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ABSTRACT

Employer Market Power in Silicon Valley*

Adam Smith alleged that employers sometimes secretly collude to reduce labor earnings. This paper examines an important case of such behavior: illegal no-poaching agreements through which information-technology companies agreed not to compete for each other's workers. Exploiting the plausibly exogenous timing of a US Department of Justice investigation, I estimate the effects of these agreements using a difference-in-differences design. Data from Glassdoor permit the inclusion of rich employer- and job-level controls. On average, the no-poaching agreements reduced salaries at colluding firms by 4.8 percent. Stock bonuses and ratings of job satisfaction were also negatively affected. These estimates are consistent with considerable employer market power.

JEL Classification: J42, K42, L41, K21, J31

Keywords: monopsony, oligopsony, employer market power, labor earnings

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* This paper was updated in March 2022. The revision reports results as the difference in average salary across colluding and non-colluding firms, rather than a per-agreement effect, but the underlying analysis has not changed substantially. In addition, the revision contains new material on the sources of employer market power in Silicon Valley. I thank Joseph Altonji, Orley Ashenfelter, Mark Borgschulte, Andrew Chamberlain, Jeffrey Clemens, Daniel Hamermesh, Ioana Marinescu, Matthew Notowidigdo, Jesse Rothstein, Todd Schoellman, Jeffrey Shrader, Evan Starr, Basit Zafar, and seminar participants at Haverford College, the IZA Junior-Senior Labor Symposium, Marquette University, the Stanford Institute for Theoretical Economics, and UC Davis for valuable comments. I am grateful to the W.E. Upjohn Institute for Employment Research for financial support of this research project, and to Glassdoor for data.

I would be very pleased if your recruiting department would stop doing this.
–Steve Jobs (Apple), in an email to Eric Schmidt (Google; 2005)

Steve, as a followup we investigated the recruiter's actions and she violated our policies. Apologies again on this... Should this ever happen again please let me know immediately and we will handle. ...On this specific case, the sourcer who contacted this Apple employee should not have and will be terminated within the hour.
–Schmidt reply to Jobs

:)
–Jobs reply to Schmidt

1 Introduction

"We rarely hear... of the combinations of masters, though frequently of those of workmen," writes Adam Smith, "But whoever imagines... that masters rarely combine [to lower wages], is as ignorant of the world as of the subject. ... These are always conducted with the utmost silence and secrecy... and when the workmen yield... they are never heard of by other people" [Smith, 1790]. Recent years have seen renewed interest in the causes and consequences of employer market power [US CEA, 2016, Yeh et al., 2022, Card, 2022], including declining unionization [Blanchard and Giavazzi, 2003, US Department of the Treasury, 2022], mergers [Marinescu, 2018], and non-compete clauses [Marx et al., 2009]. But this literature has not investigated the case Smith considered so common: secret coordination of managers aimed at reducing labor earnings. Today such behavior is difficult to study because it is typically illegal, giving firms powerful incentives to hide it from both government officials and researchers. The 2005-2009 "no-poaching" agreements among Silicon Valley technology firms provide a rare opportunity to examine the clandestine exercise of employer market power.

The following firms were party to at least one no-poaching agreement: Adobe, Apple, eBay, Google, Intel, Intuit, Lucasfilm and Pixar. Concluded at the highest levels of management, including boards and CEOs, all of the agreements prohibited participating firms from recruiting or hiring each other's employees. Managers informed recruiters which potential hires were off-limits and some recruiting departments maintained written lists. Some agreements included additional anticompetitive restraints, such as prohibitions of bidding wars.[1] Implementation was straightforward. A potential new employee can hardly avoid disclosing her recent and current employers to a prospective employer. Even if she were to withhold such information, platforms like LinkedIn allow employers to obtain it easily. Enforcement was similarly straightforward. In cases where a firm violated an agreement, its counterparty often contacted a senior manager at the violating firm, who would then put a stop to the violation [US Department of Justice, 2010b, 2012]. This use of market power was remarkably simple and cheap, relying on well-defined commitments from a small number of individuals. It required no elaborate salary schedules. The ease with which these firms coordinated stands in some contrast to the difficulty of sustaining coordination in many textbook theoretical models of firm behavior. That these firms did coordinate is surprising, as large firms should expect to face greater regulatory scrutiny than small [Basu and Dixit, 2017], and on average high-wage firms commit fewer violations of labor rights [Marinescu et al., 2021c].[2]

[1] Additional details of the agreements are discussed in Section 4.3.

[2] This is merely suggestive; labor-rights violations are an equilibrium outcome determined by worker reporting decisions, agency enforcement decisions, and firm decisions.

