Firm Decisions and Variation Across Universities in Access to High-Wage Jobs: Evidence from Employer Recruiting*

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Abstract

I analyze how firm locations affect variation across universities in labor market opportunities. I collect office locations and campus recruiting strategies for over 70 banking and consulting firms, from 2000 to 2013. I show firm location decisions create barriers to accessing high-wage employers, for students at distant universities. After firms open an office, students at nearby universities are nearly four times more likely to have on-campus access to the firm. Access increases for universities across a wide range of selectivity. Evidence suggests firms value relatively small improvements in proximity over other factors, including university selectivity. Using the *mobility report cards,* I show suggestive evidence that access to an additional firm recruiting on campus, after it opens a nearby office, raises the likelihood of top 1% earnings by .3 percentage points (4%) at ages 23-34. Additional data from universities and LinkedIn suggest initial effects on access translate into effects on hires, including relative to the counterfactual.

Intergenerational income mobility varies greatly across the United States (Chetty et al. 2014), as well as across universities (Chetty et al. forthcoming). Understanding why post-university upward income mobility is higher for some universities than for others remains an important and underexplored area for research and policy.

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This paper argues that employers often begin recruiting from a particular university because of firm-level geographic changes, exogenous to the university. These employer decisions cause variation across universities in access to firms, returns to college, and graduates' income mobility. Consider the following case studies, in which arguably exogenous firm-level geographic changes affect the jobs students obtain upon college graduation. Huron Consulting Group opened a Detroit office in 2007. In the five years before opening the Detroit office, Huron hired no more than two students per year from the University of Michigan Ross School of Business.[1] In the first two years after opening the Detroit office, Huron hired six to seven of the school's undergraduate students.

In 2004 JP Morgan Chase dramatically increased its presence in Columbus, Ohio after merging with Bank One, which had large Columbus operations. In the four years preceding the merger, JP Morgan Chase hired four to six undergraduate students from Ohio State University's Fisher College of Business. In the year of the merger they hired nine students. By three years after the merger they hired 16 students, by five years after the merger they hired 37, and by 2017 they hired 68 students, far greater than the previous Bank One hires. These increases are also large after comparing to changes for four other large finance and accounting firms at Ohio State over this period.

Because of arguably exogenous changes in office locations, students at University of Michigan and Ohio State were more likely to obtain high-wage jobs at two prestigious companies. If these universities' supply of talented labor exceeded high-wage firms' demand for their students, changes at these two firms would affect the universities' post-graduation salaries. If these firms hired students from lower parental income quintiles, these changes may also affect rates of upward income mobility.

Increasing hires from these universities may reflect a change in recruiting strategies after increasing local presence. Many employers, including finance and consulting firms, hire entry-level college-educated workers by targeting particular universities, and conducting recruiting events and interviews on campus.[2] In a recent survey of 275 firms across many industries, over 75% conducted on-campus interviews, and nearly 60% of full-time entry-level college hires were initially interviewed on campus (National Association of Colleges and Employers 2014). Students at nontargeted campuses may generally apply, but they have less access than students at targeted campuses.

This paper studies whether firm-level geographic changes, arguably exogenous to the

[1] Its previously closest office had been in Chicago. See end of paper for comparison to control firms.

[2] Based on interviews with career services personnel and employees at consulting firms (former and current), Weinstein (2018) describes the campus recruiting labor market for undergraduates, specifically for finance and consulting firms. Rivera (2011, 2012) studies hiring processes of professional services firms based on interviews and observation of a hiring committee.

universities, affect campus recruiting decisions for high-wage finance and consulting jobs. I focus on how and why campus recruiting decisions change when firms add or close offices. I then present evidence showing how these recruiting decisions and increased local presence affect hires, earnings and upward income mobility of a university's graduates using the *mobility report card* data (Chetty et al. forthcoming), as well as data from university annual reports and LinkedIn.

Using the *Internet Archive: Wayback Machine*, I assemble a new dataset of office locations and campus recruiting strategies for over 70 of the most prestigious finance and consulting firms at over 360 universities from 2000-2013.

The recruiting decisions of prestigious finance and consulting firms are particularly important for understanding upward income mobility. Based on the 2017 ACS, 23-25 year old college graduates working full-time as management analysts in consulting firms earned median wages of \$70,000 and those working as financial analysts in banking or securities firms earned median wages of \$77,000, relative to \$48,000 for the same sample working in other occupations classified as financial or business operations specialists, for firms excluding banking, securities, or consulting firms (Ruggles et al. 2019).[3] By 30-34 years old, these salary differentials are even greater with the median management analyst earning \$100,000 and the median financial analyst earning \$138,000, relative to \$63,000 for the median college graduate working in other occupations classified as financial or business operations specialists, for firms excluding banking, securities, or consulting firms. Some of these differences are clearly driven by selection, but evidence also suggests firms play an important role in wage inequality (see Card et al. 2018 for a review).

I argue that I identify the impact on student opportunities of employers' geographic location decisions, exogenous to local university characteristics. Specifically, I estimate event-study regressions, identifying changes in recruiting within a firm-university pair when the distance between that pair changes. Including firm-university, firm-year, and university-year fixed effects effectively yields a triple-difference estimate. I compare the likelihood that university j attracts firm k before and after firm k opens a nearby office, to the change in other firms' recruiting at university j, where there is no change in firm-university distance. I also compare this change to changes in firm k's recruiting at universities where there is no change in firm-university distance. Key to the identification is that timed with the move, the

[3]Sample includes employed workers, working for wages, usually working at least 40 hours per week, and at least 48 weeks per year. There were 117 individuals working as management analysts in consulting firms (management, scientific and technical consulting services) among this ACS sample. There were 68 individuals working as financial analysts in banking or securities firms (banking and related activities, or securities, commodities, funds, trusts, and other financial investments) among this sample. I use the person weights to construct median wages.

university does not become more attractive to the moving firm relative to other similar firms, for reasons other than the reduction in distance. This decrease in distance may arise because the firm found the university's market desirable for a new office, for reasons unrelated to the university, such as a location-specific increase in demand.

Firms' office location decisions affect which universities have increased access to their job opportunities. I find firms are nearly four times as likely (3.3 percentage points) to recruit at a university five or more years after they open a nearby office, relative to the year before the move. Firms do not increase recruiting prior to the move, suggesting new office locations are not driven by recruiting relationships.

New office locations increase access for students at universities across a wide range of selectivity, including Ivy League and other Barron's Tier 1 selectivity universities, through Barron's Tier 3-5 selectivity universities. My sample of universities in Barron's Tiers 2 through 5 includes close to 280 universities. It is striking that some of these prestigious firms only begin recruiting at Ivy Plus universities after opening a nearby office. Given the prestigious nature of these firms, it is also striking that these firms begin recruiting at universities outside the most selective group after opening a nearby office. Both suggest firms' willingness to tradeoff selectivity for proximity.

There are many fewer instances of firms closing offices. The results suggest firms do not immediately drop target campuses after closing local offices. However, by four years after the closing recruiting at local universities falls by nearly three percentage points (45%), though this is only significant in one of the post years and only marginally.

When firms change office locations, they change where they recruit workers. This suggests the cost of distance is large relative to other factors that could determine recruiting choices, such as university selectivity or recruiting experience at a university. The costs of recruiting further away could reflect perceived applicant location preferences, and higher offer rejection rates at universities farther from the firm's office. Applicant preferences for the local market may also give the firm monopsony power and allow it to pay lower wages. Costs of recruiting further away also include the monetary and opportunity costs from recruiting for this new location at their further-away existing target campuses. Opportunity costs of time may be quite substantial in finance and consulting, as recruiting is often conducted by bankers and consultants with high external billing rates.

These advantages of local recruiting are consistent with limited and declining interstate migration for college-educated individuals and those who are 18-24 years old (Molloy, Smith, and Wozniak (2011)). Further, only 16% of students attend a college or university in a state that did not share a border with their home state (Mattern and Wyatt 2009), consistent with the location preferences explanation.[4]

I present several additional results suggesting large costs to firms of recruiting at farther universities, relative to other factors that could determine recruiting choices. Evidence suggests the effects decay with distance for Ivy Plus and elite universities, even among universities within 200 miles of the new office, and there is some evidence that firms sacrifice selectivity to recruit at closer universities. The decay with distance even among relatively nearby universities suggests at least some role for employer costs that increase in distance.

Finally, I present suggestive analysis of office locations' effect on hires and longer-run outcomes relative to the counterfactual, using several additional data sources. I collect data on hires by firm from annual reports of two business schools. Using the case studies above, I show hires are sensitive to proximity. I show suggestive evidence that these are improvements relative to the counterfactual, using additional data collected from LinkedIn.

I also present suggestive evidence on the longer-run earnings effect of initial access to high-wage jobs. I match earnings outcomes by university and birth cohort to campus recruiting and office location changes when the cohort was in college, using the *mobility report cards* (Chetty et al. forthcoming), based on federal tax records. I focus on the probability that the university's students reach the top 1% of earnings for their birth cohort by 2014, when the individuals are 23 to 34 years old. If a 21 year-old-student has access to one additional firm recruiting at their university after it opens a nearby office, this raises the likelihood of top 1% earnings in 2014 by approximately .3 percentage points or 4%, relative to those who turned 22 before the firm began recruiting. The increase is relative to the increase at other universities in the same census region and selectivity tier.

Effects of additional recruiting firms are larger at non-Ivy Plus universities. Estimates imply that recruiting activity explains 24% of the gap in income success rates between Ivy Plus and non Ivy Plus universities. Evidence suggests students outside the top parental income quintile benefit, though not differentially, and this evidence is less conclusive.

This paper contributes to our understanding of who has access to high-wage jobs, and suggests employer location decisions affect the value and distribution of the returns to college. The results imply that one dimension of a university's value added is the set of firms to which the university enables access.[5] This suggests prospective students might consider this dimension when choosing a university, in addition to educational value added.[6]

[4]The sample is from a match between College Board data and the National Student Clearinghouse, including over 900,000 students who graduated high school in 1999, enrolled at a four-year university, and had taken the SAT, PSAT/NMSQT, or AP exam.

[5]The relationship between universities and local employers is consistent with recent findings that college major and enrollment are affected by local economic conditions (Cascio and Narayan 2015, Charles, Hurst, and Notowidigdo 2018, Weinstein forthcoming).

[6]Macleod and Urquiola (forthcoming) show theoretically that in a competitive labor market households

The paper complements several others studying universities and access to the business elite. Zimmerman (forthcoming) finds admission into elite university programs in Chile affects attainment of top jobs and incomes. MacLeod et al. (2017) find that earnings growth is positively correlated with university reputation. Weinstein (2018) shows recruiting for high-wage jobs and wages depend on a university's regional rank, conditional on absolute university quality, using cross-sectional data. Qualitative evidence suggests elite professional firms rely on elite university networks, with a potential role for geography (Gellman 2016; Rivera 2011). I show firm-university linkages are not static, and confirm an important role for proximity that in many cases leads to recruiting at less selective universities.

The paper also contributes to literature studying the impact of distance to economic opportunity on labor market outcomes.[7] I build on these literatures by focusing on the geography of an individual's university. Further, the place-based policies literature has not focused on higher-wage services industries, despite being an important target for local jurisdictions (Story, Fehr, and Watkins 2012). I show distance affects access to these firms.

Finally, I study employers' choice of where to recruit workers, an area of limited research especially relative to individual job search. Recent work highlights the role of geography in an individual's job search (Manning and Petrongolo (2017) and Marinescu and Rathelot (2018)), though evidence is lacking in how employers value proximity in search, and how this affects worker outcomes.

I show that firms in this market pursue local recruiting strategies, even in a market with highly-educated workers applying to high-wage jobs, many of which require travel. This is a market in which we may least expect search frictions. However, I show suggestive evidence consistent with considerable employer search costs that increase with distance. The results are also consistent with evidence showing limited geographic mobility even for college-educated individuals. The frictions giving rise to local recruiting strategies in this setting may give these firms monopsony power, even in a high wage, highly educated market.[8] Local recruiting strategies may further increase monopsony power, beyond local employment concentration shown in recent work.[9]

Similar to this paper, Oyer and Schaefer (2016) show geographic proximity to a given law

will often prefer high-absolute achievement schools over those with high value added.

[7] This includes the impact of individuals moving to lower-poverty areas (Chetty, Hendren, and Katz 2015, Kling, Liebman, and Katz 2007, Oreopolous 2003), the impact of policies attracting firms to local jurisdictions (see Neumark and Simpson 2015 for a review), and the spatial mismatch literature (Kain 1968, see Gobillon, Selod, and Zenou 2007 for a review).

[8] Lamadon, Mogstad, and Setzler (2019) show horizontal differentiation between employers, which may include location differences, leads to significant rents and imperfect competition in the US labor market.

[9] See Azar, Marinescu, and Steinbaum (forthcoming); Azar, Marinescu, Steinbaum, and Taska (2020); Benmelech, Bergman, and Kim (forthcoming); and Rinz (forthcoming).

school implies a law firm is more likely to hire its graduates. I build on their paper by using panel rather than cross-sectional data, allowing me to study how firm choices may affect the value and distribution of the returns to college.

1 Data

I collect a rich and unique panel dataset of locations and recruiting strategies using *The Internet Archive: Wayback Machine*. I focus on the 50 most prestigious consulting and banking firms as ranked by Vault in 2007 and 2008, respectively.[10]

For these ranked firms, I identified whether the firm's website in the Fall of each year contained information on their office locations and undergraduate target campuses, for 2000 through 2013.[11] I denote whether a firm (f) recruits at a given university (j) in a given year (t) ($Recruit_{fjt}$), for each university in Princeton Review's *The Best 376 Colleges* (2012).[12] Figure 1 gives an example of this process for the consulting firm Bain & Company in 2001.

For each firm/university pair, in each year I identify the office location with the shortest distance to the university using coordinates (described in the appendix). I define a move in as an instance in which a firm moves at least 50 miles closer to a university, and is within 200 miles, and a move out as a firm moving at least 50 miles farther from a university when it had been within 200 miles. I consider other definitions for robustness, including firms moving to or from the university's commuting zone (CZ). I merge with IPEDS to obtain university-level characteristics.

I drop singletons: firm/university pairs only in the sample for one year, and firm/year pairs with only one observation in the sample (after dropping firm/university singletons).

The *Wayback Machine* and Employer Recruiting Strategies The *Wayback Machine,* made available by the non-profit organization Internet Archive, started in 1996 as an archive of the internet constructed by automated systems routinely crawling the web. It contains 279 billion web pages (Internet Archive 2017). While the archive contains recruiting and location pages for many firms in the sample, not all pages are archived. Either the

[10] The 2007 ranking of banking firms contained very few firms.

[11] This data collection was manual for consulting firm locations and recruiting strategies of banking and consulting firms. For banking firm locations, a program was written to download webpages containing office locations for firms in the sample (using the API of the Wayback Machine to identify which webpages to download). Locations were then read from the downloaded pages.

[12] I exclude universities without IPEDS data and test scores, foreign universities, and service academies. I create one observation for the five Claremont Colleges. By focusing on universities in the Princeton Review, I do not capture universities outside this list where the firm may recruit. Recruiting at these less selective universities may be more likely driven by geographic proximity, suggesting the effects I report are underestimates.

automated web crawlers were not aware of the site's existence at the time of the crawl, or the site blocked access to automated web crawlers. I code *Recruit* as missing for all of these nonarchived pages. However, the page may not have been archived because it did not exist. This may suggest there was no active recruiting that year.[13]

In addition to some firms having unarchived or broken location pages, there is some within-firm variation across years in the types of reported locations. This could lead to mistakenly coding office openings and closings. I code location as missing for firm/years in which reporting of locations appears inconsistent with other years.[14]

This yields a dataset containing 42 consulting firms and 31 banking firms. For the main results, I keep observations associated with a given firm/year only if the firm recruited on at least one campus that year. I do this for several reasons. First, it ensures that an increase in recruiting after a move is not simply because the firm began listing recruiting targets timed with the move. Second, it restricts the sample to a more relevant set of observations where we might expect to see a change after a move. For robustness, I show results without this restriction. Implementing this restriction, and requiring data in the year before the move, yields a dataset containing 32 consulting firms and 21 banking firms.

New office locations may result from mergers or acquisitions. New target campuses timed with these new locations may be the original targets of the acquired or merging firm. However, the decision to keep the target campuses of the acquired/merging firm continues to suggest the importance of distance between the sample firm and the university. Firms could decide to abandon the recruiting strategies of the firm they acquired or merged with, and instead apply their own. If the acquiring firm is more prestigious this may also reflect an important change in students' job opportunities.

Summary Statistics Of the 53 firms, Table 1 shows 24 open offices in 46 cities, affecting 314 of 362 universities, and 789 firm/university pairs (4.8%). There are 75 total office openings, after which firms are more than 50 miles closer to at least one university and within 200 miles.

Twenty firms in the sample close offices in 36 cities, affecting 283 universities, and 505 firm/university pairs (3%). There are 41 total office closings, after which firms are more than 50 miles farther from at least one university, and had been within 200 miles. Cities experiencing move ins and move outs are distributed across the country, as are universities in the sample (Figure 2). Move ins and move outs are distributed across years in the sample

[13] Appendix Figure A1 shows the number of observations for which the recruiting page was not archived, for reasons other than being blocked or nonworking links, increases dramatically from 2007-2011. This suggests nonarchived pages may be related to the recession, and signify an absence of active recruiting.

[14] Details are available in the data files.

(Appendix Figure A2).

2 Empirical Strategy

To measure the effect of move ins, I estimate the following event-study regression:

$$Recruit_{fjt} = \alpha_0 + \alpha_{fj} + \delta_{ft} + \kappa_{jt} + \sum_{k=k_{min}}^{k_{max}} \beta_k I(t = t^* + k) MoveIn_{fj} + \epsilon_{fjt} \tag{1}$$

I estimate the analogous regression for move outs. The variable $Recruit_{fjt}$ is an indicator equal to one if firm f recruits at university j in year t. I include firm/university fixed effects (α_{fj}), ensuring I identify changes in recruiting within a firm/university pair when the closest office changes for that pair.

In the principal specifications, the variable $MoveIn_{fj}$ is an indicator for whether firm f moved more than 50 miles closer to university j in t^*, and is within 200 miles. The variable is zero for firm/university pairs that never experience a move in. I restrict to the first move in, and the years associated with it. For example, I exclude years after any subsequent move and before any previous move out. I estimate an analogous regression for move outs, where $MoveOut_{fj}$ is an indicator for firm f moving more than 50 miles farther from university j in t^*, and it had previously been within 200 miles of university j.

Each coefficient β_k measures the change in the probability that firm f recruits at university j in the k^{th} year relative to the move, relative to the year preceding the move. I censor $|t - t^*|$ at five. The estimates of β when $k < 0$ identify whether there were increases in the probability of recruiting in the years preceding the move in. If these coefficients are small and insignificant from zero, this suggests recruiting decisions are not driving location decisions. However, for firms closing offices, we may expect changes in recruiting before the closing if the firm's office is underperforming. To be in the regression sample, I require that the firm-university pair has nonmissing data in $t^* - 1$. For robustness, I show results from samples balanced on calendar year.

The central identification assumption is that a firm's recruiting at a university would not have changed for other reasons timed with the move. One potential concern is that opening an office close to a given university may be timed with an increase in recruiting for the specific firm at many universities. Alternatively, the office opening may be timed with an overall increase in recruiting firms at this specific university. To control for these possibilities, I include firm-year and university-year fixed effects in (1). This effectively yields a triple difference estimate. The effect of an office closing on recruiting is any difference beyond average changes in recruiting for the firm and university that year, for example due to a recession or university changes making it more attractive to firms. I estimate the specification with two-way clustered standard errors, at the firm and university level.[15]

I also show regression results from a similar specification that groups years into short-run (zero through four years after the move) and long-run (five or more years after the move), as well as those five or more years before the move ($PreLR$), and those one to four years before the move:

$$Recruit_{fjt} = \alpha_0 + \alpha_{fj} + \delta_{ft} + \kappa_{jt} + \beta_1 PostSR_{fjt} + \beta_2 PostLR_{fjt} + \beta_3 PreLR + \epsilon_{fjt} \quad (2)$$

3 Results

In the year a firm opens an office close to a university, students at the university are 1.4 percentage points more likely to have on-campus access to the firm, relative to the year before, and relative to the average change that year in all firms' recruiting at that university (Figure 3a). This is also relative to the average change that year in the firm's recruiting at all universities. Students are 2.7 percentage points more likely to have access by two years after the move, and 3.3 percentage points by five or more years after the move. The mean of the dependent variable is .012 in the year preceding the move, among firm/university pairs experiencing move ins. This suggests that when a firm opens an office close to a university, the university's students are nearly four times as likely to have on-campus access to the firm relative to before the move. While every firm-university pair experiencing a move has data in $t^* - 1$, additional specifications show dynamic effects between t^* and $t^* + 5^+$.[16] This is difficult to ascertain in the main specification due to imperfect balance.

There is no evidence that firms are increasingly likely to recruit at the university preceding the move. Again, all firm-university pairs experiencing moves have data in $t^* - 1$, and the average likelihood of recruiting in each pre-move year is not significantly different relative to $t^* - 1$. The robustness section discusses results balanced on calendar year.

When firms close offices, they do not immediately drop target campuses in the local

[15] These are very similar to standard errors clustered at other levels (at the firm level, at the university level, and nonclustered but robust to heteroskedasticity). There are no differences in the significance thresholds implied by two-way clustered standard errors relative to the other standard errors, except for the coefficient on t^*. For this coefficient, the two-way clustered standard errors (as well as those at the firm level, and university level) imply significance at the 5% level, while nonclustered but robust standard errors are slightly larger and imply significance at the 10% level.

[16] I estimate the regression including only firm/university pairs with data at least in $t^* - 1$, t^*, and $t \geq t^* + 5$, as well as the pairs that never experience move ins. The coefficient on t^* is .009 (not statistically significant) and the coefficient on $t \geq t^* + 5$ is .029 ($p \leq .01$), and the difference between these is significant at the 10% level.

market (Figure 3b).[17] However, four years after closing their nearest office to the university, firms are 2.7 percentage points less likely to recruit ($p \leq .1$) relative to before the move and relative to the average change that year in all firms' recruiting at that university. This is also relative to the average change that year in the firm's recruiting at all universities. The probability remains similar (2.1 percentage points) five or more years after closing their nearest office, though less precisely estimated. The mean of the dependent variable is .06 in pre-move years among firm/university pairs that experience move outs and have data in $t \geq t^* + 3$. This suggests that when a firm leaves the market, the probability of recruiting at universities in the market falls 45%. Additional specifications suggest important dynamics in the effect between t^* and $t^* + 5^+$, though the effects are not statistically different ($p = .12$).[18]

Results are similar when estimating (2) (Table 2). The results show geography is an important barrier to accessing high-wage jobs for students at distant universities.

Differential Effects by University Selectivity

Changes in office locations affect access to high-wage jobs for students at universities across a wide range of selectivity, among those in my sample of The Princeton Review's *Best 376 Colleges*. Table 2 shows results allowing for heterogeneity by university tier. I estimate (2), and include interactions between indicators for tiers of university selectivity and the pre- and post-indicators, using university tiers based on Chetty et al. (forthcoming): Ivy Plus (omitted group), including the Ivy League plus Stanford, MIT, University of Chicago, and Duke; elite (61 Barron's Tier I universities excluding Ivy Plus); highly selective (92 Barron's Tier 2 universities); selective (185 universities in Barron's Tiers 3 - 5); and nonselective or not enough information (12 universities in Barron's Tier 9 and outside Barron's index). Separately, I also interact event-study coefficients with university tier (Figure 4), though this leads to a decrease in power.[19]

There are large, statistically significant effects for all tiers in either the short-run, long-run, or both (Table 2). Magnitudes suggest that for the four years after a firm opens an office close to an Ivy Plus university, students are on average 11 percentage points more likely to have access relative to before the move ($p \leq .1$), controlling for firm-specific and university-specific changes that year. Given the mean of the dependent variable in $t^* - 1$ is .057, this

[17] Magnitudes suggest an increase in recruiting immediately after an office closing. While a firm is near a university it may be developing networks, which may continue to result in recruiting immediately following the office closing.

[18] I estimate the regression including only firm/university pairs with data at least in $t^* - 1$, t^*, and $t \geq t^* + 5^+$, as well as pairs that never experience move outs. The coefficient on t^* is -.01 ($p = .1$) and the coefficient on $t \geq t^* + 5$ is -.033 ($p \leq .05$).

[19] For example, given the small number of Ivy Plus universities, sample sizes for the event-study coefficients range from 14 to 64 in the post-period.

implies students are nearly three times more likely to have access. For the four years after a firm opens an office near one of the 61 elite, non-Ivy Plus, universities in Barron's Tier 1, its students are over 3.4 percentage points more likely to have on-campus access to the firm ($p \leq .05$). The mean of the dependent variable in $t^* - 1$ is .023, implying students at these universities are 2.5 times more likely to have access.

For the four years after a firm opens an office near a highly selective university, students are 1.9 percentage points more likely to have access, relative to before ($p \leq .05$). This implies students are 2.7 times more likely to have access based on the pre-move likelihood. At selective universities, students are on average 1.2 percentage points more likely to have access for the four years after the firm has the new office ($p = .118$). This implies students are five times more likely to have access relative to before the move.[20] Long-run effects are larger in magnitude for all tiers, with the exception of selective universities.

It is more difficult to assess the effect of office closings by tier, given they are less prevalent in the sample. While the results suggest longer-run effects, especially for elite and highly selective universities, they are not statistically significant. Appendix Figure A12 shows the event-study coefficients; however sample sizes for each coefficient are very small.

Appendix Figure A3(a) shows the universities that began attracting a recruiting firm after it opened a nearby office, but never attracted this firm preceding the move. These universities are distributed across the country, with many in areas other than the largest U.S. cities. Comparing this to Figure 2, there are still many universities in areas receiving new firms that do not attract these firms after the move. This is consistent with the low baseline probability of attracting one of these firms. Appendix Figure A3(b) shows universities that had attracted firms at least once before an office closing, but did not attract them at least once after the firm closed the local office.

In sum, these results show that firm location decisions act as a barrier to high-wage jobs for students at Ivy Plus universities, and also for those at universities well outside this most selective group.[21] This is striking for two reasons. First, it is striking that many of these prestigious firms only begin recruiting at Ivy Plus universities after opening a nearby office,

[20] The decreasing magnitude of the event-study coefficients over time (Figure 4d) should not be interpreted as a dynamic trend, due to imperfect balance. A number of pairs experiencing recruiting in t^* are not in the sample in any other post-move years. For some pairs, the firm does stop listing the target campus after listing it as a target in t^*.

[21] Interestingly, the effects of office openings are significantly larger for firms outside the highest-ten-ranked banking and consulting firms in the sample, and larger, though not statistically, for consulting firms than banking firms (Appendix Table A3). For both the higher-ranked firms and the banking firms we still see large, though not precisely estimated coefficients. There are no statistically significant differences in the short-run effect when the firm opens or closes an office in a larger city (Appendix Table A6). The long-run effect is significantly larger if the firm opened an office in a larger city, but this appears due to composition. See appendix for details.

and were not already actively recruiting on these campuses. Second, given the prestigious nature of these firms, it is striking that they do begin recruiting at universities outside the most selective group after moving nearby. Both findings suggest these firms highly value distance relative to selectivity.

4 What Distance is too Costly, and What do Firms Sacrifice for Proximity?

I further explore how and why high-wage firms value proximity in recruiting.

When firms open a new office they may face a tradeoff when choosing where to recruit new workers. They could start recruiting at universities in the local market or they could recruit at farther universities with some positive attribute, for example they may be more selective or they may be one of the firm's existing target campuses. Recruiting experience at a university may improve the quality of the firm's applicant screening or improve applicants' information about the firm, yielding more applicants and better hires. This may be amplified by alumni networks at existing target universities, which may affect screening, hiring yield, or complementarities in production.[22] These existing targets may also be more selective, or with some other positive attribute, relative to universities in the firm's new market.

Whether firms are willing to tradeoff these positive university attributes in order to recruit nearby will depend on various parameters. These include the cost of recruiting at a university without the positive attribute, for example costlier screening or less productive hires. Another parameter determining this tradeoff is the cost of distance to the target campus. This could include travel costs from recruiting further away, like opportunity costs of time or cost of accommodations. Alternatively, the cost of distance may be explained by applicant migration frictions that reduce the likelihood of offer acceptance, as firm-university distance increases. If nongeographic attributes like selectivity significantly reduce the cost or raise the benefit of recruiting, target campuses should be less sensitive to distance. If geographic costs are important, firms will sacrifice other attributes in order to recruit nearby.

Understanding how firms value geographic distance relative to other attributes helps clarify who benefits from nearby economic activity, and how close they must be to benefit. Second, it helps clarify whether firms would experience productivity gains if there were decreases in geographic search costs, applicant migration frictions, or screening costs.

I analyze the extent to which firms value geographic distance relative to other attributes with several reduced-form tests. A structural model would allow for estimating these param-

[22]See Ioannides and Loury 2004 and Topa 2011 for a review of the literature on networks in labor markets. Burks et al. 2015 show benefits of hiring through referrals.

eters more directly and performing counterfactuals. However, the reduced-form tests provide important evidence on what firms value in recruiting.

