# The death of distance in hiring

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**Abstract**

I use the staggered roll-out of an online job board to estimate the impact of the Internet on the geography of labour markets. I find that the US cities with earlier access to online recruitment experienced an increase in migration flows in and out of the city accompanied by an increase in wages. To understand the underlying mechanism, I collect a novel data set on firm recruitment across space. I show that firms hire outside of their local labour market to find workers of specific skill rather than to access cheap, unemployed labour. As a result, the introduction of online recruitment increased sorting across cities in jobs with high return to match quality. This mechanism contributed to the divergence in outcomes between US cities.

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# 1 Introduction

In theory, at least, the Internet is the perfect solution to labour market frictions. The ability to transmit large amounts of information cheaply, to communicate across a large distance, and to search through hundreds of job postings and candidate profiles should make it possible for employers to hire better, faster and more cheaply than in the analog world.

Indeed, there is some evidence that the introduction of the Internet helped job seekers find employment more quickly (Stevenson, 2009; Kuhn and Mansour, 2014; Gürtzgen, (né Nolte), Pohlan, and van den Berg, 2021), and improved efficiency in matching in the local labour market (Bhuller, Kostol, Ferraro, and Vigtel, 2022). Nevertheless, the estimated impact of the Internet on labour market outcomes seems somewhat subdued compared to the way it has revolutionised the practice of hiring, from online job adverts and online applicant profiles to the algorithms suggesting where to apply and conducting the first stage of the selection process.

In this paper, I argue the literature has ignored a crucial aspect of recruiting online: it helps workers and firms to meet *across* labour markets, not just *within* them. In the majority of countries, labour markets are predominantly local, and so are the technologies counteracting search frictions: social networks and referrals, the assistance of a caseworker in a local employment agency, advertising via billboards or simply putting up a vacancy posting in the shop window. In contrast, matching between cities and towns predominantly relies on the individual efforts of workers and firms, and as a result search frictions are larger between rather than within labour markets (Schmutz and Sidibe, 2018; Porcher, 2021; Balgova, 2022). Because the Internet doesn't just help workers and firms to find each other, it also "kills distance", it has the potential to reduce search frictions between regions by much more than the frictions within a local labour market.

Testing this hypothesis empirically is challenging for at least two reasons. The first is the near complete lack of data on recruitment in the US. While there are several surveys collecting information about how workers search for jobs – measuring the methods, their effort and the outcomes – we know relatively little about the analogous decisions on firms' side. The existing literature analysed the shift to online job search and whether it led to higher job-finding rates for workers, but the change in employers' behaviour is only implied by the fact that there are now online job vacancies to apply to. Similarly, while Manning and Petrongolo (2017) document job-seekers' preferences of search across space, no equivalent data exists for firm recruitment in other local labour markets. As a result, it is impossible to directly observe whether and how the rise of the Internet changed firms' recruitment. The second challenge lies in the nature of the Internet itself: it grew gradually from a niche idea to a technology that radically changed many aspects of life in developed economies, making it difficult to separate out the effect of online recruitment alone.

I address these challenges by exploiting the staggered introduction of a particular online job board, Craigslist, across US cities. Craigslist represented a radical shift in technology for recruiting across space. It acts as an online version of classified ads in a newspaper, offering free space to advertise job vacancies. Its online nature, however, meant that job openings that would be otherwise confined to a local newspaper were now freely viewable by anyone in the country – making it possible for workers to find job vacancies in distant labour markets, and for firms to hire them. Importantly, Craigslist websites are city-specific, allowing posters to advertise local vacancies only. This, combined with its staggered entry, generates variation in recruitment technology across time and space which I use to identify its impact on search and matching between markets. The second key feature of Craigslist was its unexpected rise in popularity between the years 2004-2006. During these 3 years, Craigslist entered 166 cities and became the most popular online job board in the US. This relatively narrow window of change allows me to isolate the labour market impact of online recruitment from the broader impact Craigslist, and the Internet in general, had on society.[1]

I estimate the impact of Craigslist on two key labour market outcomes: geographic mobility and wages.[2] To allow for causal interpretation, I rely on the website's staggered timing of entry, alongside several other complementary identification strategies. These include comparing Craigslist cities to cities entered by an unsuccessful rival and making use of the quasi-random variation in Craigslist's popularity following an earlier study by Kroft and Pope (2014). I focus on the period immediately before and after Craigslist entry, covering labour market outcomes in US cities in the years 1999 to 2008, and using a combination of different publicly available data sets on migration and pay at the city level.[3]

I find that the availability of Craigslist significantly increased the migration of workers both in and out of the affected cities. On average, gross inflows increased by 6.3% and gross outflows by 3.8%, leading to a small net increase in city-level population of about 0.07%.

[1]There are several studies documenting the impact of Craigslist on crime, health and political outcomes. Kroft and Pope (2014) show it helps the housing rental market clear faster; Djourelova, Durante, and Martin (2022); Seamans and Zhu (2014) document its negative effect on newspaper profits and political coverage; and Brenčič (2016) shows it has hit negatively the competing online job boars. There is also a large number of papers studying the effect of Craigslist's personal ads and erotic services sections on crime rates and health outcomes such as HIV; see Cunningham, DeAngelo, and Tripp (2023) for a summary.

[2]While not all movers move for job-related reasons, 60% do(Basker, 2018), and search frictions between regions have been identified as an important barrier to mobility (Schmutz and Sidibe, 2018; Balgova, 2022; Ransom, 2022). A reduction in these frictions, or a general improvement in spatial sorting, is expected to increase wages.

[3]My migration statistics come from the tax return data from the Inland Revenue Service, and the 2005-2007 waves of the American Community Survey. The data on wages comes from the Occupational Employment and Wage Statistics from the Bureau of Labor Statistics.

However, rather than driving relocation away from control to treated cities, Craigslist led to an increase in migration churn *between* treated cities – as reflected in the simultaneous increase in inflows and outflows. Furthermore, I find that this increase was driven by job posts, rather than advertising in the housing market or the general improvement in online connectivity between cities.

This increase in geographic mobility was accompanied by an increase in wages. The average worker earned \$0.12 per hour, or 0.8%, more each year following Craigslist entry. The increase was larger (1.2%) at the top decile of the wage distribution. As a consequence, Craigslist entry marked an end to the convergence in average pay between cities.

These headline results show that the emergence of online recruitment had a significant impact on workers' mobility and pay. In the second part of the paper, I expand this analysis further to show that these results are linked causally – that wages increased *because* of greater geographic mobility – and to pin down the mechanism behind Craigslist's impact on the labour market. I proceed in two steps. First, I collect and analyse a novel data set on firms' recruitment across space. This allows me to understand why firms recruit in more distant labour markets in general, and make several predictions about the impact on Craiglist on different jobs. I test these predictions by estimating the heterogeneity of the treatment effect by occupation.

I measure the spatial dimension of recruitment by analysing the placement of help-wanted ads in local newspapers in the year 1990. Because employers have to make a conscious choice about which newspaper to advertise their vacancy in, help-wanted ads uniquely capture employers' decisions over where to hire. I use a combination of machine learning and keyword search to collect a random sample of job postings from 216 digitally archived newspapers; I focus on recruitment in 1990 to avoid any overlap with the emerging online tools. My final data set contains more than 300,000 job postings from all occupation groups, covering a wide range of US cities and towns.

I find that, while the majority of newspaper help-wanted ads in 1990 were advertised locally, there was a non-negligible share (11%) of vacancies posted in another town, county, or commuting zone. This average masks a significant heterogeneity across occupations: 15 to 17% of vacancies in Management, Architecture & Engineering were posted outside of the town where the job is located, but only between 6 and 8% of help-wanted ads in Construction and Food preparation & serving were non-local. To understand *why* firms recruit non-locally, I compare labour market conditions in the location of the job to those in the location of the vacancy within each help-wanted ad. If firms post outside of their local labour market to access a large pool of cheap, potentially unemployed labour, we would expect the pay and unemployment rates to be higher in the location of the vacancy. If, on the other hand, firms recruit non-locally to find specific talent and improve the quality of the match, they might place their vacancies in locations with higher wages and a larger labour pool overall. I find that the latter is true for the average non-local vacancy: it tends to be posted in labour markets that are larger, both in terms of the size of the labour force and the total number of employers, and that are paid marginally more than the location of the job. In contrast, the difference in unemployment rates is small and negative: non-local vacancies are primarily posted in locations with lower, rather than higher, unemployment rates. Finally, I compare the relative occupational specialisations of job and vacancy locations. I find that, in general, recruitment is more likely to be local when the occupation the firm is looking for is relatively abundant in its local labour market. When that's not the case, employers advertise in newspapers in such (more specialised) locations. This serves as further evidence that non-local recruitment is primarily used to find workers of specific skill.

Given that Craigslist is a direct successor to newspaper classified ads, these patterns generate several predictions about its impact on labour market outcomes at the occupation level. First, if the underlying mechanism is an increase in matching across space, occupations with the highest migration churn should also experience the largest wage increase. I demonstrate that while in control cities, changes in wages and mobility over time are negatively correlated – as one would expect from shifts in labour supply – the relationship is positive for Craigslist cities. Second, I show that the impact of Craigslist was more pronounced in jobs which were advertised non-locally before its introduction. This makes sense for two reasons: these occupations were already recruiting cross-regionally and so should react more strongly to a fall in the price of non-local recruitment; and non-local recruitment in 1990 was most pronounced in occupations with a relatively large return to skill (or specific match). A corollary of this statement is that the growth in wages should be concentrated at the top of the wage distribution, in jobs where the quality of the match matters the most. I document this pattern holds within occupations and drives most of the observed post-Craigslist difference between occupations with historically different non-local recruitment. In other words, Craigslist's impact is concentrated in the top end of the distribution within occupations with a high return to the quality of the match.

This paper contributes to several streams of literature. I directly build on the research showing that access to the Internet has a mixed-to-positive impact on individual-level job-finding rates (Stevenson, 2009; Denzer, Schank, and Upward, 2021; Kuhn and Mansour, 2014; Kuhn and Skuterud, 2004; Gürtzgen et al., 2021) and matching efficiency and search costs at the market level (Kroft and Pope, 2014; Bhuller et al., 2022). Similarly to Bhuller et al. (2022); Denzer et al. (2021); Gürtzgen et al. (2021), I also exploit the staggered roll-out of an Internet-related technology, but, in contrast to the existing literature, I focus on its impact on search and matching between local labour markets. Methodologically, this paper is most closely related to Kroft and Pope (2014) who use the variation in the popularity of Craigslist in 2005-2007 to estimate its impact on local unemployment and housing vacancies. Their key finding that Craigslist didn't significantly lower unemployment rates in the cities it entered can be rationalised by my result that the technology increases mobility *between* markets. To the best of my knowledge, the only other paper examining the impact of online recruitment on geographic mobility is a study of broadband roll-out in Norway by Bhuller et al. (2022). They look at geographic mobility as a part of robustness checks, finding a small positive effect on commuting distance but no changes in moves between commuting zones. These different results are likely attributable to the differences in economic geography between the US and Norway: much of the latter's territory is sparsely populated, and its within-country mobility is about two-thirds of that in the US.[4]

More broadly, this paper deepens our understanding of the role of spatial search frictions in internal mobility and regional divergence in the US. Schmutz and Sidibe (2018); Ransom (2022); Balgova (2022); Porcher (2021); Fujiwara, Morales, and Porcher (2021); Wilson (2021) document how search and information frictions caused by geography influence workers' decision to move across labour markets, reducing their mobility. My findings contribute to this literature by showing how online recruitment removes some of these frictions. At the same time, however, I find that the resulting growth in mobility and wages increases, rather than decreases, regional differences. These patterns are in line with the longer-term trends in regional divergence between US regions, first described by Berry and Glaeser (2005); Ganong and Shoag (2017). Starting in the 1980s, the convergence that saw poorer regions in US catch up reversed, with high-income and low-income cities now growing at the same rates. Giannone (2022) shows that this divergence is almost entirely driven by the growing return to skill in urban areas. This paper complements this literature by describing the technological changes that made the process of matching and moving into high-return jobs easier for high-skilled workers.

Methodologically, this paper contributes to the growing literature using job vacancies as a source of data on the labour market. The overwhelming majority of these papers make use of online vacancy data, which is more easily available as well as easier to analyse (Hershbein and Kahn, 2018; Marinescu and Wolthoff, 2020; Adams-Prassl, Balgova, Waters, and Qian, 2020; Clemens, Kahn, and Meer, 2021). To the best of my knowledge, there are only two papers utilising the text of newspaper job postings. Atalay, Phongthiengtham, Sotelo, and Tannenbaum (2020) conduct a detailed text analysis of help-wanted ads from three major

[4]In 2019, the migration rate between Norway's seven regions was 1.9% compared to the 3.2% between-county mobility rate in the US. Data sources: Statistics Norway and US Census Bureau.

newspapers to map the evolution of task content of jobs. Anastasopoulos, Borjas, Cook, and Lachanski (2021) make a similar use of postings from *Miami Herald* to measure the labour demand response to the Mariel Boatlift. Compared to these papers, I rely on a larger sample of newspapers and exploit previously unused data on the spatial dimension of recruitment.

The rest of this paper proceeds as follows. In section 2, I describe the data used in my analysis, including the new data set on recruitment across space. Section 3 provides background information about Craigslist and its expansion and outlines the paper's identification strategy. I present my main empirical results in section 4. In section 5, I summarise the patterns of non-local recruitment before the introduction of the Internet, and in section 6, I use these patterns to test predictions about the mechanism driving my main results. Section 7 concludes.

# 2 Data

In this paper, I study the impact of online recruitment in the years 1999-2008 on two labour market outcomes: within-country migration and wages. I focus on these outcomes at the level of metropolitan statistical areas (MSAs) and MSA-occupation level. I use the terms "MSA" and "city" interchangeably throughout the paper. I combine the data on these outcome variables with data on Craigslist entry and popularity and city-level characteristics. I further complement it by constructing a novel dataset on non-local recruitment across US newspapers in 1990. In this section, I describe the origin, definition and construction of each variable. This information is also summarised in Table A1.

## 2.1 Outcome variables

The data on the main outcome of interest, city-level number of movers, comes from the tax statistics of the Inland Revenue Service. The annual county-to-county mobility flows are constructed from year-to-year changes in the address of residence on individual tax returns.[5] This data captures 95-98% of all tax filing population and provides an exhaustive coverage of the entire US territory.[6] I aggregate the county-level inflows and outflows to the city level using the US Census historical delineation files for MSAs in 2003. I construct two versions

[5]The year of migration is taken from the tax filing year, not the year in which the tax income was earned. The data includes individuals who filed their tax return by the September deadline, e.g. the data for 2003 moves includes all individuals who filed their tax return by September 2003 and moved since they filed their 2002 taxes (i.e. any time between January 2002 and September 2003).

[6]Not every US resident is obliged to file a tax return. The elderly and the poor are particularly under-represented, as are potentially the very wealthy individuals with complex tax arrangements who need an extension to the September filing deadline.

of the city-level flows: bilateral city-to-city flows, and total city-level inflows and outflows.

Unfortunately, the IRS data doesn't include any individual-level characteristics of the tax filers. As a result, for migration flows at the occupation level, I draw on the American Community Survey, an annual nationally representative survey of more than 3 million individuals (1% of the US population). Starting in 2005, the ACS records the respondents' city of residence in the previous year, along with the current city of residence. This allows me to extract information about migration inflows and outflows at the city-occupation level. While the sample of movers is too small to construct bilateral city migration flows, it is possible to calculate the number of total inflows and outflows at the city level for major occupation groups.

Wage information is taken from Occupational Employment and Wage Statistics, an annual survey of employers conducted by the US Bureau of Labor Statistics. This data set calculates occupation-specific wages at the metropolitan level. I focus on the mean, top and bottom deciles of hourly nominal wages. While OEWS is the most comprehensive data set on city-specific wages for the US, there are three potential issues with the data. First, wage estimates are not always available for all years and all city-occupation cells, primarily due to data confidentiality and data quality reasons. These gaps in coverage will not bias the estimation as long as the observations are missing at random. More broadly, OEWS surveys 396 out of the 922 MSAs, mostly those in the upper half of the population distribution. However, as I explain in Section 3, this is not an issue given my empirical design.[7] Second, the definition of MSA used in OEWS changed in 2005. Since this switch brought the survey in line with the 2000 Census MSA delineation, I use the 2005 MSA definitions as the baseline and manually construct a crosswalk to the 1999-2004 MSAs. Third, the data collection methodology of semi-annual overlapping panels of establishments means that any sharp changes in wages appear in the data more gradually. While this doesn't preclude the use of the data set for time-series analysis, it means that, in my case, any estimates of changes to wages based on a sudden expansion of Craigslist are likely to be biased to 0.[8]

## 2.2 Craigslist

I measure CL's expansion using 2 variables: the year the website entered a particular city, and the popularity of city-specific websites. Both of these variables come directly from the website itself. The timing of CL's expansion across US cities until June 2006 is documented

[7]Since most CL cities are also relatively large, I balance the sample of controls to focus on large cities, As a result, the missing smaller cities in OEWS wouldn't be used in the analysis anyway.

[8]Another challenge with using OWES for comparing occupation wages over time are the changes to occupation classification. However, OEWS was using the 2000 SOC throughout the time period of study, 1999-2008.

on Craigslist's About webpage[9]. To map out the following wave of expansion in November 2006 (no cities were added in 2007), I use the list of CL's websites as archived at the end of 2006 on the Internet Archive Wayback Machine, cross-checking it with similar data set constructed by Djourelova et al. (2022). The measure of intensive treatment, CL's city-specific popularity, is the number of job posts per 1,000 inhabitants posted in April 2007, as collected by scraping the CL websites in Kroft and Pope (2014). I conduct webscraping using the Internet Archive Wayback Machine to describe the occupational distribution of the posted vacancies. For both measures, I match the city of the website to its corresponding MSA following Kroft and Pope (2014).

## 2.3 Internet penetration and location characteristics

I measure Internet availability, an important factor in CL expansion, using data on the number of high-speed Internet service providers from the Federal Communications Commission. The FCC creates this statistic from compulsory semi-annual reporting by Internet service providers (so-called Form 477)[10], and it is available at ZIP code level. To aggregate it to the number of Internet service providers at the MSA level, I calculate a weighted average for each city, using ZIP code population from the 2000 Census as weights.