Prompted by a whistleblower, a US Department of Justice (DOJ) investigation began to unravel the no-poaching agreements in the first half of 2009. National media revealed the antitrust investigation on June 3, 2009 and the DOJ filed its civil complaint in US v. Adobe Systems on Sept. 24, 2010 [Helft, 2009, US Department of Justice, 2010b]. This was followed by a civil class action in 2011, with settlements in 2015 and 2018. While the DOJ did not undertake a criminal prosecution in response to the no-poaching agreements, it had the authority to do so under the Sherman Act.[3] The DOJ made this explicit in 2016 guidance for human resources departments: "Going forward, the DOJ intends to proceed criminally against naked wage-fixing or no-poaching agreements. These types of agreements eliminate competition in the same irredeemable way as agreements to fix product prices or allocate customers, which have traditionally been criminally investigated and prosecuted as hardcore cartel conduct" [U.S. DOJ and U.S. FTC, 2016].[4]

Using difference-in-differences designs, I estimate the effect of these no-poaching agreements on labor outcomes. The timing of entry into the agreements is potentially a function of unobserved economic factors that also influence labor earnings. To mitigate endogeneity concerns, I instead study exit from the agreements induced by the plausibly exogenous timing of the DOJ investigation. My research design compares outcomes at colluding firms to those at other information-technology firms, before and after the DOJ intervened. On average, the no-poaching agreements reduced salaries at colluding firms by 4.8 percent. Consistent with theory [Oyer and Schaefer, 2005], I find negative effects on stock bonuses, but no effects on cash bonuses. Survey measures of satisfaction with compensation and benefits also exhibit negative effects. My data are labor surveys from the website Glassdoor. They include employer names and detailed job classifications, salary and other compensation, and job ratings.

These results are important because the information technology sector is a large and growing part of the US economy. From 1997 to 2019, value added in this sector rose from $232 billion to $1.7 trillion [real 2012 dollars; US Bureau of Economic Analysis, 2019].[5] This paper's estimates may assume more general significance because recent evidence suggests growing scope for employer market power in the US. The DOJ identified reduced coordination costs from market concentration as a contributor to the technology-sector no-poaching agreements [US Department of Justice, 2012].[6] From 1997 to 2012, the revenue share of the

[3]Explicit collusion to depress labor compensation is illegal under the Sherman Act, and exercising market power is illegal under the Clayton Act [US Department of Justice, 2010b, Marinescu and Hovenkamp, 2018, Naidu et al., 2018].

[4]To date the DOJ has filed at least two indictments in accordance with this guidance [Jacobovitz and Kanters, 2021].

[5]Value-added figures are for "Information-communications-technology-producing industries."

[6]Smith [1790] commented in similar spirit, "The masters, being fewer in number, can combine much more

top 50 firms increased in the majority of US industries [US CEA, 2016], and workers in a majority of US occupations face labor markets that are "highly concentrated" under DOJ guidelines [Azar et al., 2020]. Growing use of arbitration and non-compete clauses may also be increasing employer market power [US CEA, 2016].

This paper contributes to the empirical literature on employer market power.[7] It complements the growing body of evidence on non-compete agreements, much of which exploits policy changes. Balasubramanian et al. [2020] evaluate a 2015 Hawaii ban on non-compete and non-solicitation clauses in the technology sector, while Lipsitz and Starr [2021] evaluate a 2008 Oregon ban on non-compete clauses for hourly workers.[8] Krueger and Ashenfelter [2017] study the prevalence of no-poaching agreements in the franchise sector. Naidu et al. [2016] use a policy reform relaxing constraints on worker mobility in the United Arab Emirates to study the effect of monopsony on earnings.[9] Another strand of research uses quasi-experimental wage variation to recover labor supply elasticities.[10] Azar et al. [2019] employ instrumental variables designs and recover firm-level labor supply elasticities consistent with employer power in US labor markets, while Dube et al. [2020] similarly find low elasticities in the labor market on Amazon's MTurk platform. Staiger et al. [2010] use a policy-mandated wage change at a subset of VA hospitals and likewise estimate elasticities consistent with employer power.

Relative to the existing empirical literature, this paper differs along several dimensions. First, to the best of my knowledge, it is the first empirical work on the earnings effects of no-poaching agreements. Such agreements may affect workers even in jurisdictions where non-competes are banned or unenforceable. Because workers do not agree to be constrained by no-poaching agreements, the possibility of compensation for the constraint is foreclosed [Kini et al., 2021]. Because the agreements are not announced, workers receive no signal to respond, e.g. by increasing job search effort. Unlike non-compete clauses, no-poaching agreements directly limit the diffusion of information to workers through recruiting calls and competing offers. Second, because my research design relies on the timing of a whistleblower tip, it avoids policy endogeneity. For example, pre-announced policy changes may generate confounding anticipatory responses, or policy timing may respond to labor market conditions. Third, this paper studies bilateral restraints among a small number of firms in the presence of a large competitive fringe, rather than a jurisdiction- or sector-level policy change. Fourth,

easily [than the workmen]..."

[7] For surveys see Boal and Ransom [1997], Bhaskar et al. [2002], Ashenfelter et al. [2010], Manning [2011], and Manning [2021].