Decay with Firm-University Distance for Universities within 200 Miles First, I test the extent to which firms value proximity by testing the effect's decay with post-move distance, by university selectivity. I estimate:

$$\begin{aligned} Recruit_{fjt} &= \alpha_0 + \alpha_{fj} + \delta_{ft} + \kappa_{jt} \\ &+ \beta_r Post\_r_{fjt} + \gamma_{r,s} Post\_r_{fjt} * Tier\_s_j \\ &+ \rho_r PreLR\_r_{fjt} + \sigma_{r,s} PreLR\_r_{fjt} * Tier\_s_j + \epsilon_{fjt} \end{aligned} \tag{3}$$

The variable $Post\_r$ is an indicator for whether firm f moved at least 50 miles closer and is within radius r of university j, where radius $r \in \{[0, 10), [10, 50), [50, 200]\}$, and tier s includes Ivy Plus, elite, highly selective, selective, and non selective/insufficient information.[23] The variable $PreLR\_r_{fjt}$ is an indicator for $t \leq t^* - 5$, where t^* is the year f moves at least 50 miles closer and is within radius r of university j. Estimated effects are relative to the four years preceding the move.

When firms open offices within 10 miles of an Ivy-Plus university, they may be in different types of locations than those within 10 miles of other universities. I include firm-urban area of closest office-year fixed effects to compare the effect of a move for universities of different tiers, conditional on the new office location.[24] As before, I include university-year fixed effects. I focus these analyses on office openings given the small sample size for closings.

Within a tier of university selectivity, the effect's decay with distance, among universities experiencing a move in, will depend on the additional cost relative to the aditional benefit of recruiting at slightly farther universities. The largest additional cost of recruiting 50 miles away from the new office, rather than five miles, is arguably the opportunity cost of time. Given firms often visit a campus many times throughout the semester, this difference in distance may amount to several hours each semester. However, recruiting at a university 50 miles away, in addition to recruiting five miles away, allows the firm to increase its applicant pool.[25] If there are fewer highly selective universities within 10 miles of the new office, the

[23]There is one nonselective college in the sample, and 11 with insufficient information. I do not show effects for these universities, but only one observation from this tier in the main sample has $Recruit = 1$.

[24]I identify each office location's urban area using the US Census Urban Area Relationship File (see appendix). Five cities in the main regression sample, and roughly 50 observations, do not match to an urban area using this procedure. However, none are associated with office location changes.

[25]If firms recruited at Ivy Plus and elite universities near the new office before its opening, this would reduce the benefit of increasing local recruiting, implying a smaller decay with distance. However, recruiting

benefit of also recruiting 10-50 miles away may be quite large.

There are two notable findings in Figures 5a and 5b, which plot $\hat{\beta}_r + \hat{\gamma}_{r,s}$ from regression (3). First, conditional on selectivity, evidence suggests firms highly value proximity, even within this radius of 200 miles. For Ivy Plus universities within 10 miles of the new office the likelihood of attracting a firm increases 18 percentage points ($p \leq .1$). While much larger in magnitude than the effects for farther universities within 200 miles, the differences are not statistically significant. For elite universities within 10 miles the increase is 13 percentage points ($p \leq .05$), and we can reject the joint test this equals the effect within 10 to 50 miles, and within 50 to 200 miles, at the 10% level.[26] Roughly 30% of the post-move observations for Ivy Plus and elite universities are within 10 to 50 miles of the new office (Figure 5c). As one example, when firms open offices in Boston, Brown University is 10 to 50 miles away. I further explore these effects with illustrative examples below.

The effect's decay with relatively small changes in distance is unlikely explained by firms' concern that applicants will reject offers due to migration frictions. Instead, it suggests employer search costs increase rapidly with distance.[27]

The decay with distance is nonexistent for highly selective and selective universities. Relative to recruiting at Ivy Plus and elite universities, recruiting at highly selective and selective universities after office openings is largely driven by new offices near fewer universities (Figure 5d). Consistent with the intuition above, this raises the benefit of recruiting further from their new office locations.

There is some evidence this value of proximity leads firms to give up selectivity for proximity. Based on the magnitudes, Ivy Plus universities 10 to 50 miles away do not experience a greater increase in recruiting than elite universities within 10 miles, though the confidence intervals do not allow us to rule out the opposite. This is noteworthy because there is a large selectivity difference between Ivy Plus and elite universities; the average proportion scoring above 700 on the SAT math is roughly double at Ivy Plus universities.

Second, while firms value proximity, it is strikingly clear that conditional on proximity

at these universities before the move was quite uncommon (5.7% and 2.3% respectively, Figure 4).

[26]The difference relative to those 10-50 miles away approaches conventional significance levels ($p = .11$). Pooling Ivy Plus and elite universities, and pooling those outside these tiers, in regression (3), the differential effect for Ivy Plus and elite universities within 10 miles, relative to within 10 to 50 miles, is significant with $p \leq .1$. Appendix Table A9 shows decay using a polynomial in distance and a continuous selectivity measure.

[27]Appendix Figure A6 shows similar results using driving distances from Google Maps for a subset of pairs, rather than coordinate-based distance. The median difference in these distances is 6.2 miles (absolute value). The decay between 0-10 and 10-50 miles for elite universities is less prominent, and slightly so for Ivy Plus universities. Magnitudes of the effect are much larger within 10 miles than farther away, though they are not statistically different. Some firm/elite university pairs are within 10 miles using coordinates, but just over ten miles using driving distance. Comparing elite universities within 20 miles using driving distance to those within 20 to 50 miles yields a differential effect of 10 percentage points, more similar to the effects in Figure 5. However, the differential effect is not significant ($p = .16$). See appendix for details.

they highly value selectivity. Among universities within 10 miles of the new office, Ivy Plus universities experience the largest increase in the likelihood of attracting a firm, followed by elite universities, and there is no significant effect for highly selective and selective universities. Positive effects for highly selective and selective universities more than 10 miles away, when offices yielding this recruiting are near fewer universities, suggests these universities are disadvantaged by proximity to more selective universities.[28]

For further intuition, I estimate (3) including only firm/university pairs that experience a nearby office opening in Boston, MA. I also include all pairs that do not experience move ins during the sample. I implement the same exercise for Houston, TX. These results also show firms value proximity conditional on selectivity and they value selectivity conditional on proximity (Appendix Figure A7). The plots also highlight two explanations for the smaller decay with distance for highly selective and selective universities. In Boston, there are many Ivy Plus and elite universities, implying smaller benefits of recruiting at less selective universities within 10 miles, reducing the decay with distance. In Houston, there are very few universities within 10 miles, raising the benefit of recruiting further away.

Do Firms Adjust Recruiting After Relatively Small Distance Changes? Second, I test firms' value for proximity by testing if they adjust recruiting even when they had been relatively close before the opening. I estimate (3) and include $Post\_r * PreFar$ interactions. The variable $PreFar$ indicates if the pre-move distance was at least 250 miles, for which car or train travel are less reasonable. Even for pairs relatively close before the opening ($<$ 250 miles), there is a positive effect on recruiting after the closer office opens, statistically significant for Ivy Plus and elite universities (Figure 5e). Pre-move distances less than 250 miles include Dallas to Houston, and New York to Boston. This suggests short distances keep firms from recruiting at universities they would otherwise target. Distance appears more valuable than other university attributes.[29] Given the proximity before the move, the lack of recruiting before the move was unlikely because of concern that applicants would reject offers due to location preferences. Effects are more likely driven by employer costs that increase with distance.

Selectivity of New Target Campuses As a final test of how firms value proximity, I test whether they appear willing to recruit at less-selective universities in order to recruit closer

[28]This is consistent with Weinstein (2018) showing the importance of a university's regional rank, conditional on absolute selectivity, for attracting recruiting firms.

[29]See online appendix for the effect of distance between the new location and the closest existing target campus. Appendix Table A9 and Appendix Figure A5 show the relationship between recruiting, pre- and post-move distance, and selectivity using a polynomial in distance and a continuous selectivity measure.

to the office. Specifically, I test whether the firm's new target campuses after office openings are less selective than the firm's other target campuses. I identify the universities that never attracted the firm before the move, but attracted the firm at least once after the move. As a measure of selectivity, I use the proportion of high math test score students at the university, described in the appendix. For each of these 40 universities with selectivity data, for the year the firm starts to recruit at these universities I compare the university's selectivity to the median selectivity of the firm's existing target campuses in that year (excluding other universities added after a move). On average, the new target is 6 percentage points less selective than the median. The median difference is -11.8 percentage points. Nearly 70% of the new targets are less selective than the firm's median target campus. New targets in smaller markets appear especially less selective, potentially because there are fewer very selective universities there (Appendix Table A8, and appendix text).

5 Alternative Explanations and Robustness

Are Local Workers More Productive?

Recruiting from local universities may instead reflect that local students are more productive, rather than reflecting search or migration frictions. This may also explain substitution away from more selective universities. Local students may have more knowledge of the local economy or local business culture. This also raises the possibility that changes in recruiting cannot be attributed to the office opening, but would have occurred without the opening given an increase in location-specific demand.

I test this hypothesis using differences in typical travel across consulting firms. For some firms, entry-level consultants are away from their home office Monday through Thursday every week, suggesting local knowledge may be less important. Additionally, some firms implement global staffing, in which a consultant whose home office is Boise, Idaho is equally likely to work on a case in South Dakota, Boston, or London relative to a consultant based in one of those offices. If firms requiring extensive travel still recruit at local universities after office openings, the importance of the student's local knowledge is unlikely the explanation. Effects are likely due to the office opening, rather than solely the location-specific demand increase.

I collect information on travel for each consulting firm in the sample, based on the careers section of the firm's website, the description of the company on Vault.com, and occasionally the Wayback Machine for no-longer-existing firms. I denote a firm as requiring extensive travel if they employ a global staffing policy, or employees generally travel frequently.

Of the 32 consulting firms in the sample, 12 are coded as requiring extensive travel (Appendix Table A4). I estimate the principal regressions limiting the sample only to firms with extensive travel. Even among these firms, when they open and close offices they adjust recruiting at local universities, suggesting students' local knowledge is not the only explanation (Appendix Table A3).[30]

The absence of pretrends in Figure 3 is further evidence against the importance of location-specific demand increases on their own, irrespective of whether there is a new office. If location-specific demand increases precede office openings, and are solely responsible for changes in recruiting, pretrends would be evident.

Additional analysis, described in the appendix, suggests the relationship between proximity and recruiting is not explained by the alma mater of initial employees at the new office.

Robustness

I alternatively define move ins as instances in which firms move within the same commuting zone (CZ) as the university, and move outs as instances when firms leave the university's commuting zone. Using this definition, there are many fewer universities that are classified as having experienced a move as this is now limited to universities in the same CZ.[31] The post-move in and pre-move out firm-university distances with the CZ definition are much smaller (Appendix Table A18). This yields similar results (Appendix Figure A11).

I also estimate a specification including a quadratic in distance, rather than using a discrete radius to identify moves. I include all observations for a firm/university pair and analyze any within-pair change in distance, rather than only those associated with the first move. I continue to restrict to pairs for which the firm targeted at least one university that year.

Within a firm/university pair, decreasing the distance between the firm and university has a positive effect on recruiting (Appendix Table A5). I evaluate the coefficients for decreases in distance at the 75th and 90th percentile of distance decreases (approximately 325 and 630 miles respectively), for firm/university pairs that are approximately 50 miles

[30] This section tests whether the results are driven by students' local knowledge, by testing if high-travel firms recruit locally after office openings. While comparing the effects for high- and low-travel firms is not part of that test, the differences are interesting. Low-travel firms more likely increase local recruiting five or more years following an office opening. These firms may differentially value students' local knowledge, have higher search frictions, or their applicants may have higher migration frictions given the fixed location.

[31] See appendix for identification of firm and university CZ. There are several additional cities where firms are now classified as moving in, as the firm-university distance fell less than 50 miles but the new location is now in the university's CZ (e.g. closest office changing from Princeton, NJ to New York City).

apart after the move (approximately the 25th percentile of firm/university distance among pairs experiencing the firm moving closer to the university).

If a firm moves 325 miles closer to the university, to a distance of 50 miles, the firm is approximately 1.7 percentage points more likely to recruit at the university. If a firm moves 630 miles closer to the university, to a distance of 50 miles, the firm is approximately 3.3 percentage points more likely to recruit at the university. These effects are similar to the main results.

I estimate an additional specification including firm-university-year observations even if the firm did not recruit on any campuses that year. This increases the number of office openings and closings (Appendix Table A12), as well as the number of new target campuses following office openings.[32] The magnitude of the effects for move ins are smaller, though similar in size relative to the baseline which is also smaller. Effects continue to be precisely estimated. The biggest difference is for office closings, as the long-run effects are precisely estimated in this specification (Appendix Table A13, Appendix Figures A8, A9). The long run may coincide with years in which the firm's website suggests they are not recruiting at any university, but in actuality they have simply changed how they publicize recruiting. The decline in recruiting at the university may not be fully captured by the average decline for the firm that year, as measured by the firm-year fixed effect. This was one reason I include in the main sample only firms recruiting on at least one campus in the given year.

Finally, given results are similar, I use this robustness sample to restrict to firm-university observations balanced on calendar year. This addresses the concern that different firm-university pairs are in the sample in each calendar year, affecting estimation of the firm-year and university-year fixed effects and potentially biasing the event-study coefficients. Given the frequency with which *Recruit* is missing due to unarchived or blocked pages, balancing on calendar year is very restrictive. For example, restricting to moves occurring in a four-year period, and requiring balance from $t^* - 2$ through $t^* + 1$, requires firm-university pairs to have seven consecutive years of data which greatly limits power. I estimate separate regressions limiting each to moves occurring in two-year periods, as well as year by year. Despite substantially limited power, the average of the effects across regressions is roughly similar in magnitude to those without balance on calendar year in Appendix Figure A8, and there are also no pretrends (Appendix Table A16, A17). Coefficients are precisely estimated in some years but in many years are imprecise due to lack of power. The online appendix describes these specifications in detail.

[32]New target campuses increase from 40 to 71. The additional 31 campuses are not included in the main sample as the firm is not recruiting on any campus in $t^* - 1$, and so this observation is dropped from the regression. The firm/university pair is then dropped since it does not have data in $t^* - 1$.

Do Firms Open Offices In Order to Recruit?

One threat to identification is that firms are opening offices in particular locations for the purposes of recruiting. In this case, there would be no causal effect of office location decisions on universities' access to firms. While this may be true for manufacturing and technology companies, it is unlikely for consulting firms and banks based on industry accounts. In their chapter of Management Consulting Today and Tomorrow, Greiner and Malernee (2018) note that management consulting firms typically open new office locations when clients ask to be served in new ways.

The effect of office closings on recruiting presents further evidence of a causal relationship between proximity and recruiting. It seems very unlikely that firms would close offices because they had decided to stop recruiting from a local university.

Second, the scale of hiring for consulting firms and banks is relatively small, further mitigating concerns they would open offices to recruit a small number of employees. To evaluate the scale of hiring, I use data from the 2007 annual report of the University of Michigan's Ross School of Business. I use 2007 since this may yield the highest estimate, given it is a year of expansion preceding the Great Recession. I use the University of Michigan as it is a large university attracting many of these firms. These data list hires by firm for firms hiring at least 10 students from the business school, for either fulltime or internship positions from any degree program. Eleven firms in my sample meet this condition. Among these companies, the average number of fulltime hires (MBA, Master's in Accounting, and BBA) was approximately 14 for consulting firms and 12 for banks. An additional 18 firms in my sample hired fewer than 10 students. It seems unlikely they were opening offices to recruit a relatively small number of employees.

I present several additional results and tests of whether the results might be driven by this reverse causal mechanism. First, there are positive effects on recruiting when firms open offices that are not immediately next to the university. When estimating (3) without heterogeneity by selectivity tier, the effect for universities 50 to 200 miles away is positive and statistically significant at the 1% level (Appendix Table A14).[33] If firms opened the office to recruit from these universities, it is likely the new office would have been much closer.[34]

Second, I analyze whether office openings are explained by university and local market

[33]Including firm-urban area of closest office-year fixed effects, instead of firm-year fixed effects yields a similar coefficient, significant at the 5% level.

[34]Appendix Table A14 shows an increase in recruiting after a move, for pairs in which the firm already recruited at other universities within 50 miles of the university (about 20% of pairs experiencing moves), although this is not precisely estimated. This increase unlikely explains the office opening, as the firm was already recruiting nearby.

changes. The principal results control for university-year fixed effects, capturing any university changes which broadly make the university a more desirable place to recruit and potentially open an office. Table 2 shows that the inclusion of these fixed effects has little effect on the R-squared. This suggests that at least during this sample period, changes in universities and their local markets over time (conditional on office location changes) explain little of the variation in recruiting. I also formally test whether university characteristics are correlated with timing of office openings or closings. I estimate regression (2), with university characteristics as dependent variables, excluding university-year fixed effects, given the dependent variables are constant within university-year cells.[35]

Universities may become slightly more selective at the same time firms move in, however, the effects are quite small and imprecise and only for some dimensions of selectivity (Appendix Table A11). Controlling for these university characteristics in regression (2), without university-year fixed effects, also has little effect on the results. However, there is also little effect on the R-squared, which is quite high (.8) given the firm-university and firm-year fixed effects (Appendix Table A6).[36] Finally, I show that the urban areas attracting new offices or losing offices are not experiencing this across many firms timed with the move in the short run (Appendix Table A7). There is some evidence that five or more years after the move there is an additional other firm that has moved in. This suggests the results are not explained by dramatic changes in the urban areas experiencing these moves. I present further evidence on this below.

Together, these tests present evidence that firms are not opening offices for the purpose of recruiting.

6 Effect of Proximity on Hires: Case Studies

The main analysis shows the effect of office locations on recruiting, an important outcome on its own but also with an arguably strong relation to hires. I complement the main analysis by studying the effect of office location decisions on hires, using a more thorough regression analysis of the two case studies described in the introduction: Huron's new Detroit office and hires from the University of Michigan, and JP Morgan Chase's increased presence in Columbus, Ohio after a merger and hires from Ohio State University.[37] I also explain

[35]Given the dependent variables are university characteristics, I cluster standard errors here at the university level.

[36]The university selectivity measure is only available starting in 2004. I analyze the effect of including university characteristics on the same post-2003 sample.

[37]JP Morgan's Columbus presence also increased slightly in 2000, after merging with Chase. However, this increased dramatically after the Bank One merger, given Bank One's large Columbus presence, even after moving its headquarters from Columbus to Chicago in 1998. As further evidence of JP Morgan's

whether increased access to these high-wage firms represents an improvement relative to the counterfactual, or simply a shift in which high-wage firms are hiring from the university.

Unlike recruiting schedules, these firms generally do not post online the number of hires by university. However, some universities report data on hires by firm. I collect annual reports from University of Michigan Ross School of Business (2002-2013) and Ohio State University Fisher College of Business (2000-2017) to create a panel of fulltime undergraduate hires by firm, which is available for the top hiring firms. For several firm/year combinations I know only an interval for the number of hires, which in many cases are quite narrow (e.g., less than two hires). In cases when this occurs for the firms in my sample, I use the midpoint of the interval, but will discuss results using the minimum and maximum. Other universities also make this type of information available, but I focus on these two because of the higher response rate and the number of years the hires data are available.

I include hires by firm from the University of Michigan for any consulting firm with data before Huron opened its Detroit office in 2007. This yields eight firms other than Huron. I include hires by firm from Ohio State for JP Morgan Chase, and four other finance/accounting firms with greatest coverage in the Ohio State data. For each of these firms, I know the exact number of hires from the university for at least 15 of the 18 years. The control groups mitigate concerns that effects are explained by industry or university changes timed with the move.

I estimate a regression with firm and year fixed effects, separately for each case study:

$$Hires_{ft} = \alpha_0 + \gamma_t + \delta_f + \sum \beta_t year\_t_t * MovingFirm_f + u_{ft} \tag{4}$$

The variable $MovingFirm$ is an indicator for Huron in the University of Michigan case study, and for JP Morgan Chase in the Ohio State case study. The main identifying assumption is that the moving firm would not have increased hires at these universities relative to other firms absent the move. I do not have data on Huron and JP Morgan hiring at other universities, which would help to rule out an overall increase in recruiting across all universities, timed with the move.

Relative to the other consulting firms in 2007 (the year Huron opens a local office), Huron hires an additional five people from Michigan's business school relative to 2006 (Figure 6a). This is also true in 2008. There is not strong evidence the effect persists during the recession, and for 2011 through 2013 we know only that Huron hired fewer than roughly 10 students. Using the interval minimum or maximum also shows similar increases immediately after the opening, but clearly yields different conclusions for 2011-2013. While Huron starts recruiting

increased Columbus presence, in 2006 Columbus is listed for the first time as a JP Morgan Investment Banking headquarters.

at University of Michigan in 2004, hires increase only after opening the local office in 2007. This may suggest proximity leads to increased recruiting intensity and hires. If so, then the estimates from the first part of the paper may underestimate the impact of proximity on access. There may be a number of universities that were targets before an office opening but experienced greater recruiting intensity after the opening.

Relative to other firms, JP Morgan Chase dramatically increases its hires from Ohio State starting in 2004, the year of the Bank One merger (Figure 6b). The initial effects are large, with JP Morgan Chase hiring an additional ten students from Ohio State relative to other firms and relative to the difference in 2003. By the end of the period, these effects are even larger, with JP Morgan Chase hiring an additional 40 Ohio State students. Using the interval minimum or maximum for number of hires yields very similar results. JP Morgan Chase does not simply absorb the previous Bank One hires. In 2003-2004, the last year Bank One is in the Ohio State data (the merger was announced in January 2004 and completed in July), they hired 9 students. There is not evidence that JP Morgan Chase was differentially increasing hiring from Ohio State in years leading up to the Bank One merger. There is also no evidence that JP Morgan Chase recruited at Ohio State before the merger. The variable *Recruit* is coded as missing in post-merger years, though the recruiting pages strongly suggest JP Morgan Chase began recruiting at Ohio State at least by 2006.[38]

Does hiring by newly local firms improve outcomes?

Students at University of Michigan and Ohio State are more likely hired by Huron and JP Morgan after these firms increase their local presence. This alone does not imply a change in career outcomes for these students, as they may have otherwise been hired by high-wage finance or consulting firms. The annual reports include hires for only the top hiring firms, making it difficult to study overall changes in initial jobs.

For additional evidence, I collect data from LinkedIn profiles for 2003 and 2009 graduates of Ohio State University's Fisher College of Business ("Fisher"), graduating before and after the JP Morgan Chase merger increasing its presence in Columbus, Ohio. I exclude individuals whose first job after Ohio State was the continuation of a pre-graduation job. I confirm a large increase in Fisher graduates with JP Morgan Chase as their initial employer after graduation. In 2003, there were three individuals (roughly 1% of graduates), while in 2009 there were 13 (roughly 2.9% of graduates). This additional 1.9% working for JP Morgan

[38]Starting in 2006, Ohio State is listed on the firm's campus events website with a set of other universities. However, none of the events were archived for the listed universities, and so *Recruit* was coded as missing. In 2004 and 2005 the campus events page was either not updated or not archived, so *Recruit* was missing for all universities.

implies an additional 8.6 students. While this is an important increase it also suggests limited ability to identify changes in initial job outcomes relative to the counterfactual.

With this caveat, I explore whether being hired by JP Morgan represented increased likelihood of working in finance, or at a large firm, relative to the counterfactual. Figure 6c shows the likelihood of a first job in financial services did not increase 1.9 percentage points (corresponding to the increased hires by JP Morgan), but increased .3 percentage points. This may imply JP Morgan hired at the expense of other finance firms. Alternatively, without JP Morgan, fewer people may have initially entered financial services in 2009, especially plausible given the financial crisis. Among 22 to 23 year old employed college graduates in the ACS, the likelihood of working in finance falls from 6% in 2004 to 5.1% in 2010.[39] Among Fisher graduates, likelihood of working in the finance-related industries of accounting and insurance falls (Figure 6c), suggesting finance employment may have fallen without JP Morgan's increased hires.

Even if JP Morgan hired at the expense of other finance firms, this may represent an improvement in outcomes as JP Morgan is a large finance firm. Had these individuals not worked at JP Morgan Chase, but instead in a reasonable counterfactual such as another finance firm, in accounting and insurance, or consumer goods and retail, they would have had a substantially smaller likelihood of working at a very large firm (Figure 6d).

7 Effect of Firms' Office Location Decisions on Longer-Run Earnings and Careers

Changes in access to high-wage firms at labor market entry have the potential to affect longer-run outcomes.[40] In this section I show how access to elite firms at labor market entry, driven by office location changes, affects earnings and career outcomes up to 10 years after graduation. This analysis is clearly more suggestive and limited in power. First, there are many factors that could affect career outcomes several years after graduation, other than

[39] I use 22-23 year old college graduates in the 2004 and 2010 surveys to capture people who would have been around graduation age in 2003 and 2009, in the year after graduation. I use the person weights. Further, annual reports from University of Michigan's business school show the proportion of new MBA graduates working in finance fell from 21.7% to 14.5% from 2003 to 2009, while the proportion of BBA graduates working in finance increased from 36.1% to 39.2%. However, the MBA graduating class is much larger than the BBA graduating class, suggesting the overall likelihood of working in finance fell for University of Michigan business school graduates.

[40] Recent work finds graduates during recessions experience long-run wage and productivity effects (Kahn 2010, Oreopoulos, Von Wachter, and Heisz 2012, Oyer 2006). Weinstein (2017) studies long-run effects of graduating from a worse regionally-ranked university, conditional on absolute university quality. Arellano-Bover (2020) finds that starting one's career at a larger firm improves lifetime income.

changes in access to high-wage firms at graduation. Second, the power of the analysis is limited by a limited number of office changes, of which a fraction yield changes in recruiting.

With these caveats, I use unique data to provide suggestive evidence. First, I merge the recruiting data with earnings outcomes by university, birth cohort, and parental income quintile from the *mobility report cards* (Chetty et al. forthcoming), based on federal tax records. Second, I use the LinkedIn data for Ohio State graduates to explore longer-run effects of JP Morgan's increased presence.

Changes in Likelihood of Reaching the Top 1%

Using the *mobility report cards* (Chetty et al. forthcoming), I match likelihood of top 1% earnings by university and birth cohort to campus recruiting and office location changes. I match to recruiting in the Fall of the calendar year the birth cohort turned 22, approximating access to new recruiting firms when the cohort was college seniors. These earnings data do not allow me to confirm that newly recruiting firms explain earnings changes. Finding an effect would be suggestive of, and consistent with, recruiting affecting labor market outcomes. I include the 1980-1991 birth cohorts, 23-34 years old at earnings measurement in 2014. Chetty et al. (forthcoming) find income percentiles are relatively stable by age 30, though they stabilize much earlier for non-Tier I graduates. Regressions include census region-university tier-year fixed effects. Thus, analyzing income percentiles before stabilization is less problematic, as the comparison is to same-tier universities. For robustness, I use different birth cohort cutoffs (Appendix Table A21).

The firms in my sample are precisely those that could enable mobility into the top 1%. For 30-34 year olds, the cutoff for the 99th percentile of individual earnings in 2014 ranged from \$143,000 to \$199,000 (2016 dollars) (Chetty et al. forthcoming). In 2016, median wages for 30-34 year old analysts at finance or consulting firms were \$100,000 to \$138,000. For 23-25 year olds, cutoffs for the 99th percentile ranged from \$65,000 to \$86,000. For 23-25 year olds working as analysts in finance or consulting firms, median wages were roughly \$70,000 to \$77,000 (2016 dollars). Given that I study the most prestigious firms in the industry, wages are likely even higher. Even if students are not still employed by these particular firms at earnings measurement, starting at these firms may affect career trajectory.

Using an event-study specification, I test for within-university changes in income success rates when there is a change in access to high-wage firms, driven by nearby office openings:

$$
\begin{aligned}
Pr(Top1\%Earnings|parentquintile = q)_{j_k ltq} = \alpha + \gamma_j \\
+ \sum_{v=-3}^{v=1} \beta_k I(t = t^* + v) RecruitPostMove_j \\
+ \kappa_{klt} + X_{jt}\rho + u_{jltq}
\end{aligned} \tag{5}
$$

The dependent variable is the proportion of students from parental income quintile q earning in the top 1% for their birth cohort in 2014, for the birth cohort that turned 22 in t and graduated from university j, in selectivity tier k and location l. Regressions contain university fixed effects, and five observations for each university (j)-senior cohort (t) pair, comprised of one observation for each parental income quintile. The variable $RecruitPostMove$ is an indicator equal to one if university j experienced a firm recruit on campus in year t^*, after opening a nearby office. Using the recruiting data, I identify t^* as the first instance starting in 2003 that the university experienced a firm recruiting on campus after opening a nearby office. I identify the first instance starting in 2003, as the matched earnings-recruiting sample starts in 2002. I require that the firm did not recruit at the university before the move, but that the firm was recruiting at other universities before the move.[41]

Regressions include only universities with $t^* \geq 2005$. This ensures three years of a pre-period in order to evaluate whether there were preexisting trends, and yields a balanced panel from $t^* - 3$ through t^*. I will also present coefficients on $t^* + 1$, identified only by universities with $t^* \neq 2013$; for $t^* = 2013$, the birth cohort corresponding to $t^* + 1$ is 1992, which is not in the data. Thus, the coefficient change between t^* and $t^* + 1$ should not be interpreted as a dynamic effect. I also balance the panel on calendar year, so the same university-quintile observations are in the control group (not experiencing new recruiting after a move) in each year.