Data on MSA characteristics comes from 3 different sources. I use OEWS for employment shares and the US Bureau of Economic Analysis for city-level GDP growth rates. I draw on the 2000 Census for the remaining variables (population, land area and density, demographic characteristics).

For county-level characteristics, I draw on annual averages from the Quarterly Census of Employment and Wages, a combination of administrative and survey data capturing labour market statistics for more than 95 percent of U.S. jobs. I use data for 1990 to correspond to my data set on non-local recruitment from the same year.

## 2.4 Non-local recruitment in 1990

I measure firms' recruitment across space from a novel source: help-wanted ads in US newspapers in 1990. I focus on hiring via newspapers because firms' decision over which newspaper to place their job posting is one of the few directly observable outcomes of their spatial recruitment strategy. I collect my data from newspapers in the year 1990 for two reasons.

[9] `https://www.craigslist.org/about/expansion`

[10] The service providers are required to report on whether they offer Internet access at speeds exceeding 200 kbps to at least one customer.

First, it allows me to avoid any overlap with the arrival of the new online recruitment tools.[11] Second, I am limited by the digitisation process of commercial newspaper archives, which predominantly focus on historical newspapers and have limited availability in the 1990s.[12]

A shortcoming of this approach is that, even in 1990, not all vacancies were advertised in a newspaper: firms also relied on informal networks, recruitment and employment agencies, trade journals, or simply on posting a piece of paper in their shop window. Furthermore, employers likely selected into the different recruitment channels non-randomly so newspapers not only fail to capture all vacancies, they are probably not fully representative of US jobs either. Despite this, the lack of survey data on vacancies has made newspaper help-wanted ads the main gauge of labour demand in the US for much of the postwar period. Until the introduction of JOLTS by the Bureau of Labor Statistics in 1998, the leading – and sometimes only – measure of open job postings was the Conference Board Help Wanted Index, which captured the number of vacancies posted in 51 metropolitan newspapers across the US.[13] My approach thus builds on and expands the standard measure of recruitment in that period.

The source of the help-wanted ads is the digital newspaper archive *Newspapers.com*. It is one the largest archives of digitised newspapers, offering more than 20,000 titles from the 18th century until the early 1990s. I randomly select four days in each month of 1990[14], using the website to search for all newspapers pages on the given day that contain the phrase "help wanted"[15]. This search results in 19,500 newspaper pages. I use a machine learning algorithm to exclude pages that do not contain readable vacancies, leaving 7,850 usable newspaper pages, and then run a customised keyword search algorithm to extract, from each help-wanted ad, information on the vacancy's occupation and job location.[16] Further details of the page scraping, data cleaning and text analysis can be found in the Appendix.

[11]The four main online recruitment websites in the US in 2000s, Craigslist, CareerBuilder, Yahoo Hotjobs and Monster.com were founded between 1994 and 1996.

[12]There appear to be two reasons for this. First, commercial digital newspaper archives are primarily marketed at customers conducting genealogy research, so newspapers from the 19th century and older are more valuable than newspapers from 30 years ago. Second, the older newspaper issues are no longer behind paywalls by their publishers. The Chronicling America project by the Library of Congress, for example, only runs to 1963.

[13]The index was used as a reliable and representative measure of labour demand in research on a wide range of topics, such as the trends in structural unemployment, productivity, and to understand labour demand over the business cycle (Abraham and Wachter, 1987). It was made redundant by the massive shift of vacancy posting online and was discontinued in 2008.

[14]I exclude public holidays but include Sundays, as many newspapers publish on Sundays, and these editions often carry the bulk of the week's help-wanted ads.

[15]Other phrases, such as "employment" and "jobs" rarely returned any new relevant pages, and contained a lot of false positives in the form of newspaper articles about the economy.

[16]Note that I do not identify the start and end of each advertisement. Instead, I analyse the whole page in one go, re-constructing each ad from the position and order of the recovered keywords.

Each help-wanted ad contains three pieces of information: the location of the job (employer), the location of the newspaper, and the job title (occupation). When the location of the job is missing, I assume the vacancy is local. To assign an occupation to each posting, I construct a database of job title keywords from the Alphabetical Index of Industries and Occupations, an exhaustive list of occupations and their related job titles developed by the US Census Bureau. It contains almost 31,000 job titles with their corresponding SOC codes, so each job title can be linked to the occupation hierarchy. The list of geographical keywords comes from a gazetteer of the US created by GeoNames[17] and allows me to identify more than 9,500 towns and cities across the US together with their GPS coordinates. These linkages uniquely identify 89.5% locations and 95.4% job titles, and make it possible to merge the vacancy data with any other information on occupations and locations, as well as calculate the distance (in km) between the location of the job and the recruitment location of the vacancy.

The main features of the dataset are described in Table 4. The final sample is fairly representative of US geography (see also Figure A3). It consists of vacancies from 216 newspapers that cover 44 (out of 50) states, 93 (out of 922) MSAs and 174 (out of 3142) counties. The vacancies themselves are even more diverse, originating from 47 states, 242 MSAs and 864 counties. In terms of occupations, the data includes 93 out of 96 occupation groups at the 3-digit SOC level. In total, the sample consists of 318,183 vacancies.

# 3 Empirical strategy

I use the expansion of a particular online job website, Craigslist (CL), to understand the impact of online recruitment on the spatial dimension of the labour market. In this section, I introduce the website and outline the empirical strategies I use to identify and estimate its impact on geographic mobility and wages.

## 3.1 Craigslist: introduction and expansion

Craigslist is a classified ads website. Just like a local newspaper, it provides space to sell and buy cars and baby clothes, advertise real estate, and look for anything from a babysitter to a romantic partner to a job. Employers wishing to fill a vacancy can advertise under "jobs" (or "gigs" for ad-hoc, short-term work), an online version of a help-wanted ads section. The website also offers a "resumes" section that allows individuals looking for work to upload their CVs.

[17] https://www.geonames.org/

However, CL is not a typical online recruitment platform. Unlike other online major recruitment websites such as Monster.com, CareerBuilder or Indeed, it doesn't help employers screen applicants, use AI to enhance the quality of job posts or offer an online infrastructure for interested job-seekers to apply directly on the platform. Instead, CL still functions as an online version of a local newspaper classified ads section: it helps to distribute information and leaves the rest of the matching process to the workers and employers. In fact, the website's design has changed relatively little since its inception in 1995. The second major difference is that Craigslist operates a dedicated website for each geographic location. Users are asked to post on their local CL website, and posting on multiple city-specific sites is explicitly prohibited.[18] As a result, even though CL job posts are accessible to job-seekers in any household with an Internet connection, only those employers located in a city with a dedicated CL website are able to use it to advertise their vacancies.

These features of CL are a key part of my identification strategy. First, while CL entry makes transmission of information across space considerably faster and cheaper, the lack of other search-and-match-enhancing capabilities allows me to separately identify the effect of the Internet as an information transmitter rather than a direct improvement in matching online. Second, the localised nature of CL websites creates significant geographic heterogeneity in treatment, making it possible to compare treated and control cities (and jobs).

The easy navigation of the website, together with the fact that most posts on CL were free[19], resulted in CL becoming the leading classified-ads website in the US and one of the most visited websites in general during the period studied in this paper. In 2006, CL ranked 47th in terms of unique number of monthly visitors, and 7th in terms of monthly page views – double that of Amazon.com.[20] Kroft and Pope (2014) estimate that in 2007, CL held a two-thirds market share on online job posts from among the 4 major online recruitment websites (CL, Monster, CareerBuilder, and Yahoo/Hotjobs) which, in turn, captured the majority of all online vacancies. CL achieved this level of success in a relatively narrow window between 2004 and 2007. The website was founded by Craig Newmark in 1995 and served only the San Francisco Bay Area until 2000 when it started its slow expansion across other major

[18]Early on, CL was relying on a system of manual flagging of posts that violated its terms of use. In later years, duplicate postings would be automatically blocked. Source: `https://www.craigslist.org/about/help/faq`

[19]Until 2008, CL only charged for job postings in its 11 biggest markets (San Francisco Bay Area, Boston, Chicago, Los Angeles, New York, Orange County, Portland, Sacramento, San Diego, Seattle, and Washington DC), plus a fee for brokered apartment rental listings in New York. Source: `https://web.archive.org/web/20080228034906/https://www.craigslist.org/about/help/posting_fees`. Cajner and Ratner (2016) show that the change in CL's pricing policy at the end of 2012 significantly reduced the number of online job posts, and substantially drove the divergence between JOLTS and the Help-Wanted Online Index.

[20]Source: `https://web.archive.org/web/20170909054652/https://www.forbes.com/2006/12/08/newspaper-classifield-online-tech_cx-lh_1211craigslist.html`

US cities (see Figure A1). This expansion sped up in 2005, when CL entered 41 MSAs, and was accompanied by a sharp growth in the number of users and monthly visits that wasn't mirrored in that of its main competitors (Kroft and Pope, 2014; Djourelova et al., 2022).[21]

I exploit this sharp and unexpected growth in CL use and availability to identify the impact of online recruitment on labour market outcomes. For this strategy to work, CL's expansion shouldn't be driven by expected growth in the local labour markets it is planning to enter. Importantly, anecdotal evidence as well as several studies (Kroft and Pope, 2014; Djourelova et al., 2022) have shown that CL's expansion and popularity were primarily driven by the idiosyncratic opinions of its CEO and owner rather than strategic profit-maximisation or predictions about economic growth. In a 2015 interview with the San Francisco Chronicle, Craig Newmark (the owner) explained that the entry into a new city is determined by demand from potential local users and Jim Buckmaster's (the CEO) perception and intuition about the city.[22] He identifies just two relevant city characteristics that determine CL's entry – the city's population size and broadband availability.

I present the descriptive statistics of CL and non-CL cities in the first three columns of Table 1. CL's city entry strategy is reflected in the characteristics of CL cities: they are significantly larger, both in terms of population and land area, than cities where CL didn't enter by 2007. CL cities are also more densely populated, more ethnically and racially diverse, younger, and with a somewhat higher employment share and wages. Interestingly, however, CL and non-CL cities do not differ in their migration in- and out-flows, which hover at around 2% annually.

Nevertheless, systematic differences between CL and non-CL cities aren't necessarily evidence of CL strategically entering high-wage, high-employment, or high-growth cities. I test this hypothesis for the years 2004-2007 directly in Table 2. Each column of the table summarises the results of a probit regression of CL entry on city characteristics before 2004. The first two columns confirm Craig Newmark's statement that CL is significantly more likely to enter large cities with a large number of Internet service providers, a reliable proxy for the availability and use of the Internet (Djourelova et al., 2022). In column (3), I regress CL entry on realised employment and GDP growth over the 4 years prior to CL entry (2000-2003), and in column (4), I regress it on the predicted growth in these variables for the years 2004-2007.[23] Neither realised nor predicted city growth explains CL entry.

[21]Kroft and Pope (2014) use Corzen.com proprietary data to track the number of posts in a subset of CL websites alongside the main competitors (Monster, Careerbuilder, Yahoo) over time. Djourelova et al. (2022) draw on data from Comscore, a nationally representative survey of web browsing behaviour, to compare CL's traffic to that of Monster and eBay.

[22]Source: `http://www.sfgate.com/cgi-bin/article.cgi?f5/c/a/2004/08/15/NEWMARK.TMP.`

[23]I run a linear dynamic forecast of city-level employment and GDP growth using the lagged dependent variable and the following city characteristics from the 2000 US Census, based on Glaeser and Shapiro (2001):

Column (5), which simultaneously controls for all variables, shows that if anything, higher predicted GDP growth decreases the probability that CL will enter the city over the given time period. Overall, even though population size and the growth in Internet availability are strong predictors of CL entry, all the variables combined explain only 15.5% of the variation in CL entry, supporting the anecdotal evidence that CL entry was primarily driven by the enthusiasm of local supporters and the idiosyncratic preferences of CL management.

## 3.2 Main empirical strategy: staggered rollout of Craigslist

My main identification strategy for estimating the impact of CL entry is based on the staggered rollout of CL across US cities between 2004 and 2006. As explained in the previous section, CL entry in US cities varied across time, and this entry wasn't associated with higher past or predicted economic conditions at the city level. As a result, I identify the treatment effect on wages and migration by comparing city-level outcomes between treated and control (never-treated and not-yet-treated) cities, taking into account the staggered timing of CL entry. I estimate the following two-way fixed-effects dynamic difference-in-differences regression:

$$Y_{ct} = \alpha_c + \beta_t + \sum_{j=-2}^{j=3} \gamma_j CL_c + + \sum_{j=0}^{j=3} \delta_j CL_c + \theta_t X_c + \epsilon_{ct} \tag{1}$$

where $Y_{ct}$ is the outcome for city $c$ in year $t$, $\alpha_c$ and $\beta_t$ are city- and year-fixed effects, and $\delta_j$ are the parameters of interest, estimating the difference between treated and control cities for up to 3 years after the treatment. $\theta_t X_c$ is the interaction of year-FE and baseline city-level Internet availability (in levels and growth), allowing for differential growth rates of cities with different connectivity in 2001. This specification will produce an unbiased estimate of the average treatment on the treated for each year after treatment as long as, in the absence of treatment, wages and mobility in CL cities would have evolved in the same way as in non-CL cities (parallel trends assumption). While this assumption cannot be tested empirically, coefficients $\gamma_j$ allow me to test whether the treated and control cities evolved similarly before the treatment.

Furthermore, I construct the control group so that the two groups are comparable in pre-treatment *level* characteristics. Using Mahalanobis distance matching, I select a subsample of never-treated cities that are similar to treated cities along a range of characteristics in the year 2000 (population size, land area, population density, racial and ethnic composition,

education attainment, population density, the share of workers working in the metropolitan city, the age of city buildings, the number of cars per household, the length of the average commute.

age, employment share). I present the balance test for the matched sample in Table 3. The first 3 columns summarise the baseline characteristics of treated and control cities and their difference. The matching procedure does a good job of pairing cities that are similar in terms of migration rates, demographics, and employment share. The only remaining statistically significant differences are in population size and density: treated cities in the balanced sample are on average twice as large as the control cities, with a correspondingly higher population density. As a result, workers in the treated cities are paid on average 50c an hour more. This difference in city size stems from CL's focus on large cities: by the end of 2006, CL had entered 164 out of the 200 most populous cities in the US (see Figure A2). As a consequence, restricting control and treated cities to match on population size would lead to a very small control group.

To rule out the possibility that the estimated treatment effects are driven by city size rather than CL entry, I perform two robustness checks. First, I impose common support in population size on the matched control and treated cities. Second, I only estimate the dynamic diff-in-diff on (all) CL cities, identifying the treatment effect solely from the staggered timing of CL entry. In addition, I address the question of the appropriate control group addressed directly in the second empirical strategy (see next section).

Another threat to identification is anticipation effects. However, I argue that these are less relevant in this particular setting. CL entry into a city was unannounced (**?**), and in general, CL didn't rely on advertisement to attract users and boost its viewership, making it difficult for employers to increase wages (or workers to bargain for higher pay) in anticipation of CL entry. Anticipation effects are even less plausible for migration flows in and out of the treated cities, since my main hypothesis is that CL websites provided the mechanism that made moving easier, and movers had little to gain from moving in anticipation of CL entry.

I focus on CL entry in the years 2004-2006 for two reasons. First, it offers a cleaner definition of "treatment": Kroft and Pope (2014) demonstrate that CL popularity in terms of pageviews and users only started to grow substantially in 2005. As a result, cities where CL entered before this date had access to the technology, but a much smaller user base likely resulted in a weaker effective treatment. This argument is closely related to the second motivation for focusing on entries starting in 2004: treatment effect heterogeneity. The treatment effect of CL entry for the earlier years might be different not only because of differences in general CL popularity but also because the CL entry in its first few years was heavily concentrated among the few largest MSAs in the country.[24] Including these cities in the treatment group would lead the standard dynamic two-way fixed-effects specification to

[24] Cl entered all 10 largest MSAs by 2003. Following its first location in San Francisco Bay Area, it entered the 3 largest MSAs (New York, Los Angeles and Chicago) in the first expansion wave in 2000.

produce biased estimates of the treatment effects.

## 3.3 Alternative empirical strategies

**Externally determined control group** The chief potential weakness of the empirical strategy outlined above is that CL picked cities to enter based on predicted strength growth in the local economy. Even though I have shown, in Table 2, that the past nor forecast employment and GDP growth can explain CL entry, it still might be the case that CL management was able to cherrypick the cities that would experience faster wage growth and mobility in the future. This is particularly concerning given that there is no publicly available data explaining how exactly Craig Newmark and Jim Buckmaster selected which cities to operate in. The ideal set of control cities would consist of near-identical MSAs that CL should have entered but didn't. Crucially, they should not only be similar to CL cities in terms of their baseline characteristics, but also in their market potential and profitability.

In this section, I present an externally identified set of control cities that plausibly fit the characteristics of an ideal control group. I then re-estimate the dynamic diff-in-diff specification (regression (1)) using this externally determined control, complementing my main empirical strategy.

This second identification strategy originates in a short-lived involvement of eBay, the online marketplace, with Craigslist in 2004-2008. In the early 2000s, eBay developed an interest in the online classified business; the company saw it as a natural next step in its growing portfolio of various trade-related online services. When in 2003 a disgruntled Craigslist stake-holder decided to sell his 28.4% share in the company, eBay seized the opportunity and became a minority shareholder. eBay's objective was to eventually purchase the entire company, but following strong opposition from the other two owners (Craig Newmark and Jim Buckmaster, the CEO), eBay decided to use its minority stake to at least "learn the "secret sauce" of Craigslist's success, presumably so that eBay could spread that sauce all over its own competing classifieds site.".[25] Over the following 3 years, eBay's representatives on CL's board of directions passed on non-public financial statements, capacity projections, and other private Craigslist documents on to its internal team tasked with launching Kijiji.com, eBay's version of classified ads website.