[8] Nonsolicitation clauses prohibit former employees of a firm from soliciting its clients.

[9] Prior to the reform workers faced monopsony in the strict sense: just one potential employer.

[10] A closely related set of papers infers labor supply elasticities from recruitment and separation elasticities [Manning, 2021].

to the best of my knowledge this is the first paper in the economics literature to examine the secret and illegal exercise of employer market power.

More broadly, my results contribute to the economic literature on white-collar crime descended from Sutherland [1940].[11] The prevalence of such crime is difficult to assess, but prominent examples occur with regularity: in 2008 the Madoff Ponzi scheme came to light; and in 2012 the US began to investigate the rigging of the LIBOR by investment banks.[12] Over the same period white-collar prosecutions have declined, falling more than 50 percent since 2011 and reaching a record low in 2020 [TRAC Reports, 2019, 2021].[13] A rational model of crime like Becker [1968] predicts increased lawbreaking in response to reduced enforcement, and this argues for the importance of research on this topic. Mark Cohen has investigated the total social costs of white-collar crime using contingent valuation methods [Cohen, 2015, 2016]. Much of the economic research on white-collar crime studies tax evasion. Notable examples include Slemrod [2004] and the work of Gabriel Zucman [Zucman, 2013, Alstadsæter et al., 2019].[14] The remaining literature is rather idiosyncratic. Levitt [2006] studies non-payment for donuts and bagels in office settings where individual payments are unobserved.[15] Fisman and Miguel [2007] find that diplomats from more corrupt countries incur more unenforceable parking tickets near the UN, while Bourveau et al. [2021] find indirect evidence of increased insider trading by company directors after the election of a French President to whom they are linked. My study adds to the small branch of this literature on criminal violations of antitrust statutes [Gallo et al., 1994]. It also contributes by recovering a causal estimate of the impact on victims. Because victims of white-collar crime are typically many and difficult to identify, such estimates are frequently unavailable.[16]

The rest of the paper proceeds as follows. Section 2 describes my data, Section 3 presents estimating equations, and Section 4 discusses empirical results. Section 5 considers the sources of employer market power in Silicon Valley. Section 6 evaluates policy implications and concludes.

[11] Edwin Sutherland coined the phrase "white-collar crime" in a 1939 address, published as Sutherland [1940], and antitrust violations are one of the four types of such crime discussed in Sutherland [1945].

[12] LIBOR stands for "London Interbank Overnight Rate." Interest rates on many debt instruments are indexed to LIBOR.

[13] TRAC data begin in 1986.

[14] See Slemrod [2007] for a review.

[15] Contrary to the priors of many economists, Levitt finds average payment equal to 90 percent of the posted price.

[16] Sutherland [1945] writes, "The effects of a white collar crime upon the public are diffused over a long period of time and perhaps over millions of people, with no person suffering much at a particular time." In cases where white-collar crime has led to corporate bankruptcy, the effect on debt and equity holders is fairly clear and typically has not prompted econometric investigation, at least in academic publications. Examples include the Enron and Madoff bankruptcies. Impacts on victims of tax evasion, insider trading, fraud, and other common forms of white-collar crime are much more difficult to estimate.

2 Data

2.1 Description

My primary data come from Glassdoor, an online aggregator of wage and salary reports contributed by workers. Reports cover employer, work location, job, salary, and experience. The chief strengths of these data, relative to public data sets like the Current Population Survey, are the inclusion of employer names and detailed job classifications. Glassdoor uses machine-learning models to classify users' jobs at three increasingly granular levels: general occupation, specific occupation, and job. The ten most frequent categories in my sample under each classification are in Table A5. As described by the company, the machine-learning model groups jobs using job search and clicking behavior on the Glassdoor website. Importantly, salary information is not an input into the model. The Glassdoor salary variable is not censored at high values. For users that report monthly or hourly earnings (15 percent of my sample), I impute an annual salary by assuming a 40-hour work week and 50 work weeks per year.[17] Some users report non-salary compensation variables, including stock and cash bonuses. I convert all nominal amounts to 2009 US dollars using the chained personal consumption expenditures deflator from the US Bureau of Economic Analysis. The data also include age, education, and gender for a subset of users. While some Glassdoor reports are unincentivized, others are incentivized by a "give-to-get" model: complete access to the website's aggregate salary and job satisfaction data requires a survey response that passes quality checks. Users may submit multiple reports for the same or different jobs. The resulting sample is non-random, and I discuss sample selection in Section 2.3 below. My estimation sample comprises all Glassdoor reports by regular, full-time employees[18] 2007-2018 in US industries containing at least one colluding firm: "Computer Hardware & Software", "Internet", and "Motion Picture Production & Distribution." All colluding firms are represented. All non-colluding firms for which Glassdoor reports exist are included. Table A1 provides descriptive statistics for the entire sample. Table A2 compares colluding and non-colluding firms 2015-2018, by which time effects of the no-poaching agreements had likely dissipated. The two groups are unconditionally similar on base salary, with both averages near $98,000, but colluding firms show higher stock and cash bonuses during this period. Means of demographic variables are similar. Note that my identification strategy does not rely upon such unconditional comparisons of levels; Section 3 discusses assumptions that are invoked. Figure A1 shows mean salaries for colluding firms over time.