New office locations may reflect broader local demand shocks that increase income success rates at all universities in the area, regardless of campus recruiting. This would bias the effect of a new recruiting firm after it opens a nearby office. I address this by including census

[41] Given the small number of universities experiencing recruiting after a move in the shorter earnings panel, I do not require the firm-university pair was in the main recruiting sample in $t^* - 1$. However, I require that $Recruit \neq 1$ in the pre-period, and it is zero (not missing) in one of the pre-move years while the firm is recruiting at other universities. I continue to include only firm-university-year observations for which the firm recruited that year at some university. I also attempt to address some of the missing recruiting observations to increase power. Specifically, I code $Recruit = 1$ if $Recruit$ is missing in a post-move year, but equals one the next year, and meets the pre-move recruiting conditions above. This results in the same set of universities experiencing new recruiting, but changes in t^* for seven universities. Not making this adjustment yields results with similar patterns, though the results are less precise for $t^* + 1$, and more positive in t^*, suggesting the positive effect of moves for these universities occurs in the later year (Appendix Table A15).

region-university tier-year fixed effects (κ_{klt}). This identifies the effect relative to the change in income success rate for same-cohort students graduating from same-tier, same-region universities that did not experience an increase in recruiting. I also show results using more narrow geographies (state and CZ), but power here is quite limited. I define selectivity tier as in Chetty et al. (forthcoming): Ivy Plus; other universities in Barron's Tier 1; highly selective public; highly selective private; selective public; selective private; nonselective; and universities with insufficient information.[42]

Changes in income success rates may be correlated with changes in student composition at the university, and these may be correlated with changes in recruiting. To address this, X includes the following time-varying university characteristics: proportion of students in each parental income quintile, proportion with parental income in the top 10, 5, 1, and .1%, percent female, and ln(students in the university's cohort (across all parental income quintiles)). I weight observations by the number of students in the university-birth cohort-parental income quintile cell, and I exclude universities for which the income success rate is measured across several universities since recruiting differs across these universities as well. I cluster standard errors at the university level.

For the cohort turning 22 in the year the firm started recruiting on campus, there is no significant change in the likelihood of upper-tail income success (Column 1, Table 3). However, for those who were 21 at the time the firm started recruiting on campus, this likelihood increases .28 percentage points ($p \leq .1$), relative to students who turned 22 in the year before the firm started recruiting and relative to students at similarly selective universities in the same region. Weighting observations as in the regression, the mean of the dependent variable in $t^* - 1$ is .071, among universities identifying the effect in $t^* + 1$. Thus, the coefficient implies a 4% increase in upper-tail income success. This coefficient on $t^* + 1$ is estimated for all but one of the 27 universities which identify the effect in t^*, as there is one university which experiences a move in 2013. There is no significant increasing trend in upper-tail income success preceding the firm's recruiting.[43]

Column 2 shows that including state-university tier-birth cohort fixed effects leads to a very imprecise estimate, and the universities providing identification falls from 27 to 17. Inclusion of the state-university tier-birth cohort fixed effects has a very small effect on the

[42] Highly selective and selective are as defined earlier. Two universities in the regression sample have $t^* - 3$ through t^*, but do not identify the effect as there is no other same-tier, same census region university.

[43] The coefficient on $t^* - 4$ is negative and significant while none of the other lags are significant (Appendix Table A23), among those identified by the same set of universities that also identify the coefficient on $t^* + 1$. This appears largely due to universities with $t^* = 2006$, as adding universities with $t^* \geq 2007$ results in the magnitude falling by 50%. Omitting universities with $t^* = 2006$ also yields a non-significant coefficient on $t^* - 4$. The coefficient on $t^* - 11$ is also negative and significant, but this is only identified by universities with $t^* = 2013$, and these do not identify the effect on $t^* + 1$.

R-squared, compared to including census region-university tier-birth cohort fixed effects. While power to identify an effect falls, this specification suggests these fixed effects provide little additional explanatory power, reducing concerns the results are driven by local shocks affecting all universities in the market. Comparing to similarly selective universities in the same CZ yields only six universities providing identification.

The coefficient on $t^* + 1$ in column (1) implies that for the median-sized university-year-parent quintile cell, an additional .72 students reach the top 1%, after one of these firms opens a nearby office and starts recruiting. For universities with median-sized university-year-parent quintile cell, for each parent income quintile, this implies an additional 6.2 students reach the top 1%, if all quintiles experience the same effect.[44]

With some assumptions described in the appendix, the firms in my sample hiring from University of Michigan Ross School of Business (Ross) hire an average of three bachelor's in business (BBA) students into fulltime jobs. This would likely be larger at universities attracting firms after a nearby office opening given these universities are much larger than the Ross BBA graduating class in 2007 (364 students relative to roughly 2200 for universities with median-sized quintiles described above), and given this does not include hires from University of Michigan outside the business school (for example among Economics majors). At Ross these firms hire .008 individuals per student enrolled, whereas the effect above implies an additional .0028 individuals per total students enrolled reach the top 1% after the recruiting increase. These magnitudes seem important and reasonable in this setting. This also implies the effects are relatively persistent, as income measurement is up to 10 years after graduation.

On-campus recruiting may differentially benefit students who would not otherwise have access to high-wage jobs. There are two natural dimensions along which recruiting may have differential effects. First, the benefits may be greater outside Ivy Plus universities, based on overall prevalence of recruiting or differences in networks. Second, students from lower parental income quintiles may benefit more from this recruiting, if their personal networks are less likely to provide access.

The Chetty et al. (2020) data show that likelihood of reaching the top 1% conditional on parental income quintile is substantially higher at the Ivy Plus universities, even relative to the other elite universities in Barron's Tier 1. Students at Ivy Plus universities may have access to many high-wage firms recruiting on campus already, and they may have access to these firms even without recruiting through alumni networks.

[44] The median-sized university-year-parent quintile cell in $t^* - 1$, among universities identifying the effect in $t^* + 1$, is roughly 258; by quintile (from one to five) it is: 99, 144, 219, 363, and 1374. For the average-sized quintile (616), an additional 1.7 students reach the top 1%, implying an additional 8.6 students from the university. There are three universities that experience two additional firms recruiting in t^*.

Column 3 shows the magnitude increases substantially when omitting the Ivy Plus universities. For those who were 21 at the time the firm started recruiting on campus, the likelihood of upper-tail income success increases .38 percentage points ($p \leq .05$) or 6%, relative to students who turned 22 in the year before the firm started recruiting and relative to the change among same-cohort students at similarly selective universities in the same region. This coefficient is statistically different at the 5% level from the coefficient for Ivy Plus universities (column 5), which shows no effect but is also underpowered with only four universities providing identification. While not significant, we also see positive effects when including state-university tier-birth cohort fixed effects (column 4), though the universities providing identification falls from 23 to 14. Comparing to similarly selective universities within the same CZ yields only four universities providing identification.

On average, Ivy Plus universities attracted roughly 10.2 recruiting firms in 2006 among the firms in my sample, while universities in other tiers attracted roughly .9.[45] Treating the estimate in Table 3 column 3 as the causal impact of attracting an additional firm at non-Ivy Plus universities, we identify the impact of exogenously increasing recruiting to the level at Ivy Plus universities, without any other changes at non-Ivy Plus universities. This would increase the income success rate at non-Ivy Plus universities by 3.5 percentage points. The overall gap in income success rates between Ivy Plus and non Ivy Plus universities is 14.6 percentage points for the cohort of 22 year olds in 2006, suggesting recruiting activity accounts for 24% of this gap.[46]

On-campus recruiting may differentially benefit students from lower parental income quintiles, if this is their principal channel of accessing high-wage jobs. Wealthier students may have networks providing access to high-wage jobs even without campus recruiting. If so, then high-wage firms in the university's local market, that recruit on campus, would help explain university variation in upward income mobility. Alternatively, these firms may differentially hire high income students when they recruit on campus, having little effect on mobility of students from less affluent families. This would be consistent with qualitative analysis showing that professional service firms value cultural signals in hiring, and these are typically associated with upper-middle class individuals (Rivera 2012). Panel B shows results from estimating regression (5), separately for each parental income quintile.

There are large and statistically significant effects of newly recruiting firms for students

[45] I use 2006 as I compare income success rates across university tiers, raising the importance of using a year in which income percentiles have stabilized. The cohort of 22 year olds in 2006 is 30 at income measurement in 2014.

[46] This is the gap in likelihood of top 1% incomes conditional on parental income quintile, weighted by number of students in the parental income quintile cell at the university (which equals the gap in likelihood of top 1% incomes, not conditional on parental income quintile, weighted by total students at the university).

from the fourth parental income quintile, implying a 13.5% increase in likelihood of reaching the top 1% by 2014. While this effect is larger in magnitude than the effect for other quintiles, the confidence intervals for the other quintiles are large. The effect for the fourth parental income quintile is not statistically different from the effect for the highest quintile, or the lowest quintile. The results suggest students outside the top parental income quintile do benefit from additional recruiting on campus, though there is not conclusive evidence they benefit more than students from the top quintile. When restricting to universities outside the Ivy Plus tier, we see similar results, though the effects are statistically significant for both the fourth and fifth quintiles (Appendix Table A20).

Given these firms should mainly affect mobility to the top 1%, if there were much larger positive effects on mobility to the top 20% ($25,000 to $58,000 in 2015 dollars) this may suggest other changes at these universities. There is no effect on mobility to the top 20% in $t^* + 1$, relative to the change at other same-tier universities in the same region (Appendix Table A10). However, the confidence interval for this effect includes .0028, the increased likelihood of reaching the top 1%. There may be university-specific trends in income success rates, and these may affect, or be correlated with, recruiting decisions of high-wage firms. I find no changes in composition by sex or parental income in $t^* + 1$ relative to $t^* - 1$, which is when we see the increase in income success rates (Appendix Table A19). Lack of change in nearly all of these observables reduces concerns that university-specific changes in unobservables explain the income success effect.[47]

Firms recruiting after nearby office openings may reflect a labor demand shock more localized than the census region, for example in the state or CZ. Changes in income success would then be experienced by all universities in the state or CZ, even those not attracting these firms. The very minimal change in the R-squared from including state-university tier-birth cohort fixed effects provides some evidence against this. As noted above, power is very limited when including CZ-university tier-cohort fixed effects in (5), instead of census region-university tier-cohort fixed effects. However, as an additional test I analyze changes in CZ economic characteristics timed with the increase in firms recruiting after nearby office openings, relative to the change that year at same-tier universities in the same census region.

I estimate (5) with $y_{j_k l t}$ equal to economic characteristics in the university's CZ (l) in the year the birth cohort turned 22 (t), clustering standard errors at the CZ level. I obtain county-level economic characteristics from the Bureau of Economic Analysis Regional Economic Accounts, and aggregate to the CZ. The event study variables in this specifica-

[47]In t^* there are increases in share female, share in the first parental income quintile, with decreases in the share with high income parents. Preceding this new recruiting, there is some evidence of an increase in the share from wealthier families. Importantly, these trends do not continue past $t^* - 1$, and so comparing $t^* + 1$ to $t^* - 1$ is not problematic.

tion measure years relative to the increase in firms recruiting after a nearby office opening. The increase in recruiting firms is not timed with differential changes in the university's CZ, relative to the census region, in per capita net earnings, per capita personal income, per capita unemployment compensation, or average wages (Appendix Table A22). This mitigates concerns that when there was an increase in recruiting, job opportunities were improving broadly in the CZ that year, potentially affecting future income success rates for all graduating students in the CZ. We also do not see differential changes in these variables in the years following the increase in recruiting. This mitigates concerns that there was a local shock timed with the recruiting increase, that impacted the economy in the years between the shock and income measurement, benefiting all CZ graduates throughout the post-shock period (but not those graduating just before the shock).

There are long-run differential trends in population and employment growth in these commuting zones, preceding and following the increase in recruiting firms after nearby office openings. However, if these were responsible for the change in income success rates in $t^* + 1$ relative to $t^* - 1$, we would expect differences in income success rates in $t^* - 3$, $t^* - 2$, and t^* relative to $t^* - 1$. We do not see pretrends in income success rates in Table 3, providing further evidence suggesting the change in $t^* + 1$ in Table 3 is due to the additional recruiting firm, and not due to changes in the local labor market affecting all universities in that market.

Together these results on income success rates are underpowered due to data constraints and I do not wish to overstate their conclusiveness. However, they present some evidence that location decisions, exogenous to the universities, may affect variation across universities in upward income mobility.

JP Morgan Chase Recruiting and Longer-Run Effects on Ohio State Graduates: Evidence from LinkedIn

I use the LinkedIn data for Ohio State graduates to additionally evaluate longer-run impacts. For the 2003 graduates, this is 16 years after graduation, and 10 years after graduation for 2009 graduates. Had individuals not started at JP Morgan Chase, they would have been less likely to currently work at a large financial services firm (Figure 6e). Roughly 40% of students starting at JP Morgan Chase in 2009 currently work at a large financial services firm, relative to 25% among those starting at other financial services firms, and even lower among those starting outside financial services.

Second, I show those starting at JP Morgan Chase in 2009 are more likely to currently live in the state of New York (over 15%), relative to those starting at other financial services firms (under 10%), or in accounting and insurance, or consumer goods and retail. For people working in finance, this may be an important signal of career growth. Interestingly, individuals starting their career at JP Morgan Chase are equally likely to currently live in Ohio, relative to those starting in other financial services firms, or in other industries. These results are consistent with initial access to JP Morgan Chase having longer-run impacts on Ohio State graduates relative to the counterfactual.

8 Conclusion

This paper studies how employer location decisions affect variation across universities in labor market opportunities. For 2000 to 2013, I collect data on office locations and recruiting strategies of over 70 prestigious finance and consulting firms.

Firm location decisions create barriers to accessing high-wage jobs, for students at distant universities. After opening an office, firms are nearly four times as likely to recruit at nearby universities. Access increases for students at universities across a wide range of selectivity. After closing offices, recruiting at previously nearby offices falls 45%, though there are far fewer instances of office closings and this evidence is less conclusive. With two case studies, and data from university annual reports and LinkedIn, I show suggestive evidence that increased local presence also translates into effects on hires by high-wage firms, and does not simply shift which high-wage firms are hiring. Finally, I show suggestive evidence that increased local presence of high-wage firms affects longer-run income success, using *mobility report cards* (Chetty et al. forthcoming) and LinkedIn data. These effects are larger at universities outside the Ivy Plus tier, and account for nearly 25% of the gap in income success rates between Ivy Plus and other universities.

I show that in choosing where to recruit workers, the costs of distance relative to other university attributes appear large, even for high-wage firms recruiting young, college-educated workers. Studying recruiting for lower-wage jobs is also of interest, including for understanding income mobility. For these jobs, firms may more likely recruit locally because of stronger applicant migration frictions in those markets, or reduced benefit of recruiting at more selective national universities. However, the opportunity cost of travel may also be lower for firms recruiting for lower-wage jobs, which may lead them to recruit slightly farther away.

The results suggest that some universities may have higher upward income mobility than others because of employer office location decisions exogenous to universities. This has important implications for how students select universities, and teachers, parents, and policymakers advise those decisions. The results also suggest that local economic development policies, which attract firms to municipalities or states, may improve access to high-quality jobs for people in those markets.

Using richer individual-level data, future research could further identify the effect of the university's local market, and the composition of firms recruiting at the university, on students' labor market outcomes. Access to individual-level data with university attended, earnings, and employer would facilitate this research, and this may have important implications for improving individuals' initial access to high-wage firms and longer-run labor market outcomes. In particular, these data could further identify the effect of access or proximity to high-wage firms while in college on the likelihood of being hired by those firms, post-graduation incomes, and the extent to which this improves intergenerational income mobility.

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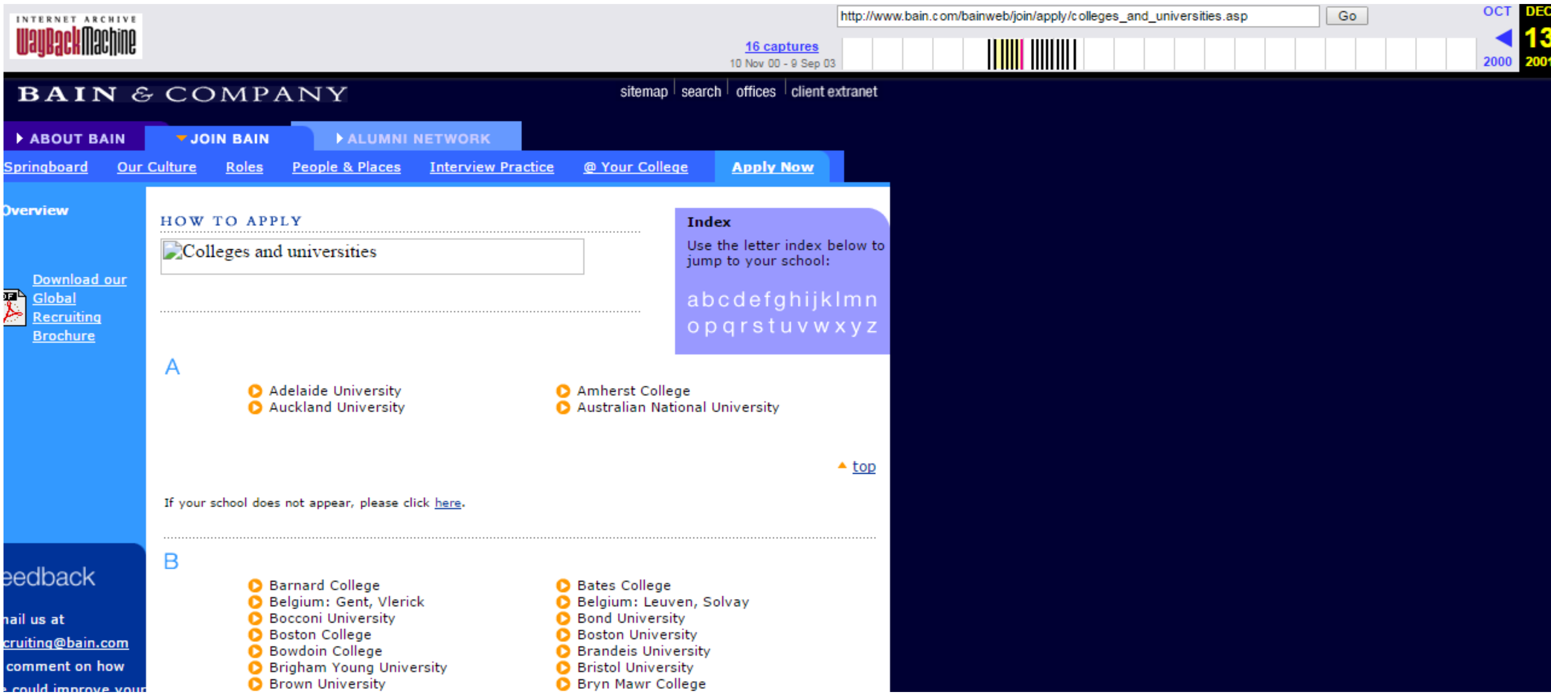
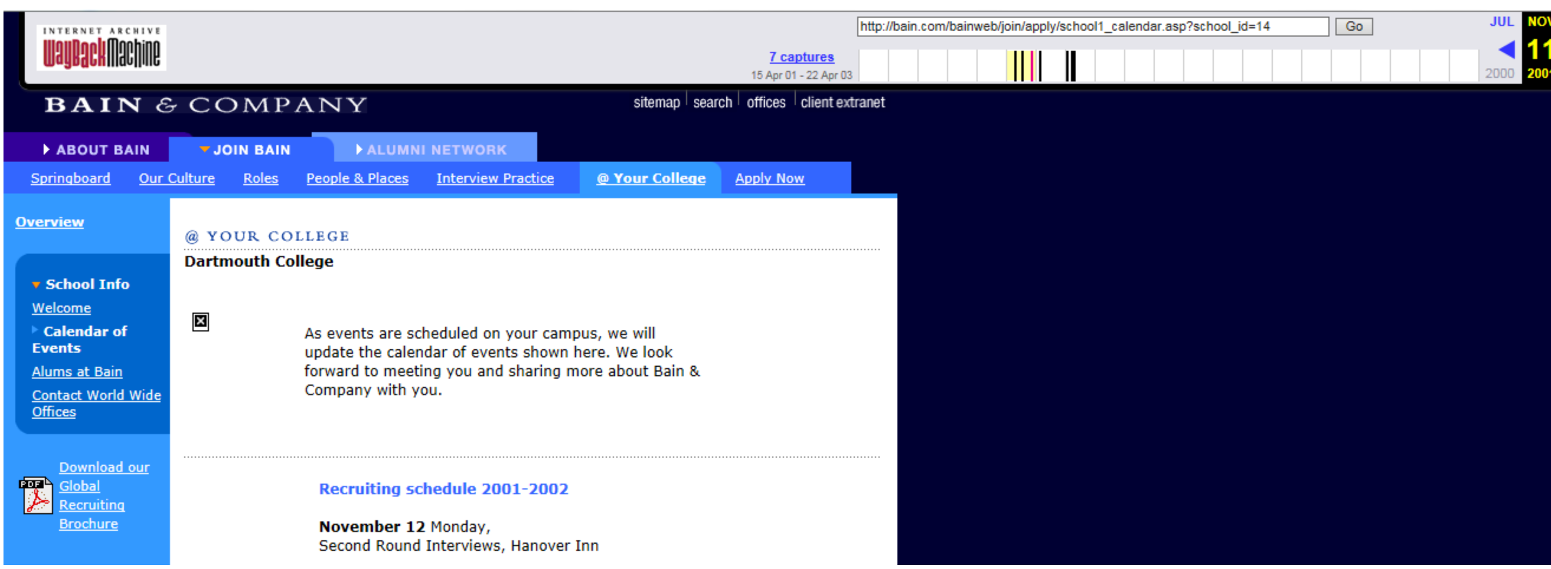
Figure 1: Data Collection from *The Internet Archive Wayback Machine*: Bain & Company Recruiting Pages

(a) University-Specific Links



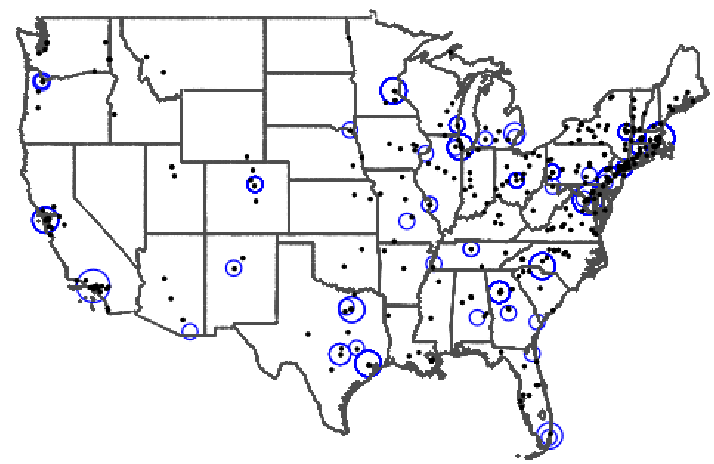
(b) Dartmouth-Specific Link



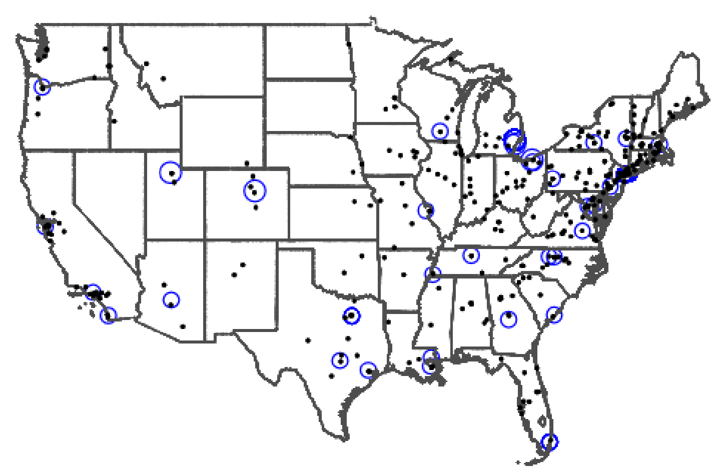
Note: This figure gives an example of the data collection for the consulting firm Bain & Company in 2001, using *The Internet Archive Wayback Machine*.

Figure 2: Cities Experiencing Move Ins and Move Outs, and Universities in the Sample

(a) Move Ins



(b) Move Outs



Note: These maps show all universities in the sample (solid dots) as well as cities experiencing move ins (in (a)) and move outs (in (b)). These are cities in which a firm opens an office (a) or closes an office (b). In addition, in (a) this move puts them at least 50 miles closer, and within 200 miles of at least one university. In (b) this move puts them at least 50 miles farther from at least one university when they had been within 200 miles of the university. I limit to the first move in and first move out experienced by each firm/university pair. Marker sizes are weighted based on how many firms move in or out of the city, based on these definitions of move in and move out. See text for details.

Figure 3a: Office Openings and Recruiting at Local Universities

Mean *Recruit*$_{t-1}$ = .012

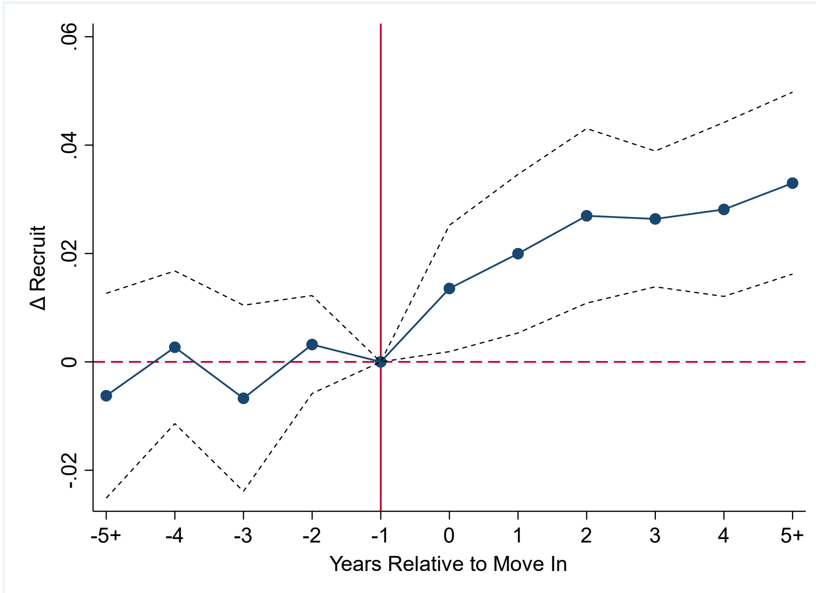
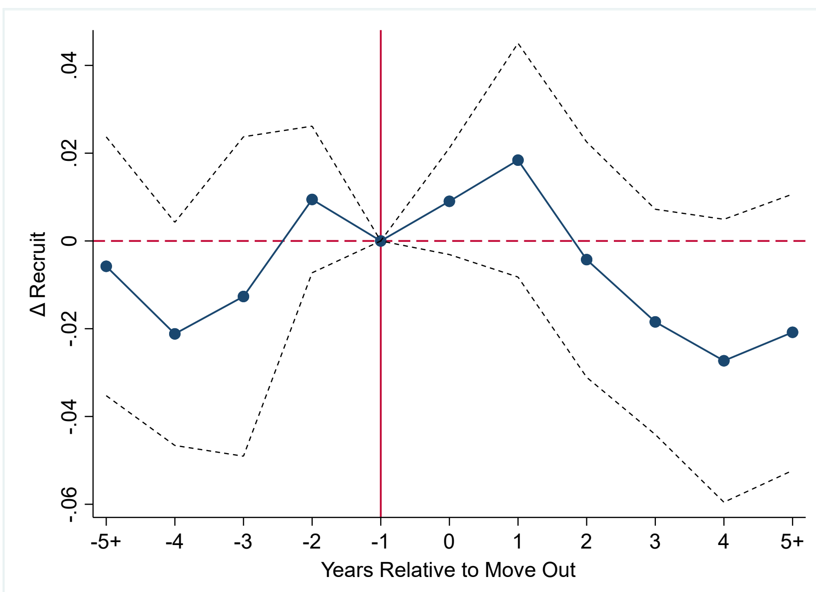


Figure 3b: Office Closings and Recruiting at Local Universities

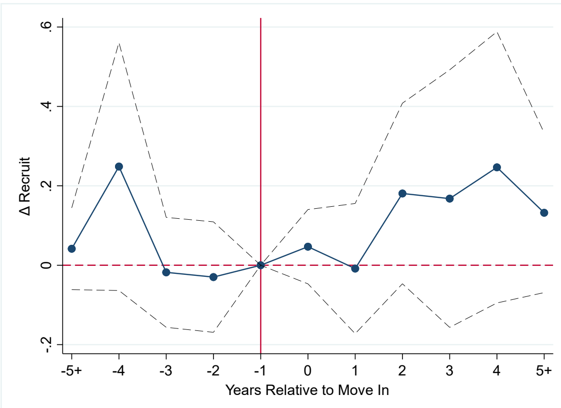
Mean *Recruit*$_{t-1}$ = .04



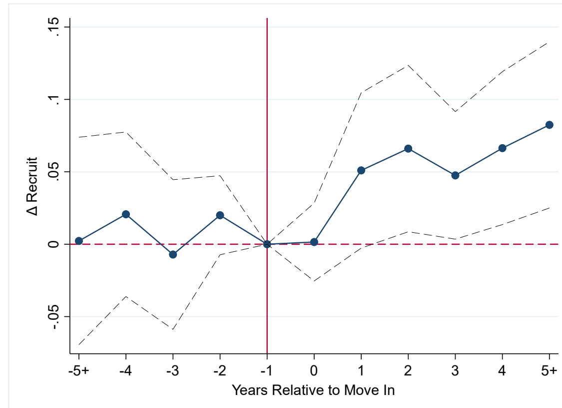
Note: These figures show the results of a regression of *Recruit* on indicators for period from the move in (Figure 3a) or move out (Figure 3b). Move ins are instances in which a firm moves at least 50 miles closer to a university, and the firm-university distance is within 200 miles. Move outs are instances in which the firm moves at least 50 miles farther from a university, when it had been within 200 miles. I limit to the first move in and first move out experienced by a firm/university pair. The dependent variable in the regression is an indicator for whether firm *f* recruits at university *j* in time *t*. The regression includes firm-university pair fixed effects, firm-year fixed effects, and university-year fixed effects. To be in the regression sample, firm-university pairs experiencing moves must have data in *t-1*. Dashed lines show 95% confidence intervals. See text for details.