Kijiji launched at the end of June 2007 across 220 US cities: in 174 out of 224 CL cities, plus another 37 cities that CL hadn't entered yet. These non-CL Kijiji cities form a near-ideal control group for CL cities, for three reasons. First, eBay decided to launch

[25] This is a quote from a ruling from the Supreme Court of Delaware on eBay v. Craigslist. The statement is based on internal eBay communication that was presented in the trial. Source: `https://caselaw.findlaw.com/court/de-supreme-court/1558886.html#footnote_33`

Kijiji after it became clear they wouldn't be able to acquire Craigslist, and used private Craigslist data to decide which cities to enter. Given the strong for-profit motivation of eBay, especially in contrast to the "business as a community service"[26] philosophy behind Craigslist, Kijiji cities can be interpreted as the cities CL should have entered to maximise profits and growth. Second, eBay's development of Kijiji didn't affect CL's operations. CL directors were unaware of the Kijiji launch until less than a month before the website went live, and they didn't know that some part of Kijiji strategy was based on internal CL documents until eBay sued CL over the breach of fiduciary duty in 2008 and their activity was revealed in the legal discovery. Third, even though Kijiji officially launched in June 2007, it didn't start to fully compete with CL until 2008. This means that the Kijiji launch didn't significantly alter CL operations and userbase until after the time period studied in this paper.[27]

I present the characteristics of non-CL Kijiji cities in column (4) of Table 1. Two main patterns stand out. First, non-CL Kijiji cities are substantially different from other non-CL cities (column (5)): they are more than 5 times larger in terms of their population, have higher population density, are somewhat younger, and have larger employment shares and wages. Second, all of these attributes make them relatively much more comparable to CL cities. I make this point explicitly in columns (4)-(6) of Table 3, where I compare CL and non-CL cities within the Kijiji group. While non-CL Kijiji cities are still somewhat smaller than CL cities, this difference is substantially smaller than for the matched treatment and control cities, and it is significant only at 10%. The two groups of cities are statistically indistinguishable in all other characteristics except for migration shares, where non-CL Kijiji cities experience somewhat greater in- and out-flows than CL cities. The same pattern is visible when we compare the control group (non-CL Kijiji cities) to all CL cities (presented in column (1)): the population size difference disappears, and the two groups are near-identical in their demographics and labour market characteristics.

**Craigslist popularity** The second alternative identification strategy is based on Kroft and Pope (2014) who exploit the variation in CL's popularity across cities rather than the timing of its entry. The advantage of their approach is twofold. First, it identifies the impact of CL within the cities that CL entered, circumventing the problem of endogeneity of CL entry. Kroft and Pope (2014) argue that the popularity of the website was driven by chance much more than its entry (which was, after all, decided by the company's CEO). Second, given that the impact of any technology depends on the extent of its usage, focusing on the

[26]Source: `https://caselaw.findlaw.com/court/de-supreme-court/1558886.html#footnote_33`
[27]In fact, Kijiji never fully took off in the US and the website was shut down in 2010.

intensive margin is probably closer to the ideal experiment in which some cities randomly use CL and others don't. In other words, the estimation using staggered CL entry identifies an intention-to-treat effect, while the estimation using CL popularity identifies a treatment effect.

I combine the two approaches and estimate dynamic difference-in-differences using CL popularity as a continuous treatment. I compare cities where CL became more popular to cities where it was less popular or it never entered, using the dynamic specification to control for the staggered CL entry. I measure CL popularity using the scraped number of job posts across city websites in April 2007, normalised by city population, from Kroft and Pope (2014). As in my main empirical strategy, I restrict my attention to cities that are matched on baseline covariates. I run the following version of regression (1):

$$Y_{ct} = \alpha_c + \beta_t + \sum_{j=-2}^{j=3} \gamma_j Posts_c + + \sum_{j=0}^{j=3} \delta_j Posts_c + \epsilon_{ct} \tag{2}$$

where, as before, $\delta_j$ is the coefficient of interest and $\gamma_j$ allows us to test for parallel trends before treatment. Unlike in the binary main specification, the interpretation of these coefficients is somewhat more complicated. Callaway, Goodman-Bacon, and Sant'Anna (2021) show that $\delta_j$ can be interpreted as a weighted average of causal response at a particular treatment dose, i.e. the change in outcomes due to a change in treatment dose.[28] However, in order to interpret $\delta_j$ as the average causal response of CL popularity on labour market outcomes, we need to be willing to make several assumptions about the nature of the treatment.

First, I need to assume so-called strong parallel trends: high-CL-popularity cities would develop similarly to low-CL-popularity cities had CL been less popular there. In other words, not only do non-CL cities need to be a good counterfactual for CL cities, but low-CL-popularity cities must be a valid counterfactual to high-CL-popularity cities. This assumption is satisfied if there is no selection into the intensity of treatment: CL popularity mustn't be driven by expected wage growth or mobility at the city level. While Kroft and Pope (2014) show that, unlike CL entry, the number of posts isn't correlated with population size or Internet availability, and that, anecdotally, CL management was surprised by where CL became popular, CL still might have been used more enthusiastically in cities where its returns were higher – for example in tight labour markets which would have seen fast wage

[28] The alternative interpretation is that $\delta_j$ corresponds to the average treatment effect on the treated, i.e. the average *level* difference between treatment and no treatment. However, as the authors show, TWFE estimates of $\delta_j$ are weighted dose-specific ATT effects, using weights that are not always sensible or non-negative.

growth anyway. To address this issue of selection bias, I instrument the number of job posts with the number of normalised personal posts (community, services, for sale) in the same city. This IV will be valid if CL popularity in one type of posts spills over into the popularity of other types of posts, but, at the same time, CL popularity in personal posts mustn't be related to local labour market conditions.[29]

The validity of the first assumption depends on the validity of the instrumental variable. While it's not possible to prove the exclusion restriction holds, it is supported by the anecdotal evidence that the website's popularity was difficult for CL management to predict, as described in Kroft and Pope (2014). Regarding the instrument's relevance, I document the first-stage relationship between the number of personal posts and the number of job posts in Figure 1. Panel (a) of the figure shows that there was significant variation in CL popularity. About half of the CL cities only recorded 1 job post per 1,000 inhabitants in April 2007, but the top quartile of cities has double the average number of posts. In panel (b), I show that CL popularity in job postings was closely related to CL popularity in personal posts: in other words, cities where CL was a popular platform for personal ads were also the cities with a high number of job postings, making it a relevant instrument for job post popularity.

The second necessary assumption for the correct interpretation of $\delta_j$ concerns the homogeneity of the treatment effect. The dynamic diff-in-diff specification in equation (2) aggregates individual dose-specific causal responses. This aggregation can be interpreted as an average causal response to CL popularity when the treatment effect is homogeneous across (i) the timing of the treatment, and (ii) the treatment intensity. In other words, I assume that a city with CL entry in 2004 would have reacted to its treatment in the same way had CL entered in 2006; and that the causal response function of CL popularity is linear, so that low CL popularity and high CL popularity result in the same relative causal response. The linearity assumption is imposed by the functional form of regression (2). Probably the main reason why assumption (i) might not be satisfied is if CL popularity depends on how long the website has been available in the given city. If CL popularity grows over time, the older CL cities will be treated more intensely than the ones where CL entered relatively recently (note that I cannot capture the changes in CL popularity over time because the measure of CL posts is only available for a single month at the end of the studied time period[30]). Two pieces of evidence make this relatively unlikely. First, the period of treatment under study, 2004-2006, is chosen precisely because it was a time of rapid CL expansion and popularity growth across the US; CL websites reached relatively high levels of popularity within a few

[29] In the language of Callaway et al. (2021), a valid instrument will remove the selection bias term from the estimates of average causal response.

[30] The number of posts is measured in April 2007 for all MSAs that CL entered by June 2006. This means that this sample excludes the 61 MSAs CL entered in November 2006.

months of entering the market. Second, I show in panel (c) of Figure 1 that average CL popularity varied only weakly across the three years of entry in my sample. While CL entered its most popular cities in 2004, only 5 cities from this entry cohort experienced higher popularity than the most popular CL city of 2005. Moreover, mean popularity is statistically the same for the 2005 and 2006 entries, suggesting that the intensity of treatment wasn't significantly lower for cities where CL entered late.[31]

# 4 The impact of Craigslist on mobility and wages

This section summarises the main empirical findings of the paper. I show that Craigslist's entry into different US cities caused an increase in migration inflows and outflows from these cities, and raised wages, especially at the top of the distribution. The complementary identification strategies and robustness checks provide evidence that this treatment effect is mainly driven by Craigslist's use as an online recruitment tool.

## 4.1 Impact on geographic mobility

I present the main results of the impact of Craigslist on city-level aggregate migration inflows and outflows in Figure 2. The dependent variables are the annual totals of inflows and outflows of individuals from other US cities. I start by plotting the raw data in panels (a) and (b). They show the results of a dynamic difference-in-differences regression for all Craigslist entries after 2003, using the cities that CL never entered before 2008 as the control.[32] These raw results suggest that CL entry increased both moves in and out of the affected cities, although we do see some level differences between the treated and control cities before the treatment: CL cities had somewhat lower inflows and somewhat higher outflows at the baseline.

To address this, I estimate my main empirical specification, regression (1). I run it on a sample that matches CL and non-CL cities on their baseline covariates, and I allow mobility to vary across cities with different Internet availability since this has been identified as one of the key factors of CL entry. I present these estimates in panels (c) and (d) of Figure 2. The plots show that CL entry significantly increases both the number of inflows and

[31] This finding is at odds with the patterns reported in Kroft and Pope (2014), who find that the time of entry is one of the few predictors of CL popularity in a given city. This discrepancy is entirely driven by the differences in samples. While Kroft and Pope (2014) look at all CL entries since 1995, I restrict my attention to entries in or after 2004. The fact that the first cities CL entered were significantly different from the average US city – and we would thus expect CL popularity to differ - is one of the main reasons why I restrict my sample this way.

[32] In other words, this sample excludes the 28 cities where CL entered between 1995 and 2003.

outflows from the city. By year 2 after CL entry, the treated cities experience on average 1113 more individuals moving into the city, and 643 more individuals moving out of the city compared to cities without CL; this effect increases over time. Given the average number of in- and out-movers per year in a CL city (17,701 and 16,761, respectively, in the year before treatment), this CL-induced increase corresponds to a 6.3% increase in inflows and a 3.8% increase in outflows at the city level.

I explore these patterns further by running a series of robustness checks and alternative identification strategies. In Table 5, I present the difference-in-difference estimator for a range of alternative specifications[33], starting by replicating my preferred specification in column (1). In column (2), I allow for city-specific linear trends in mobility (instead of allowing for different trends based on Internet availability), and in column (6), I show the results hold when I include CL's first entries between 2000 and 2003. Because these early cities are bigger, the size of the effects in terms of the number of in- and out-migrants is also larger. In column (5), I show that my results carry through when mobility flows are expressed in logs rather than levels.[34]

One of the potential caveats of my analysis is the fact that CL focused on entering large cities (CL cities are significantly larger than non-CL cities even in the matched sample; see Table 3). I address this point in several ways. First, in column (4), I impose common support in population size to the cities in the matched sample. This removes some of the largest CL cities from the treatment group, reducing the overall size of the treatment effects, but the results remain similar to the baseline estimates in column (1) and are statistically significant. Second, I restrict my sample to CL cities only, so that the treatment effect is identified from the comparison to not-yet-treated cities. I present these results in column (3), again finding an increase in inflows and outflows from the affected cities. Finally, I turn to Kijiji cities as an externally determined control group. These results are presented in column (7). I find that inflows and outflows from CL cities increase compared to non-CL Kijiji cities, although these differences are not statistically significant due to the large variance in outcomes of Kijiji cities.

The estimates presented so far are measuring the intention-to-treat treatment effect of Craigslist entry. However, as I explain in Section 3.3, the actual usage of CL varied signifi-

[33]The parameter presented in Table 5 can be interpreted as an average treatment effect of CL entry on city mobility. Recent literature has shown that this estimate corresponds to a *meaningful* ATT only under a set of relatively restrictive assumptions on the homogeneity of the treatment effect across treatment cohorts and time. As a result, I also present the dynamic diff-in-diff estimates (corresponding to versions of regression specifications (1) in Table A2 in the Appendix. While the size of the estimated effect varies as expected, the qualitative results of an increase in migration inflows and outflows hold.

[34]I study changes in the number of movers rather than the changes in mobility growth because geographic mobility was stable during the period in question; see also Figure A6.

cantly across the cities in entered, ranging between 0.2 job posts per 1,000 inhabitants per month in the cities where CL was the least popular to 9 job posts in Santa Barbara, its most popular city (Kroft and Pope, 2014). Furthermore, Craigslist was just an online job board: it also carried sales, housing and personal ads. As the next step, I thus estimate the effect of the actual number of CL job posts (normalised per 1,000 inhabitants) on migration inflows and outflows. Because job posting might be endogenous to local labour market conditions, I instrument it using a normalised measure of personal posts put on CL within the city at the same time. The resulting estimates complement the CL entry estimates by measuring the average causal response of city-to-city mobility to a unit increase in the number of CL *job* posts.

The coefficients are presented in panels (A) and (B) in Table 6. I estimate the average causal response to be about 400 individuals, i.e. one extra job post per 1,000 inhabitants increases the number of migrants in and out of the city by about 400. While this might seem like a large number, the magnitude is in line with the treatment effects of CL entry estimated in Table 5 (313 for outflows and 960 for inflows in the baseline specification) and with the fact that the average number of job posts in a CL city was 1.18.

Overall, these results consistently show that Craigslist increased both inflows and outflows of workers from other US cities. The scale of the net effect varies somewhat across specifications, but it is positive and weakly significant across most specifications; in my preferred specification in column (1), the average city population increases by about 0.07% annually. Such an increase, while large as a reaction to a website entry, doesn't correspond to a substantial change in city size. Instead, Craigslist increased migration *churn*, helping more people to move in but also to leave.

I explore this result further by analysing the impact of CL entry on bilateral city-to-city migration flows. A disadvantage of studying aggregate flows is that the total flow in or out of city doesn't just depend on the city characteristics, but also on the conditions of all other potential destination cities. Analysing bilateral flows circumvents this issue by holding the origin and destination city fixed. I assign each city-to-city flow to one of four categories, depending on whether it was the origin city, the destination city, or both that were treated by CL entry. I then estimate the standard dynamic difference-in-differences specification (1), using bilateral flows where neither the origin nor the destination city were treated as control.[35] I plot the results in Figure 3. They show that the increase in mobility

[35] In line with my preferred specification, I run this analysis on a sample of cities matched on baseline covariates. A bilateral flow is included in this balanced sample if both cities are included. The regression controls for time, origin and destination fixed effects, and includes time FE interacted with baseline Internet availability in the destination and origin city. In the "both-treated" category, I drop the year when one of the cities was treated first.

due to CL entry is entirely driven by an increase in the flows between CL cities.[36] This result rationalises why CL entry increases both inflows into the city and outflows from the city. Higher inflows are the result of a direct effect of online recruitment becoming available. The higher migration outflows are the consequence of the direct (inflow) effect in other CL cities since workers in CL cities rely on CL in other cities to help them find jobs. In other words, CL entry into one city increases the usage of CL in other cities, too, contributing to migration churn within CL cities.

## 4.2 Impact on wages

I repeat the dynamic difference-in-differences analysis of Craigslist entry for city-level wages. I focus on three moments: mean, top decile and bottom decile of nominal hourly wages, which allows me to analyse whether CL entry – and the subsequent increase in geographic mobility – had an impact on local wage distribution.

The raw data, plotted in the three left-hand panels of Figure 4 shows that wages in CL and non-CL cities were on different paths before CL expansion. Specifically, wages in non-CL cities were growing faster in the early 2000s, converging to wages in the (often larger) to-be-CL cities. CL entry put an end to this convergence, reversing the trend especially, in the upper half of the wage distribution.

As the next step, I estimate my preferred specification regression, in which I impose the control and treatment cities to be comparable at the baseline, and allow for different time fixed effects for cities with different baseline Internet availability. I also allow for a linear trend in wages of CL cities to take into account the convergence pattern in the pre-treatment period. The results are plotted in the right-hand side panels of Figure 4. The confirm the patterns in the raw data: Craigslist caused a relative increase in wages in the cities it entered, especially at the mean and top decile of the distribution. The annual increase was 0.9% for the bottom decile and 1.2% for the top decile of wages.

[36]Not all bilateral migration flows are non-zero. While this is not an issue in my estimation per se since I measure migration flows in levels (rather than logs), it does raise the question of whether a linear regression is the best fit for the underlying data-generating process. I explore this question in Figures A4 and A5 in the Appendix. In Figure A4, I show that 2.3% of all possible city-to-city flows are greater than 0. While this share is much higher among CL-to-CL flows than bilateral flows where only one or neither city is treated, it is also almost constant over time: only 0.09% of flows change from zero to non-zero (or back) year on year. This highly constant nature of bilateral flows, combined with the flow fixed effects included in the baseline dynamic diff-in-diff, means that a switching from and to 0 flows is not an important part of my findings. However, the relatively large overall share of zero flows does suggest that a Poisson regression might be more appropriate. I present the estimates in panels (b), (d) and (f) of Figure A5. In the other three panels of this Figure, I present the estimates of a linear dynamic diff-in-diff, restricting the sample to positive flows only. While the standard errors are relatively large for both specifications, they also estimate a positive impact of CL entry on city-to-city migration flows, especially for flows where both the destination and origin city are treated.

In Tables A3 and 7, I summarise the results of my robustness checks and alternative identification strategies. I employ different de-trending (city-specific linear trends, column (2)), impose common support in city size (column (4)), expand the sample to include early CL entry before the year 2004 (column (6)), and re-estimate the regression in logs (column (5)). I also identify the treatment effect off of the staggered timing of CL entry only (column (3)) and use eBay's Kijiji cities to construct an alternative, externally-determined control group (column (7)). Throughout, these results confirm that CL entry had a significant impact on city wages. Depending on the specification, the treatment effect on average and bottom decile wages sometimes turns insignificant or weakly negative, highlighting the fact that the CL impact was the largest for the highest-paid jobs.