A second Glassdoor data set contains user ratings of jobs and job attributes: career

[17] Excluding these observations does not meaningfully change my estimates; see Section 4.2.

[18] Temporary, part-time and contract workers are excluded.

opportunities, compensation and benefits, senior leadership, and work-life balance. Ratings range from one to five stars.[19] These data begin a year later, in 2008. Users are a subset of those who contribute salary reports. Figure A2 is a histogram of compensation ratings and Table A3 provides descriptive statistics for the full sample of ratings. Table A4 reports means for colluding and non-colluding firms 2015-2018. Mean ratings are higher at colluding firms on all dimensions. These ratings data should be approached with care. Users face no incentive to minimize misreporting, and many of the standard critiques of stated-preference measures apply. For example, three stars might have different meanings to different users, or on different dimensions. Bearing these caveats in mind, it is interesting to study these ratings because they plausibly reveal some aspects of users' information sets. Green et al. [2019] show that changes in Glassdoor ratings predict future earnings surprises, which argues for their informativeness.

2.2 Measurement error & misreporting

Self-reported data naturally raise the question of measurement error. Karabarbounis and Pinto [2018] investigate by comparing Glassdoor data to the Quarterly Census of Income and Wages (QCEW) and the Panel Study of Income Dynamics (PSID). Industry-level correlations for mean salary are .87 and .9, respectively. The authors conclude, "...the wage distribution (conditional on industry or region) in Glassdoor represents the respective distributions in other datasets, such as QCEW and PSID fairly well." Martellini et al. [2021] find close agreement between Glassdoor and the US Department of Education's College Scorecard, which is based on administrative tax data. Sockin [2021] estimates a correlation of .92 between Glassdoor industry-occupation means and the corresponding means from the CPS Annual Social and Economic Supplement.

More generally, previous research suggests survey respondents report annual pre-tax earnings with good accuracy. Using the Displaced Worker Supplement to the Current Population Survey (CPS), Oyer [2004] finds mean reporting error of +5.1% and median error of +1.3%. Both mean and median error are smaller for respondents reporting annual earnings, as 85 percent of respondents in my data do. Similarly, Bound and Krueger [1991] compare CPS reports to Social Security earnings records and find a signal-to-noise ratio of .82 for men, .92 for women. Abowd and Stinson [2013] relax the assumption that administrative data are accurate and survey data are measured with error. They estimate similar reliability statistics for the Survey of Income and Program Participation and Social Security earnings data. Using the same two data sets, Kim and Tamborini [2014] find reporting error is smaller for

[19] Half-stars were permitted for attributes (but not for overall ratings) 2008-2012.

workers with undergraduate and graduate degrees, who comprise 93 percent of my sample (Table A1).

One might worry about misreporting driven not by users, but by firms. Journalists have documented attempts by some firms to induce sudden waves of high ratings from their employees. For example, the firm Guaranteed Rate engineered a sharp increase in its rating in September-October 2018 [Winkler and Fuller, 2019]. Following the public disclosure of the DOJ investigation in June 2009, the firms that participated in the no-poaching agreements had reason to falsely increase Glassdoor salary reports and job ratings. They also had reasons to refrain from such behavior. Colluding firms were under press scrutiny and DOJ investigation. They would have expected a class action to follow. Attempts to manipulate Glassdoor data might have leaked to the press or emerged in discovery. Moreover Glassdoor was less prominent during the period in question than it is today, reducing the return to risky manipulation. The examples of firm-driven misreporting in Winkler and Fuller [2019] are all from 2016 or later, and their aggregate data show an increase in the share of five-star ratings from 2015. Glassdoor has strong incentives to police firm-driven manipulation, which degrades the value of its site to job seekers, and does so using both human moderators and machine-learning algorithms [Winkler and Fuller, 2019]. There is an additional concern, however: colluding firms might have discouraged employees from posting negative ratings or low salaries during the collusive period. Potentially consistent with such a story, Sockin and Sojourner [2020] find that employees are less likely to reveal negative information when employer retaliation is more probable. Several features of this paper's setting militate against this concern, however. The no-poaching agreements were illegal and secret. Employees had no reason to believe that their salary reports and job ratings were sensitive. Attempts by management to discourage Glassdoor submissions would have risked arousing employee curiosity, particularly as salaries at colluding firms remained high in absolute terms. Section 3 returns to the question of firm-driven misreporting in the context of an event study.