Figure 4: Changes in Recruiting After Office Openings, by University Tier

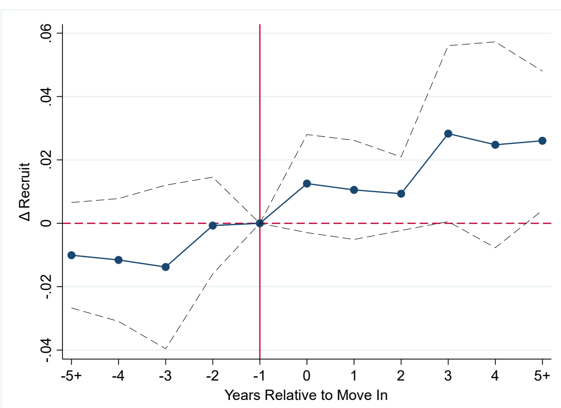
(a) 12 Ivy Plus Universities
Mean $Recruit_{t-1}$ = .057



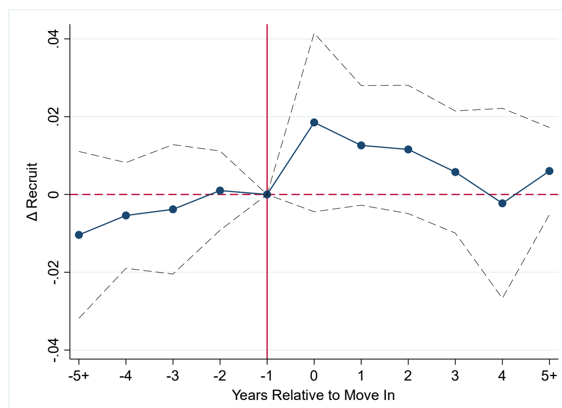
(b) 61 Barron's Tier 1 Universities, Excluding Ivy Plus
Mean $Recruit_{t-1}$ = .023



(c) 92 Barron's Tier 2 Universities (Highly Selective)
Mean $Recruit_{t-1}$ = .011



(d) 185 Barron's Tiers 3-5 Universities (Selective)
Mean $Recruit_{t-1}$ = .003

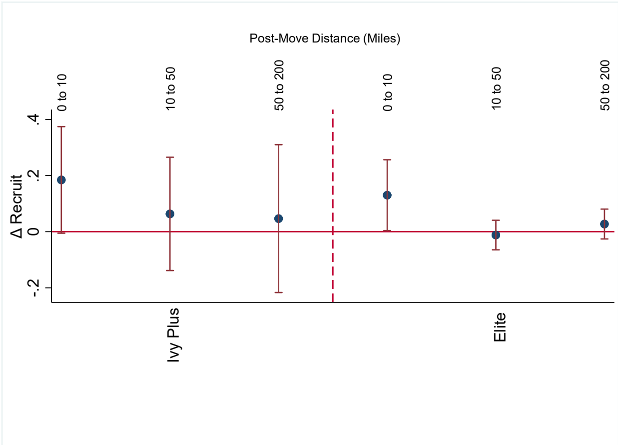


Note: Figures are all from the same regression of *Recruit* on indicators for period from the year in which firm *f* moved closer to *j*, interacted with university tier. Categories of university tier are: "Elite" (Barron's Tier 1 excluding Ivy Plus), "Highly Selective" (Barron's Tier 2), "Selective" (Barron's Tiers 3-5), and "No Tier/Insufficient Information"; Ivy Plus is the omitted category. The regression includes firm-university pair fixed effects, firm-year fixed effects, and university-year fixed effects. To be in the regression sample, firm-university pairs experiencing moves must have data in *t-1*. Dashed lines show 95% confidence intervals. See text and notes to Figure 3 for details.

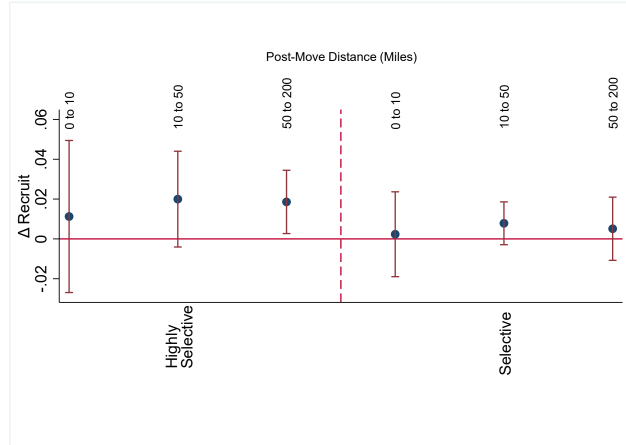
Figure 5: Effect of Nearby Office Openings, by Post- and Pre-Move Distance and University Tier

Effects by Post-Move Distance

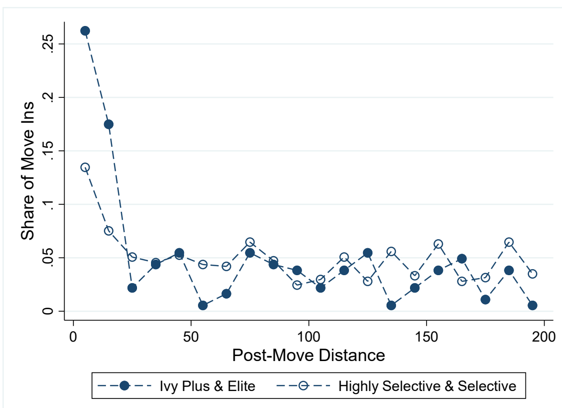
(a) Ivy Plus and Elite Universities



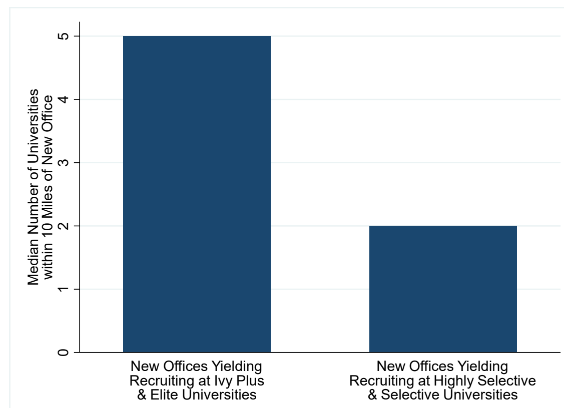
(b) Highly Selective and Selective Universities



(c) Distances After Move In

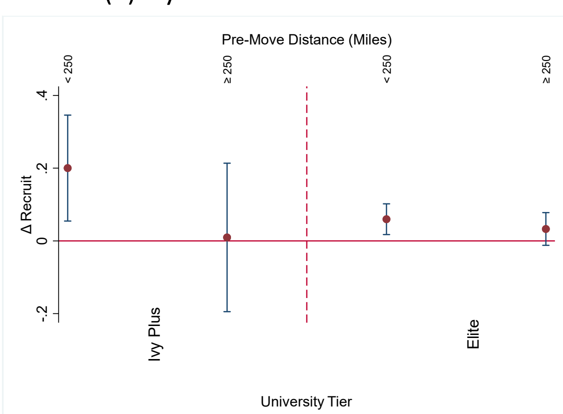


(d) Fewer Universities within 10 Miles When Recruit Outside Barron's Tier I

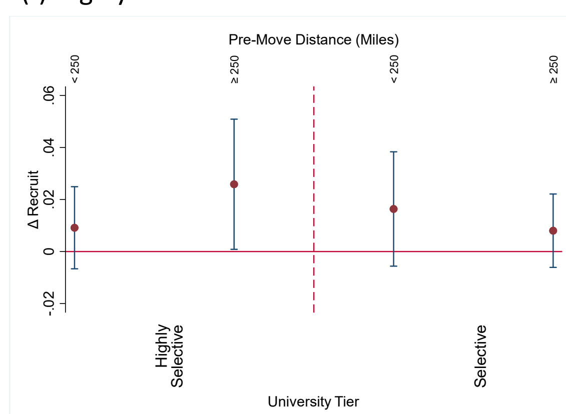


Effects by Pre-Move Distance

(e) Ivy Plus & Elite Universities



(f) Highly Selective & Selective Universities

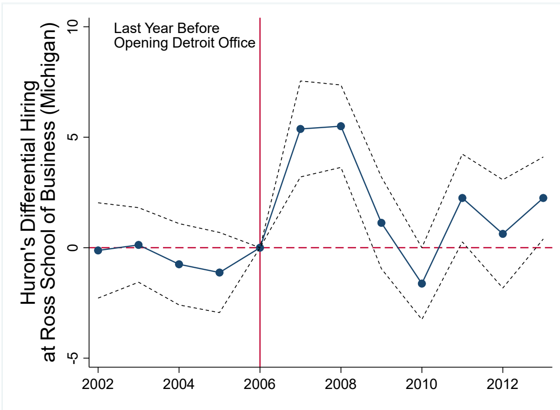


Note: Plots (a) and (b) show linear combinations of regression coefficients, from the same regression, identifying the effect of a move, for varying post-move distances, by university tier. Regressions also include firm-university, firm-urban area of closest office-year, and university-year fixed effects. To be in the regression sample, firm-university pairs experiencing moves must have data in *t-1*. Bars show 95% confidence intervals. Plot (c) shows a histogram of firm-university distance following an office opening, for firm-university pairs experiencing move ins, separately for Ivy Plus/elite universities and highly selective/selective universities. Plot (d) identifies moves to urban areas after which the firm started recruiting on at least one campus, and shows differences in number of nearby universities depending on whether the new target was Ivy Plus/elite or highly selective/selective. I identify new targets for which Recruit ≠ 1 in the pre-move period, and equals zero in at least one pre-move year. Plots (e) and (f) show linear combinations of coefficients, from the same regression including interactions of post move, an indicator for pre-move distance ≥ 250 miles, and indicators for university selectivity tier, as well as firm-university, firm-year, and university-year fixed effects. See paper for details.

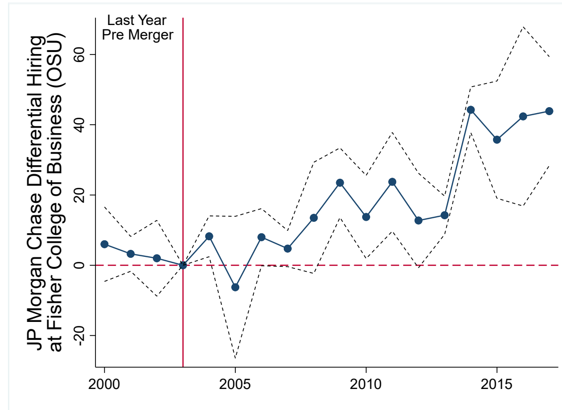
Figure 6

Panel A: Hires by Firm for Two Case Studies, Based on Business School Annual Reports

(a) Huron Hires from U. Michigan Business School

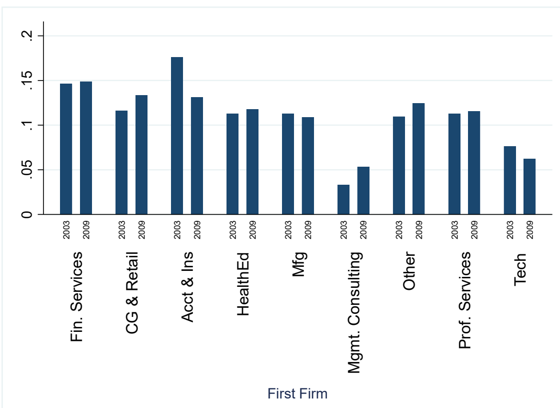


(b) JP Morgan Hires from Ohio State U. Business School

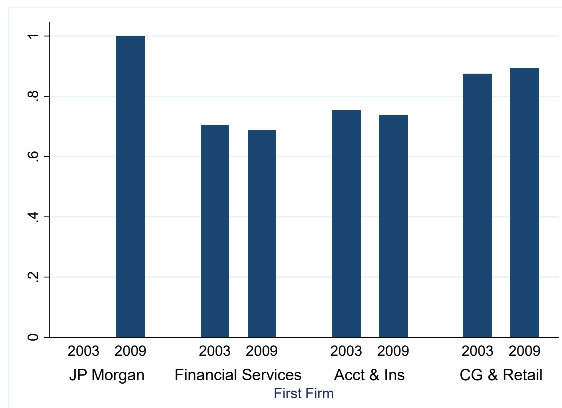


Panel B: Outcomes for Ohio State Graduates Before (2003) and After (2009) JP Morgan Increases Local Presence, by First Firm Industry and Based on LinkedIn Profiles

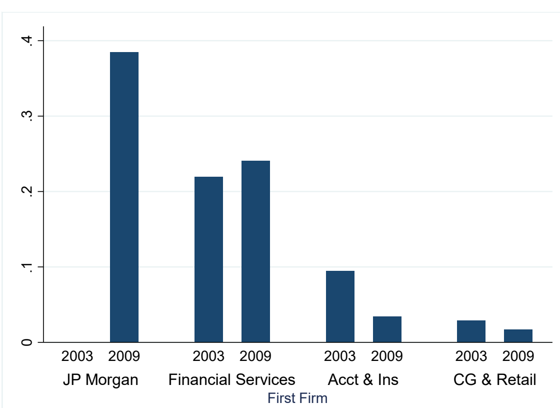
(c) Y = Industry Share, 1st Industry



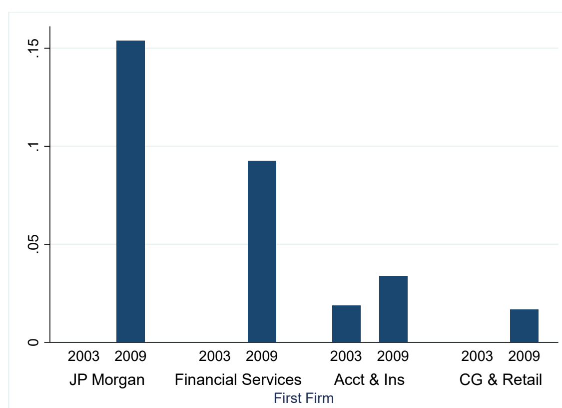
(d) Y = 1st Firm > 5000 Employees



(e) Y = Current (2019) Firm in Financial Services & > 5000 Employees



(f) Y = Current (2019) State is New York



Note: Figure (a) shows the differential change in hires surrounding Huron Consulting opening an office in Detroit, MI, and (b) for JP Morgan Chase increasing its presence in Columbus, OH. Coefficients show the differential change in hires relative to the base year for Huron from U. Michigan relative to eight other consulting firms with data before 2007 (a) and JP Morgan Chase from Ohio State U. relative to four finance/accounting firms with greatest coverage in the data (b). For firm/years in which I know only an interval for hires, I show results using the interval midpoint. Dashed lines show 95% confidence intervals. Figures (c) through (f) are based on LinkedIn profiles for graduates of Ohio State's business school in 2003 and 2009. Graduates in 2003 preceded the JP Morgan Chase merger with Bank One, which increased the company's local presence. I include only individuals whose first post-graduation job was not the continuation of their pre-graduation job. Thirteen graduates (of 450) in 2009 were originally employed by JP Morgan Chase, compared to three (of 301) in 2003. Thus, these results should be treated as very suggestive. I do not show results for JP Morgan hires in 2003 given the very small sample. See paper for details.

Table 1: Move Ins and Move Outs in the Sample

# Firms	53
# Consulting Firms	32
# Banking Firms	21
# Universities	362
# Office Openings	75
# Office Closings	41
# Firms with ≥ 1 Move In	24
# Universities with ≥ 1 Move In	314
# Firm/University Pairs with ≥ 1 Move In	789
# Cities with ≥ 1 Move In	46
# Firms with ≥ 1 Move Out	20
# Universities with ≥ 1 Move Out	283
# Firm/University Pairs with ≥ 1 Move Out	505
# Cities with ≥ 1 Move Out	36
Cities with Greatest Move Ins (#)	
Los Angeles, CA	5
Boston, MA	4
Washington, DC	4
Cities with Greatest Move Outs (#)	
Denver, CO	2
Detroit, MI	2
Cleveland, OH	2
Salt Lake City, UT	2
Troy, MI	2

Note: Move ins are defined as instances in which a firm moves at least 50 miles closer to a university and is within 200 miles. Move outs are defined as instances in which a firm moves at least 50 miles farther from a university and was within 200 miles. Moves are limited to the first move in and first move out. The sample drops singletons, including firm/university pairs only in the sample for one year, and firm/year pairs with only one observation in the sample (after dropping firm/university pairs that are singletons). See text for details.

Table 2: The Effect of Office Openings and Closings on Recruiting at Local Universities

	(1)	(2)	(3)	(4)	(5)	(6)
Y = *Recruit*		Move Ins			Move Outs	
Post Move, Short Run						
All Universities	0.019***	0.021***		0.004	0.005	
	(0.005)	(0.005)		(0.009)	(0.007)	
Ivy Plus University			0.111*			0.055**
			(0.064)			(0.025)
Elite University (Barron's Tier I, Excl. Ivy Plus)			0.0341**			0.00161
			(.015)			(.02)
Highly Selective University (Barron's Tier 2)			0.0193**			0.0119
			(.008)			(.013)
Selective University (Barron's Tiers 3-5)			0.0118			0.000458
			(.007)			(.007)
Post Move, Long Run						
All Universities	0.025***	0.031***		-0.016	-0.015	
	(0.007)	(0.008)		(0.013)	(0.015)	
Ivy Plus University			0.125			-0.004
			(0.082)			(0.007)
Elite University			0.0732**			-0.0352
			(.028)			(.057)
Highly Selective University			0.0280**			-0.0209
			(.012)			(.034)
Selective University			0.00792*			-0.0044
			(.005)			(.007)
N	90,764	90,764	90,764	89,715	89,715	89,715
R-squared	0.741	0.768	0.769	0.744	0.771	0.771
Firm-University Fixed Effects	Y	Y	Y	Y	Y	Y
Firm-Year, University-Year Fixed Effects	N	Y	Y	N	Y	Y
Dependent Variable Mean						
t^**-1*	0.012			0.040		
Ivy Plus			0.057			0.222
Elite			0.023			0.104
Highly Selective			0.011			0.037
Selective			0.003			0.007

Note: *** p<0.01, ** p<0.05, * p<0.1. Each column is a separate regression. In parentheses are two-way clustered standard errors, by firm and university. All regressions include firm/university pair fixed effects. Columns 1 and 4 include year fixed effects, while the remainder include firm-year fixed effects and university-year fixed effects. Move ins are instances in which a firm moves at least 50 miles closer to a university, and the firm-university distance is within 200 miles. Move outs are instances in which the firm moves at least 50 miles farther from a university, when it had been within 200 miles. The variable *Post Move, Short Run* is an indicator for the year of the move, and the four years following the move. The variable *Post Move, Long Run* is an indicator for five or more years following the move. The regressions also include an indicator for five and more years before the move (coefficients not shown). Effects are relative to the four years preceding the move. To be in the regression sample, firm-university pairs experiencing moves must have data in t-1. Columns 3 and 6 show linear combinations from a regression that includes the post-move indicators interacted with university tier. Categories of university tier are: "Elite" (Barron's Tier 1 excluding Ivy Plus), "Highly Selective" (Barron's Tier 2), "Selective" (Barron's Tiers 3-5), and "No Tier/Insufficient Information"; Ivy Plus is the omitted category. I drop singletons, defined in Table 1, and limit to the first move in and first move out experienced by a firm/university pair.

Table 3: On-Campus Access to Recently Relocated Firms at Graduation, and the Effect on Incomes in 2014 (at age 23-34)

Y =Pr(Top 1% Earnings|parent quintile = q)$_{jltq}$	(1)	(2)	(3)	(4)	(5)
$t = t^* - 3$	-0.0008	-0.0031	-0.0004	-0.0023	0.0064
	(0.0016)	(0.0024)	(0.0017)	(0.0025)	(0.0081)
$t = t^* - 2$	-0.0012	-0.0027	0.0001	-0.0008	-0.0052
	(0.0014)	(0.0019)	(0.0014)	(0.0018)	(0.0072)
$t = t^*$	-0.0001	-0.0011	0.0001	-0.0005	-0.0016
	(0.0013)	(0.0014)	(0.0014)	(0.0015)	(0.0084)
$t = t^* + 1$	0.0028*	0.0004	0.0038**	0.0023	-0.0120
	(0.0016)	(0.0021)	(0.0015)	(0.0018)	(0.0073)
N	15,960	15,960	15,240	15,240	720
University Tier	All	All	Non Ivy	Non Ivy	Ivy
Universities Providing Identifying Variation	27	17	23	14	4
R-squared	0.8520	0.8662	0.7758	0.7981	0.5829
Dep. Var. Mean, $t^* - 1$	0.0738	0.0738	0.0614	0.0614	0.183
Region-University Tier-Cohort (*t*) FE	Y	N	Y	N	Y
State-University Tier-Cohort (*t*) FE	N	Y	N	Y	N

	Parental Income Quintile				
Y =Pr(Top 1% Earnings|parent quintile = q)$_{jltq}$	1	2	3	4	5
$t = t^* - 3$	-0.0038	-0.0044	-0.0009	0.0036	-0.0012
	(0.0051)	(0.0033)	(0.0040)	(0.0034)	(0.0021)
$t = t^* - 2$	-0.0044	-0.0017	-0.0058	-0.0008	-0.0001
	(0.0059)	(0.0038)	(0.0042)	(0.0028)	(0.0023)
$t = t^*$	0.0078	-0.0035	-0.0036	0.0007	0.0003
	(0.0048)	(0.0040)	(0.0034)	(0.0029)	(0.0020)
$t = t^* + 1$	0.0006	-0.0033	-0.0027	0.0069**	0.0037
	(0.0044)	(0.0031)	(0.0038)	(0.0027)	(0.0025)
N	3,132	3,132	3,132	3,132	3,132
Universities Providing Identifying Variation	27	27	27	27	27
R-squared	0.6834	0.7251	0.8109	0.8678	0.9529
Dep. Var. Mean, $t^* - 1$	0.0421	0.0417	0.0501	0.0513	0.0898
Region-University Tier-Cohort (*t*) FE	Y	Y	Y	Y	Y

Universities in Panel A Column 1 Attracting Recruiting Firms After Nearby Office Openings, by University Tier

Ivy Plus	4
Elite (Barron's Tier 1, Excluding Ivy Plus)	12
Highly Selective (Barron's Tier 2)	8
Selective (Barron's Tiers 3-5)	3

Notes: *** p<0.01, ** p<0.05, * p<0.1. Regressions include university fixed effects, and standard errors are clustered at the university level. Observations in Panel A are at the university-parental income quintile-birth cohort level. Dependent variable is the fraction of students from parental income quintile q in the top 1% of the income distribution for their birth cohort in 2014, for the birth cohort that turned 22 in calendar year t and graduated from university j in selectivity tier k, and location l. For $k =$ -3 to 1, the variable $t=t^*+k$ indicates whether the calendar year t in which the birth cohort turned 22 is the kth year relative to the university first attracting a Fall recruiting firm, after opening a nearby office. I identify the first instance starting in 2003, and require the firm did not recruit at the university before the move, but recruited at other universities in the pre-move period. I use the 1980-1991 birth cohorts. Time-varying controls include the fraction of the cohort that is female, the fraction with parental income in quintile 1, in quintile 2, in quintile 3, in quintile 4, in quintile 5, in the top 10%, 5%, 1%, and .1%, and ln(students at the university across all parental income quintiles). The dependent variable and university characteristics are from the mobility report cards (Chetty et al. forthcoming). Observations are weighted by the count in the university-birth cohort-parental income quintile cell. Region denotes census region, and university tiers include Ivy Plus; others in Barron's Tier 1; highly selective (Barron's Tier 2) public; highly selective private; selective (Barron's Tiers 3-5) public; selective private; nonselective; and universities with insufficient information. Panel B reports results from a similar regression, estimated separately by parent income quintile. See text for details.

Appendix Table 1: Firms in Main Sample

Consulting Firms

A. T. Kearney
Accenture
Advisory Board
Analysis Group
Arthur D. Little
Bain & Company
Booz & Company
Booz Allen Hamilton
Cambridge Associates
Charles River Associates
Cornerstone Research
Corporate Executive Board
Dean & Company
First Manhattan Consulting Group
FTI Consulting
Gallup
Hewitt Associates
Huron Consulting Group
Kurt Salmon
Marakon
McKinsey & Company
Mercer
Mitchell Madison Group
Navigant
NERA Economic Consulting
OC&C Strategy Consultants
Oliver Wyman
Parthenon Group
PRTM
Putnam Associates
The Boston Consulting Group
ZS Associates

Banks

ABN AMRO
Bank of America
Brown Brothers Harriman
Citi
Cowen Group
Deutsche Bank
Evercore Partners
Gleacher & Company
Jefferies & Company
Lazard
Macquarie Group
Morgan Stanley
Perella Weinberg Partners
Piper Jaffray Companies
Raymond James Financial
Robert W. Baird & Co.
Rothschild
Thomas Weisel Partners Group
U.S. Bancorp
Wachovia
William Blair & Company

Firm Decisions and Variation Across Universities in Access to High-Wage Jobs: Evidence from Employer Recruiting
Online Appendix

Russell Weinstein*

February 25, 2021

Data

There are 46 consulting firms listed in the 2007 Vault top 50 ranking by prestige. Four of these 46 are not in the full sample, either because automated web crawlers were blocked, or the page was nonarchived, in all sample years. Deloitte Consulting and Watson Wyatt were not included because their pages could not be crawled by robots in any of the sample years. Towers Perrin was not included because robots could not crawl the pages listing the firm's locations. Strategic Decisions Group was not included because the pages were not archived in any of the sample years. I collect data for one firm not listed in the top 50 in 2007 because it split from a top 50 firm in 2008 (Booz). In addition, BearingPoint is not included as there is only one year with nonmissing recruiting and location data, yielding a total of 42 consulting firms in the full sample.

There are 43 banking firms in the 2008 Vault top 50 ranking by prestige of commercial banks and financial services companies. Data were not available for four firms: Goldman Sachs, Blackstone, Deloitte, or UBS. There were duplicate listings of two firms in the Vault ranking. There were two listings for JP Morgan (JP Morgan

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Investment Bank and JPMorgan Chase & Co.), and the data were collected for JP Morgan as a whole. There were also two listings for Citi (Citi Institutional Clients Group and Citigroup Inc.), and the data were collected for Citi as a whole. For three firms, recruiting pages were identified but missing recruiting or location information prevented their inclusion. KPMG says to contact the university regarding recruiting in each year. Wells Fargo has inconsistent location information in each year except 2004 and 2005, and in 2005 it says to contact the university while in 2004 the page is not archived. RBS does not give relevant location data, so this firm is not used in the analysis.

In the case of mergers or acquisitions, I collect the post-merger or post-acquisition target campuses if the original sample firm remains in the name of the new firm, or the target campuses can be separated from the parent firm. I collect the pre-merger target campuses for the original sample firm. This is relevant for Oliver Wyman and Hewitt.

From 2000-2002, Oliver Wyman existed as a firm. The firm was renamed Mercer Oliver Wyman in 2003. In 2006, Mercer Oliver Wyman merged with Mercer Management and Mercer Delta to form Oliver Wyman. The recruiting data for Oliver Wyman consists of the target campuses for Oliver Wyman from 2000-2002, for Mercer Oliver Wyman from 2003-2006, and Oliver Wyman from 2007 forward. Thus, for this firm I do not include the target campuses for the other companies that merged in 2006.

Hewitt was bought by Aon in October 2010, and a new firm Aon Hewitt was formed. The values of *Recruit* for Hewitt consist of Hewitt's target campuses through 2009, and Aon Hewitt's target campuses from 2010 forward. While PRTM retained its name after being acquired by PwC, the recruiting strategies could not be separated from PwC as a whole, which has many other divisions. I only collect data on recruiting strategies for PRTM through 2010, the year before it was acquired.