Finally, in panels C, D and E of Table 6, I examine how city-level wages reacted to the *intensity* of treatment as measured by the number of job posts per 1,000 inhabitants in a given city. The estimated average causal response is positive: one additional job post per 1,000 inhabitants increases the average wage by about \$0.1 per hour (0.7%), the bottom 10th percentile wage by \$0.05 per hour (0.8%), and the wage at the top 10th percentile by \$0.3 (1.1%). These results suggest that the wage impact of CL entry was driven by the cities where CL became popular and widely used, supporting the hypothesis that CL entry increased wages by facilitating a different search and matching in the labour market.

Overall, the results of this section suggest that the availability of a new tool for recruitment and job search caused greater geographic churn between the treated cities, and led to a significant increase in local wages, especially at the top of the wage distribution. In the rest of the paper, I explore the hypothesis that these two results are causally related and explore the underlying mechanisms.

# 5 Understanding firm recruitment

In this section, I collect novel data on recruitment across space to explain why firms use Craigslist to advertise their vacancies. I show that non-local hiring is predominantly driven by the search for specific skills, rather than to hire cheaply or more quickly.

## 5.1 Motivation and methodology

One possible explanation for the simultaneous increase in migration and wages in cities following Craigslist entry is that it was driven by greater sorting and matching across space. If the sudden increase in availability of online recruitment allowed firms to hire more cheaply from other cities, we would expect more city-to-city moves and higher city-level wages, especially at the top end of the wage distribution – in line with the estimates from the previous section.

A key assumption underlying this explanation is that firms use online recruitment to find specific talent or better matches outside of their local labour market. Whilst this may sound obvious, it is not necessarily true: the Internet may instead be used to access larger pools of unemployed to hire faster and at lower wages.[37] Understanding why firms recruit across space is thus crucial for evaluating the hypothesis that Craigslist improved spatial sorting between cities.

In this section, I address this question by analysing cross-regional recruitment by firms as measured from newspaper vacancy postings in the year 1990. In the absence of survey data[38], firms' posting of help-wanted ads across newspapers is unique in allowing us directly observable instances of firms' decisions over where to hire. Studying vacancy postings on the Internet doesn't reveal the same information because the Internet doesn't have a location; data on actual hires confounds the recruitment choice with the decision of workers over whether to accept the job. In contrast, the decision to advertise a particular job in a particular newspaper had to be done consciously by the employer, and as such can be informative about why firms recruit non-locally in the first place.

I use newspaper help-wanted ads from 1990 to avoid any overlap with posting on the Internet (Craigslist, for example, was founded in 1995). Each posted advertisement contains 3 pieces of information: the location of the employer (job), the place where the vacancy was posted (derived from the newspaper it was posted in), and the occupation of the job. By merging this information with characteristics of the job and newspaper location, I am able to describe under which circumstances, and for which jobs, were employers willing to go the extra length of recruiting outside of their local labour market. The dataset contains 318,183 help-wanted ads from 216 newspapers across 44 US states. For more information about the creation of the data set and the data itself, see Section 2.4.

[37]A third use of online recruitment is of course to match more efficiently locally. However, given (i) the significant impact of CL on the geographic mobility of workers, and (ii) the fact that a firm posting its vacancy online cannot a priori prevent applications from outside of its local labour market, it is fair to assume that online recruitment did have an impact on cross-regional hiring and that firms recruiting online were doing so with the knowledge that they are also posting their vacancy outside of their local labour market. In other words, it is reasonable to assume that firms that have a strong distaste for hiring across space would not choose to post their vacancy online.

[38]Survey data on recruitment is scant in general. The most recent dataset, the Employment Opportunities Pilot Projects, is from 1980. To the best of my knowledge, there is no existing survey dataset on the geographic dimension of recruitment.

## 5.2 Patterns in cross-regional recruitment

The newspaper vacancy data shows that while the majority of job postings in 1990 were local, a non-negligible share was not. 11% of vacancies were posted in a newspaper located in a different town, and about a half of these non-local vacancies were plausibly advertised in another local labour market: 5% (of all vacancies) were posted in another county, and about 2% were advertised outside of the commuting zone (a map of the average recruitment distance for each newspaper in the sample is plotted in Figure A3).[39]

Non-local recruitment varied significantly across occupations. As I show in Figure 5, the most local occupation was construction, with less than 6% of vacancies advertised outside of its local town. In the most widely recruited occupations (architects and engineers, managers, and social service workers), the non-local shares were up to three times as large. The heterogeneity across occupations is the first piece of evidence of the drivers of cross-regional recruitment: it is mostly higher-wage, higher-education occupations that are likely to be advertised in a distant newspaper.

To explore the drivers of non-local recruitment explicitly, I compare, within each help-wanted ad, the labour market characteristics of the job location and newspaper location. I calculate a percentage difference in the local average wages and their growth, the size of the labour force, the competitiveness of the labour market (as measured by the number of establishments) and the percentage point difference in local unemployment rates. If firms recruit non-locally primarily to tap into a large pool of cheap labour, they would post their help-wanted ads in markets with lower pay, higher unemployment rate, and fewer competing establishments. If they are using non-local recruitment to find specific talent, we should instead find that they advertise in higher-pay markets with a large labour force.

I plot the labour market comparisons in panel (a) of Figure 6. The main determinant of where firms post their vacancies is the size of the labour market: recruitment markets are about 3 percentage points larger, both in terms of the labour force and the number of establishments than the labour markets where the job is located. However, firms don't seem to be drawn to large pools of unemployed: they tend to advertise in labour markets with weakly lower unemployment rates than those in their local labour market. Pay doesn't seem to matter much either: firms advertise in markets with somewhat higher pay (but lower wage growth). Put together, these patterns suggest that firms recruit non-locally to access larger markets rather than cheap abundant labour.

However, these patterns don't hold across all occupations. In panel (b) of Figure 6, I focus on the differences in average pay, and in panel (c) on the differences in unem-

[39] Interestingly, these figures are comparable to the annual migration rates in 1990 across counties (5-6%), metropolitan statistical areas and state boundaries (both around 3%) (Molloy, Smith, and Wozniak, 2011).

ployment rates, calculated separately for each occupation.[40] Both figures show significant variation across jobs. For some occupations, such as Building and Grounds Maintenance, and Food Preparation and Serving, employers do target higher-unemployment, lower-wage regions when recruiting cross-regionally. In other occupations, e.g. Social Services and Business and Financial Operations, the placement of non-local vacancies is the opposite, in low-unemployment-high-pay areas. Comparison with Figure 5 shows that it is the occupations least likely to recruit non-locally which focus on markets with lower wages and higher unemployment. This explains the overall pattern in panel (a): the majority of non-local recruitment happens in large labour markets, regardless of their pay.

If most non-local recruitment focuses on finding workers with specific skills, we should also see that firms direct their job posts to places where such skills are more abundant. I test this by estimating the relationship between non-local recruitment and the employment share of the vacancy's occupation in the firm's local labour market. Firms that are hiring for an occupation that is relatively abundant in their local labour market (as measured by the occupation's employment share) should be less likely to recruit non-locally. The results in column (1) of Table 8 support this hypothesis: the higher the occupation's share of employment in the local labour market, the less likely is a vacancy posted elsewhere. In column (2) of the table, I go one step further and estimate the relationship between the occupation employment share in the local market and the occupation employment share in the recruitment market, conditional on hiring non-locally. I find that this relationship is also negative: the lower the availability of a particular occupation in the local labour market, the higher it is in the market where the vacancy is posted. In other words, firms post their vacancies in labour markets that specialise in the occupation they are recruiting for.

# 6 Impact of CL on labour market outcomes: heterogeneity by occupation

The previous section of this paper has shown that when employers recruit non-locally it is to find workers of specific skill rather than to access large pools of cheap labour. As a consequence, if Craigslist effectively lowered the costs of recruiting non-locally, we should expect an increase in migration churn and wages in the local markets it has entered – exactly in line with the treatment effects estimated in Section 4. However, this hypothesis generates several other testable predictions, which I examine in this section. First, I confirm that

[40]Note that the data on pay and unemployment rates is still at the county (rather than county-occupation) level. These two panels thus analyse how the pattern of posting varies by occupation, but it doesn't take into account the within-county differences in labour market characteristics.

the increases in geographic mobility and wages are causally related: the markets with the largest increase in migration churn are also the markets where wages grew the most. Second, I show that the impact of CL entry is greatest in occupations where the value of the match matters most, i.e. in occupations that were recruiting non-locally before the introduction of the Internet. Put together, these findings provide evidence on *why* Craigslist entry changed the geography of local labour markets.

**Occupation-specific impact on wages** I start by showing that the baseline results of CL entry on wages also hold within cities and across occupations. I estimate the baseline regression (1) using city-occupation-specific wages as the dependent variable, progressively adding city- and occupation- fixed effects, and controlling for city- and occupation-specific linear trends in wages. The results are summarised in Table 9. Column (1) contains time- and occupation-fixed effects only, estimating the impact of CL entry on wages across different cities within an occupation. This specification demonstrates that the increase in average city wages isn't driven by a compositional change in workers at the city level: the positive impact of CL entry holds within each occupation. In the other four columns of the table, I also control for city fixed effects. This allows me to compare the impact of CL entry across occupations within a single city, effectively controlling for any general increases in wages at the city level. In column (3), I allow for occupation-specific linear growth in wages, and in column (4), I also add city-specific linear wage growth. Finally, in column (5), I turn to one of the alternative identification strategies in which I use Kijiji cities as an externally determined control group. The estimated effects of CL entry are consistently positive and significant, especially 1-2 years after entry.

As the next step, I explore the differences in treatment effects between occupations. I split occupations into 2 groups, low and high non-local recruitment, depending on whether its pre-Internet share of non-local recruitment in newspapers was below or above the median. There are two ways to interpret this categorisation: jobs that are recruiting cross-regionally at the baseline might be more prone to switching to new, cheaper technology (such as Craigslist); or we can use non-local recruitment share as a proxy for the relative importance of match value. Either way, we would expect the occupations with high non-local recruitment before the introduction of the Internet to react more strongly to Craigslist entry. To test this hypothesis, I re-run the baseline dynamic diff-in-diff regression separately for high- and low-non-local recruitment occupations. I plot the results in Figure 7. The top panel, focusing on the average wage, shows that while Craigslist entry increases the average wage in all occupations, this increase is significantly more pronounced in occupations with relatively high non-local recruitment share. By year 4 after treatment, wages in high non-local recruitment jobs have increased about three times more than wages in low non-local recruitment occupations. The other two panels of Figure 7 demonstrate that this average difference between the two groups is driven almost entirely by wages at the 90th percentile of city-occupation wages. Panel (b), which replicates the analysis for the bottom decile of the wage distribution within each occupation and city, finds virtually no difference – wages in both groups increase by the same amount. On the other hand, at the top decile (panel (c)), CL entry causes wages in the high non-local recruitment occupations to increase by much more than the top wages in occupations that historically recruited relatively more locally. This result – that not only is the impact of CL greater in occupations that recruit more non-locally in general but that this impact is concentrated at the top of the wage distribution – further supports the hypothesis that online recruitment technologies, such as Craigslist, facilitated greater spatial sorting and better matching across space.

**Occupation-specific impact on geographic mobility** Ideally, I would like to replicate my analysis of CL impact on migration inflows and outflows at the city-occupation level. Unfortunately, this is not possible due to the lack of migration data at this level of detail. The IRS data I use for my baseline city-level analysis doesn't include any demographic information about the movers, and the alternative – survey data from the American Community Survey – only started to record city-level moves in 2005, after Craigslist entry in a large number of cities. As a result, there is no "before" data for occupation-specific moves that would allow me to replicate my baseline dynamic diff-in-diff specification.[41]

In the absence of detailed time series data, I focus on analysing the relationship between the changes in migration flows and wages *after* CL entry. I use ACS data to calculate the number of workers of a particular occupation moving into a given city annually between 2005 and 2007, during the treatment period studied in my baseline analysis. I calculate the change in inflows over these three years within each city-occupation cell and compare it to an analogous change in wages over the same time period.[42]This comparison allows me to test whether the occupations that experienced the largest migration inflows are also the occupations that saw the largest increase in wages. Put differently, if it is indeed greater spatial sorting that caused the positive impact of CL on wages, then we should see a positive relationship between these two outcomes.

[41]Technically since CL entered some cities in 2006, I could estimate the baseline diff-in-diff regressions for the subgroup of these cities. While feasible in theory, there are 2 problems with this approach. First, because ACS is a survey of 1% US population, it doesn't capture movers of each occupation into each city every year; in other words, the panel is unbalanced, and data for both years is available for only about 1/6 of all the possible city-occupation cells. The second problem is that the data starts in 2005, giving us only 1 year before treatment.

[42]The wage data is the same as in the previous section, taken from the OEWS dataset.

In Figure 8, I present a binned scatterplot of post-CL entry changes in migration flows and wages at city-occupation level. The graph shows that, in line with the prediction, the relationship between the two labour market outcomes is positive for CL cities: the occupations that experienced the largest increase in migration inflows are also the occupations that saw the largest increase in wages. Importantly, this pattern does not exist within control (non-CL) cities: high inflow of workers within an occupation led to relatively *smaller* increase in wages.

I test this relationship formally by estimating the following regression of wages on migration:

$$\Delta Wage_{co} = \alpha + \beta \Delta Migration_{co} + \gamma \Delta Migration_{co} CL_c + \lambda_o + \kappa_c + \epsilon_{co} \tag{3}$$

where $\Delta$ corresponds to the 2007-2005 difference in the given variable, $CL$ is a binary treatment dummy treatment, and $\lambda_o$ and $\kappa_c$ are occupation- and city-fixed effects, respectively. The coefficient of interest is $\gamma$ which allows the relationship between the changes in migration and the changes in wages to vary between treated and control cities. I estimate it to be positive and statistically significant (0.0004812, with a p-value of 0.001), while $\beta$, the correlation in control cities is significantly negative (-0.00034, p-value of 0.007). (I plot the estimated relationship between migration and wages, and report the full regression results in Figure A7.) These regression results confirm the patterns shown in the scatterplot above. They show that the impacts of CL on migration and wages are likely causally related. Furthermore, the negative relationship in control cities is consistent with the standard model of labour supply and demand, in which higher labour supply from particular workers causes their wages to relatively decline. Viewed through this lens, the positive relationship for treated cities suggests that the changes in wages observed there were driven by an increase in labour demand, rather than supply – driven, for example, by a more efficient spatial matching technology.

# 7 Discussion and conclusion

In this paper, I show that the Internet didn't just make it easier to search, it also substantially reduced the effective distance between geographically separate labour markets. The introduction of one of the first – and, at one time, the most popular – online job boards in the US led to a significant increase in migration flows in and out of the affected cities. Instead of increasing city population, Craigslist increased migration churn, helping workers sort between different locations. As a result, pay went up, especially at the top of the wage distribution.

I uncover these patterns using a combination of different identification strategies and a novel data set on recruitment. I primarily estimate the impact of Craigslist from the variation in its expansion across time and space, but I complement my analysis with a comparison of Craigslist cities to those entered by an unsuccessful rival online job board. I also make use of the data on Craigslist's actual use to strengthen my claim that the impact of Craigslist on the labour market was predominantly due to its help-wanted ads rather than housing or personal classifieds. To understand the mechanism behind Craigslist's impact, I collect the first data set describing how firms recruit across space. I show that the motivation for cross-regional hiring is to find workers of specific skill rather than to access a large pool of unemployed labour. As a result, when the introduction of online recruitment made non-local hiring cheaper, it was the occupations that historically hired this way the most that experienced the largest increase in migration and wages.

However, the results of this paper come with several caveats. First, Craigslist offers relatively rudimentary online recruitment tools compared to what is on offer today: it allows employers to advertise vacancies online, but it doesn't help to match them with potential hires, nor does it allow employers and candidates to directly communicate or manage the early stages of the recruitment process. As such, the estimated effect of Craigslist might be different from the impact of more sophisticated, AI-powered online recruitment tools.[43] Second, my detailed heterogeneity analysis is just a proxy for directly observing the search and recruitment methods of the moving workers and the changes in the wages of movers. Third, online recruitment is likely to have far-reaching general equilibrium effects, not just within the labour market but across the wider economy. For example, Steffen Altmann and Sebald (2023) document significant negative spillovers of job-search advice in Denmark, and Atasoy (2013) shows how the expansion of broadband availability across the US increases firm size. My identification strategy was specifically designed to exclude such general equilibrium effects.

Overall, while the results of this paper show the significant ways in which technology can have a positive impact on the labour market, they also contain some reasons for caution. First, even though the movers in my analysis most likely benefited from relocation and higher pay, the overall wage and regional inequality increased. Policymakers who value regional convergence per se may need to think about ways to harness the efficiency gains of online recruitment while minimising its negative impacts on inequality.

[43] In particular, the rise of hiring online has been accompanied by an anecdotal increase in the number of applications to each job, and per each job seeker. This simultaneous increase might lead to significant congestion effects in the market, negating much of the improvement thanks to the higher efficiency of the matching function. There is also a large emerging literature on the consequences of algorithm-based selection in recruitment, see for example Hoffman, Kahn, and Li (2017).

Second, it is unclear why this technology, offering a near-costless advertisement across much larger labour markets, hasn't been adopted universally. Carnevale, Jayasundera, and Repnikov (2014) estimate that about 70% of all vacancies in the US are posted online, which means that about a third of all job openings are not; low-wage, low-skill vacancies are particularly under-represented. This gap in coverage has potentially serious consequences for the equality of opportunity and geographic mobility, but the question of why certain types of employers avoid online recruitment remains so far unexplored in the literature. Given the growth in working from home at the opposite end of the job spectrum and the opportunity it offers to workers to free themselves from the tyranny of geography, the importance of this and related questions about the interaction between the spatial frictions and technology will only increase.