2.3 Sample selection

The plaintiffs' expert report from the civil class action [Leamer, 2012] contains some data that are useful in evaluating selection into my Glassdoor sample. Leamer [2012] Fig. 5 gives firms, jobs, years, and nominal compensation for the named plaintiffs. While these observations are not randomly selected, that does not imply that they are not representative. Indeed all but one of the observations for the named plaintiffs are close to the corresponding fitted values from Leamer's econometric model, estimated using complete administrative data from defendant firms. With one exception (described below), they are representative despite their non-random selection. One named plaintiff earned \$118,226 in salary and \$3,445 in other compensation as a Computer Scientist at Adobe in 2008. Matching on firm, job, and year, the corresponding Glassdoor means ($n = 17$) are \$127,240 and \$11,917. A second named plaintiff earned an average of \$109,363 in salary and \$30,641 in other compensation as a Software Engineer at Intel 2008-2011. The corresponding Glassdoor means ($n = 233$) are \$111,914 and \$15,565. A third named plaintiff held multiple positions at Intuit. In 2008 he earned \$91,300 in salary and \$83,877 in other compensation as a Software Engineer. (This observation is far from the corresponding fitted value of roughly \$110,000 from the Leamer model, perhaps because of the large non-salary compensation.) The corresponding Glassdoor means ($n = 12$) are \$94,210 and \$9,320. In 2009 he earned \$94,000 in salary and \$38,553 in other compensation as a Software Engineer II. The corresponding Glassdoor means ($n = 3$) are \$103,506 and \$10,071. The mean salary difference between the administrative and Glassdoor data is \$5,995. These observations suggest that the Glassdoor data are useful measurements of salaries at colluding firms. The Glassdoor measures of non-salary compensation are noisier, at minimum, and potentially less representative.[20] Leamer's Exhibit 2 permits a few comparisons of report frequencies by job for Pixar Animation. The top five jobs by count of worker-years are "Technical Director," "Animator," "Software Engineer," "Artist–Story," and "Artist–Sketch." In Glassdoor data the top five Pixar jobs by worker-years are "Technical Director," "Production Coordinator," "Software Engineer," "Senior Software Engineer," and "Animator." While these lists do not match perfectly, they are similar.

The above comparisons are suggestive, but quite limited in scope. The Occupational Employment Statistics from the Bureau of Labor Statistics permit a broader set of comparisons at the occupation-year level, including both treatment and control firms. Figure 1 presents a scatter plot of occupation-years, where occupations are defined by year-2010 Standard Occupational Classification (SOC) codes. Vertical coordinates are nominal mean salaries from my Glassdoor sample. Horizontal coordinates are nominal mean salaries from BLS OES data. The 45-degree line provides a benchmark, but complete agreement is not expected, as Glassdoor occupations were not designed to map exactly onto SOC codes. A local linear fit through the scatter shows the empirical relationship between OES and Glassdoor means and the bands around it represent the 95 percent confidence interval.[21] Glassdoor means are slightly above their OES analogs: the average occupation-year difference is approximately \$3,600. But overall the local linear fit hews closely to the 45-degree line throughout the uncensored range of the BLS data.[22] While the Glassdoor sample is not randomly drawn,

[20]For non-salary compensation, the mean difference between administrative and Glassdoor data is -\$13,170.

[21]The local linear fit is constructed with an Epanechikov kernel (the Stata default) and \$3,000 bandwidth.

[22]Beyond \$145,600 some OES means are top-coded, making agreement between the two data sources much less likely.

Figure 1 provides evidence that it is nonetheless reasonably representative.

3 Empirical strategy

I begin from the following difference-in-differences equation.

$$\ln\left(Salary_{iejlt}\right) = \boldsymbol{\alpha_{ej}} + \boldsymbol{\beta_{jt}} + \boldsymbol{\gamma_{lt}} + \delta Agreement_{et} + \varepsilon_{iejlt} \tag{1}$$

Bold font denotes a vector and indices are i for user, e for employer, j for job, l for location (state), and t for year. The parameters in $\boldsymbol{\alpha_{ej}}$ control for cross-sectional differences across employer-job groups. The parameters in $\boldsymbol{\beta_{jt}}$ control for arbitrary job-year time trends, in $\boldsymbol{\gamma_{lt}}$ for arbitrary location-year time trends. The treatment variable $Agreement_{et}$ is a duration-weighted indicator for having at least one no-poaching agreement in force. For example, if a firm had 1 agreement in force for 4 of 12 months, $Agreement_{et} = \left(\frac{4}{12}\right)1 + \left(\frac{8}{12}\right)0$.[23] It follows that δ is the effect of having at least one no-poaching agreement in force for a full year. Intensive-margin variation in the number of agreements is not used, and one can interpret δ as the average of heterogeneous effects from different agreement counts.[24] Parameters are estimated using using the ordinary least squares procedure of Guimaraes and Portugal [2010], which performs well in the presence of high-dimensional fixed effects. Standard errors are clustered in two dimensions, general occupation and employer, except where otherwise noted. This allows for arbitrary covariances in the error term within occupation and employer, both cross-sectionally and over time.