Appendix Table A1 shows the firms in the sample, the years in which each firm is in the sample, and the reason for any missing years. I define a firm to be in the sample if the firm is in the sample for at least one university that year. Firms may not be in the sample if they are missing location information or recruiting information, or if they have not yet been founded or have exited. There are several reasons why for a given firm $Recruit_{fjt}$ may be missing for some universities and not others. These include event dates listed as TBA, and nonworking university-specific links when others were accessible or clearly not attracting firms.[1]

Six of the 42 consulting firms, and four of the 31 banking firms entered or exited during the sample period. $Recruit_{fjt}$ is set to missing for these firms in the years they were not active.

Geographic Data

I calculate distance between firms and universities by computing the lengths of the great circle arcs connecting each university and each office location for a given firm, located on the surface of a sphere. The arc length, measured in degrees, is then converted to statute miles as measured along a great circle on a sphere with radius 6371 kilometers, the mean radius of the earth. These calculations are performed using the *arclen* and *deg2sm* commands in MATLAB. I then identify the closest location of each firm to each university.

I obtain the latitude and longitude of the office locations using the Census Gazetteer place and county subdivision files. For cities that could not be merged, I manually obtained the latitude and longitude. I obtain the university's latitude and longitude from IPEDS.

I obtain the urban area for each city by merging the city to a census place or county subdivision using the TIGERweb data files from the US Census to obtain the geoid, and then merging on geoid to the Urban Area Relationship Files from the US Census.

I obtain the 1990 commuting zone (CZ) for each firm's office locations by identifying the county in which the city is located, using the StatsAmerica City-to-County Finder (http://statsamerica.org/CityCountyFinder/). For cities that are in multiple counties, I use the county with a greater share of the city's population. I then merge county to 1990 commuting zone using the crosswalk from David Dorn (https://www.ddorn.net/data.htm). I obtain CZ for each university using the *mobility report card* data (Chetty et al. forthcoming). For universities that share a Super OPEID with other universities, the CZ in the data is not necessarily the CZ of the particular university. Further, some universities in the recruiting data are not in the *mobility report card* data. For these universities, I merge county from IPEDS to CZ

[1] A separate appendix with coding details, including why $Recruit_{fjt}$ is listed as missing in each case, is available upon request.

again using the Dorn crosswalk.

Calculating the Share of High-Scoring Students at a University

I test whether university characteristics change around the time of move ins or move outs. Among the variables I consider are the share of students scoring above 700 on the SAT Math or 30 on the ACT Math. I calculate this share using the 25th and 75th percentiles of the Math SAT and ACT score distribution for entering students from IPEDS. Assuming test scores are distributed normally, I obtain from the percentiles the mean and standard deviation of each test score distribution at each university. Using the normal CDF, and weighting by the percent of students reporting each exam, I calculate the percent at each university scoring above 700 on the Math SAT or above 30 on the Math ACT.[2] I determine the university's regional rank based on this percentage, where regions are defined using the Bureau of Economic Analysis OBE regions (combining New England and the Mideast).

Illustrative Examples: Office Openings in Boston and Houston

To provide further intuition for the results showing heterogeneity by distance, I estimate equation (3) in the paper including only firm/university pairs that experience a nearby office opening in Boston, MA. I also include all firm/university pairs that do not experience move ins during the sample. I implement the same exercise for Houston, TX openings. After firms open a Boston office, students at Boston-area universities are more likely to have on-campus access to these firms through their recruiting activities (Appendix Figure A7). There are five firms opening Boston offices during the sample, and eight instances in the regression sample of these firms

[2]However, if the test score percentiles for a particular test are missing, I assume the weight on the non-missing test is one. Otherwise, I am implicitly assuming that the percent above the threshold on the missing test is zero. Any concerns that this places too much weight on the non-missing test are mitigated because the percent reporting the non-missing test when there is a test with missing percentiles is approximately 87%.

beginning to recruit at universities where they had not recruited before they opened their Boston office.

Second, for universities with relatively similar selectivity, firms highly value proximity. Two firms begin recruiting at Harvard, three at MIT, but none at Brown (10-50 miles away) and one at Dartmouth (50-200 miles away) both of which are also Ivy Plus universities. There is also some evidence that firms appear willing to give up selectivity in order to recruit at closer universities. Firms start recruiting at Tufts and Boston University within 10 miles, but not at Brown, an Ivy League university 10 to 50 miles away. For each of the firms that starts recruiting locally after opening their Boston office, the office previously closest to the university was in New York or New Jersey. Relatively short distances were keeping these firms from recruiting at these highly selective Boston-area universities.

Third, conditional on distance, these firms highly value university selectivity. Among very local universities, Ivy Plus universities experience dramatically larger effects from new Boston offices than other universities, including elite universities.

Similarly, after firms open a Houston office students at Houston-area universities are more likely to have on-campus access to these firms through their recruiting activities. There are three firms opening offices in Houston, and three instances in the regression sample of firms beginning to recruit at local universities which they had not targeted before. More than the Boston plot, this figure suggests firms are willing to give up proximity in order to recruit at universities with some other attribute, especially when there are a limited number of students in the immediate area. More firms start recruiting in Austin (about 150 miles away) rather than at Rice which is in Houston and more selective. Firms are willing to give up on proximity and selectivity to recruit at a university with some other attribute (perhaps most dramatically the number of students). In recruiting at UT Austin (Highly Selective), firms are also passing up University of Houston (Selective) which is immediately next to the new office and Texas A&M which is about 50 miles closer than Austin, and also a large university in Barron's Highly Selective tier.

Do Firms Adjust Recruiting Even if they have an Existing Target Campus Nearby?

The paper assesses the firm's value for distance using the firm-university distance. An additional strategy is to test how the effect of an office opening varies with distance between the firm's new location and its closest existing target. If firms adjust recruiting at local universities even when they have a relatively nearby campus where they have recruiting experience, this also suggests a high value on distance.

I estimate equation (3) in the paper, but additionally include $Post\_r * ClosestTargetFar$ interactions. The variable $ClosestTargetFar$ is an indicator for whether the firm's closest existing target to their new location is at least 250 miles. While the magnitude suggests a positive effect on local recruiting at Ivy Plus, elite, and selective universities even when the firm has an existing target campus relatively nearby, we also cannot rule out zero effect (Appendix Figure A13). However, for highly selective universities, firms are significantly more likely to recruit after a move even when they have a nearby existing target campus. The costs of recruiting slightly farther away at existing target campuses are greater than the benefit of recruiting at a university where the firm has experience.

These estimates have large confidence intervals, and I do not wish to overinterpret the findings. However, the difference across tier may be attributed to differences in the locations yielding new recruiting. For locations yielding new recruiting at highly selective and selective universities, the average size of the urban area is smaller by approximately 500,000.[3] These may be less attractive for applicants at existing target campuses in larger cities, even though they are within 250 miles.[4] Airline or train travel may also be more difficult between these locations, increasing employer search costs.

[3]While the median and higher percentiles of the urban area population are roughly the same for those yielding recruiting at Ivy Plus/elite and highly selective/selective, the lower percentiles are much different.

[4]Appendix Table A9 and Appendix Figure A5 show the relationship between distance to closest existing target campus and recruiting, using a polynomial in distance to closest target.

Selectivity of New Target Campuses

New targets may be especially less selective in smaller markets. First, there are fewer very selective universities in smaller markets. I define large cities as those with urban area population greater than or equal to the 25th percentile of the post-move regression sample (approximately 4.5 million people). The 90th percentile of university selectivity in smaller markets is 36% lower than the 90th percentile in larger markets. New targets may also be less selective in smaller markets because of distance between these markets and existing targets, location preferences in or against these markets, or fewer competing firms.

Given there are only 10 new targets in smaller markets, it is difficult to test for differences between new targets in small versus larger markets. With this caveat, regression results suggest when firms open offices in smaller cities, new targets are 16.8 percentage points less selective than the firm's median target (Appendix Table A8). In large cities, selectivity of new target campuses is slightly lower than their median target campus. The difference between smaller and larger markets is statistically significant at the 10% level.[5]

Some of this may be explained by larger distance to existing targets in smaller markets (column 2), although estimates are imprecise. Conditional on whether the office is opened in a big city, and on the distance to existing targets, there is a positive association between competing firms and the new target's selectivity (column 3). This also reduces the coefficient on the indicator for new office in a large city. The sample size is very small to separate these effects, and to identify a causal relationship. However, it is consistent with firms being willing to add less selective targets when they can offer lower wages due to lower competition. Alternatively, places with fewer competing firms may have fewer selective universities, conditional on population, and firms may always recruit from the most selective universities in the market.

[5]Appendix Table A8 also shows results using ln(urban area population) instead of an indicator for big city.

Is the Value of Proximity Explained by Founders' Alma Mater?

In deciding which employees to charge with opening a new office, firms may choose those who attended universities in the area of the new office. If these alma maters are in the immediate vicinity of the new office, this may explain the very local recruiting rather than employer search costs.

I evaluate this explanation using data collected from LinkedIn. For each firm/city pair where the firm adds a local target campus after opening a nearby office, I collect data on the firms' employees. Specifically, I identify the employees of the firm who currently work in the office that was opened during the sample period. Of these, I identify the employees who were employed by the firm at least one year prior to the year in which this office was opened. This is meant to approximate the set of employees who were sent to open the office, "founders", rather than those who were hired shortly afterward. For these employees, I collect data on their bachelor's and master's universities.

The search yielded data on education for 70 employees at 18 firms, covering 23 of the 40 firm-university pairs that added target campuses after opening a nearby office. Approximately 80% of these new targets were not the alma maters of any of the office "founders". This suggests alumni recruiting does not explain the strong relationship between proximity and recruiting.

Robustness: Driving Distances

The principal distance measure is based on latitude and longitude. For robustness, I use driving distances from Google Maps for a subset of the firm/university pairs. Specifically, this information is collected for firm/university pairs that are in the regression sample and experience at least one move in or one move out, and for whom the distance is within 100 miles. I limit to the pairs within 100 miles as this is mainly a robustness check for how the effects of a move decay with distance.

For each of these university/office city pairs, I use the distance from Google Maps between the university and the city hall of the firms' city. For cities where there is no city hall, I use distance from Google Maps between the university and the city name. I use the Google Maps distance associated with the shortest travel time. For all other pairs, I use the distance measure based on latitude and longitude. Appendix Figure A6 shows the equivalent of Figure 5 in the paper, instead using the Google Maps distance if it was collected.

Robustness: Balanced Samples

I report results from separate regressions which analyze office openings that occur in two-year windows: $t^* \in [y_1, y_1 + 1]$, starting with $y_1 = 2002$.[6] I require balance from $t^* - 2$ through $t^* + 1$. Given that I require balance on calendar year, this implies that I require every firm-university pair to have data starting in $y_1 - 2$ through $y_1 + 2$.[7] In each regression I include firm-university pairs with $t^* \in [y_1, y_1 + 1]$ as well as those that never experience moves during the sample.[8] Given the small number of firms in each regression, I cluster at the university level rather than presenting two-way clustered standard errors at the firm and university level. However, the principal results showed little difference in the two-way clustered standard errors relative to the standard errors clustered at the university level.

Not surprisingly, restricting the data in this way leads to a reduction in power. However, the results still suggest positive effects by $t^* + 1$ for $t^* \leq 2007$, though only precise for moves in 2002 and 2003. The average of the effects across the regressions are of roughly similar magnitude as the results in Appendix Figure A8, which also uses the sample not requiring firms to have recruited that year. There are also no pretrends. The results are closer to zero for move ins during and after the Great Recession, consistent with smaller results using the main sample during this period.

Given the number of office closings is much smaller, these balance restrictions result in analyzing very few events. To maximize the number of events I minimize the balance requirements by estimating separate regressions which analyze office closings in a given year: $t^* = y$. I require balance in $t^* - 2$, $t^* - 1$, and $t^* + 4$, since the results

[6] Given the sample years, one year group must include three years, and I group together three years surrounding the Great Recession–2008-2011. Appendix Table A17 also shows results from separate regressions for each move in year, requiring balance in $t^* - 2$, $t^* - 1$, and $t^* + 1$.

[7] For example, for the 2002-2003 year group, I require every firm-university pair has data from 2000 through 2004, so that the firm-university pairs in the data are the same for $t^* - 2$ through $t^* + 1$ for $t^* = 2002, 2003$.

[8] I also include those with $t^* \geq y_2 + 5$, so that the event study coefficients for those pairs do not contribute to the coefficients on $t^* - 2$ through $t^* + 1$, but they do contribute to firm-year and university-year fixed effects.

in Appendix Figure A8 show negative effects of closings starting around this period. Requiring balance on calendar year implies every firm-university pair has data in the same three years: $y-2, y-1, y+4$.[9] Even setting the restrictions in this way yields very few office closings per year. We see large, negative effects of closings in 2003 through 2005 that are precise in two of those years. Effects are closer to zero when the closings occur during or just before the Great Recession.

Robustness: What do Firms Sacrifice for Proximity

The principal results use discrete bins of distance and selectivity tiers to evaluate how firms value proximity relative to selectivity. In this section, I present specifications using the proportion of high-test score students as a measure of selectivity, and a polynomial in distance. This allows me to more finely evaluate the proximity-selectivity tradeoff, for example the difference in selectivity needed to compensate the firm for recruiting further away.

First, I test how the effects of an office opening decay with distance between the firm and university after the move, controlling for university selectivity and the distance before the move. Specifically, I estimate:

$$\begin{aligned} Recruit_{fjt} = \alpha_0 + \alpha_{fj} + \delta_{ft} + \kappa_{jt} + \beta Post_{fjt} + \gamma Post_{fjt} * f(PostDist_{fj}) \\ + \rho Post_{fjt} * f(PreDist_{fj}) + \kappa Post_{fjt} * ln(Selectivity_j) + \epsilon_{fjt} \end{aligned} \quad (1)$$

Because there were not dramatic differences in short- and long-run effects, for simplicity I include only an indicator for the years following an opening. I interact $Post$ with a cubic in firm-university distance after the move, and a cubic in firm-university distance before the move. Recall $Post_{fjt} = 1$ only if the firm-university distance after the move is less than 200 miles. The coefficients γ identify the differential effect of an office opening by distance after the move, conditional on opening an office within 200 miles of the university. I also interact $Post$ with the log of the university's selectivity

[9] For the regression studying closings in 2003, all firm-university pairs (both those experiencing moves in 2003 and those not experiencing moves in 2003) must have data in 2001, 2002, and 2007. In each regression I include firm-university pairs with $t^* = y$, as well as pairs that never experience a move out during the sample, as well as those with $t^* \geq y+7$ so the event-study coefficients do not contribute to the coefficients in t^*-2, t^*-1, or t^*+4.

(in 2006). Selectivity is the proportion of students scoring above 700 on the SAT Math or 30 on the ACT math. I show the results in Appendix Figures A4 and A5 and Appendix Table A9.

I use a similar specification to test how the effect of an office opening varies with distance between the firm's new location and its closest existing target. I estimate:

$$\begin{aligned} Recruit_{fjt} = \alpha_0 + \alpha_{fj} + \delta_{ft} + \kappa_{jt} + \beta Post_{fjt} + \gamma Post_{fjt} * f(PostDist_{fj}) \\ + \rho Post_{fjt} * f(ClosestTargetDist_{fj}) + \kappa Post_{fjt} * ln(Selectivity_j) + \epsilon_{fjt} \end{aligned} \quad (2)$$

This is similar to regression (1) but includes a cubic in distance between the firm and the closest existing target, rather than the firm-university distance before the move. I show the results in Appendix Figure A5 and Appendix Table A9.

Heterogeneity Exercises

As discussed in the paper, I test whether the effect of a move on recruiting differs for the highest-ten-ranked firms in the sample. Including the highest-ten-ranked firms in the sample implies I include ranks worse than 10 because some of the high-ranked firms are not in the regression sample. Limiting the regressions to the top ten ranked firms by Vault reduces the sample, but generally yields similar results. Booz & Company is not included in the regressions by rank because it spun off one of the original Vault-listed firms.

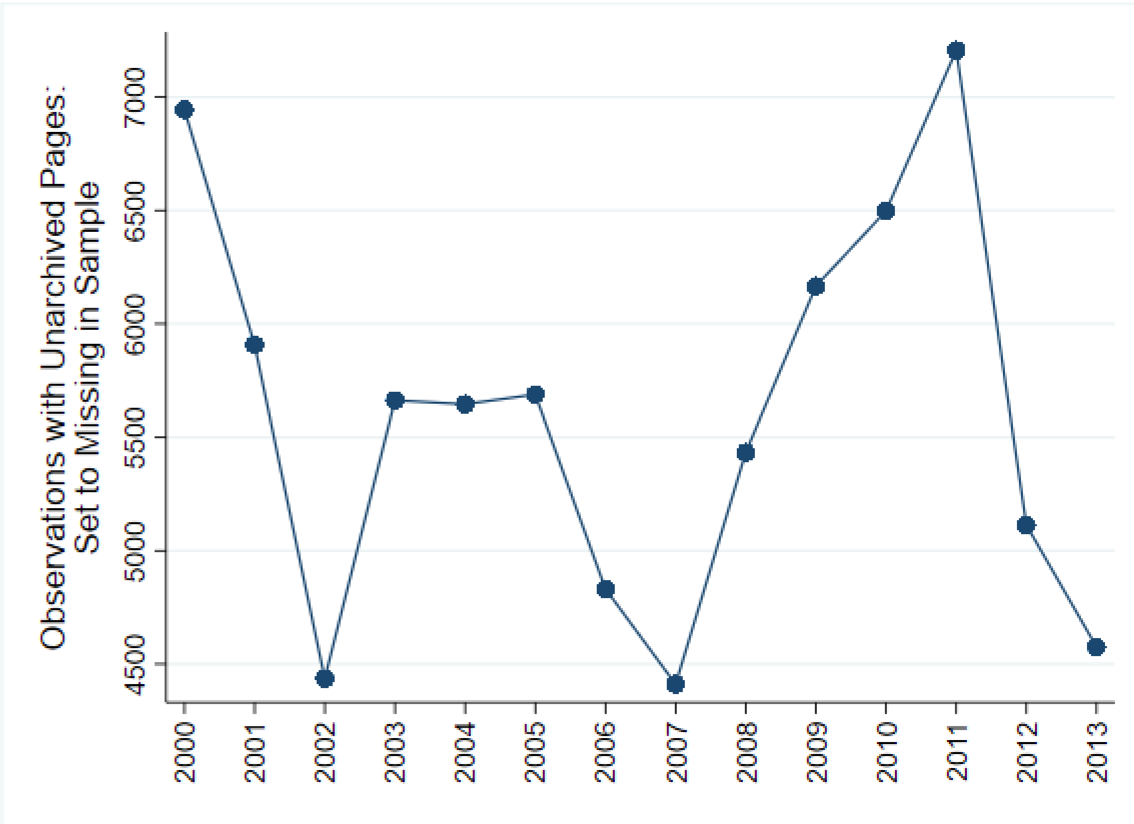
I report in the paper that I find no statistically significant differences in the short-run effect when the firm opens or closes an office in a larger city. However, the long-run effect of a move on recruiting is significantly larger if the firm opened an office in a larger city, but this appears due to composition. Among pairs with data at least five years after the move, the pre-move likelihood of recruiting is higher for those in which the firm opened an office in a larger city. This is not true when not restricting to pairs with data in the long run.

Calculating Average Number of Hires from University of Michigan

With some assumptions, the firms in my sample hiring from University of Michigan Ross School of Business hire an average of 3 bachelor's in business (BBA) students into fulltime jobs. As described in the paper, the annual report for the school in 2007 lists hires by firm for firms hiring at least 10 students from the business school, for either fulltime or internship positions from any degree program.

Among the firms in the main regression sample, seven consulting firms and four banks hire at least ten Michigan students and so I observe actual hires for these firms. There are 12 banking firms and 6 consulting firms in the main sample hiring or recruiting from University of Michigan Ross School of Business, but hiring fewer than 10 students across all degree levels. For these firms I do not know actual number of BBA hires. Using the actual BBA hires for the seven consulting firms and five banking firms for which I have data, I calculate the percent of all hires that are BBA versus MBA students, separately for finance and consulting firms. For finance firms this is .5 and for consulting firms this is .33. To calculate the average BBA hires for the firms in my data at University of Michigan, for firms where I do not know the exact number of hires I assume these firms hire five total students and of those five they hire the same percent BBA students as those in the same industry for which I have data. Because of data availability, these numbers also do not include any hires from schools other than the business school at University of Michigan.

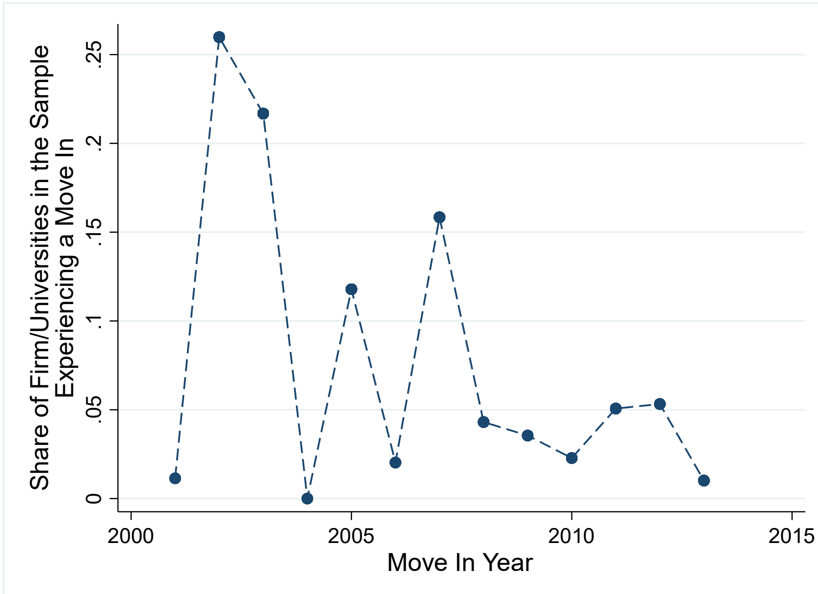
Appendix Figure A1: Observations with Unarchived Pages by Year



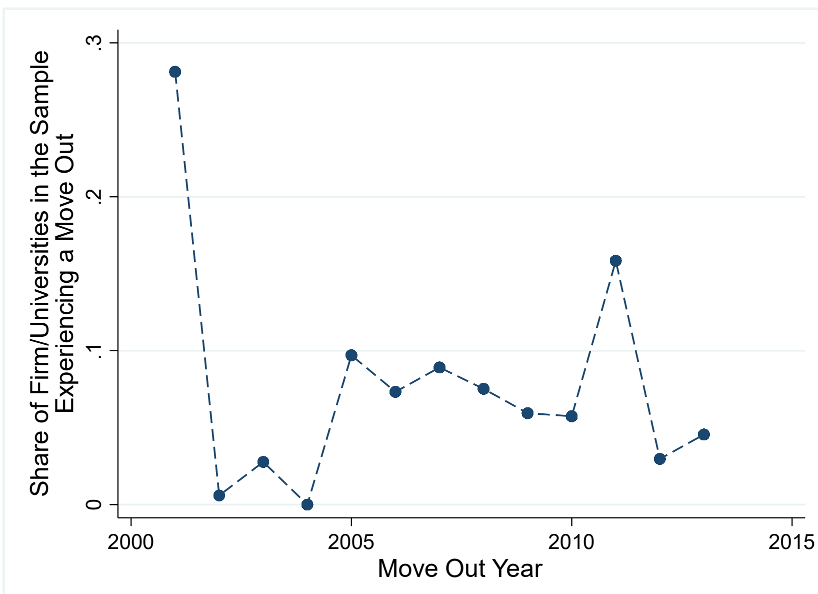
Note: This plot shows the number of firm/university pairs in each year whose recruiting page was not archived. I set the *Recruit* variable equal to missing for these observations.

Appendix Figure A2: Office Openings and Closings by Year

(a) Office Openings by Year



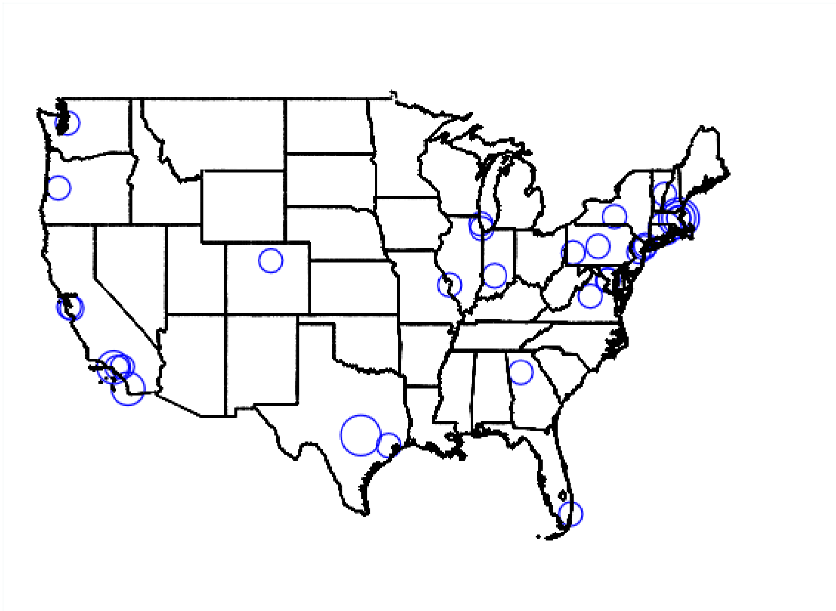
(b) Office Closings by Year



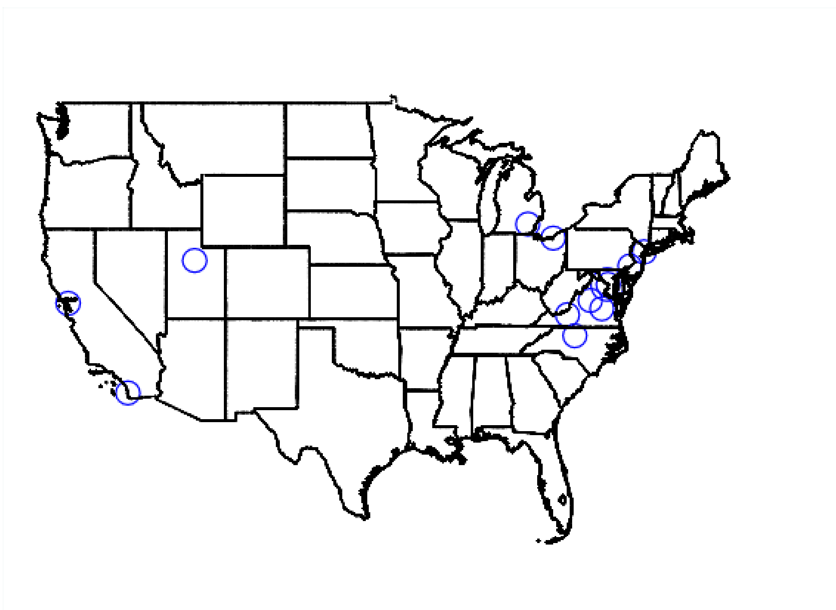
Note: These figures show histograms of the year in which firm/university pairs experience openings (a) and closings (b) among the pairs in the sample that experience openings (a) and closings (b). I limit to the first move in and first move out experienced by each firm/university pair. See text for details.

Appendix Figure A3: Changes in Recruiting Following Office Openings and Closings

(a) Universities Attracting Recruiting Firms After Nearby Office Openings



(b) Universities Losing Recruiting Firms After Nearby Office Closings

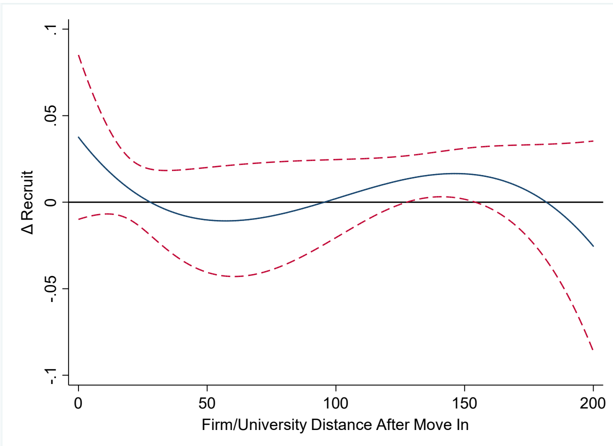


Note: Plot (a) shows the universities that began attracting a recruiting firm after it opened a nearby office, but had not attracted this firm before the office opening. Plot (b) shows the universities that had attracted the firm at least once before it closed its local office, but did not attract the firm at least once after the office closing. See text for details.