Table 1: Descriptive statistics

| | CL cities | | Non-CL cities | | |
|---|---|---|---|---|---|
| | (1) All mean/sd | (2) 2004-2006 entry mean/sd | (3) All mean/sd | (4) Kajiji cities mean/sd | (5) Other cities mean/sd |
| *Panel A: Mobility and demographics* | | | | | |
| Gross inflows | 17675.22 | 9360.34 | 2074.96 | 10806.59 | 1580.97 |
| | (27204.21) | (9199.45) | (6056.41) | (21445.90) | (2972.92) |
| Gross outflows | 17769.97 | 9152.59 | 2045.01 | 9468.78 | 1625.01 |
| | (31286.14) | (7938.97) | (5226.04) | (15273.66) | (3563.51) |
| Inflow share, % | 2.24 | 2.23 | 2.01 | 2.69 | 1.98 |
| | (1.10) | (1.09) | (1.14) | (1.11) | (1.13) |
| Outflow share, % | 2.25 | 2.27 | 2.14 | 2.53 | 2.12 |
| | (0.96) | (0.98) | (0.95) | (0.98) | (0.95) |
| Population (1,000s) | 906.88 | 435.57 | 86.30 | 367.74 | 70.38 |
| | (1833.31) | (390.44) | (166.30) | (553.50) | (87.13) |
| Land area (km-sq) | 3024.88 | 2656.18 | 1352.29 | 2382.52 | 1294.01 |
| | (2989.56) | (2821.76) | (1980.57) | (4430.41) | (1731.84) |
| Population density | 2840.54 | 2284.48 | 922.84 | 2136.36 | 854.19 |
| | (2652.78) | (1049.88) | (849.86) | (1531.56) | (738.71) |
| Population share, white (%) | 79.81 | 81.18 | 83.72 | 83.19 | 83.75 |
| | (11.93) | (11.11) | (15.18) | (11.78) | (15.36) |
| Population share, black (%) | 10.49 | 10.12 | 8.97 | 7.83 | 9.03 |
| | (10.39) | (10.66) | (13.75) | (9.54) | (13.95) |
| Population share, hispanic (%) | 10.24 | 9.71 | 7.48 | 8.54 | 7.42 |
| | (15.66) | (16.17) | (13.55) | (11.53) | (13.66) |
| Median age | 34.35 | 34.27 | 36.27 | 35.77 | 36.30 |
| | (3.66) | (3.82) | (3.73) | (3.84) | (3.73) |
| Share under 18 (%) | 25.38 | 25.37 | 25.48 | 25.34 | 25.48 |
| | (3.15) | (3.31) | (3.00) | (3.12) | (2.99) |
| Share over 75 (%) | 5.80 | 5.85 | 6.70 | 5.92 | 6.75 |
| | (1.62) | (1.61) | (1.77) | (1.92) | (1.76) |
| N | 223 | 195 | 691 | 37 | 654 |
| | mean/sd | mean/sd | mean/sd | mean/sd | mean/sd |
| *Panel B: Labor market* | | | | | |
| Employment share (%) | 45.29 | 45.17 | 41.30 | 43.34 | 40.38 |
| | (11.58) | (11.50) | (14.65) | (20.02) | (11.56) |
| Hourly wage, mean | 14.66 | 14.27 | 13.87 | 14.27 | 13.70 |
| | (1.75) | (1.43) | (1.74) | (1.45) | (1.84) |
| Hourly wage, bottom 10% | 7.80 | 7.63 | 7.65 | 7.94 | 7.52 |
| | (1.15) | (1.10) | (0.88) | (0.82) | (0.88) |
| Hourly wage, top 10% | 23.33 | 22.74 | 21.99 | 22.47 | 21.78 |
| | (3.62) | (3.49) | (2.31) | (1.90) | (2.45) |
| N | 193 | 166 | 87 | 27 | 60 |

*Note:* The table summarises city characteristics in 2000. Column (1): all MSAs that Craigslist entered before 2008. Column (2): MSAs that Craigslist entered between 2004 and 2006. Column (3): all MSAs without a Craigslist website by 2007. Column (4): MSAs without a Craigslist website but with a Kijiji website in 2007. Column (5): MSAs without either Craigslist or Kijiji website by 2008.

Table 2: Predicting Craigslist entry

| | Craigslist entry | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Population, log, 2002 | 2.300*** | | | | 1.576*** |
| | (0.171) | | | | (0.307) |
| Population growth, 2002-03 | 9.378 | | | | 5.369 |
| | (11.35) | | | | (27.74) |
| N. of Internet service providers, 2002 | | 0.560*** | | | -0.146 |
| | | (0.0563) | | | (0.118) |
| Growth in ISP, 2002-03 | | 0.275*** | | | 0.363* |
| | | (0.0860) | | | (0.194) |
| GDP growth, 2000-03 | | | -0.0314 | | 0.0253 |
| | | | (0.0614) | | (0.0688) |
| Employment growth, 2000-03 | | | 0.105 | | 0.0583 |
| | | | (0.118) | | (0.183) |
| Predicted GDP growth, 2004-07 | | | | -0.248 | -0.611* |
| | | | | (0.224) | (0.331) |
| Predicted employment growth, 2004-07 | | | | 0.116 | 0.185 |
| | | | | (0.215) | (0.358) |
| Constant | -28.39*** | -3.942*** | 0.882*** | 1.546** | -16.98*** |
| | (2.028) | (0.297) | (0.225) | (0.627) | (3.529) |
| Observations | 886 | 888 | 233 | 233 | 233 |
| Pseudo $R^2$ | 0.476 | 0.145 | 0.003 | 0.005 | 0.155 |

*Note:* Predicting CL entry between 2004 and 2006 using population levels and growth up to 2 years before the entry (column (1)), the levels and growth in the availability of high-speed Internet connection (column (2)), city-level growth in employment and real GDP (column (3)), and predicted growth of employment and GDP over the years 2004-2007 (column (4)). Column (5) uses all the predicting variables simultaneously. Standard errors are in parentheses. $*p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

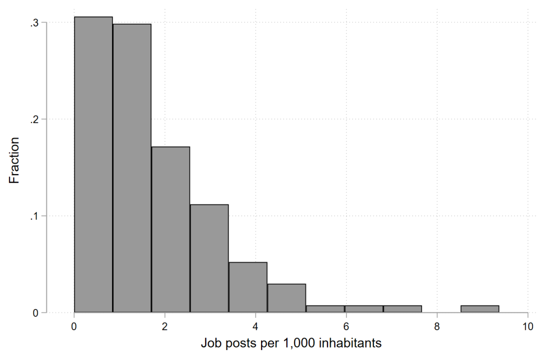
Table 3: Balance test for the matched sample

| | All cities | | | Kijiji cities | | |
|---|---|---|---|---|---|---|
| | Craigslist cites | Non-CL cities | Diff. | Craigslist cites | Non-CL cities | Diff. |
| Inflow share, % | 2.13 | 2.24 | 0.11 | 2.14 | 2.67 | 0.51** |
| | (0.99) | (1.15) | (0.16) | (0.97) | (1.21) | (0.20) |
| Outflow share, % | 2.04 | 2.19 | 0.15 | 1.99 | 2.32 | 0.37** |
| | (0.94) | (1.26) | (0.16) | (0.87) | (0.91) | (0.16) |
| Population (1,000s) | 478.53 | 215.70 | -262.83*** | 553.58 | 454.41 | -162.48* |
| | (400.30) | (148.69) | (56.25) | (411.52) | (684.34) | (85.23) |
| Population density | 2356.71 | 1879.16 | -477.55*** | 2414.52 | 2263.27 | -244.32 |
| | (1041.96) | (1234.81) | (172.01) | (1011.57) | (1575.37) | (213.53) |
| Population share, white (%) | 80.28 | 81.95 | 1.66 | 79.25 | 82.12 | 3.46 |
| | (11.16) | (13.07) | (1.84) | (11.33) | (12.68) | (2.10) |
| Share under 18 (%) | 25.66 | 25.34 | -0.32 | 25.90 | 25.67 | -0.46 |
| | (2.95) | (2.20) | (0.44) | (2.92) | (2.37) | (0.57) |
| Share over 75 (%) | 5.81 | 6.03 | 0.22 | 5.80 | 5.96 | -0.00 |
| | (1.44) | (1.36) | (0.22) | (1.45) | (1.63) | (0.32) |
| Employment share (%) | 44.12 | 40.93 | -3.19 | 44.34 | 41.79 | -2.55 |
| | (11.43) | (14.54) | (1.93) | (10.96) | (19.55) | (2.69) |
| Hourly wage, mean | 14.28 | 13.75 | -0.54** | 14.43 | 14.27 | -0.16 |
| | (1.43) | (1.39) | (0.22) | (1.41) | (1.45) | (0.30) |
| Hourly wage, bottom 10% | 7.63 | 7.60 | -0.03 | 7.68 | 7.94 | 0.26 |
| | (1.10) | (0.75) | (0.16) | (1.19) | (0.82) | (0.24) |
| Hourly wage, top 10% | 22.77 | 21.83 | -0.94* | 22.95 | 22.47 | -0.49 |
| | (3.49) | (2.04) | (0.51) | (3.65) | (1.90) | (0.72) |
| Observations | 167 | 53 | 220 | 134 | 27 | 183 |

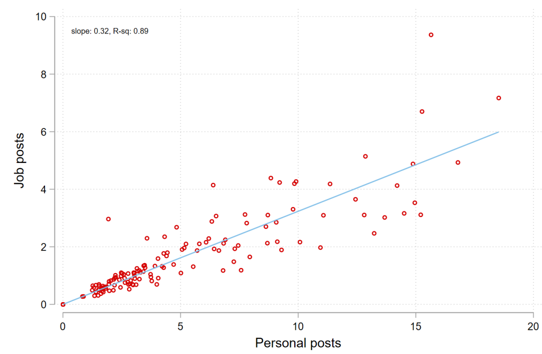
*Note:* The comparison of city-level baseline characteristics (year 2000) for the matched sample of treated and control cities. The first three columns present the characteristics of CL and non-CL cities and calculate their difference. The last three columns repeat the exercise for the subsample of Kijiji cities. Standard errors are in parentheses. $*p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Figure 1: Craigslist popularity

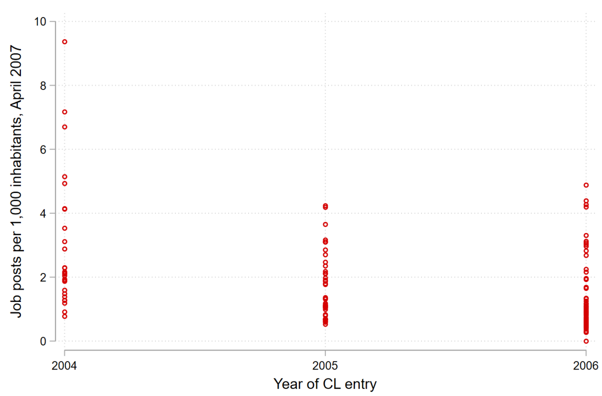
(a) Distribution of the number of job posts across cities




(b) First-stage relationship between job posts and personal posts




(c) CL popularity across cities, by year of entry




*Notes:* Descriptive statistics of CL popularity, as measured by the number of job posts per 1,000 inhabitants in April 2007. Data from Kroft and Pope (2014). Panel (a) plots the distribution of the number of job posts across cities where CL entered after 2003. Panel (c) further breaks this distribution down by the year of CL entry. Panel (b) shows the first-stage relationship between the normalised number of job posts and the normalised number of personal posts per city.

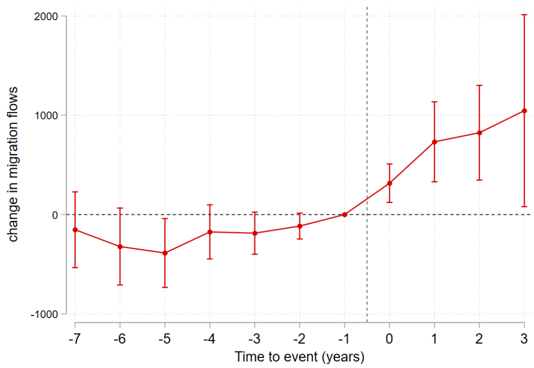
Table 4: Description of the newspaper vacancy dataset

| variable | # of unique values | |
|---|---|---|
| | in the dataset | in the population |
| *newspapers* | 216 | |
| locations | 200 | 9548 |
| counties | 174 | 3142 |
| MSAs | 93 | 922 |
| states | 44 | 50 |
| *vacancies* | 318 183 | |
| occupations | 93 | 96 |
| locations | 2699 | 9548 |
| counties | 864 | 3142 |
| MSAs | 242 | 922 |
| states | 47 | 50 |

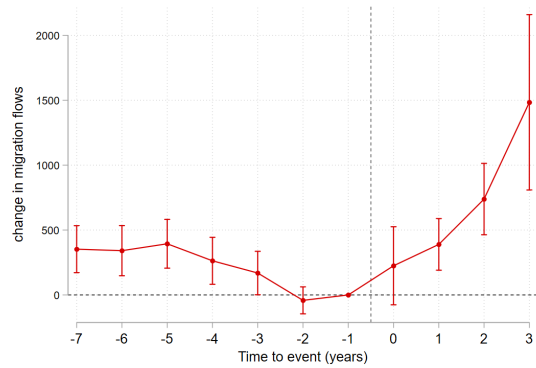
*Notes:* Descriptive statistics of the data on vacancy posting from help-wanted newspaper ads in 1990. The two panels summarise the number of unique values in the dataset and in the population, at the newspaper and vacancy level.

Figure 2: Impact of CL entry on aggregate migration flows

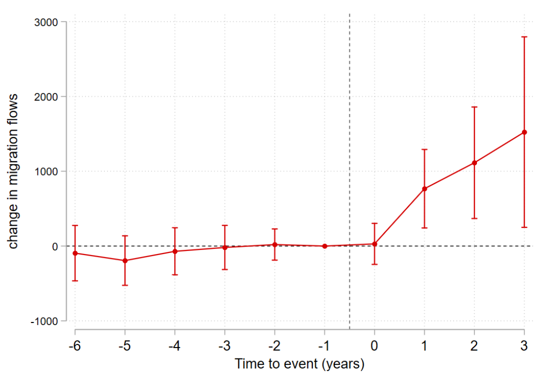
(a) Inflows, all cities



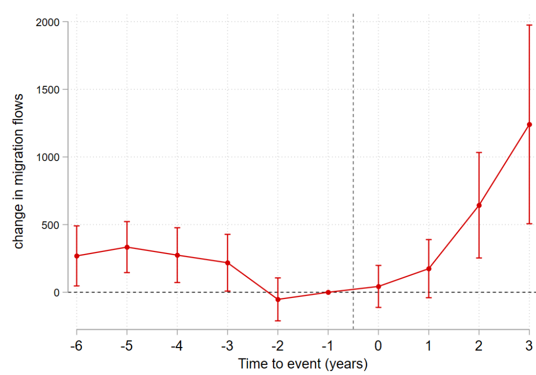

(b) Outflows, all cities




(c) Inflows, baseline estimates




(d) Outflows, baseline estimates



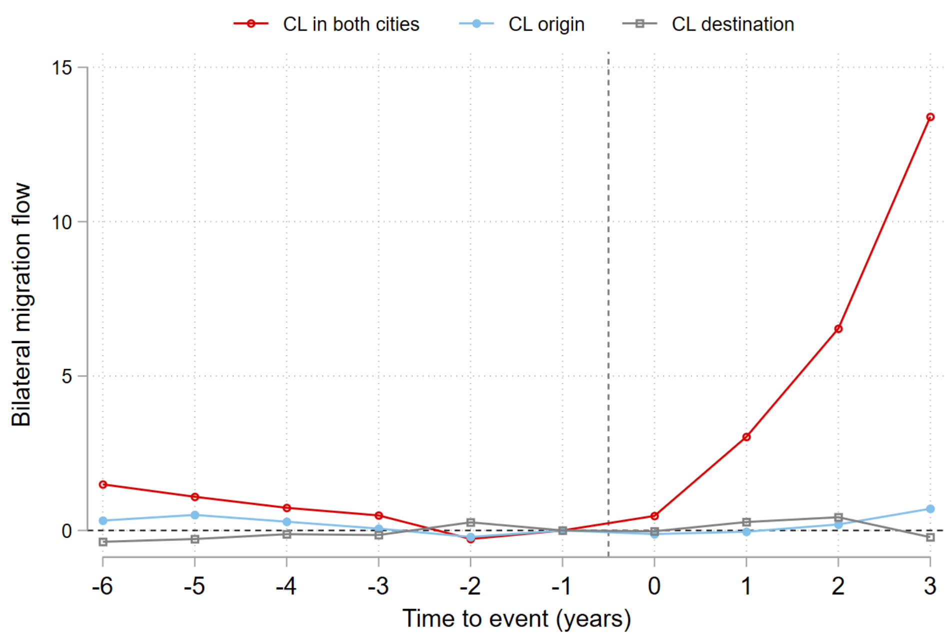

*Notes:* Panels (a) and (b) show event study on all cities, using city and time fixed effects. Panels (c) and (d) are my preferred specification, using a sample of cities balanced on baseline covariates and including baseline levels and growth of Internet service providers interacted with time FE.

Table 5: Impact of Craigslist entry on aggregate migration flows, robustness checks

| | Balanced sample | | | | All data | | Kijiji cities |
|---|---|---|---|---|---|---|---|
| | (1) Levels | (2) Levels | (3) Levels | (4) Levels | (5) Log | (6) Levels | (7) Levels |
| *Panel A: Gross Inflows* | | | | | | | |
| Treatment effect | 960.0*** | 659.8** | 1388.9** | 663.1*** | 0.0310*** | 941.2*** | 484.7 |
| | (290.8) | (298.2) | (558.6) | (214.0) | (0.0106) | (291.9) | (633.7) |
| *Panel B: Gross Outflows* | | | | | | | |
| Treatment effect | 313.4** | 260.6* | 200.6 | 235.2** | 0.0374*** | 1251.1*** | 200.3 |
| | (142.1) | (135.8) | (227.4) | (110.9) | (0.00839) | (343.4) | (711.7) |
| *Panel C: Net Inflows* | | | | | | | |
| Treatment effect | 646.6** | 399.2 | 1188.3** | 427.9* | 0.154 | -309.9 | 284.3 |
| | (279.1) | (363.7) | (550.7) | (221.1) | (0.0968) | (347.9) | (775.1) |
| N | 2200 | 2200 | 1670 | 1980 | 3577 | 9160 | 2110 |
| MSA FE | YES | YES | YES | YES | YES | YES | YES |
| Time FE | YES | YES | YES | YES | YES | YES | YES |
| Linear MSA trend | | YES | | | | | |
| Covariates × time | YES | | | YES | | YES | YES |
| CL cities only | | | YES | | | | |
| City size common support | | | | YES | | | |
| All CL entries | | | | | | YES | |

*Note:* This table summarises the results of a TWFE regression of CL entry on city-level migration inflows and outflows. The presented coefficient estimates the annual change in migration flows at city level following CL entry, averaged across different years after entry. Column (1) corresponds to my baseline preferred specification, using a matched sample of treated and control cities and including an interaction between period FE and baseline Internet service availability. Column (2) uses city-specific linear growth trends instead. Column (3) re-runs the baseline specification from column (1) but only on CL-cities. Column (4) re-runs the baseline specification further imposing common support over population size of treated and control cities. Columns (5) and (6) are estimated on the entire sample of cities; column (5) uses log, rather than levels, of migration flows, and column (6) includes cities that were treated in the years 2000-2003. Column (7) re-runs the baseline specification on Kijiji cities only. Standard errors are in parentheses, clustered at city level throughout. $*p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Figure 3: Impact of CL entry on city-to-city (bilateral) migration flows




*Notes:* Event study of bilateral migration flows (in levels) on CL entry. The control group for all 3 treatment groups are bilateral flows that were never treated (i.e. both destination and origin cities didn't have CL in this time period). The destination and origin cities for all bilateral flows are balanced on baseline covariates. The regression specification includes origin and destination FE, time FE, and baseline Internet access interacted with time dummies, in line with the preferred specification for aggregate migration flows. For confidence intervals, see the Appendix.