The event study in Figure 2 provides a preliminary view of the treatment effect and allows for evaluation of identifying assumptions. This figure is constructed from a variant of equation (1), in which treatment is a firm-level ever-treated indicator interacted with year indicators, and the 2015 treatment-control difference is normalized to zero.[25] The treatment group is comprised of Adobe, Apple, eBay, Google, Intel, Intuit, Lucasfilm and Pixar. Table A6 lists the most frequently observed control-group firms, starting with Amazon, Microsoft,

[23] Details for each treated firm are in Appendix A. Duration weighting matters only for Intel, as all other colluding firms were party to at least one agreement by 2007, when my Glassdoor data begin. Sources in court records disagree over whether Intel's first no-poaching agreement began before or during 2007. Eliminating duration weighting and using an unweighted indicator–at least one agreement in any portion of the year–produces no meaningful change in my estimates (see Table A8, column five).

[24] Firm-level agreement counts range from 1 to 3, and the mean among colluding firms is very close to 2.

[25] In a more typical difference-in-differences event study where untreated observations of later-treated units precede treated observations, it is common to normalize relative to the last pre-treatment year. In my setting one could plausibly normalize relative to several different untreated post-treatment years and it is not clear how best to choose. Visual inspection of Figure 2, however, reveals that this choice is not consequential: normalizing relative to any year 2014-2018 would not meaningfully change the figure.

and Cisco. Well-known consumer-facing firms like Uber and Facebook are represented, as are more business-facing firms like Qualcomm and VMware. My data begin in 2007, by which time all colluding firms were party to at least one agreement, so there is no staggered entry into treatment.[26] The effect of the no-poaching agreements is visible in the left-hand region of Figure 2, where treatment-group salaries are below control-group salaries by approximately five percent. Estimates from 2007 through 2009 are statistically significant at the five percent level. The vertical line just after 2009 marks the end of the treatment period. DOJ documents indicate that the no-poaching agreements ended in 2009, but that at least some continued after the investigation was publicly revealed in June [US Department of Justice, 2012]. Therefore I assume that all agreements continued through the end of that year. Treatment-group salaries began to converge to control-group salaries after 2009, but estimates remain substantially negative in 2010 and 2011. By 2012 estimates are consistent with full convergence. As Figure 2 illustrates, my identification strategy relies not on the potentially endogenous introduction of no-poaching agreements, but rather on the plausibly exogenous DOJ investigation that ended them. Because neither entry into nor exit from treatment is staggered in my sample, the problems reviewed by de Chaisemartin and D'Haultfoeuille [2022] do not arise.

Figure 2 also allows indirect evaluation of the common trends assumption required for a difference-in-differences design to identify the causal effect of the no-poaching agreements. In the 2007-2009 period covered by the agreements estimates are roughly constant, consistent with common time trends for treated and control firms. In the post-treatment period 2012-2018 there is more variance in point estimates, but there is no evidence of different trends in the two groups.[27] Taken together, the event study results imply that the magnitudes of my estimates based on equation (1) are likely biased downward. My specification ignores the 2010-2011 transition, during which salaries at treatment-group firms may have been reduced by lingering effects of the no-poaching agreements. While this is undesirable, defining treatment based on observed salary dynamics would induce endogeneity.

The second important identifying assumption for my research design is the Stable Unit Treatment Value Assumption (SUTVA), or more colloquially the "no spillovers" assumption. Theory is ambiguous on the existence and sign of such spillovers in my setting. Qualitative evidence obtained by the DOJ in discovery shows that the no-poaching agreements reduced

[26] In Figure 2 Intel observations are coded as treated treated throughout the period 2007-2009.

[27] More specifically, the common trends assumption pertains to Y_{0i}, the potential outcome in an untreated state. The untreated period 2012-2018 is most directly informative about Y_{0i}, because so long as treatment effects have dissipated one observes Y_{0i} for both treatment and control firms. The estimates 2007-2009 suggest $Y_{1i}|Agreement = 1$ and $Y_{0i}|Agreement = 0$ move in parallel, but this is informative about common trends in Y_{0i} only under the additional assumption of a time-invariant treatment effect.

the number of firms potentially bidding for a given worker at a colluding firm, but did not change the number of firms potentially bidding for a given worker at a non-colluding firm [US Department of Justice, 2010b]. This does not suggest large spillovers. Section 4.2 conducts several empirical tests, including regressions limited to less concentrated labor markets and regressions in which control-group firms are in different output markets, and finds no evidence for substantial bias from spillovers.