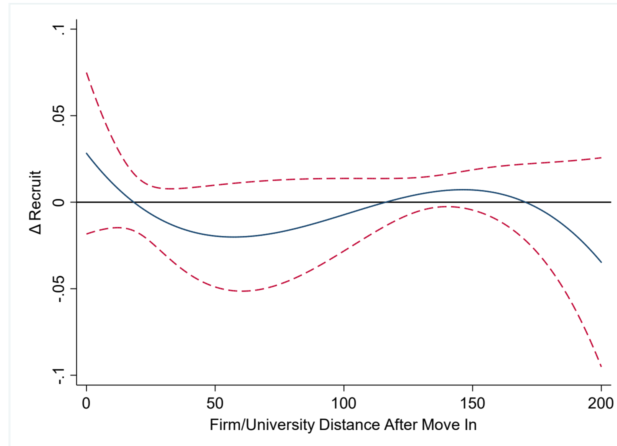
Appendix Figure A4

Effect of Nearby Office Openings on Recruiting, by Firm-University Distance After the Move

(a) Median-Selectivity Universities



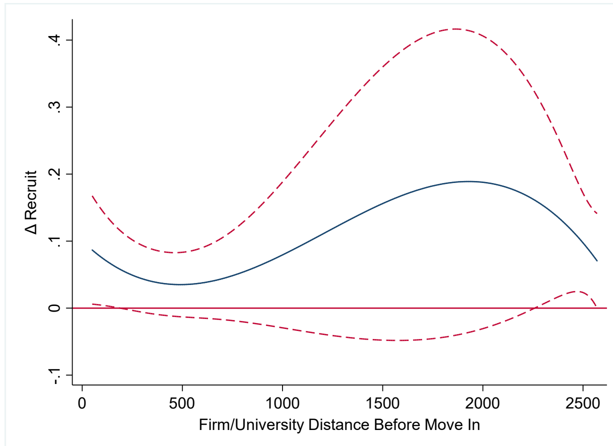
(b) University Selectivity at the 25th Percentile



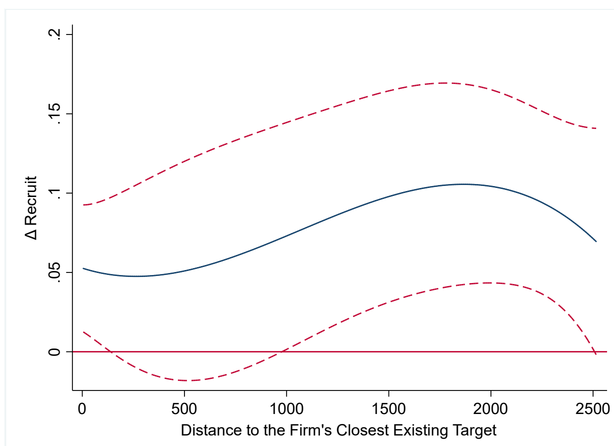
Note: Plots show linear combinations of regression coefficients for median (a), or 25th percentile (b) selectivity universities, whose pre-move-in distance was at the median for the sample, for different levels of the post-move-in distance. Regressions include firm-university, firm-urban area of closest office-year, and university-year fixed effects, an indicator for post-move, and this indicator interacted with a cubic in pre-move distance, a cubic in post-move distance, and ln(university selectivity). Dashed lines are 95% confidence intervals for these combinations. See paper for details.

Appendix Figure A5

(a) Effect of Nearby Office Changes on Recruiting, by Firm-University Distance Before the Office Opening



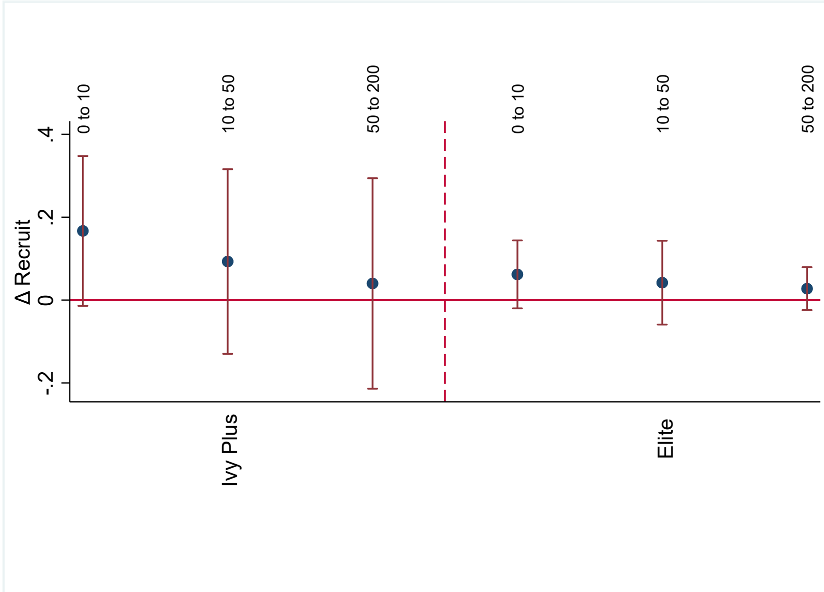
(b) Effect of Nearby Office Changes on Recruiting, by Firm Distance to its Closest Existing Target Campus, After Office Openings



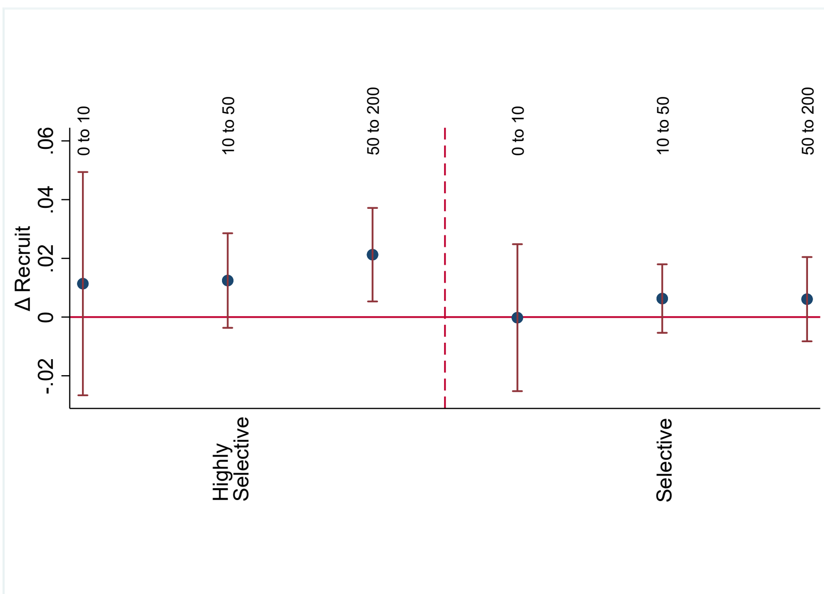
Note: Plot (a) shows linear combinations of regression coefficients for median-selectivity universities, whose post-move-in distance was zero, for different levels of the pre-move-in distance. Regressions include firm-university, firm-urban area of closest office-year, and university-year fixed effects, an indicator for post-move, and this indicator interacted with a cubic in pre-move distance, a cubic in post-move distance, and ln(university selectivity). These regressions are the same as those described in Appendix Figure A4. Plot (b) shows linear combinations of regression coefficients for median-selectivity universities, whose post-move-in distance was zero, for different levels of distance between the firm and its closest existing target campus to its new location. Regressions include firm-university, firm-year, and university-year fixed effects, an indicator for post-move, and this indicator interacted with a cubic in post-move distance, a cubic in distance between the new office location and the closest existing target campus, and ln(university selectivity). Dashed lines are 95% confidence intervals for these combinations. See paper for details.

Appendix Figure A6: Effect of Nearby Office Openings, by Post-Move Distance using Driving Distance

(a) Ivy Plus and Elite Universities



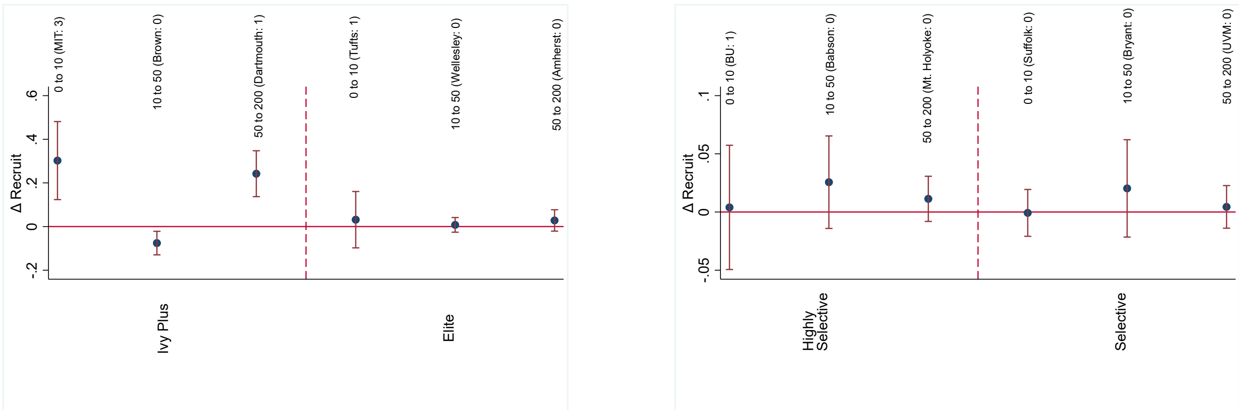
(b) Highly Selective and Selective Universities



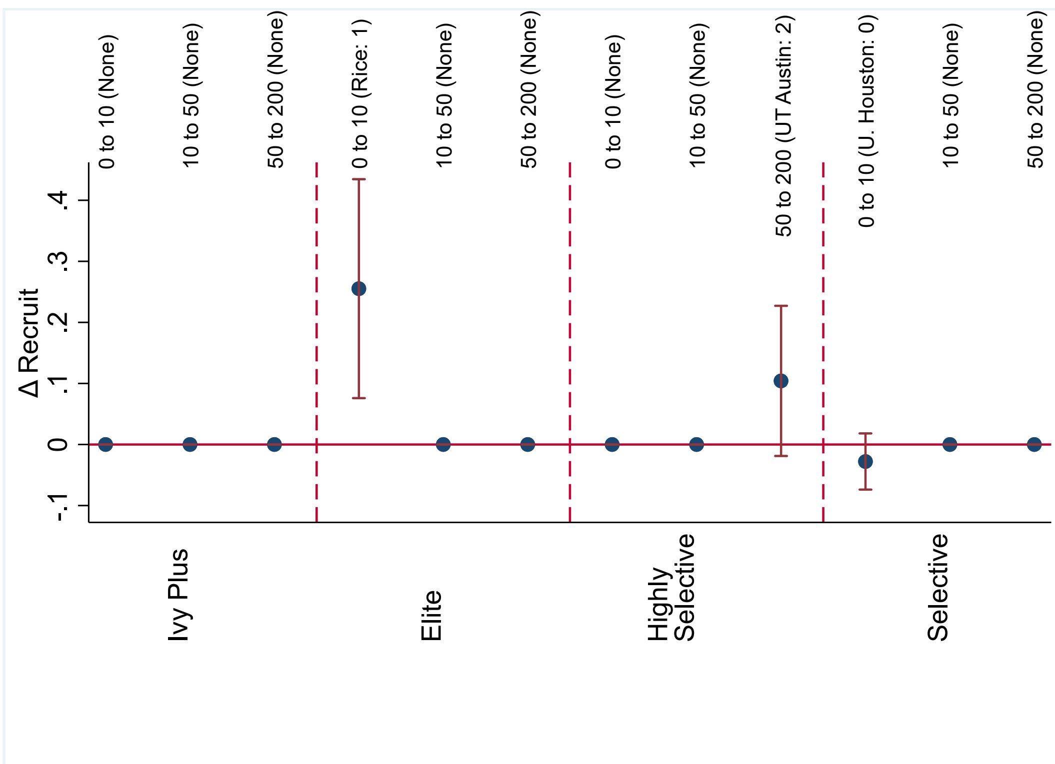
Note: This plot is equivalent to Figure 5 in the paper, but uses driving distances from Google Maps for firm/university pairs that experienced a move in, and were within 100 miles of each other using distance based on latitude and longitude. For all other pairs, I use distance based on latitude and longitude. Bars are 95% confidence intervals. See appendix and Figure 5 for details.

Appendix Figure A7: Illustrative Examples: Effect of Office Openings on Recruiting, by Distance to University

(a) New Offices in Boston, MA



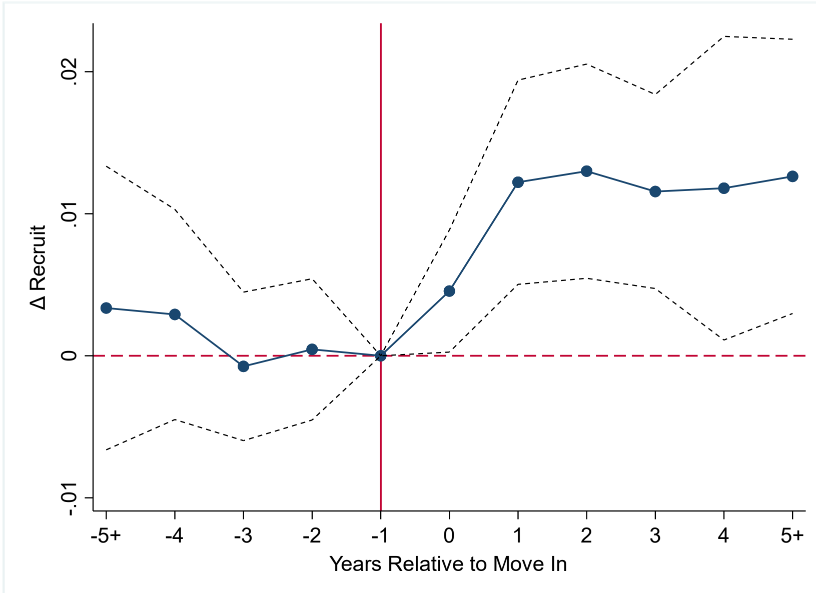
(b) New Offices in Houston, TX



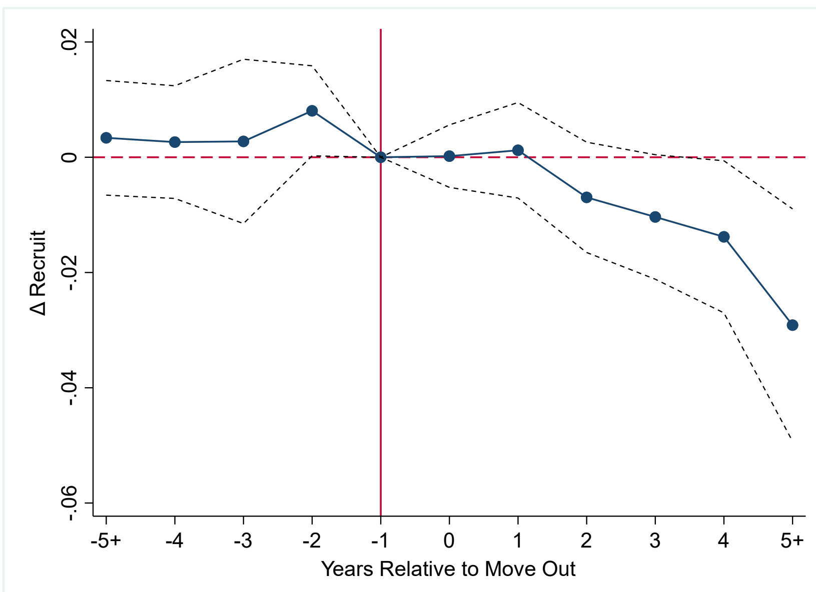
Notes: Coefficients in these plots are similar to those in Figure 5, but include only firm/university pairs for which the firm's new office was in Boston, MA (a), or Houston, TX (b), as well as all firm/university pairs that did not experience any move ins during the sample period. I list one example university for each radius and selectivity tier, and list the number of firms starting to recruit there following a move in parentheses. In cases with multiple universities per radius/tier, I list the university attracting the most firms after moves if any exist. Bars show 95% confidence intervals. See text and Figure 5 notes for details.

Appendix Figure A8: Changes in Recruiting After Office Openings and Closings, Sample Including Firm-University-Year Pairs in which the Firm Does not Recruit at any University that Year

(a) Office Openings



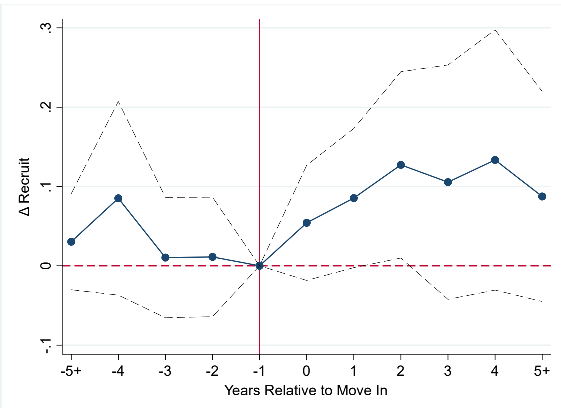
(b) Office Closings



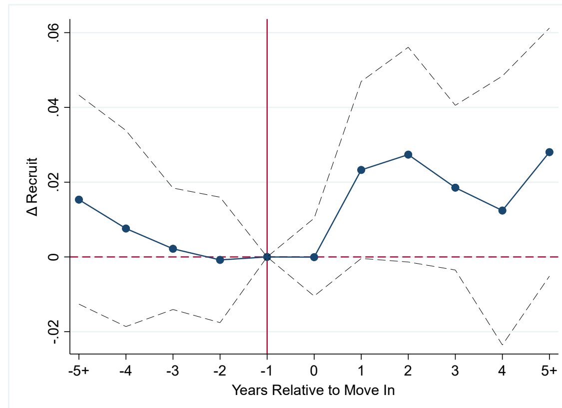
Note: These figures are similar to those in Figure 3, but using the robustness sample including firm-university-year pairs even if the firm is not recruiting on any campus that year. Dashed lines show 95% confidence intervals. See text and Figure 3 for details.

Appendix Figure A9: Changes in Recruiting After Office Openings, by University Tier, Sample Including Firm-University-Year Pairs in which the Firm Does not Recruit at any University that Year

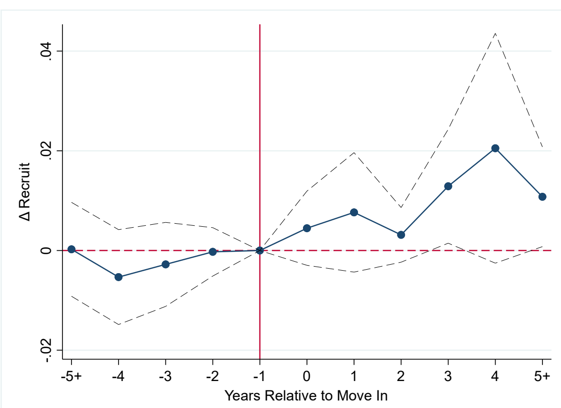
(a) 12 Ivy Plus Universities
Mean $Recruit_{t-1}$ = .023



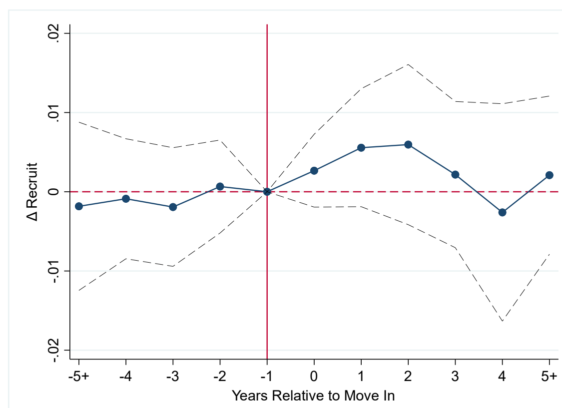
(b) 61 Barron's Tier 1 Universities, Excluding Ivy Plus
Mean $Recruit_{t-1}$ = .009



(c) 92 Barron's Tier 2 Universities (Highly Selective)
Mean $Recruit_{t-1}$ = .004



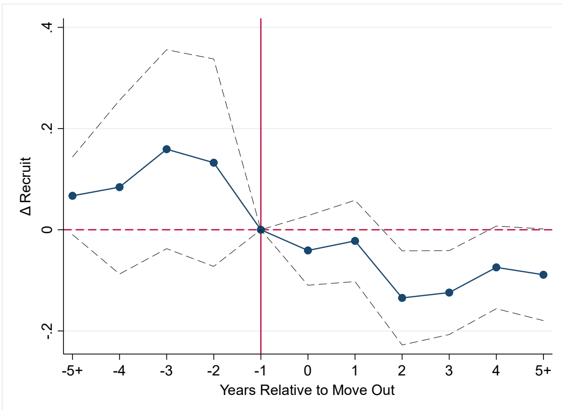
(d) 185 Barron's Tiers 3-5 Universities (Selective)
Mean $Recruit_{t-1}$ = .001



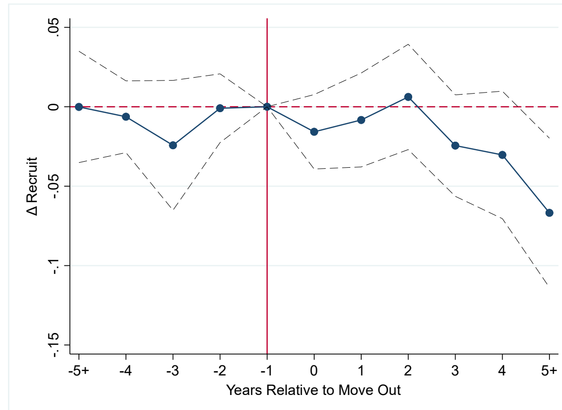
Note: These figures are similar to those in Figure 4, but using the robustness sample including firm-university-year pairs even if the firm is not recruiting on any campus that year. Dashed lines show 95% confidence intervals. See text and notes to Figure 4 for details.

Appendix Figure A10: Changes in Recruiting After Office Closings, by University Tier, Sample Including Firm-University-Year Pairs in which the Firm Does not Recruit at any University that Year

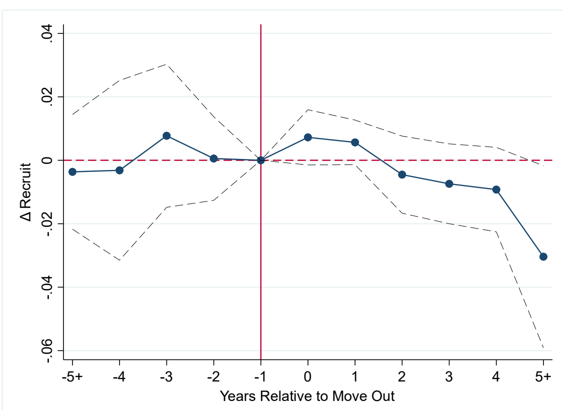
(a) 12 Ivy Plus Universities
Mean *Recruit*$_{t-1}$ = .089



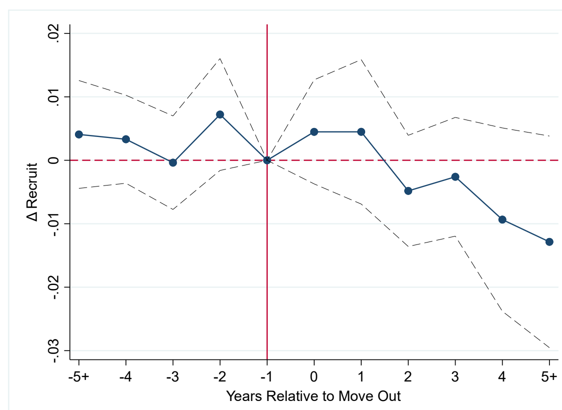
(b) 61 Barron's Tier 1 Universities, Excluding Ivy Plus
Mean *Recruit*$_{t-1}$ = .047



(c) 92 Barron's Tier 2 Universities (Highly Selective)
Mean *Recruit*$_{t-1}$ = .014



(d) 185 Barron's Tiers 3-5 Universities (Selective)
Mean *Recruit*$_{t-1}$ = .003

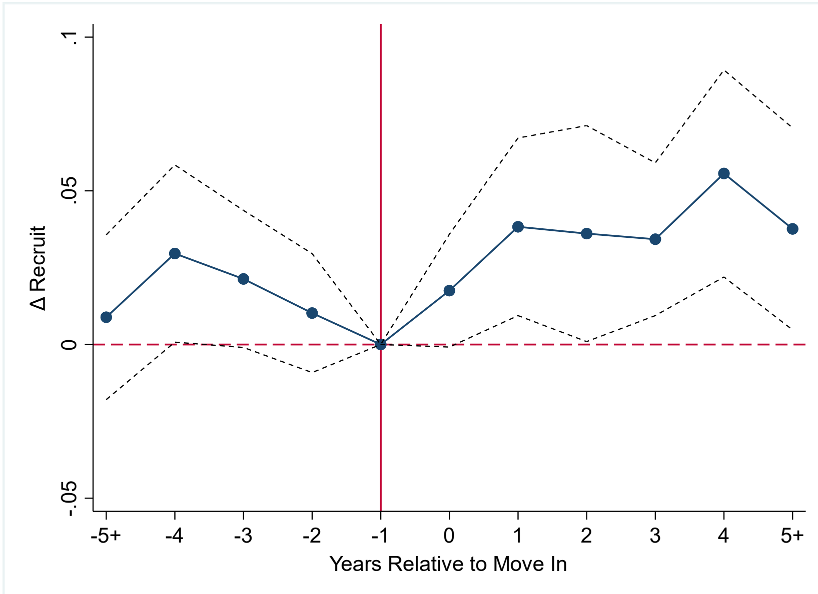


Note: These figures are similar to those in Appendix Figure A12, but using the robustness sample including firm-university-year pairs even if the firm is not recruiting on any campus that year. Dashed lines show 95% confidence intervals. See text and notes to Appendix Figure A12 for details.

Appendix Figure A11: Defining Moves Using Firm and University Commuting Zones

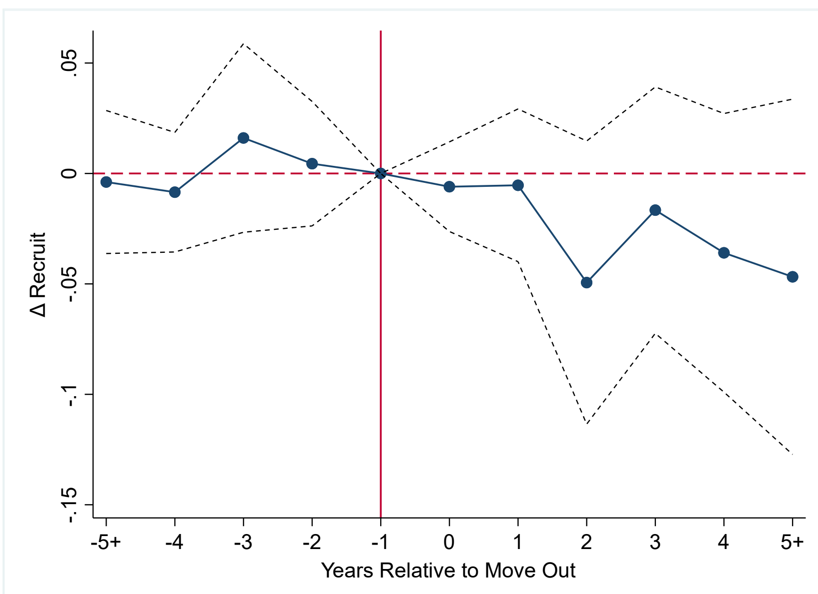
(a) Office Openings and Recruiting at Local Universities

Mean *Recruit*$_{t-1}$ = .026



(b) Office Closings and Recruiting at Local Universities

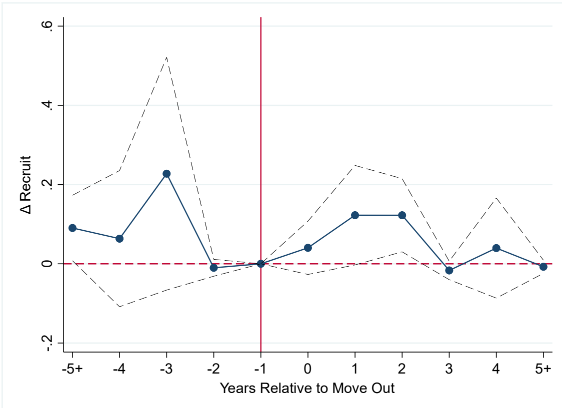
Mean *Recruit*$_{t-1}$ = .047



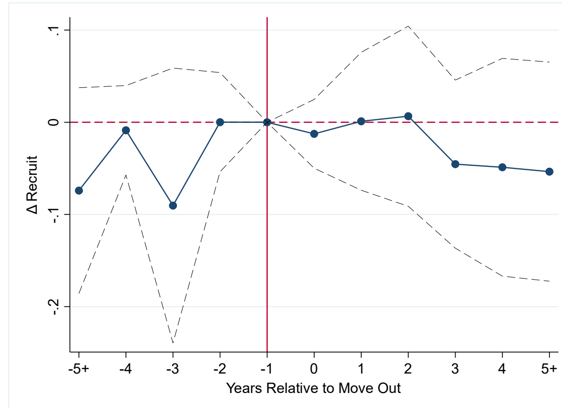
Note: These figures show the results of a regression of *Recruit* on indicators for period from the move in (a) or move out (b). Regressions are analogous to those estimated for Figure 3, except for the definition of moves. Move ins are instances in which a firm locates within the same commuting zone as the university. Move outs are instances in which the firm leaves the university's commuting zone. I limit to the first move in and first move out experienced by a firm/university pair. The dependent variable in the regression is an indicator for whether firm *f* recruits at university *j* in time *t*. The regression includes firm-university pair fixed effects, firm-year fixed effects, and university-year fixed effects. To be in the regression sample, firm-university pairs experiencing moves must have data in *t-1*. Dashed lines show 95% confidence intervals. See text for details.