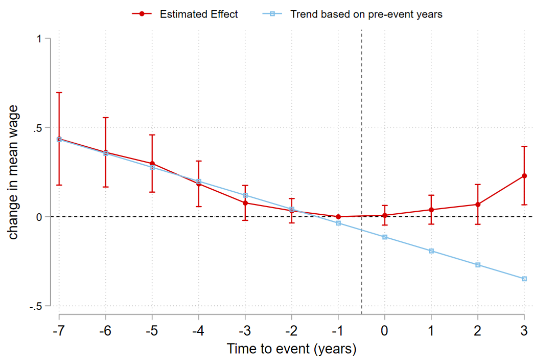
Table 6: The impact of the number of Craigslist job posts on migration flows and wages

| | OLS | | IV | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *Panel A: aggregate outflows* | | | | |
| Job posts per 1,000 inhabitants | 297.8** | 175.6* | 432.3*** | 355.1** |
| | (120.8) | (103.9) | (164.7) | (178.7) |
| *Panel B: aggregate inflows* | | | | |
| Job posts per 1,000 inhabitants | 372.5** | 165.8 | 391.6** | 141.0 |
| | (149.8) | (136.9) | (176.9) | (188.3) |
| *Panel C: mean hourly wage* | | | | |
| Job posts per 1,000 inhabitants | 0.0991*** | 0.0960*** | 0.115*** | 0.116*** |
| | (0.0178) | (0.0201) | (0.0224) | (0.0254) |
| *Panel D: bottom 10% hourly wage* | | | | |
| Job posts per 1,000 inhabitants | 0.0455*** | 0.0465*** | 0.0552*** | 0.0574*** |
| | (0.00882) | (0.00912) | (0.0110) | (0.0115) |
| *Panel E: top 10% hourly wage* | | | | |
| Job posts per 1,000 inhabitants | 0.296*** | 0.283*** | 0.332*** | 0.327*** |
| | (0.0453) | (0.0473) | (0.0591) | (0.0655) |
| N | 2354 | 1590 | 2356 | 1590 |
| 1st-stage F | 239.67 | 371.62 | 239.67 | 371.62 |
| MSA FE | YES | YES | YES | YES |
| Time FE | YES | YES | YES | YES |
| Covariates time | | YES | | YES |
| Matched sample | | YES | | YES |

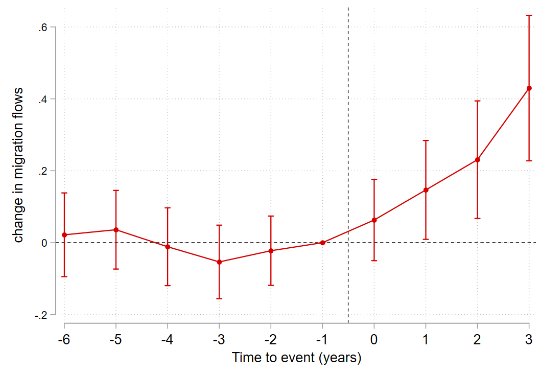
*Notes:* The results of a difference-in-differences regression of aggregate migration and wages on the number of CL posts per 1,000 inhabitants, set to 0 for non-CL cities. Each coefficient corresponds to a different diff-in-diff regression. All specifications include city and time fixed effects; columns (2) and (4) also control for time FE interacted with baseline Internet availability, and are estimated on a balanced matched sample. The IV columns use the normalised number of personal posts as an instrument for the number of job posts. Standard errors are in parentheses, clustered at city level throughout. $*p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Figure 4: Impact of CL entry on city-level hourly wages

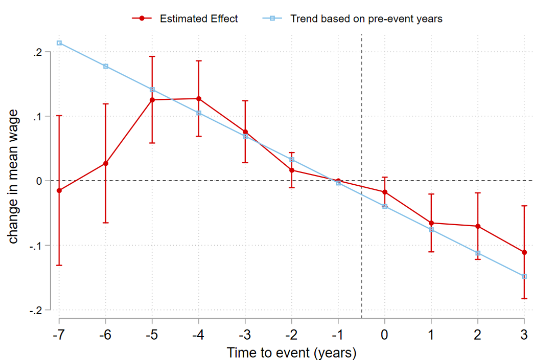
(a) Mean wage, all cities



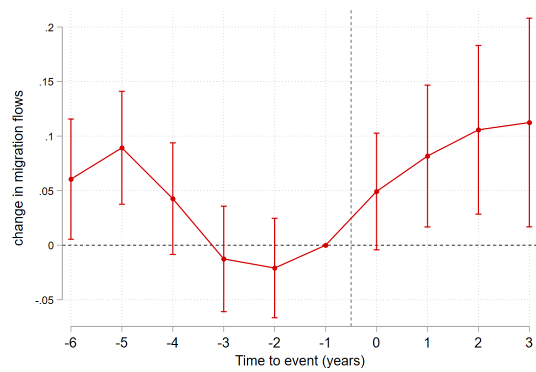

(b) Mean wage, preferred specification



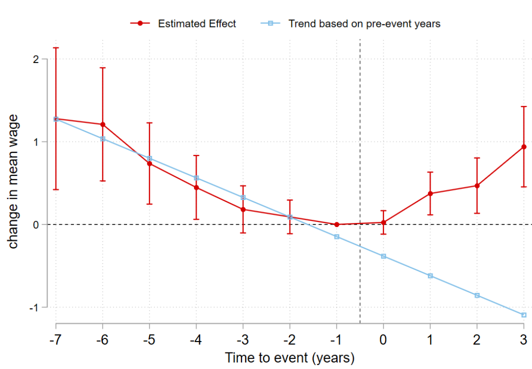

(c) Bottom 10%, all cities



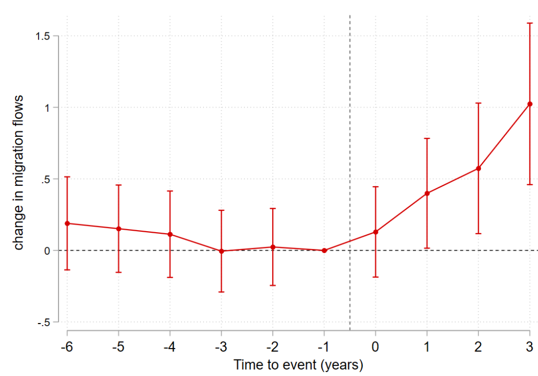

(d) Bottom 10%, preferred specification




(e) Top 10%, all cities




(f) Top 10%, preferred specification



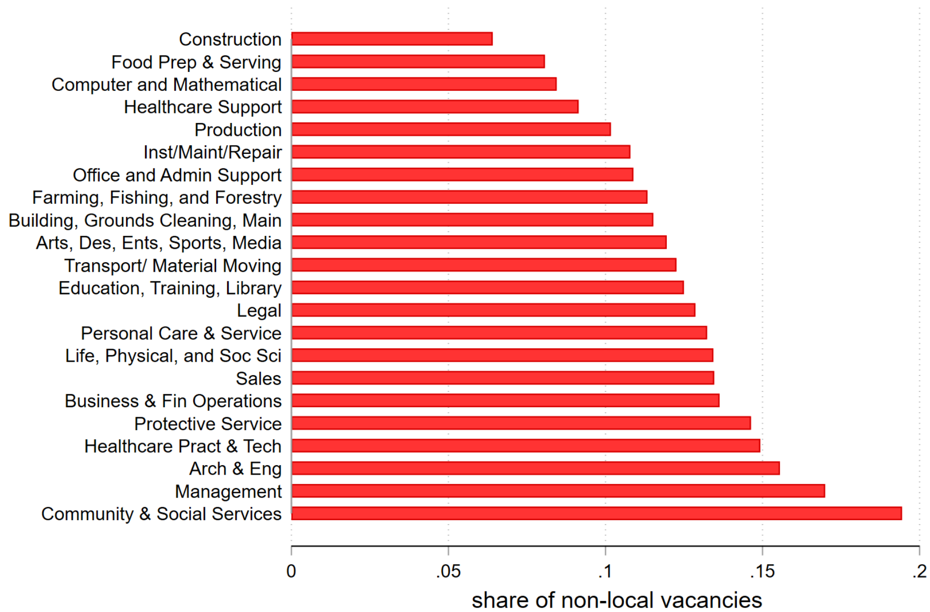

*Note:* Wages are city-level averages of hourly wage, in logs. "All cities" corresponds to raw event study with MSA and time FE on all cities. The blue line corresponds to linear trend extrapolated from the pre-treatment observations. "Preferred specification" corresponds to event study with city-specific linear trend. This corresponds to diff-in-diff in column (5) in Table 3.

Table 7: Impact of Craigslist entry on average hourly wages, event study parameters for robustness checks

| | Balanced sample | | | | All data | | Kijiji cities |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| | Levels | Levels | Levels | Levels | Log | Levels | Levels |
| *Panel A: Mean hourly wage* | | | | | | | |
| Treatment effect at t+0 | 0.0630* | 0.0482 | -0.0595* | 0.0790** | 0.00222 | 0.0393 | 0.0256 |
| | (0.0372) | (0.0395) | (0.0321) | (0.0393) | (0.00181) | (0.0328) | (0.0392) |
| Treatment effect at t+1 | 0.147** | 0.110** | -0.0628 | 0.169** | 0.00540* | 0.105* | 0.105 |
| | (0.0656) | (0.0501) | (0.0429) | (0.0669) | (0.00280) | (0.0535) | (0.0718) |
| Treatment effect at t+2 | 0.231** | 0.151** | -0.0659 | 0.247** | 0.00752* | 0.156** | 0.186* |
| | (0.0964) | (0.0647) | (0.0444) | (0.0982) | (0.00394) | (0.0772) | (0.110) |
| Treatment effect at t+3 | 0.430*** | 0.236** | 0.0194 | 0.473*** | 0.0114** | 0.298** | 0.341** |
| | (0.152) | (0.0926) | (0.0699) | (0.161) | (0.00539) | (0.120) | (0.163) |
| *Panel B: Bottom 10% hourly wage* | | | | | | | |
| Treatment effect at t+0 | 0.0492*** | 0.0633*** | 0.0200 | 0.0582*** | -0.00125 | -0.00506 | -0.0261 |
| | (0.0142) | (0.0211) | (0.0148) | (0.0151) | (0.00192) | (0.0126) | (0.0159) |
| Treatment effect at t+1 | 0.0817*** | 0.116*** | 0.0387* | 0.0971*** | -0.00561 | -0.0286 | -0.0740** |
| | (0.0279) | (0.0267) | (0.0230) | (0.0297) | (0.00384) | (0.0254) | (0.0330) |
| Treatment effect at t+2 | 0.106** | 0.169*** | 0.0498** | 0.128*** | -0.00553 | -0.0343 | -0.0978** |
| | (0.0415) | (0.0345) | (0.0234) | (0.0435) | (0.00511) | (0.0343) | (0.0470) |
| Treatment effect at t+3 | 0.112* | 0.221*** | 0.0382 | 0.157** | -0.0121* | -0.0642 | -0.137** |
| | (0.0637) | (0.0494) | (0.0301) | (0.0692) | (0.00682) | (0.0489) | (0.0644) |
| *Panel C: Top 10% hourly wage* | | | | | | | |
| Treatment effect at t+0 | 0.129 | 0.0717 | -0.189** | 0.166* | 0.00284 | 0.0723 | 0.0972 |
| | (0.0887) | (0.109) | (0.0731) | (0.0921) | (0.00281) | (0.0860) | (0.111) |
| Treatment effect at t+1 | 0.399** | 0.254* | -0.162 | 0.446*** | 0.0150*** | 0.417** | 0.556** |
| | (0.164) | (0.138) | (0.101) | (0.162) | (0.00474) | (0.164) | (0.224) |
| Treatment effect at t+2 | 0.573** | 0.262 | -0.229** | 0.618** | 0.0187*** | 0.555** | 0.845** |
| | (0.243) | (0.178) | (0.102) | (0.239) | (0.00655) | (0.232) | (0.331) |
| Treatment effect at t+3 | 1.024** | 0.382 | -0.106 | 1.100*** | 0.0267*** | 0.939** | 1.277*** |
| | (0.395) | (0.255) | (0.175) | (0.410) | (0.00905) | (0.364) | (0.483) |
| N | 2129 | 2129 | 1620 | 1919 | 3046 | 3046 | 1892 |
| MSA FE | YES | YES | YES | YES | YES | YES | YES |
| Time FE | YES | YES | YES | YES | YES | YES | YES |
| Linear treatment trend | YES | | | YES | YES | YES | YES |
| Linear MSA trend | | YES | | | | | |
| Covariates × time | YES | | | YES | | YES | YES |
| CL cities only | | | YES | | | | |
| City size common support | | | | YES | | | |
| All CL entries | | | | | | YES | |

*Note:* This table summarises the results of a dynamic diff-in-diff version of the robustness checks in Table A3. See the note for more details. Standard errors are in parentheses, clustered at city level throughout. $*p < 0.10$, $**$ $p < 0.05$, $***$ $p < 0.010$

Figure 5: Share of non-local recruitment in 1990, by occupation




*Note:* Each bar represents the share of newspaper help-wanted ads advertised in a newspaper not located in the same town as the job (employer).

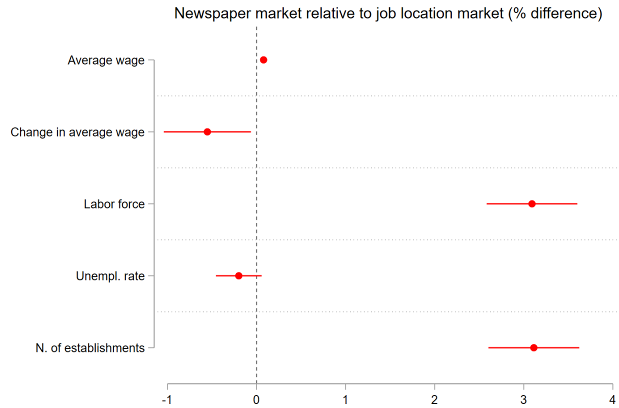
Table 8: Direction of non-local recruitment

| | (1)<br>Non-local hiring | (2)<br>Occup. share in the hiring market |
|---|---|---|
| Local occupation employment share | -1.393*** | -0.0864** |
| | (0.327) | (0.0304) |
| Constant | 0.0341*** | 0.00320*** |
| | (0.000891) | (0.0000670) |
| Observations | 1243 | 1550 |
| $R^2$ | 0.020 | 0.078 |

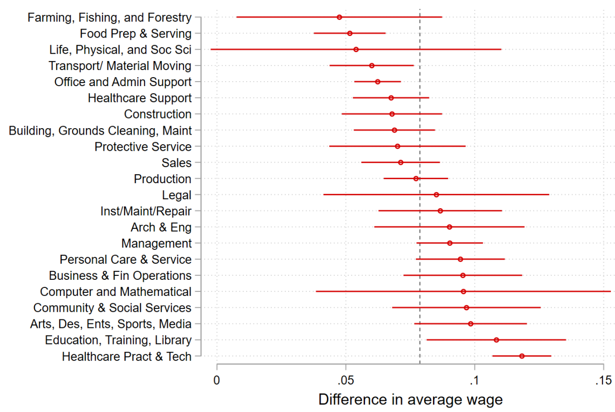
*Note:* Regressions analysing the placement of help-wanted ads across different cities. Column (1): regression of the share of non-local hiring at occupation level (dependent variable) on the employment share of the given occupation in the location of the job. Column (2): regression of the employment share of an occupation in the location of recruitment (dependent variable) on the employment share of the given occupation in the location of the job for non-local vacancies. Standard errors in parentheses, clustered at occupation level. $*p < 0.10$, $**\ p < 0.05$, $***\ p < 0.010$

Figure 6: Differences in labour market conditions between the location of the job and the location of recruitment, for non-local vacancies

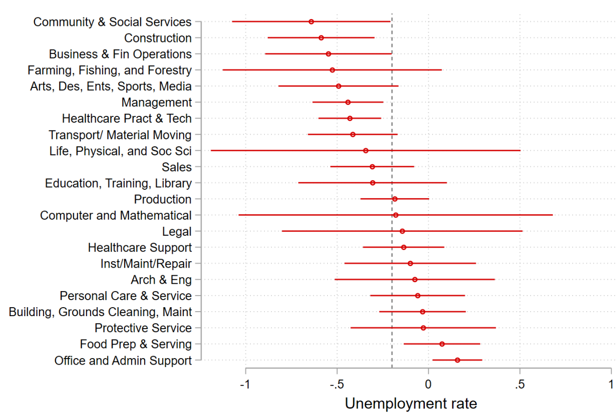
(a) Average differences across occupations




(b) Difference in county-level average pay, by occupation




(c) Difference in county-level unemployment rate, by occupation




*Note:* Unconditional differences in labour market characteristics at the county of the job location and the county of the newspaper location for no-local vacancies. Panel (a): the difference in average hourly wage, annual growth in hourly wage, the size of the labour force, the unemployment rate, and the number of establishments. Panel (b): the difference in average hourly wage by major occupation groups. Panel (c): the difference in unemployment rate by major occupation groups. The line corresponds to 95% confidence interval.