Lastly Figure 2 speaks to the possibility of employer-driven misreporting. As discussed in Section 2.2, treated firms might have wished to artificially increase their Glassdoor salary means after 2009. I cannot exclude the possibility that the positive (but not statistically significant) point estimate for 2013 reflects such behavior. Misreporting in 2013 would have posed considerable risks to treated firms, however, as the class action against them had not yet been settled. More importantly, the 2013 treatment-control difference has relatively little influence on the pooled estimator of Equation 1 because of the long post-treatment period. Figure 2 shows that the difference between 2007-2009 salaries and 2014-2018 salaries is about five percent. Given my empirical strategy, only a campaign of misreporting sustained over the entire post-treatment period could introduce large bias. Employer campaigns documented by journalists have lasted just one to two months [Winkler and Fuller, 2019]. A prolonged campaign by treated firms would have spawned material legal peril, and would have been unlikely to evade Glassdoor's quality-control mechanisms.

4 Empirical results

4.1 Primary results & initial robustness

Table 1 presents estimated effects of full-year participation in the no-poaching agreements. Column one ("Primary") corresponds exactly to equation (1). This is my preferred specification because it employs rich cross-sectional and time-series controls while maintaining a large, plausibly representative sample. The estimated effect is approximately -4.8 percent. It is statistically significant at the one percent level, and the 95 percent confidence interval runs from -8.0 percent to -1.7 percent. The magnitude of this no-poaching effect is striking for a number of reasons. Affected employees were well educated and highly paid. In the estimation sample, thirty-one percent of workers have an advanced degree and the mean salary is $93,158 (2009 US$). One might expect such characteristics to make these workers less vulnerable than others to employer market power. For example, Naidu et al. [2018] comment, "Wage suppression...often affects low-income earners the most as they have the fewest options and least bargaining power," while describing computer programmers as able to switch jobs "with relative ease." In addition, many firms remained outside the agreements and one might expect this competitive fringe to mute or eliminate salary effects (see Section 5 for discussion).

The reports of expert witnesses from the class action against the colluding firms provide initial benchmarks for the estimates in Table 1. Said experts had access to administrative labor compensation data from defendant firms, but not from other firms, making construction of counterfactual compensation difficult. The research design employed in Leamer [2012] may be thought of as a single difference, comparing agreement periods to pre- and post-agreement periods after adjustment for sector-level growth. The resulting firm- and year-specific effects on total compensation range -1.6 to -20.1 percent [Leamer, 2012]. While this range is admittedly wide, it does include my primary estimate from Table 1. To the best of my knowledge the defendants' expert report, authored by Dr. Kevin M. Murphy, remains under seal at the request of the defense [Koh, 2013b]. No redacted version is available. However in certifying the plaintiff class Judge Lucy Koh quoted the Murphy report's conclusions: "'Defendants argue that, when Dr. Murphy disaggregated the Conduct Regression, he received dramatically different results. See id. at 12-13; Murphy Rep. ¶ 117 (finding that Lucasfilm and Pixar "show[ed] no 'undercompensation' but instead 'overcompensation'... throughout the period," Google, Adobe, and Intel showed overcompensation in some years, and Apple showed "much smaller" undercompensation)"' [Koh, 2013a]. My primary estimate is inconsistent with an average null effect or "overcompensation." To the best of my knowledge the DOJ did not produce its own damage estimates.

Previous academic research on employer market power has estimated effects with magnitudes broadly similar to that of my primary estimate in Table 1. Azar et al. [2020] find that a 10 percent increase in concentration (Herfindahl-Hirschman Index: HHI) is associated with a .3 to 1.3 percent decrease in wages, while Marinescu et al. [2021b] estimate a .5 percent causal decrease from a similar concentration change.[28] Benmelech et al. [2020] find that a one-standard-deviation increase in HHI is associated with a 1 to 2 percent decrease in wages, and that the relationship is stronger in more recent data. Balasubramanian et al. [2020] find that a Hawaii law banning noncompete and nonsolicitation clauses increased average labor earnings by 0 to 2.2 percent.[29] The smaller magnitude of these estimates, relative to Table 1, is potentially consistent with a positive compensating differential for a contractually

[28] Marinescu et al. [2021b] implement an instrumental variables design in French data.

[29] These results are not perfectly comparable to Table 1 because: 1) the same Hawaii statute that banned noncompetes also banned nonsolicitation clauses, which prohibit former employees of a firm from soliciting its clients; and 2) the Hawaiian labor market is geographically isolated. Balasubramanian et al. [2020] also use cross-sectional variation in state laws to estimate the additional effect of noncompetes on tech workers, relative to non-technology workers in the same state.

agreed noncompete clause.[30] Prager and Schmitt [2021] find that hospital mergers that led to large concentration increases reduced the wages of skilled workers by 4 to 6.8 percent. Finally Naidu et al. [2016] find that when migrant workers in the United Arab Emirates were allowed to change employers at the end of their initial contract, their earnings increased by 10 percent. The estimates in Table 1 are toward the higher end of the interval defined by recent empirical work and suggest considerable employer market power; Section 5 discusses the sources of such power.