Appendix Figure A12: Changes in Recruiting After Office Closings, by University Tier

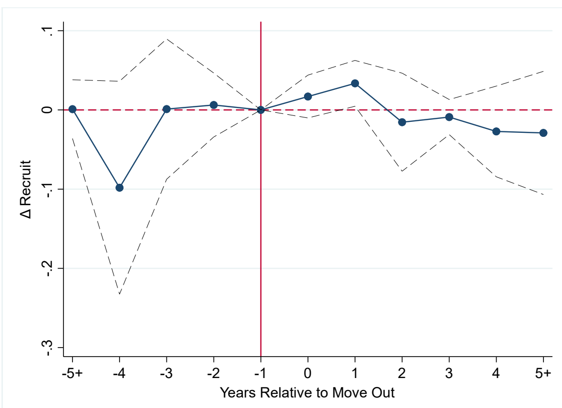
(a) 12 Ivy Plus Universities
Mean $Recruit_{t-1}$ = .22



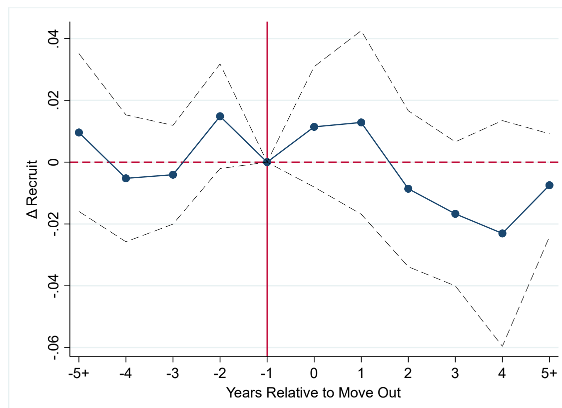
(b) 61 Barron's Tier 1 Universities, Excluding Ivy Plus
Mean $Recruit_{t-1}$ = .10



(c) 92 Barron's Tier 2 Universities (Highly Selective)
Mean $Recruit_{t-1}$ = .037



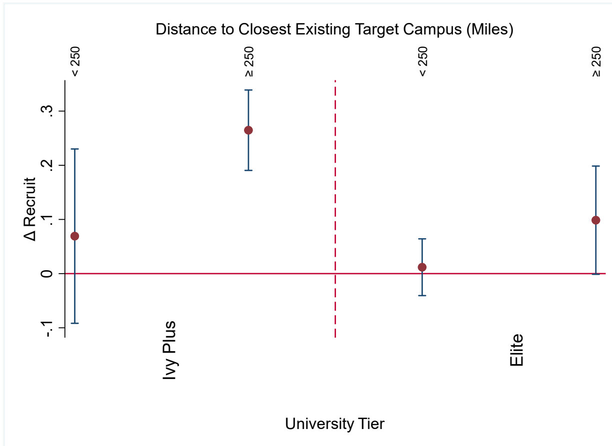
(d) 185 Barron's Tiers 3-5 Universities (Selective)
Mean $Recruit_{t-1}$ = .007



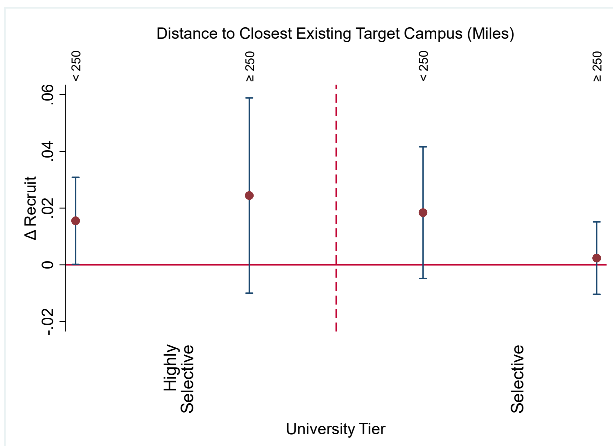
Note: Figures are all from the same regression of *Recruit* on indicators for period from the year in which firm *f* moved farther from *j*, interacted with university tier. Categories of university tier are: "Elite" (Barron's Tier 1 excluding Ivy Plus), "Highly Selective" (Barron's Tier 2), "Selective" (Barron's Tiers 3-5), and "No Tier/Insufficient Information"; Ivy Plus is the omitted category. The regression includes firm-university pair fixed effects, firm-year fixed effects, and university-year fixed effects. To be in the regression sample, firm-university pairs experiencing moves must have data in *t-1*. Sample sizes for the event-study coefficients are very small. For Ivy Plus universities, the coefficient on t* is the only post-move coefficient with a sample size over ten. While sample sizes are larger for other tiers, they are still relatively small and in some post-move years are between 20 and 40. For selective universities, the magnitude becomes less negative in t* + 5+ , likely because only one of the universities losing access still appears in the regression sample five or more years after the move. Dashed lines show 95% confidence intervals. See text and notes to Figure 3 for details.

Appendix Figure A13: Distance to Closest Existing Target Campus and Recruiting After Office Opening

(a) Ivy Plus & Elite Universities



(b) Highly Selective & Selective Universities



Note: Figures (a) and (b) are from the same regression, and show linear combinations of regression coefficients for different levels of distance to the firm's closest existing target campus, by university tier. The regression is similar to that for Figure 5e and 5f, but instead of interactions with pre-move distance ≥ 250 miles, I interact with an indicator for whether the closest existing target campus was ≥ 250 miles away after the move. Bars show 95% confidence intervals. See paper for details.

Appendix Table A1: Firms in Sample, by Year

	Total Years in Full Sample	Years in Full Sample	Total Years in Main Sample	Years in Main Sample	Reason Missing Years in Full Sample?	Vault Rank
Consulting Firms						
Oliver Wyman	13	2001-2013	13	2001-2013	No page archived	7
ZS Associates	12	2000-2005, 2007-2012	12	2000-2005, 2007-2012	No page archived	28
Dean & Company	11	2000-2009, 2011	11	2000-2009, 2011	2010: No location, 2012-2013: No page archived	41
NERA Economic Consulting	11	2000, 2003, 2005-2013	11	2000, 2003, 2005-2013	Blocked Robots	22
Bain & Company	10	2000-2007, 2011-2012	10	2000-2007, 2011-2012	No page archived	3
PRTM	11	2000-2010	10	2001-2010	Acquired by PwC in 2011	43
The Boston Consulting Group	12	2001-2005, 2007, 2009-2013	10	2001-2004, 2007, 2009-2013	2006: Singleton 2008: No page archived	2
Corporate Executive Board	10	2000-2008, 2010	9	2000-2007, 2010	Error loading page	34
Marakon	13	2000-2001, 2003-2013	9	2003-2006, 2008-2012	Contact university	12
First Manhattan Consulting Group	12	2000-2008, 2010-2012	8	2000, 2002-2007, 2010	No page archived	35
Huron Consulting Group	12	2002-2013	8	2004-2005, 2007-2011, 2013	Formed in 2002	47
Analysis Group	8	2006-2013	7	2007-2013	Error loading page	40
Parthenon Group	13	2000-2006, 2008-2013	7	2000, 2008-2013	Contact university	9
Booz Allen Hamilton	7	2000, 2007-2009, 2011-2013	6	2000, 2007-2009, 2011-2012	Error loading page	4
Gallup	12	2000-2003, 2005, 2007-2013	6	2008-2013	No page archived	38
McKinsey & Company	6	2007-2009, 2011-2013	6	2007-2009, 2011-2013	2001-2002: Contact University 2004-2006: Blocked Robots	1
Putnam Associates	12	2000-2009, 2011-2012	6	2005-2008, 2011-2012	No page archived	46

	Total Years in Full Sample	Years in Full Sample	Total Years in Main Sample	Years in Main Sample	Reason Missing Years in Full Sample?	Vault Rank
Cornerstone Research	9	2000-2004, 2006, 2008, 2011-2012	5	2001-2004, 2008	Blocked Robots	33
Kurt Salmon	8	2000, 2005-2011	5	2005-2008, 2010	2001: Contact university 2002-2004: Error loading website	36
Mitchell Madison Group	11	2003-2013	5	2005-2008, 2012	No website found	48
Navigant	6	2005-2007, 2010, 2012-2013	5	2005-2007, 2012-2013	Blocked Robots	32
A. T. Kearney	10	2004, 2006-2013	4	2004, 2006, 2008, 2010	2000: No page archived, 2001-2003: Contact firm, 2005: Singleton	14
Cambridge Associates	6	2000-2001, 2009-2011, 2013	4	2009-2011, 2013	No page archived	23
Hewitt Associates	9	2000-2004, 2006-2009	4	2000-2003	2005: No page archived, 2010: Data combined with Aon	18
OC&C Strategy Consultants	7	2004-2007, 2011-2013	4	2006-2007, 2012-2013	2000-2003: Broken links; 2008: Contact university; 2009-2010: No page archived	45
Advisory Board	4	2000, 2002, 2012-2013	3	2000, 2012-2013	No page archived	39
Booz & Company	6	2008-2013	3	2010-2012	Split from Booz Allen Hamilton in 2008	NR
FTI Consulting	7	2004-2007, 2009, 2012-2013	3	2007, 2012-2013	2001-2003, 2008: Error loading page 2010-2011: Contact university	50
Accenture	2	2012-2013	2	2012-2013	Contact university	16
Arthur D. Little	9	2003-2008, 2010, 2012-2013	2	2008, 2010	No page archived	30
Charles River Associates	5	2000-2001, 2010, 2012-2013	2	2000, 2013	Error loading page	24
Mercer	3	2004, 2006, 2008	2	2004, 2006	2000-2003: No page archived; 2007, 2009-2013: No Location;	8
Capgemini	3	2002, 2004, 2013	0	None	Contact university	13 (27?)

	Total Years in Full Sample	Years in Full Sample	Total Years in Main Sample	Years in Main Sample	Reason Missing Years in Full Sample?	Vault Rank
Gartner Inc.	11	2000-2002, 2004, 2007-2013	0	None	No page archived	15
Giuliani Partners	11	2002-2008, 2010-2013	0	None	Formed in 2002	42
L. E. K. Consulting	8	2001-2008	0	None	Blocked Robots	11
LECG Corporation	4	2000, 2008-2010	0	None	Liquidated in March, 2011	29
Mars & Co.	12	2000-2003, 2005-2011, 2013	0	None	No page archived	25
Monitor Group	3	2000, 2011-2012	0	None	Acquired by Deloitte in January, 2013	5
PA Consulting Group	9	2003-2005, 2007, 2009-2013	0	None	No page archived	49
Roland Berger	9	2001-2002, 2006-2009, 2011-2013	0	None	No page archived	17
Stern Stewart & Co.	7	2001-2006, 2010	0	None	No page archived	37
Banks						
Lazard	11	2000-2010	10	2001-2010	2011-2013: Contact university	8
Raymond James Financial	12	2000-2002, 2004-2010, 2012-2013	8	2000, 2002, 2004-2009	2003, 2011: No page archived	41
Wachovia	8	2000-2007	8	2000-2007	2008: Acquired by Wells Fargo	18
ABN AMRO	8	2000-2007	7	2000-2002, 2004-2007	2007: Acquired	40
Jefferies & Company	14	2000-2013	7	2005-2010, 2013		22
Morgan Stanley	10	2001-2002, 2005-2009, 2011-2013	6	2002, 2006-2009, 2011	2000: No page archived 2003-2004: Error loading page 2010: No page archived	3
Piper Jaffray Companies	9	2000-2005, 2007, 2010, 2012	6	2001-2003, 2007, 2010, 2012	2006, 2008-2009, 2011: No page archived	27

	Total Years in Full Sample	Years in Full Sample	Total Years in Main Sample	Years in Main Sample	Reason Missing Years in Full Sample?	Vault Rank
Cowen Group	7	2000-2006	5	2000, 2003-2006	2007-2010: No page archived 2011-2012: Contact university 2013: No page archived	39
William Blair & Company	7	2001-2004, 2006, 2012-2013	5	2001-2004, 2006	2000: No location 2005: Mentions recruiting, but says positions filled 2007-2011: No page archived	36
Bank of America	4	2006-2007, 2012-2013	4	2006-2007, 2012-2013	2000-2005, 2008, 2010: No location	15
Citi	10	2000-2009	4	2000-2001, 2003, 2007	2010-2011: Blocked Robots 2012-2013: No page archived	7 (13)
Macquarie Group	9	2000-2004, 2006-2009	4	2006-2009	2005: Contact university	47
Rothschild	9	2002-2003, 2005-2008, 2011-2013	4	2005, 2011-2013	2000-2001: Error loading page 2009-2010: Blocked robots	19
Deutsche Bank	7	2001-2003, 2008-2011	3	2008, 2010-2011	2000, 2004-2007, 2012-2013: No page archived	12
Evercore Partners	10	2000-2006, 2010-2012	3	2004, 2011-2012	2007-2009: No page archived	25
Gleacher & Company	13	2000-2005, 2007-2013	3	2010-2012	2006: No location	45
U.S. Bancorp	11	2002-2004, 2006-2013	3	2006, 2009-2010	2000-2001: No page archived	46
Brown Brothers Harriman	6	2000-2005	2	2002, 2005	2006-2013: No page archived	37
Perella Weinberg Partners	6	2006-2009, 2012-2013	2	2012-2013	Founded in 2006 2010-2011: Contact university	23
Robert W. Baird & Co.	5	2007-2011	2	2007-2008	2000-2006: No page archived 2012-2013: Contact university	42
Thomas Weisel Partners Group	2	2008-2009	2	2008-2009	2000-2007: No location 2010: Acquired by Stifel Financial	28
Allen & Company	6	2007-2008, 2010-2013	0	None	2000-2006, 2009: No page archived	33
Barclays	5	2009-2013	0	None	2000-2008: No US locations	17
BNP Paribas	4	2001-2002, 2006, 2013	0	None	2000: Error loading page 2003-2005, 2008-2012: No page archived	34
Greenhill & Co.	8	2006-2013	0	None	2000-2005: No page archived	16

	Total Years in Full Sample	Years in Full Sample	Total Years in Main Sample	Years in Main Sample	Reason Missing Years in Full Sample?	Vault Rank
Houlihan Lokey	2	2007, 2009	0	None	2000-2005, 2010-2013: No location 2008: Page unarchived	21
HSBC	10	2004-2013	0	None	2000-2001: No page archived 2002-2003: No page archived	20
JP Morgan Chase & Co.	3	2000, 2006-2007	0	None	2001-2002: No page archived 2003: No Location 2004: No page archived 2005, 2008-2010: No page archived 2011-2013: Blocked robots	5 (11)
Keefe Bruyette & Woods	14	2000-2013	0	None		38
Morgan Keegan & Co.	12	2001-2012	0	None	2000, 2013: No location	44
RBC Capital Markets	2	2012-2013	0	None	2000-2001: No website found 2002-2005: No location 2006-2009: No page archived 2010-2011: No page archived	29

Note: The explanation "No page archived" may reflect that there is no recruiting page at all or that the archived recruiting page does not have the necessary information (i.e. discusses recruiting but not specific target campuses). The explanation "No location" may reflect that the firm's locations were unarchived, or inconsistencies in how/what type of locations were reported. The explanation "Contact university" reflects that the firm tells interested students to contact their university to determine if the firm recruits on their campus. The explanation "Blocked robots" reflects that the site blocked access to automated web crawlers. The explanation "Singleton" reflects there was only one observation for the firm in that year. Vault Rank is the rank from 2007 for consulting firms, and from 2008 for banking firms because the 2007 banking ranking contained very few firms. The question mark in the rank cell for Capgemini is because the firm was included twice in the rankings. The difference between the full and main sample is that in the main sample I require the firm recruited on at least one campus in the given year.

Appendix Table A2: The Effect of Office Openings and Closings on Recruiting at Local Universities

Outcome: Recruit		
	Panel A: Move Ins	
(t=t)Move*		0.014**
	Pairs with data: 734	(0.006)
(t=t + 1)Move*		0.020***
	Pairs with data: 498	(0.007)
(t=t + 2)Move*		0.027***
	Pairs with data: 541	(0.008)
(t=t + 3)Move*		0.026***
	Pairs with data: 505	(0.006)
(t=t + 4)Move*		0.028***
	Pairs with data: 403	(0.008)
(t=t + 5^+)Move*		0.033***
	Pairs with data: 474	(0.008)
(t=t - 2)Move*		0.003
	Pairs with data: 480	(0.005)
(t=t - 3)Move*		-0.007
	Pairs with data: 326	(0.009)
(t=t - 4)Move*		0.003
	Pairs with data: 293	(0.007)
(t=t - 5^+)Move*		-0.006
	Pairs with data: 253	(0.009)
Observations		90,764
R-Squared		0.768
	Panel B: Move Outs	
(t=t)Move*		0.009
	Pairs with data: 451	(0.006)
(t=t + 1)Move*		0.018
	Pairs with data: 243	(0.013)
(t=t + 2)Move*		-0.004
	Pairs with data: 190	(0.013)
(t=t + 3)Move*		-0.018
	Pairs with data: 129	(0.013)
(t=t + 4)Move*		-0.027*
	Pairs with data: 128	(0.016)
(t=t + 5^+)Move*		-0.021
	Pairs with data: 121	(0.016)
(t=t - 2)Move*		0.009
	Pairs with data: 436	(0.008)
(t=t - 3)Move*		-0.013
	Pairs with data: 203	(0.018)
(t=t - 4)Move*		-0.021
	Pairs with data: 81	(0.013)
(t=t - 5^+)Move*		-0.006
	Pairs with data: 120	(0.015)
Observations		89,715
R-Squared		0.771

Note: *** p<0.01, ** p<0.05, * p<0.1. In parentheses are two-way clustered standard errors, by firm and university. Regressions include firm-university, firm-year, and university-year fixed effects. The year of the move is *t**. Below each variable, I list the number of firm/university pairs with that variable equal to one. In the year preceding the move there are 892 firm/university pairs for move ins, and 745 pairs for move outs. See Figure 3 and text for details.

Appendix Table A3: Office Openings and Closings and Recruiting at Local Universities: Heterogeneity

Outcome: Recruit	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Move Ins			
Post Move, Short Run	0.004	0.034***	0.021***	0.011	0.012*	0.031**
	(0.006)	(0.012)	(0.007)	(0.011)	(0.007)	(0.014)
Post Move, Long Run	0.012	0.045***	0.037***	0.014	0.016	0.067***
	(0.011)	(0.013)	(0.009)	(0.017)	(0.011)	(0.018)
Observations	36,115	53,569	58,762	31,997	24,492	33,908
R-Squared	0.760	0.806	0.820	0.732	0.837	0.836
			Panel B: Move Outs			
Post Move, Short Run	0.002	0.011*	0.002	0.006	-0.014	0.021**
	(0.013)	(0.007)	(0.007)	(0.012)	(0.012)	(0.011)
Post Move, Long Run	-0.024	-0.013	-0.019	0.001		-0.010
	(0.018)	(0.024)	(0.033)	(0.028)		(0.036)
Observations	34,812	53,944	56,996	32,719	22,148	34,462
R-Squared	0.761	0.810	0.826	0.733	0.855	0.838
Firms	High Rank	Low Rank	Consulting	Banking	High Travel	Low Travel

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. In parentheses are standard errors clustered at the university level. All regressions include firm/university pair fixed effects, firm-year fixed effects, and university-year fixed effects. The variable *Post Move, Short Run* is an indicator for the year of the move, and the four years following the move event *(t*, t*+1, t*+2, t*+3, t*+4)*. The variable *Post Move, Long Run* is an indicator for five or more years following the move. See Table 2 for definition of move ins and move outs. Column 1 includes only firms whose Vault ranking by industry was among the ten highest (best) of the firms in that industry in the sample. Column 2 includes only the firms with lower Vault rankings. Column 3 includes only consulting firms, while column 4 includes only banking firms. Column 5 includes consulting firms denoted as requiring extensive travel, while column 6 includes consulting firms denoted as requiring less extensive travel. In Panel B, column (5) there are no observations with Post Move, Long Run equal to one. See text for details.

Appendix Table A4: Extent of Travel at Consulting Firms in the Sample

Firms Requiring Extensive Travel or with Global Staffing	Firms Requiring Less Extensive Travel or with Local Staffing
McKinsey & Company	Bain & Company
The Boston Consulting Group	Mercer
Booz & Company	A. T. Kearney
Oliver Wyman	Parthenon Group
Huron Consulting Group	Navigant
First Manhattan Consulting Group	ZS Associates
Marakon	NERA Economic Consulting
PRTM	Hewitt Associates
Mitchell Madison Group	Cornerstone Research
Arthur D. Little	Cambridge Associates
Kurt Salmon	Charles River Associates
Accenture	Corporate Executive Board
	The Advisory Board Company
	Analysis Group
	Gallup
	Putnam Associates
	Dean & Company
	Booz Allen Hamilton
	FTI Consulting
	OC&C Strategy Consultants

Notes: Designations are based on firm websites, Vault.com, and both of these sites accessed through The Wayback Machine. Local staffing refers to assigning cases to consultants in the area of their local offices. Global staffing refers to case assignments that do not depend on the location of the consultant's home office. The particular texts which determined these designations are available from the author upon request. See text for details.

Appendix Table A5: The Effect of Office Openings and Closings on Recruiting at Local Universities: Quadratic in Distance

Outcome: Recruit	
Distance	-0.0054***
	[0.0017]
$Distance^2$	0.0002***
	[0.0001]
Effect of Moving ≈326 miles closer, to a distance of ≈52 miles	
	0.0169***
	[.005]
Effect of Moving ≈633 miles closer, to a distance of ≈52 miles	
	0.0329***
	[.01]
N	101,254
R-squared	0.7613

Note: *** p<0.01, ** p<0.05, * p<0.1. In parentheses are two-way clustered standard errors, by firm and university. All regressions include firm/university pair fixed effects, firm-year fixed effects, and university-year fixed effects. Distance denotes the distance between the university and the firm's closest office to the university. Moving 326 miles closer is the 75th percentile of distance differences among universities experiencing firms moving closer. Moving 633 miles closer is the 90th percentile of distance differences among universities experiencing firms moving closer. Distance between firm and university of 52 miles is the 25th percentile of distance among firm/university pairs experiencing the firm moving closer to the university. See text for details.

Appendix Table A6: The Effect of Office Openings and Closings on Recruiting at Local Universities, Robustness and Heterogeneity

	Outcome: Recruit	(1)	(2)	(3)
	Panel A: Move Ins			
(1)	Post Move, Short Run	0.021***	0.021***	0.023**
		(0.007)	(0.007)	(0.010)
(2)	Post Move, SR*New Office in Big City			-0.002
				(0.011)
(3)	Post Move, Long Run	0.027***	0.027***	0.016**
		(0.009)	(0.009)	(0.007)
(4)	Post Move, LR*New Office in Big City			0.018*
				(0.010)
	Observations	64,479	64,479	90,764
	R-Squared	0.796	0.796	0.768
	Panel B: Move Outs			
(1)	Post Move, Short Run	0.001	0.001	0.008*
		(0.007)	(0.007)	(0.005)
(2)	Post Move, SR*Closed Office in Big City			-0.011
				(0.015)
(3)	Post Move, Long Run	-0.024***	-0.024***	-0.012
		(0.008)	(0.008)	(0.014)
(4)	Post Move, LR*Closed Office in Big City			-0.009
				(0.012)
	Observations	62,983	62,983	89,715
	R-Squared	0.800	0.800	0.771
	University Characteristics	Y	N	N

Note: *** p<0.01, ** p<0.05, * p<0.1. In parentheses are two-way clustered standard errors, by firm and university. Regressions in columns 1 and 2 include firm/university pair fixed effects and firm/year fixed effects, and include only years including and after 2004. See Appendix Table A11 for the set of university characteristics included as controls in column 1. To avoid dropping observations missing one of the control variables, I set missing values to a common value and include an indicator for whether the observation is missing the value for that variable. Regressions in column 3 include firm/university pair fixed effects, firm/year fixed effects, and university/year fixed effects. I define big cities as cities in an urban area with population above 4.5 million (the 25th percentile for pairs in the regression sample experiencing move ins). See notes to Table 2 for description of the Post Move variables, and see text for further details.

Appendix Table A7: Office Openings and Closings and Changes in the Urban Area

	(1)	(2)
	# Sample Firm Offices in Urban Area of City Experiencing the Move	
Outcome:		
Post Move, Short Run	0.539	-0.073
	(0.517)	(0.838)
Post Move, Long Run	1.962	-0.025
	(1.340)	(1.586)
Observations	6,641	2,856
R-Squared	0.989	0.993
Move	Move In	Move Out

Note: *** p<0.01, ** p<0.05, * p<0.1. In parentheses are standard errors clustered at the level of the urban area into which (column 1), or from which (column 2), the firm moved. There are 37 clusters in column 1 and 39 in column 2. Regressions include only firm/university pairs that eventually experience a move. The dependent variable is the number of sample firm offices in the urban area corresponding to the city into which the firm moves and Post Move becomes one for the pair (column 1), or away from which the firm moves and Post Move afterward becomes one for the pair (column 2). All regressions include firm/university pair fixed effects and year fixed effects. The variable *Post Move, Short Run* is an indicator for the year of the move, and the four years following the move event *(t*, t*+1, t*+2, t*+3, t*+4)*. The variable *Post Move, Long Run* is an indicator for five or more years following the move. See Table 2 for definition of move ins and move outs.

Appendix Table A8: Selectivity of New Target Campuses Relative to the Firm's Median Target Campus

Y = Selectivity - Selectivity of Firm's Median Target	(1)	(2)	(3)	(4)	(5)	(6)
New Office in Big City	0.144*	0.113	-0.0747			
	(0.0791)	(0.0939)	(0.124)			
Distance Between New Office and Closest Existing Target		-0.00694	-0.0132			
		(0.00920)	(0.00898)			
# Firms with Office in Same Urban Area as New Office			0.00606**			
			(0.00295)			
Ln(Urban Area Population) for New Office				0.0726*	0.0757*	0.0150
				(0.0425)	(0.0421)	(0.0972)
Ln(Distance Between New Office and Closest Existing Target)					-0.0503*	-0.0505*
					(0.0288)	(0.0292)
Ln(# Firms with Office in Same Urban Area as New Office)						0.0666
						(0.0928)
Constant	-0.168***	-0.115	-0.155	-1.181*	-0.953	-0.231
	(0.0587)	(0.0950)	(0.0949)	(0.647)	(0.642)	(1.219)
Observations	40	40	40	40	40	40
R-squared	0.054	0.068	0.187	0.055	0.106	0.122

Note: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. Sample includes firm/university pairs that never attracted the firm before the move, but attracted the firm at least once after the move. These firm/university pairs are included in the year the firm started to recruit at the university. Big city equals one if the city population is greater than or equal to the 25th percentile of the post-move regression sample (4.5 million people). Number of firms with office in same urban area as new office refers to the number of firms in the sample (combining finance and consulting firms) with offices in the same urban area as the firm's new office. See text for details.

Appendix Table A9: The Effect of Office Openings and Closings on Recruiting at Local Universities, by a Continuous Measure of Distance

Panel A: Y = *Recruit*	Move Ins	
Post Move	0.127***	0.078***
	(0.047)	(0.025)
Post Move*ln(Selectivity)	0.012**	0.012**
	(0.005)	(0.005)
Interactions with Distance when Proximity = 1		
Post Move*Distance when Proximity = 1	-0.002	-0.002
	(0.001)	(0.001)
Post Move*(Distance when Proximity = 1)^2	2.37e-05	2.07e-05
	(1.45e-05)	(1.33e-05)
Post Move*(Distance when Proximity = 1)^3	-7.75e-08*	-6.73e-08
	(4.53e-08)	(4.20e-08)
Interactions with Distance when Proximity = 0		
Post Move*Distance when Proximity = 0	-2.917e-04*	
	(1.596e-04)	
Post Move*(Distance when Proximity = 0)^2	3.74e-07	
	(2.37e-07)	
Post Move*(Distance when Proximity = 0)^3	-1.03e-10	
	(7.15e-11)	
Interactions with Distance to Closest Existing Target		
Post Move*Distance to Closest Target		-4.15e-05
		(8.43e-05)
Post Move*(Distance to Closest Target)^2		8.99e-08
		(8.82e-08)
Post Move*(Distance to Closest Target)^3		-2.814e-11
		(2.373e-11)
Observations	89,296	89,502
R-squared	0.773	0.768

Note: *** p<0.01, ** p<0.05, * p<0.1. In parentheses are two-way clustered standard errors, by firm and university. Regression for column (1) includes firm-university, firm-urban area of closest office-year, and university-year fixed effects, an indicator for post-move, and this indicator interacted with a cubic in pre-move distance (Proximity = 0), a cubic in post-move distance (Proximity = 1), and ln(university selectivity). Regression for column (2) includes firm-university, firm-year, and university-year fixed effects, an indicator for post-move, and this indicator interacted with a cubic in distance to closest existing target campus, and interacted with a cubic in distance when proximity = 1, and interacted with ln(selectivity). Sample sizes are smaller in column (1) because of singletons when using the firm-urban area of closest office-year fixed effects, and because some locations are not matched to urban areas. See text for details.