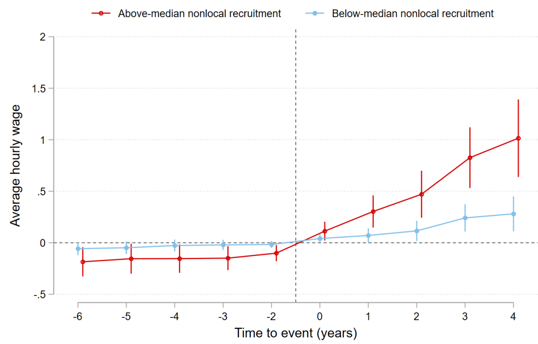
Table 9: The impact of CL entry on occupation-city-specific wages

| | Balanced sample | | | | Kajiji cities |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Treatment effect at t+0 | 0.0496 | -0.0332 | -0.0661* | 0.0328 | 0.0264 |
| | (0.0761) | (0.0455) | (0.0338) | (0.0343) | (0.0345) |
| Treatment effect at t+1 | 0.381*** | 0.0259 | -0.0219 | 0.0989 | 0.103* |
| | (0.0706) | (0.0678) | (0.0503) | (0.0614) | (0.0614) |
| Treatment effect at t+2 | 0.678*** | 0.0870 | 0.0248 | 0.133 | 0.156* |
| | (0.105) | (0.0911) | (0.0671) | (0.0910) | (0.0921) |
| Treatment effect at t+3 | 1.363*** | 0.312** | 0.252*** | 0.249** | 0.347*** |
| | (0.158) | (0.124) | (0.0874) | (0.126) | (0.131) |
| Treatment effect at t+4 | 1.880*** | 0.393** | 0.330*** | 0.236 | 0.398** |
| | (0.245) | (0.179) | (0.119) | (0.175) | (0.184) |
| N | 34353 | 34353 | 34353 | 34353 | 29167 |
| Time FE | YES | YES | YES | YES | YES |
| MSA FE | | YES | YES | YES | YES |
| SOC FE | YES | YES | YES | YES | YES |
| Covariates time FE | YES | YES | YES | | |
| Linear MSA trend | | | | YES | |
| Linear SOC trend | | | YES | YES | |

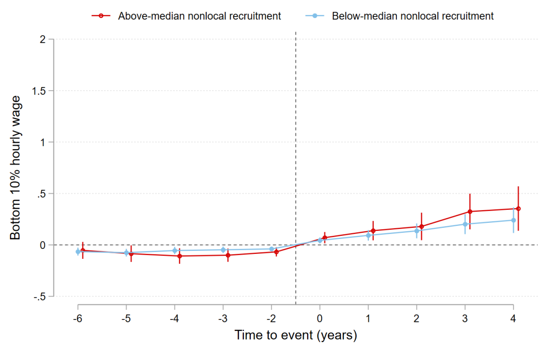
*Note:* Dynamic difference-in-differences regressions of city×occupation wages on Craigslist entry. All specifications include time and occupation fixed effects; specifications in columns (1) - (4) are estimated on a matched sample of treated and control cities. Column (1) also includes the standard interaction between time fixed effects and Internet availability (in levels and growth) at city level at the baseline. Column (2) adds city fixed effects. Column (3) further adds a linear occupation-specific trend. Column (4) uses a city-specific linear trend instead of the interaction between time fixed effects and Internet availability. Column (5) includes city- and occupation- fixed effects and is estimated on the sample of Kijiji cities. Standard errors in parentheses, clustered at occupation and city level. $*p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Figure 7: Impact of CL entry on wages, by non-local hiring in 1990

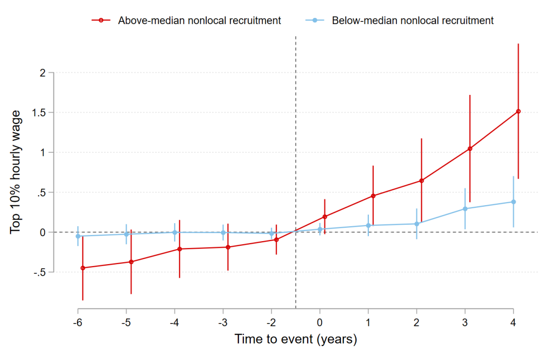
(a) Average hourly wage




(b) Bottom 10% hourly wage

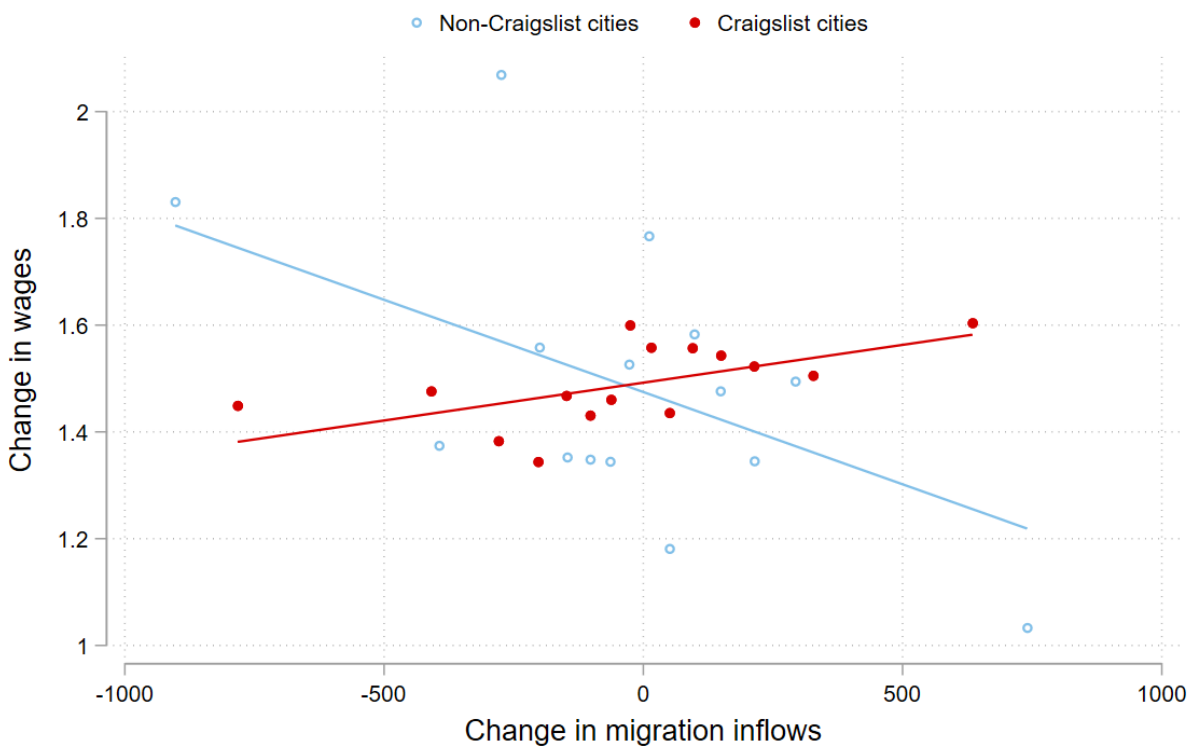



(c) Top 10% hourly wage




*Notes:* The results of an event study regression for city-occupation-specific wages. The regressions include city-, time- and occupation- fixed effects, as well as occupation- and treatment-group- specific linear trends. The sample consists of treated and control cities matched on baseline covariates. Non-local hiring is measured at occupation level from the dataset of newspaper help-wanted ads in 1990. Standard errors are clustered at occupation and city level. $*p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.010$

Figure 8: Relationship between changes in wages and gross migration inflows at occupation level within Craigslist and non-Craigslist cities




*Notes:* Binscatter of changes in city-occupation-specific wages and city-occupation-specific migration inflows. The changes are calculated between years 2005-2007. The specification includes city- and occupation- fixed effects. Red line and filled circles: Craigslist cities. Blue line and empty circles: non-Craigslist cities.

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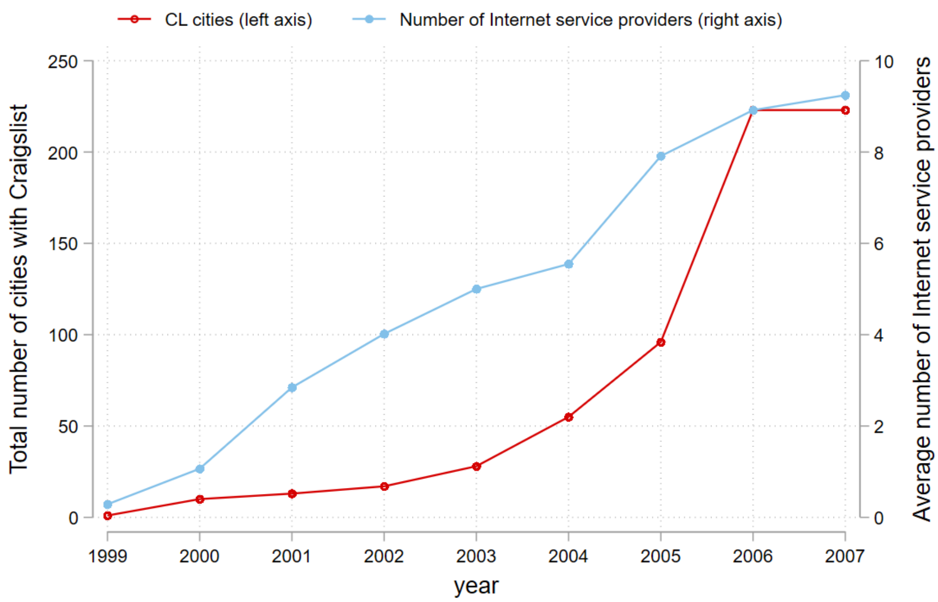
# A Appendix

Table A1: Variable definitions and data sources

| Variable | Description | Source |
|---|---|---|
| | *City-level variables* | |
| City-level migration | Number of individuals who changed city of residence between two fiscal years | SOI Tax Statistics, Inland Revenue Service |
| City $\times$ occupation migration | Number of individuals who changed their city of residence between two calendar years | American Community Survey |
| Wage | Mean, 10th percentile and 90th percentile of city or city $\times$ occupation hourly wage, annual statistic | Occupational Employment and Wage Statistics |
| Broadband availability | The number of high-speed Internet service providers within a city | Form 477 data, Federal Communications Commission |
| Employment share | Total employment (excl. solo-employment) as a share of total population | Occupational Employment and Wage Statistics |
| GDP growth | Annual growth in real Gross Domestic Product at city level | U.S. Bureau of Economic Analysis |
| Population | Annual estimate of the number of inhabitants | US Census Bureau, Population Division |
| Land area | Area of the city in squared miles | 2000 Census |
| Population density | Average number of inhabitants per squared mile | 2000 Census |
| Population share, by race/ethnicity | % of city population that is white/black/Hispanic | 2000 Census |
| Median age | | 2000 Census |
| Population share under 18 | % of city population under 18 years of age | 2000 Census |
| Population share over 75 | % of city population over 75 years of age | 2000 Census |
| | *County-level variables* | |
| Average wage | Average weekly wage based on the 12-monthly employment levels and total annual wage levels | Quarterly Census of Employment and Wages |
| Number of establishments | Annual average of quarterly establishment counts for a given year | Quarterly Census of Employment and Wages |
| Labour force | Number of individuals in the labour force | Local Area Unemployment Statistics |
| Unemployment rate | County-level unemployment rate (%) | Local Area Unemployment Statistics |

Figure A1: Craigslist expansion and Internet availability




*Notes:* The red line (left-hand side axis) plots the expansion of Craigslist until 2007 across US cities. The blue line (right-hand side axis) captures the average number of Internet service providers available at the city level. The number of ISP is measures at the ZIP-code level and aggregated by taking a population-weighted average within a city. This figure replicates Djourelova et al. (2022) at the city level.

Figure A2: CL and non-CL cities in descending order by population size, 2000

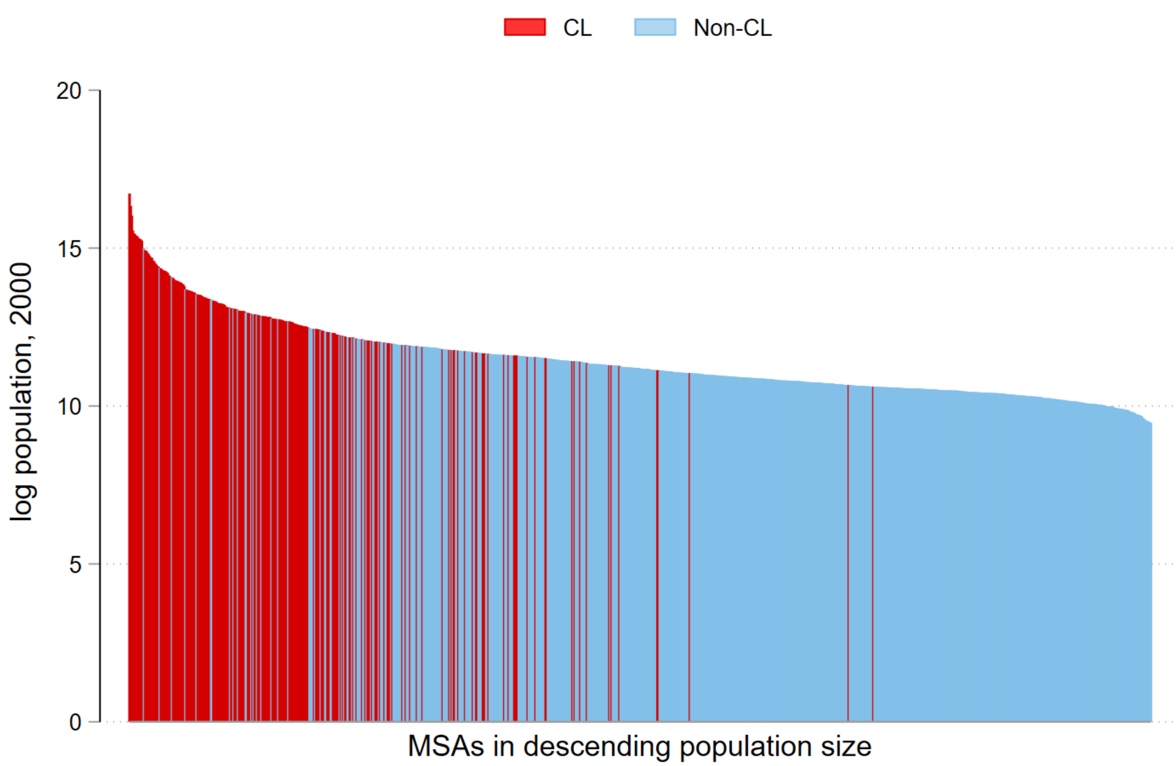
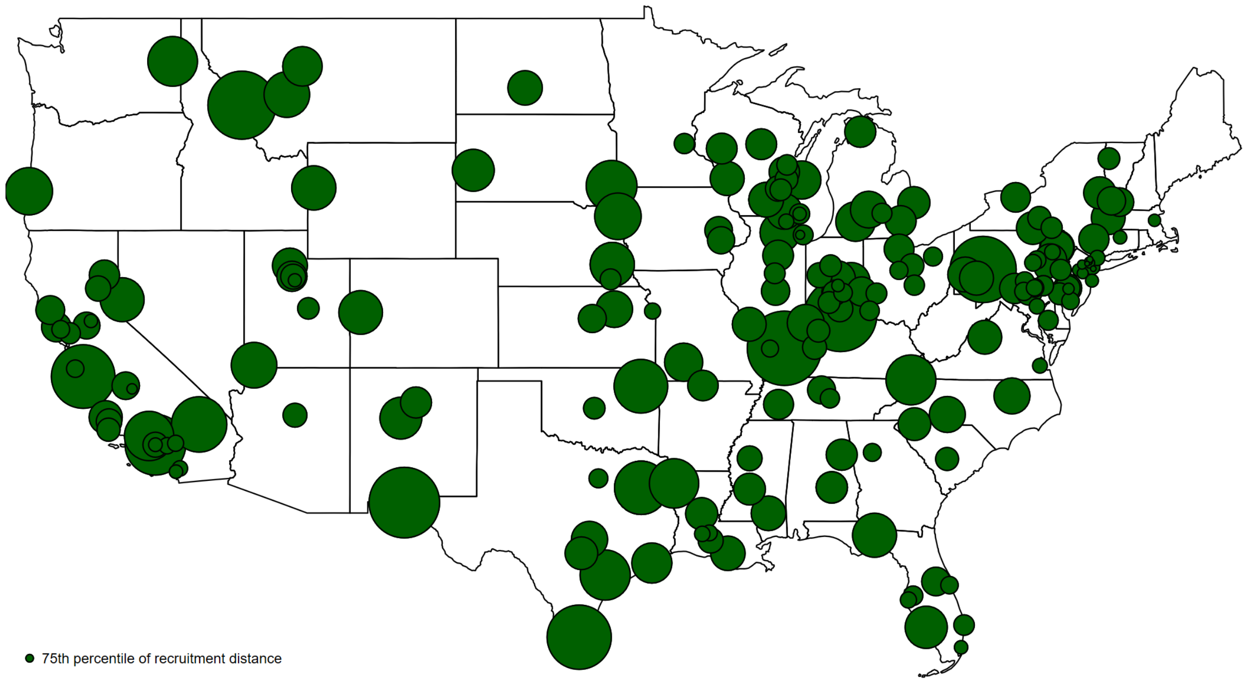


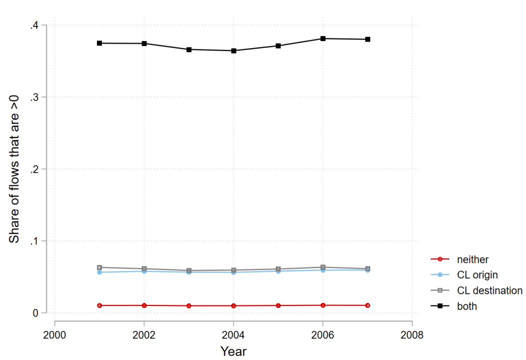
Figure A3: Local labour markets in the newspaper data




*Notes:* Each circle on the map represents a newspaper in my dataset of help-wanted ads. The size of the circle corresponds the geographic radius of help-wanted ads advertised in a given newspaper, measured at the 75th percentile of recruitment distance.

Figure A4: Positive bilateral flows

(a) Positive bilateral flows




(b) Changes to/from positive flows

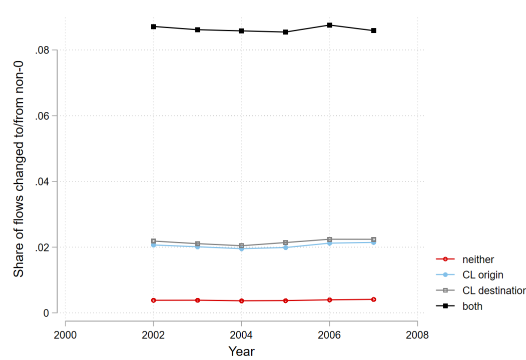
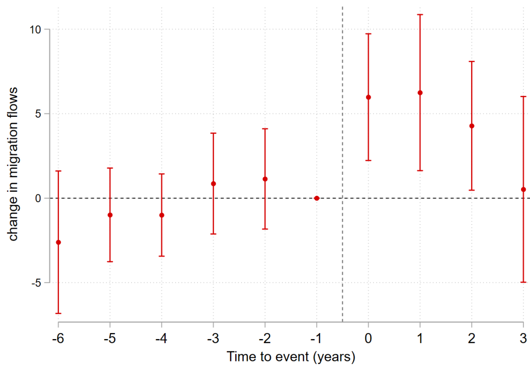
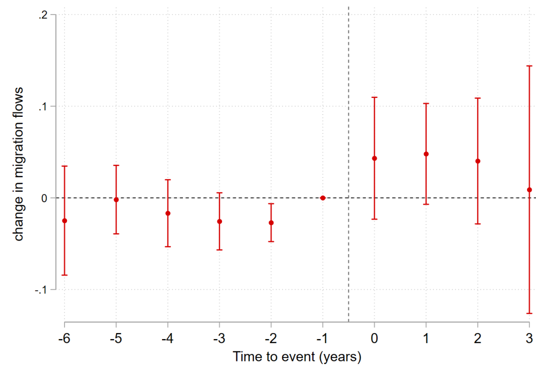


Figure A5: Impact of CL entry on bilateral migration flows

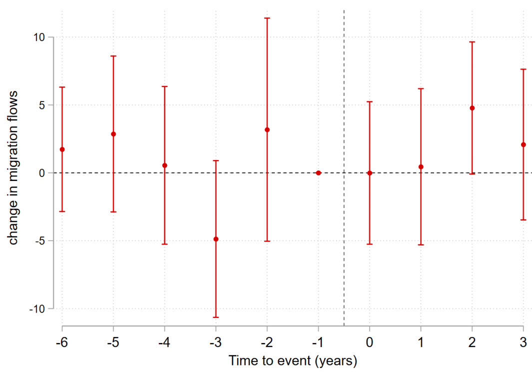
(a) Both cities CL, flows>0



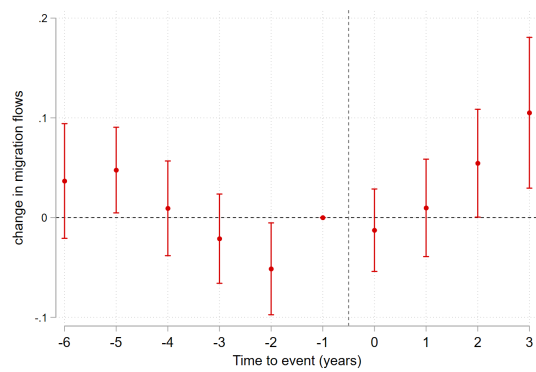

(b) Both cities CL, Poisson



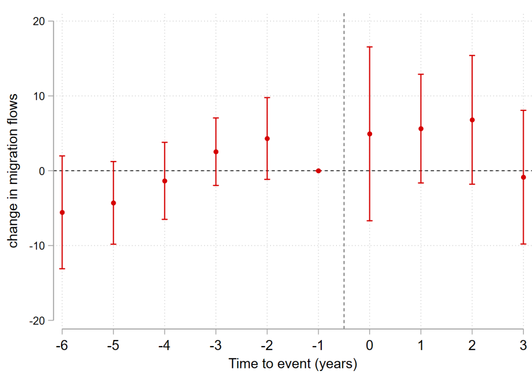

(c) CL origin, flows>0



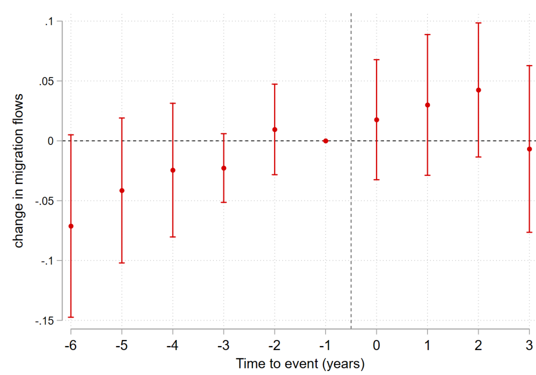

(d) CL origin, Poisson




(e) CL destination, flows>0



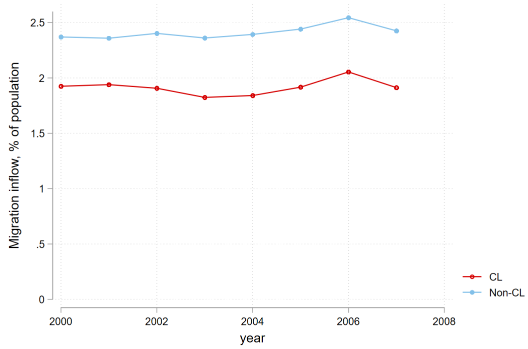

(f) CL destination, Poisson




*Notes:* The left-hand side panels contain only positive (non-0) bilateral flows. The right-hand side contains the full sample and is estimated using a Poisson regression. All regressions contain city and time FE and baseline Internet availability interacted with time dummies. The sample used is migration flows between cities such that both origin and destination are drawn from the matched sample.

Figure A6: Trends in aggregate migration flows

(a) Inflows




(b) Outflows

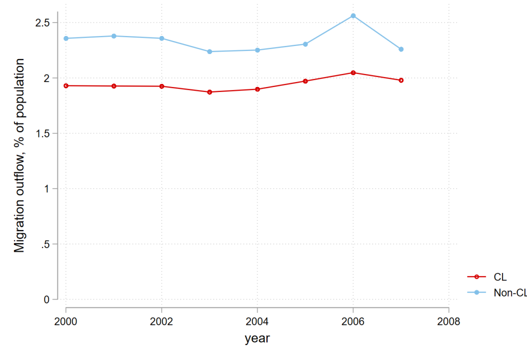
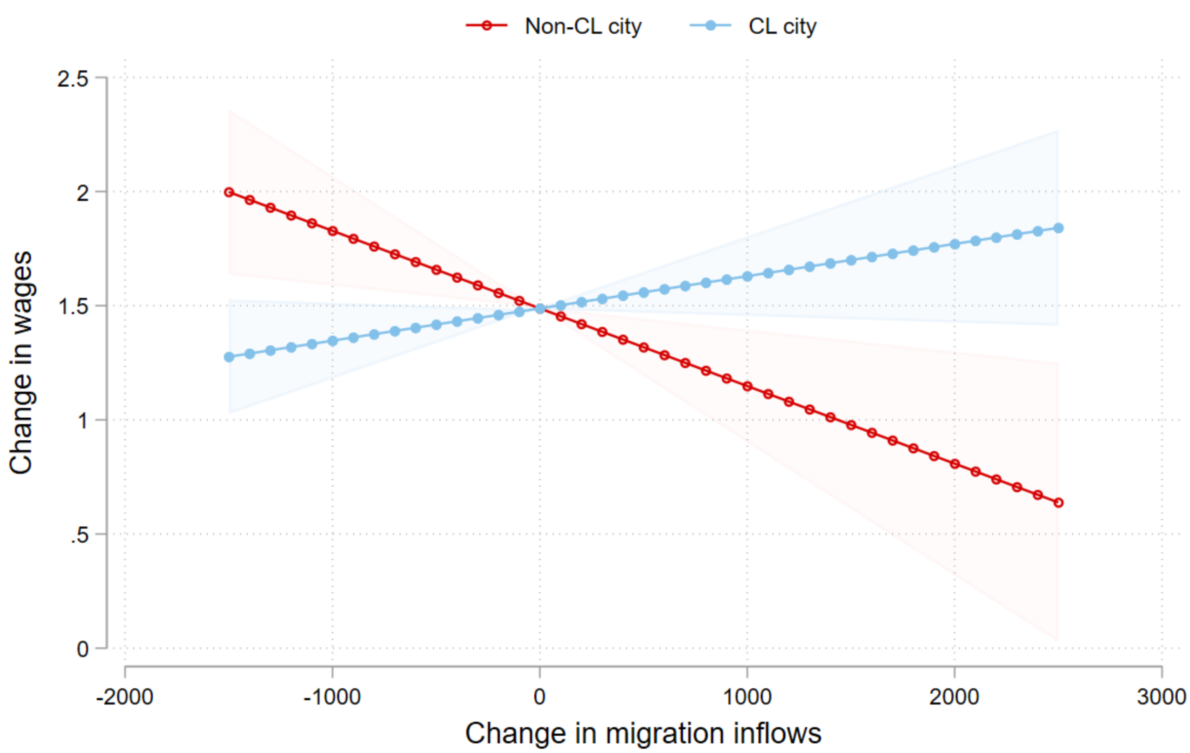



Figure A7: Estimated relationship between changes in wages and gross migration inflows at occupation level within Craigslist and non-Craigslist cities




*Notes:* Predicted margins from a regression of changes in city-occupation-specific wages on city-occupation-specific migration inflows. The changes are calculated between years 2005-2007. The specification includes city- and occupation- fixed effects. The slope for control cities is -0.00034 (0.0001243), and the difference between the slope for control and treated cities is 0.0004812 (0.0001482). The regression's R-squared is 0.3762, and the sample size is 1,624. The red line and filled circles in the plot: Craigslist cities. Blue line and empty circles: non-Craigslist cities. Standard errors are clustered at city level. The shaded areas correspond to confidence intervals at 95% statistical significance.

Table A2: Impact of Craigslist entry on aggregate migration flows, event study parameters for robustness checks

| | Balanced sample | | | | All data | | Kijiji cities |
|---|---|---|---|---|---|---|---|
| | (1) Levels | (2) Levels | (3) Levels | (4) Levels | (5) Log | (6) Levels | (7) Levels |
| *Panel A: Gross Inflows* | | | | | | | |
| Treatment effect at t+0 | 28.92 | -97.37 | 316.7 | 129.8 | 0.0214** | 150.4 | -238.1 |
| | (140.0) | (153.3) | (376.3) | (140.9) | (0.00860) | (194.1) | (364.7) |
| Treatment effect at t+1 | 766.5*** | 479.5* | 1757.2** | 671.6*** | 0.0387*** | 536.6* | 55.18 |
| | (267.8) | (268.6) | (859.2) | (223.9) | (0.0109) | (283.3) | (574.9) |
| Treatment effect at t+2 | 1113.7*** | 502.2 | 2899.1** | 560.7** | 0.0279** | 658.7** | 378.6 |
| | (380.3) | (320.5) | (1290.2) | (231.9) | (0.0121) | (323.5) | (740.2) |
| Treatment effect at t+3 | 1523.4** | 112.6 | 3639.8** | 517.5 | 0.0194 | 1877.3** | 1390.1 |
| | (649.8) | (633.8) | (1620.2) | (384.7) | (0.0161) | (836.6) | (1114.7) |
| *Panel B: Gross Outflows* | | | | | | | |
| Treatment effect at t+0 | 42.98 | 245.0** | 161.8 | 82.14 | 0.0179** | 148.3 | -847.9 |
| | (79.22) | (98.25) | (169.5) | (77.70) | (0.00795) | (185.5) | (905.1) |
| Treatment effect at t+1 | 174.4 | 538.2*** | 453.4 | 194.3** | 0.0175** | 497.1** | -246.1 |
| | (109.6) | (168.6) | (393.4) | (88.02) | (0.00802) | (209.0) | (821.6) |
| Treatment effect at t+2 | 643.1*** | 1135.2*** | 1131.4 | 478.6*** | 0.0328*** | 1324.7*** | 1009.7 |
| | (198.9) | (247.6) | (712.0) | (144.1) | (0.00924) | (372.2) | (1077.4) |
| Treatment effect at t+3 | 1240.5*** | 1835.1*** | 1842.0* | 798.8*** | 0.0621*** | 2850.5*** | 2405.7* |
| | (374.5) | (383.2) | (991.3) | (254.3) | (0.0129) | (678.3) | (1396.7) |
| *Panel C: Net Inflows* | | | | | | | |
| Treatment effect at t+0 | -14.05 | -342.3* | 154.9 | 47.66 | 0.0769 | 2.099 | 609.8 |
| | (146.8) | (196.6) | (356.0) | (137.6) | (0.0995) | (209.0) | (805.3) |
| Treatment effect at t+1 | 592.1* | -58.69 | 1303.8 | 477.3** | 0.132 | 39.46 | 301.3 |
| | (301.2) | (362.9) | (794.3) | (238.2) | (0.128) | (298.4) | (633.0) |
| Treatment effect at t+2 | 470.6 | -633.0 | 1767.7 | 82.13 | 0.138 | -666.0* | -631.0 |
| | (379.7) | (412.1) | (1145.4) | (246.1) | (0.118) | (396.6) | (963.2) |
| Treatment effect at t+3 | 282.9 | -1722.5** | 1797.8 | -281.3 | 0.0489 | -973.2 | -1015.6 |
| | (673.7) | (738.4) | (1473.0) | (460.2) | (0.154) | (950.5) | (1396.2) |
| N | 2200 | 2200 | 1670 | 1980 | 3577 | 9160 | 2110 |
| MSA FE | YES | YES | YES | YES | YES | YES | YES |
| Time FE | YES | YES | YES | YES | YES | YES | YES |
| Linear MSA trend | | YES | | | | | |
| Covariates × time | YES | | | YES | | YES | YES |
| CL cities only | | | YES | | | | |
| City size common support | | | | YES | | | |
| All CL entries | | | | | | YES | |

*Note:* This table summarises the results of a dynamic diff-in-diff version of the robustness checks in Table 5. Column (1) corresponds to my baseline preferred specification. Column (2) uses city-specific linear growth trends. Column (3) re-runs the baseline specification from column (1) but only on CL-cities. Column (4) re-runs the baseline specification further imposing common support over population size of treated and control cities. Columns (5) and (6) are estimated on the entire sample of cities; column (5) uses log migration flows, and column (6) includes cities that were treated in the years 2000-2003. Column (7) re-runs the baseline specification on Kijiji cities only. Standard errors are in parentheses, clustered at city level throughout. $*p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.010$

Table A3: Impact of Craigslist entry on average hourly wages, robustness checks

| | Balanced sample | | | | All data | | Kijiji cities |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| | Levels | Levels | Levels | Levels | Log | Levels | Levels |
| *Panel A: Mean hourly wage* | | | | | | | |
| Treatment effect | 0.125*** | 0.0770** | 0.182*** | 0.134*** | 0.00352 | 0.0214 | -0.0239 |
| | (0.0473) | (0.0345) | (0.0492) | (0.0500) | (0.00220) | (0.0503) | (0.0638) |
| Average wage | 15.960 | 15.960 | 15.960 | 15.960 | 2.763 | 15.960 | 15.960 |
| *Panel B: Bottom 10% hourly wage* | | | | | | | |
| Treatment effect | 0.0664*** | 0.0820*** | 0.0527** | 0.0764*** | -0.00302 | -0.0213 | -0.0508* |
| | (0.0219) | (0.0187) | (0.0229) | (0.0239) | (0.00317) | (0.0219) | (0.0261) |
| Average wage | 6.886 | 6.886 | 6.886 | 6.886 | 1.926 | 6.886 | 6.886 |
| *Panel C: Top 10% hourly wage* | | | | | | | |
| Treatment effect | 0.348*** | 0.220** | 0.545*** | 0.364*** | 0.0109*** | 0.145 | 0.0849 |
| | (0.118) | (0.0948) | (0.133) | (0.122) | (0.00409) | (0.176) | (0.236) |
| Average wage | 28.236 | 28.236 | 28.236 | 28.236 | 3.330 | 28.236 | 28.236 |
| Observations | 2129 | 2129 | 1620 | 1919 | 3046 | 3046 | 1892 |
| MSA FE | YES | YES | YES | YES | YES | YES | YES |
| Time FE | YES | YES | YES | YES | YES | YES | YES |
| Linear treatment trend | YES | | | YES | YES | YES | YES |
| Linear MSA trend | | YES | | | | | |
| Covariates × time | YES | | | YES | | YES | YES |
| CL cities only | | | YES | | | | |
| City size common support | | | | YES | | | |
| All CL entries | | | | | | YES | |

*Note:* This table summarises the results of a TWFE regression of CL entry on city-level hourly nominal wages. The presented coefficient estimates the annual change in average wage at city level following CL entry, averaged across different years after entry. Column (1) corresponds to my baseline preferred specification, using a matched sample of treated and control cities and including linear treatment trend and an interaction between period FE and baseline Internet service availability. Column (2) uses city-specific linear growth trends instead. Column (3) re-runs the baseline specification from column (1) but only on CL-cities. Column (4) re-runs the baseline specification further imposing common support over population size of treated and control cities. Columns (5) and (6) are estimated on the entire sample of cities; column (5) uses log, rather than levels, of wages, and column (6) includes cities that were treated in the years 2000-2003. Column (7) re-runs the baseline specification on Kijiji cities only. Standard errors are in parentheses, clustered at city level throughout. $*p < 0.10$, ** $p < 0.05$, *** $p < 0.010$