Appendix Table A10: On-Campus Access to Recently Relocated Firms at Graduation, and the Effect on Earning in the Top Quintile in 2014 (at 23-34 Years Old)

Y =Pr(Top 20% Earnings|parent quintile = q)$_{jltq}$	(1)	(2)	(3)	(4)	(5)
$t = t^* - 3$	-0.0032	-0.0003	-0.0034	0.0012	0.0142
	(0.0026)	(0.0030)	(0.0027)	(0.0030)	(0.0102)
$t = t^* - 2$	-0.0079***	-0.0070***	-0.0077***	-0.0064**	0.0042
	(0.0025)	(0.0027)	(0.0027)	(0.0028)	(0.0101)
$t = t^*$	-0.0046*	-0.0066**	-0.0047*	-0.0072**	-0.0044
	(0.0026)	(0.0033)	(0.0027)	(0.0033)	(0.0096)
$t = t^* + 1$	-0.0043	-0.0159***	-0.0035	-0.0152***	-0.0194
	(0.0043)	(0.0045)	(0.0044)	(0.0043)	(0.0165)
N	15,960	15,960	15,240	15,240	720
University Tier	All	All	Non Ivy	Non Ivy	Ivy
Universities Providing Identifying Variation	27	17	23	14	4
R-squared	0.8055	0.8255	0.7955	0.8167	0.7767
Dep. Var. Mean, $t^* - 1$	0.536	0.536	0.524	0.524	0.645
Region-University Tier-Cohort (*t*) FE	Y	N	Y	N	Y
State-University Tier-Cohort (*t*) FE	N	Y	N	Y	N

	Parent Income Quintile				
Y =Pr(Top 20% Earnings|parent quintile = q)$_{jltq}$	1	2	3	4	5
$t = t^* - 3$	0.0098	-0.0049	-0.0018	0.0045	-0.0054
	(0.0128)	(0.0080)	(0.0101)	(0.0065)	(0.0037)
$t = t^* - 2$	-0.0185*	-0.0080	-0.0137	-0.0140**	-0.0017
	(0.0101)	(0.0077)	(0.0103)	(0.0061)	(0.0028)
$t = t^*$	-0.0080	-0.0010	-0.0139	-0.0062	-0.0014
	(0.0150)	(0.0079)	(0.0093)	(0.0057)	(0.0039)
$t = t^* + 1$	-0.0194*	0.0048	-0.0105	0.0040	-0.0049
	(0.0109)	(0.0081)	(0.0122)	(0.0083)	(0.0050)
N	3,132	3,132	3,132	3,132	3,132
Universities Providing Identifying Variation	27	27	27	27	27
R-squared	0.8284	0.8686	0.8905	0.9138	0.9464
Dep. Var. Mean, $t^* - 1$	0.437	0.453	0.478	0.505	0.579
Region-University Tier-Cohort (*t*) FE	Y	Y	Y	Y	Y

Universities in Panel A Column 1 Attracting Recruiting Firms After Nearby Office Openings, by University Tier

Ivy Plus	4
Elite (Barron's Tier 1, Excluding Ivy Plus)	12
Highly Selective (Barron's Tier 2)	8
Selective (Barron's Tiers 3-5)	3

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the university level. Regressions are similar to Table 3, but the dependent variable is the fraction of students from parental income quintile q in the top 20% of the income distribution for their birth cohort in 2014, for the birth cohort that turned 22 in calendar year t and graduated from university j in selectivity tier k, and location l. See text and Table 3 for details.

Appendix Table A11: Changes in University Characteristics Around Office Openings

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Share Scoring > 700 on SAT Math or 30 on ACT Math	Percent Admitted	SAT Verbal, 75th Percentile	ACT English, 75th Percentile	Number of Students	Out of State Tuition	Percent Black	Percent Hispanic	Regional Rank
Post Move, Short Run	0.002	0.007	1.321	0.137*	-37.079	336.558***	-0.001	0.001	0.354
	(0.003)	(0.019)	(0.856)	(0.075)	(52.152)	(113.545)	(0.001)	(0.001)	(0.367)
Post Move, Long Run	0.008*	0.036	2.002	0.235*	-149.893	1,184.082***	0.001	0.001	-0.340
	(0.005)	(0.023)	(1.357)	(0.130)	(114.716)	(257.927)	(0.001)	(0.001)	(0.719)
Observations	64,507	68,375	62,057	37,036	90,575	89,426	90,575	90,575	64,507
R-Squared	0.976	0.471	0.967	0.949	0.993	0.973	0.989	0.982	0.972

Note: *** p<0.01, ** p<0.05, * p<0.1. In parentheses are standard errors clustered at the university level. All regressions include firm/university pair fixed effects and firm/year fixed effects. See Table 2 for definitions of Post Move, Short Run and Post Move, Long Run. Number of observations differs with each dependent variable because these variables are not reported by the universities in some years. The first four columns, and the last column, are estimated using only years greater than or equal to 2004, since this is when these variables become available. Singletons are dropped based on the regression sample in each column. The dependent variable in column 3 is the 75th percentile of SAT Verbal scores for entering students at the university, ranging from 470 to 800. The dependent variable in column 4 is the 75th percentile of ACT English scores for entering students at the university, ranging from 18 to 35. See text for details.

Appendix Table A12: Summary Statistics: Robustness Sample Including Firm-University-Year Pairs in which the Firm Does not Recruit at any University

# Firms	73
# Consulting Firms	42
# Banking Firms	31
# Universities	362
# Office Openings	233
# Office Closings	100
# Firms with ≥ 1 Move In	40
# Universities with ≥ 1 Move In	359
# Firm/University Pairs with ≥ 1 Move In	2,228
# Cities with ≥ 1 Move In	135
# Firms with ≥ 1 Move Out	38
# Universities with ≥ 1 Move Out	346
# Firm/University Pairs with ≥ 1 Move Out	1,229
# Cities with ≥ 1 Move Out	70

Note: This table is similar to Table 1, but provides statistics for the robustness sample which includes firm-university-year pairs even if the firm is not recruiting on any campus that year. See text and Table 1 for details.

Appendix Table A13: The Effect of Office Openings and Closings on Recruiting at Local Universities, Sample Including Firm-University-Year Pairs in which the Firm Does not Recruit at any University

	(1)	(2)	(3)	(4)	(5)	(6)
Y = *Recruit*	Move Ins			Move Outs		
Post Move, Short Run						
All Universities	0.011***	0.009***		-0.008	-0.006	
	(0.004)	(0.003)		(0.005)	(0.004)	
Ivy Plus University			0.073*			-0.135**
			(0.040)			(0.067)
Elite University (Barron's Tier I, Excl. Ivy Plus)			0.013			-0.007
			(.008)			(.009)
Highly Selective University (Barron's Tier 2)			0.009***			0.0006
			(.003)			(.005)
Selective University (Barron's Tiers 3-5)			0.0035			-0.00122
			(.003)			(.003)
Post Move, Long Run						
All Universities	0.011*	0.012**		-0.019***	-0.030***	
	(0.007)	(0.005)		(0.007)	(0.010)	
Ivy Plus University			0.072			-0.158***
			(0.059)			(0.040)
Elite University			0.026*			-0.060**
			(.015)			(.024)
Highly Selective University			0.011*			-0.030**
			(.006)			(.012)
Selective University			0.002			-0.014
			(.004)			(.008)
N	194,724	194,724	194,724	189,688	189,688	189,688
R-squared	0.599	0.637	0.637	0.604	0.641	0.642
Firm-University Fixed Effects	Y	Y	Y	Y	Y	Y
Firm-Year, University-Year Fixed Effects	N	Y	Y	N	Y	Y
Dependent Variable Mean						
t^*-1	0.004	0.004	0.005	0.017	0.017	0.017
Ivy Plus			0.023			0.089
Elite			0.009			0.047
Highly Selective			0.004			0.014
Selective			0.001			0.003

Note: *** p<0.01, ** p<0.05, * p<0.1. In parentheses are two-way clustered standard errors, by firm and university. This table is similar to Table 2, but using the robustness sample including firm-university-year pairs even if the firm is not recruiting on any campus that year. See Table 2 and text for details.

Appendix Table A14: The Effect of Office Openings and Closings on Recruiting at Local Universities

	(1)	(2)	(3)
Y = *Recruit*	Move Ins	Move Outs	Move Ins
Post Move*Distance when Proximity = 1 within			
0 to 10 miles	0.061**	-0.039	
	(0.030)	(0.042)	
10 to 50 miles	0.009	0.010	
	(0.006)	(0.008)	
50 to 200 miles	0.018***	0.009**	
	(0.005)	(0.003)	
Post Move			0.013
			(0.018)
Post Move*Closest Target > 50 Miles Away			0.011
			(0.022)
N	90,764	89,715	90,764
R-squared	0.768	0.771	0.768
Firm-University Fixed Effects	Y	Y	Y
Firm-Year, University-Year Fixed Effects	Y	Y	Y

Note: *** p<0.01, ** p<0.05, * p<0.1. In parentheses are two-way clustered standard errors, by firm and university. Regressions include an indicator for post interacted with three bins of post-move distance (column 1) or pre-move distance (column 2). Regressions also include interactions between these distance bins and an indicator for at least five years before the move. Column 3 includes an indicator for post move, an indicator for at least five years before the move, and these interacted with an indicator for whether the closest existing target campus is more than 50 miles away after the move. See text for details.

Appendix Table A15: On-Campus Access to Recently Relocated Firms at Graduation, and the Effect on Incomes in 2014 (at 23-34 Years Old), Not Imputing Missing Recruiting Values

Y =Frac. in Top 1%, Conditional on Parent Quintile	(1)	(2)	(3)
t-3	-0.0004	0.0000	0.0064
	(0.0016)	(0.0017)	(0.0081)
t-2	-0.0002	0.0013	-0.0052
	(0.0013)	(0.0012)	(0.0072)
t	0.0021	0.0027*	-0.0016
	(0.0013)	(0.0014)	(0.0084)
t+1	0.0022	0.0032**	-0.0120
	(0.0015)	(0.0014)	(0.0073)
N	15,960	15,240	720
University Tier	All	Non Ivy	Ivy
Universities Providing Identifying Variation	27	23	4
R-squared	0.8520	0.7757	0.5829
Dep. Var. Mean, *t-1*	0.0733	0.0608	0.183
University Fixed Effects	Y	Y	Y
Region-University Tier-Cohort (*t*) FE	Y	N	N
State-University Tier-Cohort (*t*) FE	N	Y	N
CZ-University Tier-Cohort (*t*) FE	N	N	Y

	Parent Income Quintile				
Y =Frac. in Top 1%, Conditional on Parent Quintile	1	2	3	4	5
t-3	-0.0054	-0.0036	0.0002	0.0027	-0.0006
	(0.0046)	(0.0030)	(0.0037)	(0.0028)	(0.0022)
t-2	-0.0055	0.0019	-0.0079**	0.0035	0.0002
	(0.0047)	(0.0038)	(0.0040)	(0.0028)	(0.0023)
t	0.0010	0.0001	-0.0018	0.0044	0.0027
	(0.0053)	(0.0043)	(0.0033)	(0.0027)	(0.0021)
t+1	-0.0047	-0.0027	-0.0026	0.0093***	0.0021
	(0.0043)	(0.0030)	(0.0035)	(0.0028)	(0.0024)
N	3,132	3,132	3,132	3,132	3,132
University Tier	All	All	All	All	All
Universities Providing Identifying Variation	27	27	27	27	27
R-squared	0.6833	0.7258	0.8107	0.8676	0.9529
Dep. Var. Mean, *t-1*	0.0449	0.0411	0.0495	0.0498	0.0893
University Fixed Effects	Y	Y	Y	Y	Y
Region-University Tier-Cohort (*t*) FE	Y	Y	Y	Y	Y

Universities in Panel A Column 1 Attracting Recruiting Firms After Office Opening within 10 Miles, by University Tier	
Ivy Plus	4
Elite (Barron's Tier 1, Excluding Ivy Plus)	12
Highly Selective (Barron's Tier 2)	8
Selective (Barron's Tiers 3-5)	3

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the university level. This table is similar to Table 3, but I do not impute missing values for *Recruit* . See text for details.

Appendix Table A16: Event Study Regressions Balanced on Calendar Year, Office Openings

Y = Recruit	(1)	(2)	(3)	(4)	(5)
t-2*	0.012*	0.002	0.008	0.002	0.002
	(0.007)	(0.002)	(0.008)	(0.005)	(0.004)
*t**	0.014	-0.006**	-0.002	0.021	0.0005
	(0.009)	(0.003)	(0.005)	(0.015)	(0.002)
t+1*	0.035***	0.007	0.012	0.002	-0.004
	(0.013)	(0.010)	(0.009)	(0.004)	(0.003)
Years of Office Openings	2002-2003	2004-2005	2006-2007	2008-2010	2011-2012
Number of Firms with Openings	9	7	8	5	4
Number of Office Openings	12	24	19	10	5
Mean *Recruit* , *t*-1*	0.015	0.006	0.004	0	0
N	34,170	30,790	28,950	25,950	25,680
R-squared	0.706	0.771	0.765	0.720	0.841

Note: Regressions show results using only firm-university observations balanced on calendar year, for firm-university pairs that experience moves, as well as those that never experience moves. Each column shows results restricting to office openings in different years. Regressions include firm-university pairs experiencing moves in those years and firm-university pairs never experiencing moves in the sample. I also include firm-university pairs that experience moves at least five years after the last year in the year group, so those pairs do not contribute to the event-study coefficients in t-2 through t+1. I require balance from t-2 through t+1,where t refers to the year of the move. Given that I require balance on calendar year, this implies that I require every firm-university pair to have data starting two years before the first move in year for the group, through two years after the first move in year for the group. For example, for the 2002-2003 year group, I require every firm-university pair has data from 2000 through 2004, implying the same firm-university pairs contribute to the firm-year and university-year fixed effects in each year, and the same firm-university pairs contribute to coefficients on t-2 through t+1, for t=2002,2003. Standard errors are clustered at the university level.

Appendix Table A17: Event Study Regressions with a Balanced Sample on Calendar Year

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: Office Openings											
t-2*	0.011	0.005	0.001	-0.00003	0.032	0.0002	0.007	0.002	-0.020**	0.002	0.001
	(0.009)	(0.006)	(0.003)	(0.002)	(0.020)	(0.005)	(0.005)	(0.020)	(0.008)	(0.003)	(0.001)
t+1*	0.051***	0.006	0.013	0.022	0.025	0.014	0.003	0.008	-0.007	-0.006*	-0.004
	(0.019)	(0.006)	(0.015)	(0.015)	(0.022)	(0.010)	(0.006)	(0.020)	(0.005)	(0.003)	(0.004)
Year of Opening	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012
Firms with Openings	5	6	2	7	6	7	6	3	4	2	2
Office Openings	9	13	13	13	7	17	7	6	8	3	2
Mean, *Recruit* , *t*-1*	0.008	0.017	0	0.006	0	0.004	0	0	0	0	0
N	23,421	27,324	25,290	26,655	25,278	23,445	25,320	23,736	24,138	17,394	19,530
R-squared	0.708	0.766	0.767	0.742	0.756	0.826	0.702	0.793	0.776	0.831	0.909
Panel B: Office Closings											
t-2*	0.021	-0.012**	-0.005	0.003	-0.012**	-0.003					
	(0.020)	(0.006)	(0.004)	(0.007)	(0.006)	(0.004)					
t+4*	-0.043**	-0.051	-0.066*	0.015	0.0001	0.001					
	(0.020)	(0.049)	(0.037)	(0.010)	(0.007)	(0.002)					
Year of Closing	2003	2004	2005	2007	2008	2009					
Firms with Closings	2	2	2	2	4	3					
Office Closings	3	3	2	2	6	3					
Mean, *Recruit* , *t*-1*	0.069	0.044	0.077	0	0.023	0					
N	23,715	22,707	17,808	17,937	22,338	21,804					
R-squared	0.708	0.746	0.762	0.757	0.715	0.770					

Note: Regressions show results using only firm-university observations balanced on calendar year, for firm-university pairs that experience moves, as well as those that never experience moves. Each column shows results restricting to office openings or closings in the specified year, y. Regressions include firm-university pairs experiencing moves in that year and firm-university pairs never experiencing moves in the sample. In Panel A, I also include firm-university pairs with t ≥ y + 4, so those pairs do not contribute to the event-study coefficients in t-2, t-1, or t+1. In Panel B, I include firm-university pairs with t ≥ y + 7, so those pairs do not contribute to the event-study coefficients in t-2, t-1, or t+4. In Panel A, I require balance in t-2, t-1, and t+1, where t refers to the year of the move. In Panel B, I require balance in t-2, t-1, and t+4. Given that I require balance on calendar year, in Panel A this implies that I require every firm-university pair to have data in the same three years: y-2, y-1, and y+1. In Panel B, I require every firm-university pair to have data in y-2, y-1, and y+4. Standard errors are clustered at the university level.

Appendix Table A18: Summary Statistics Using CZ Definition of Move

	Main Definition	CZ Definition
# Office Openings Yielding a Move In for ≥ 1 University	75	80
# Office Closings Yielding a Move Out for ≥ 1 University	41	50
Firm-University Pairs in *t*-1*	892	420
Pre-Move In Distance (Miles)	466	321
	[415]	[405]
Post-Move In Distance (Miles)	84	15
	[63]	[15]
Pre-Move Out Distance (Miles)	87	17
	[62]	[15]
Post-Move Out Distance (Miles)	330	229
	[172]	[187]

Note: These columns compare summary statistics using the main definition of moves to those from defining a move as moving within or from the same CZ as the university. See text for details.

Appendix Table A19: Change in University Characteristics Surrounding Firm Recruiting After Relocation

	Share with Parent Income in Quintile					Share with Parent Income in Top					
Y =	1	2	3	4	5	10%	5%	1%	0.10%	Share Female	Ln(Students)
t-3	-0.0008	0.0011	-0.0001	0.0025	-0.0028	-0.0036	-0.0048*	-0.0031**	0.0007	-0.0064*	0.0131
	(0.0015)	(0.0017)	(0.0022)	(0.0022)	(0.0046)	(0.0040)	(0.0029)	(0.0012)	(0.0005)	(0.0037)	(0.0176)
t-2	-0.0009	-0.0013	-0.0008	0.0017	0.0014	-0.0034	-0.0032	-0.0011	0.0011***	-0.0011	-0.0037
	(0.0013)	(0.0015)	(0.0018)	(0.0020)	(0.0037)	(0.0031)	(0.0025)	(0.0013)	(0.0004)	(0.0027)	(0.0079)
t	0.0022*	-0.0011	0.0006	0.0000	-0.0017	-0.0049*	-0.0048*	-0.0011	0.0003	0.0060**	-0.0020
	(0.0013)	(0.0013)	(0.0019)	(0.0025)	(0.0030)	(0.0029)	(0.0027)	(0.0013)	(0.0005)	(0.0028)	(0.0107)
t+1	-0.0013	-0.0001	-0.0006	-0.0009	0.0029	-0.0010	-0.0009	0.0003	0.0001	0.0020	-0.0117
	(0.0016)	(0.0015)	(0.0021)	(0.0020)	(0.0044)	(0.0036)	(0.0035)	(0.0014)	(0.0005)	(0.0032)	(0.0164)
N	3,132	3,132	3,132	3,132	3,132	3,132	3,132	3,132	3,132	3,132	3,132
R-squared	0.9521	0.9489	0.9257	0.9415	0.9709	0.9774	0.9818	0.9768	0.9249	0.9646	0.9939
University Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Region-Univ. Tier-Cohort FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the university level. This table is similar to Table 3, but the dependent variables are university characteristics shown in the top row. The dependent variables are at the university-birth cohort level, and so observations here are at the university-birth cohort level rather than university-birth cohort-parental income quintile level as in Table 3 Panel A.

Appendix Table A20: On-Campus Access to Recently Relocated Firms at Graduation, and the Effect on Incomes in 2014 (at 23-34 Years Old)

Y =Pr(Top 1% Earnings|parent quintile = q)$_{jltq}$	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$t = t^* - 3$	-0.0008	-0.0031	0.0064	-0.0004	-0.0023	0.0072	0.0064
	(0.0016)	(0.0024)	(0.0056)	(0.0017)	(0.0025)	(0.0072)	(0.0081)
$t = t^* - 2$	-0.0012	-0.0027	-0.0037	0.0001	-0.0008	0.0014	-0.0052
	(0.0014)	(0.0019)	(0.0045)	(0.0014)	(0.0018)	(0.0037)	(0.0072)
$t = t^*$	-0.0001	-0.0011	-0.0069*	0.0001	-0.0005	-0.0034	-0.0016
	(0.0013)	(0.0014)	(0.0037)	(0.0014)	(0.0015)	(0.0035)	(0.0084)
$t = t^* + 1$	0.0028*	0.0004	-0.0126*	0.0038**	0.0023	-0.0045	-0.0120
	(0.0016)	(0.0021)	(0.0065)	(0.0015)	(0.0018)	(0.0043)	(0.0073)
N	15,960	15,960	15,960	15,240	15,240	15,240	720
University Tier	All	All	All	Non Ivy	Non Ivy	Non Ivy	Ivy
Universities Providing Identifying Variation	27	17	6	23	14	4	4
R-squared	0.8520	0.8662	0.8811	0.7758	0.7981	0.8244	0.5829
Dep. Var. Mean, $t^* - 1$	0.0738	0.0738	0.0738	0.0614	0.0614	0.0614	0.183
Region-University Tier-Cohort (t) FE	Y	N	N	Y	N	N	Y
State-University Tier-Cohort (t) FE	N	Y	N	N	Y	N	N
CZ-University Tier-Cohort (t) FE	N	N	Y	N	N	Y	N

	Parent Income Quintile				
Y =Pr(Top 1% Earnings|parent quintile = q)$_{jltq}$	1	2	3	4	5
$t = t^* - 3$	-0.005	-0.002	-0.003	0.004	-0.001
	(0.005)	(0.003)	(0.004)	(0.004)	(0.002)
$t = t^* - 2$	-0.001	-0.001	-0.004	-0.003	0.002
	(0.006)	(0.004)	(0.004)	(0.003)	(0.002)
$t = t^*$	0.009*	-0.005	-0.001	-0.001	0.001
	(0.005)	(0.004)	(0.003)	(0.003)	(0.002)
$t = t^* + 1$	0.003	-0.002	-0.001	0.005**	0.005**
	(0.004)	(0.003)	(0.004)	(0.003)	(0.002)
N	3,024	3,024	3,024	3,024	3,024
University Tier	Non Ivy	Non Ivy	Non Ivy	Non Ivy	Non Ivy
Universities Providing Identifying Variation	23	23	23	23	23
R-squared	0.593	0.641	0.746	0.804	0.923
Dep. Var. Mean, $t^* - 1$	0.0369	0.0362	0.0440	0.0454	0.0774
Region-University Tier-Cohort (t) FE	Y	Y	Y	Y	Y

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the university level. Panel A regressions are similar to those in Table 3 Panel A, but with different fixed effects in each column. Panel B regressions are similar to those in Table 3 Panel B, but including only Non Ivy Plus universities. See text and Table 3 for details.

Appendix Table A21: On-Campus Access to Recently Relocated Firms at Graduation and the Effect on Incomes in 2014, Restricting to Different Birth Cohorts

Y =Pr(Top 1% Earnings|parent quintile = q)$_{jltq}$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$t = t^* - 3$	-0.0008	-0.0007	-0.0015	-0.0014	-0.0014	-0.0016	-0.0023	-0.0033
	(0.0016)	(0.0017)	(0.0014)	(0.0014)	(0.0014)	(0.0014)	(0.0018)	(0.0022)
$t = t^* - 2$	-0.0012	-0.0013	-0.0012	-0.0011	-0.0012	-0.0015	-0.0019	-0.0031
	(0.0014)	(0.0014)	(0.0015)	(0.0014)	(0.0014)	(0.0014)	(0.0017)	(0.0024)
$t = t^*$	-0.0001	0.0001	0.0005	0.0004	0.0004	0.0004	-0.0007	-0.0008
	(0.0013)	(0.0013)	(0.0013)	(0.0014)	(0.0014)	(0.0014)	(0.0016)	(0.0018)
$t = t^* + 1$	0.0028*	0.0032*	0.0024	0.0025	0.0028*	0.0019	0.0031	0.0039**
	(0.0016)	(0.0017)	(0.0017)	(0.0017)	(0.0017)	(0.0017)	(0.0021)	(0.0018)
N	15,960	14,630	13,300	12,015	10,680	9,345	8,010	6,675
Birth Cohorts	1980-1991	1980-1990	1980-1989	1980-1988	1980-1987	1980-1986	1980-1985	1980-1984
Universities Providing Identifying Variation	27	26	24	23	23	22	17	12
R-squared	0.852	0.852	0.852	0.853	0.857	0.861	0.863	0.866
Region-University Tier-Cohort (*t*) FE	Y	Y	Y	Y	Y	Y	Y	Y

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the university level. Regressions are the same as in Table 3, Panel A, but with different birth cohorts. See text and Table 3 for details.

Appendix Table A22: Change in Commuting Zone Characteristics Surrounding Firm Recruiting After Relocation

Y =	Ln(CZ Per Capita Income)	Ln(CZ Per Capita Net Earnings)	Ln(CZ Per Capita Unemployment Compensation)	Ln(CZ Population)	Ln(CZ Avg. Wage)	Ln(CZ Employment)
t-3	-0.0081	-0.0097	0.0477	-0.0096**	-0.0063*	-0.0116**
	(0.0062)	(0.0061)	(0.0322)	(0.0038)	(0.0036)	(0.0050)
t-2	-0.0039	-0.0067	0.0183	-0.0055**	-0.0047**	-0.0065***
	(0.0042)	(0.0045)	(0.0179)	(0.0021)	(0.0023)	(0.0024)
t	-0.0004	0.0043	-0.0066	0.0037*	-0.0001	0.0060
	(0.0037)	(0.0045)	(0.0140)	(0.0021)	(0.0030)	(0.0038)
t+1	-0.0078	-0.0075	0.0231	0.0097**	-0.0043	0.0095
	(0.0063)	(0.0061)	(0.0352)	(0.0038)	(0.0050)	(0.0067)
t+2	-0.0184***	-0.0182**	0.0484	0.0132**	-0.0082	0.0136
	(0.0069)	(0.0072)	(0.0371)	(0.0055)	(0.0056)	(0.0090)
t+3	-0.0173**	-0.0169*	0.0665*	0.0160**	-0.0075	0.0171
	(0.0071)	(0.0088)	(0.0366)	(0.0071)	(0.0063)	(0.0112)
t+4	-0.0144*	-0.0143	0.0423	0.0206**	0.0027	0.0212
	(0.0078)	(0.0103)	(0.0305)	(0.0091)	(0.0054)	(0.0129)
t+5	-0.0057	-0.0047	-0.0129	0.0239**	0.0056	0.0262*
	(0.0093)	(0.0114)	(0.0312)	(0.0113)	(0.0056)	(0.0146)
t+6	0.0032	0.0044	0.0354	0.0282*	0.0045	0.0366*
	(0.0093)	(0.0128)	(0.0384)	(0.0158)	(0.0064)	(0.0201)
t+7	-0.0089	-0.0091	-0.0444	0.0136*	0.0032	0.0202*
	(0.0080)	(0.0094)	(0.0385)	(0.0081)	(0.0069)	(0.0107)
t+8	-0.0206	-0.0179	-0.0666*	0.0188*	0.0121*	0.0123
	(0.0138)	(0.0159)	(0.0401)	(0.0103)	(0.0072)	(0.0163)
N	3,132	3,132	3,132	3,132	3,132	3,132
R-squared	0.9893	0.9863	0.9732	0.9998	0.9953	0.9997
University Fixed Effects	Y	Y	Y	Y	Y	Y
Region-Univ. Tier-Year FE	Y	Y	Y	Y	Y	Y

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the commuting zone level. This table is similar to Table 3, with the same included covariates, but the dependent variables are characteristics of the university's CZ in the year the birth cohort turned 22, shown in the top row. The year *t** corresponds to the year in which the firm began recruiting after opening a nearby office, and birth cohorts are matched to the year in which they turned 22. The dependent variables are at the university-birth cohort level, and so observations here are at the university-birth cohort level rather than university-birth cohort-parental income quintile level as in Table 3 Panel A. Observations are weighted by the total students in the cohort that year, across all parental income quintiles.

Appendix Table A23: On-Campus Access to Recently Relocated Firms at Graduation and the Effect on Incomes in 2014

Y =Pr(Top 1% Earnings|parent quintile = q)$_{jltq}$	(1)
$t = t^* - 11$	-0.0193**
	(0.0086)
$t = t^* - 10$	-0.0026
	(0.0097)
$t = t^* - 9$	-0.0049
	(0.0059)
$t = t^* - 8$	0.0060
	(0.0073)
$t = t^* - 7$	-0.0001
	(0.0067)
$t = t^* - 6$	-0.0048
	(0.0032)
$t = t^* - 5$	-0.0019
	(0.0029)
$t = t^* - 4$	-0.0045**
	(0.0021)
$t = t^* - 3$	-0.0008
	(0.0016)
$t = t^* - 2$	-0.0012
	(0.0014)
$t = t^*$	-0.0001
	(0.0013)
$t = t^* + 1$	0.0028*
	(0.0016)
$t = t^* + 2$	0.0010
	(0.0021)
$t = t^* + 3$	0.0027
	(0.0026)
$t = t^* + 4$	0.0052*
	(0.0032)
$t = t^* + 5$	0.0050
	(0.0032)
$t = t^* + 6$	0.0064
	(0.0042)
$t = t^* + 7$	0.0135***
	(0.0045)
$t = t^* + 8$	0.0142***
	(0.0041)
N	15,960
R-squared	0.852

Notes: *** p<0.01, ** p<0.05, * p<0.1. Regression is the same as Table 3, Panel A, column 1 but shows all event study coefficients. See text and Table 3 for details.