The following approximate calculation estimates aggregate damages based on salary alone. The plaintiffs' expert report claims 109,048 members of the class and $52 billion in affected earnings [Leamer, 2012]. From column one of Table 1, the exact percentage change in salary is $e^{-.048} - 1 = -.047$, or 4.7 percent. Earnings in the absence of the agreements would then have been $\frac{\$52bn}{1-.047} = \$54.56bn$ and employee losses were approximately $\$2.56bn$, or $4,700 per employee-year.[31] This estimate should be viewed as a lower bound. It excludes not only non-salary compensation, but also additional job search costs incurred by affected workers. Even ignoring these omissions, my damage estimate is substantially greater than the $435 million the defendants paid to settle the case [Elder, 2015, Settlement Website, 2018].[32] This gap raises the question of whether civil penalties will meaningfully deter future exercise of employer market power.[33] Recall that the no-poaching agreements created not only civil, but criminal liability (see Section 1). Other recent, prominent, white-collar crimes provide alternative benchmarks for my damage estimates. The $\$2.56bn$ salary losses from the no-poaching agreements are smaller than estimates of losses from the Madoff investment fraud scheme, which range from $\$13.2bn$ [Lewis, 2010] to $\$17.5bn$ [Peterson-Withorn, 2021]. The difference in magnitudes is partly attributable to the longer 13-year duration of the Madoff scheme, relative to the roughly 5-year duration of the no-poaching agreements.[34] The salary damages in Silicon Valley are substantially greater than the estimated $\$93mn$ in ille-

[30] That is, contractual noncompetes may both strengthen employer market power and require a compensating differential, with the net effect of these two forces being negative. As described in Section 1, however, non-competes and no-poaching agreements differ on several dimensions and one cannot draw strong conclusions from a simple comparison of magnitudes.

[31] These calculations can instead be performed in levels, using an estimate from Table A8, column four. For the larger all-employee class [Leamer, 2012], damages are then (109,048 employees)(-$5925.5/yr)(5 years), approximately $3.23 billion in 2009 dollars. Alternatively one can assume that only technical and creative salaries were affected (59,550 employees). From the triple-difference regression of Table A9, the exact percentage effect of the agreements is $e^{-.049} - 1 = .048$. Earnings in the absence of the agreements would have been $\frac{\$33bn}{1-.048} = \$34.66bn$ and employee losses were roughly $\$1.66bn$.

[32] Apple, Google, Intel and Adobe settled together for $415 million in 2015. The other defendants settled for $20 million.

[33] This remains true even if one allows for considerable uncertainty in my estimate, non-settlement losses, and overstatement of affected earnings by the plaintiffs.

[34] For details on the timing and duration of the no-poaching agreements, see Section A.

gal profits generated by insider trading at the Galleon Group [U.S. Securities and Exchange Commission, 2012].

Theory predicts that earnings damages represent a transfer from labor to owners of other factors [Shy and Stenbacka, 2019]. An estimate of the attendant deadweight loss is beyond the scope of this paper.[35] Given the high mean salary among affected workers, one could argue that the welfare consequences of earnings lost to the no-poaching agreements are relatively small. For many technology workers this argument is unconvincing because high urban housing costs greatly reduce the real purchasing power of six-figure nominal salaries. For example, in June 2018 the US Department of Housing and Urban Development revised its eligibility threshold for low-income housing assistance to $117,400 for Marin, San Mateo, and San Francisco counties [Sciacca, 2018].

The remaining columns in Table 1 evaluate the robustness of the primary estimate. To test for selection into treatment on observables, column two presents estimates for the subsample in which I observe demographic variables. Controls are as in equation (1), with the addition of a female indicator, age, age squared, and a set of educational attainment indicators. The resulting estimate remains -4.8 percent. Column three presents a more saturated specification with job-employer-MSA and job-year-MSA fixed effects.[36] These controls rule out bias from time-varying selection at the job-MSA (job-location) level. The resulting estimate, -5.7 percent, is modestly larger than the estimate from my primary specification and statistically significant at the one percent level. Selection on time-varying unobservables, conditional on job-employer-MSA and job-year-MSA fixed effects, remains a potential threat to identification. As a check of this concern, column four estimates this highly saturated specification using the subsample of "give to get" reports (described in Section 2.1). Previous research has found that "give to get" mitigates selection of employees with highly positive or negative views of their jobs [Marinescu et al., 2021a]. If intense feelings about one's job are correlated with determinants of earnings or misreporting, then limiting the sample to "give to get" reports will reduce bias. This sample also reduces the probability of bias from employer-driven misreporting (see Sections 2.2 and 3). A user who visits Glassdoor and simply volunteers a report, for example because of employer pressure, never faces the "give to get" mechanism. In column four the resulting estimate is modestly larger, at -7.1 percent, and statistically significant at the one percent level, but I cannot reject a null hypothesis of equality with my primary estimate at any conventional size.

Table 2 examines non-salary compensation, including cash and stock bonuses. While

[35] Sources of deadweight loss potentially include excess unemployment (idle human capital), decreased worker-firm match quality (misemployment), impaired labor- or product-market function from lost trust, and additional monitoring by workers, firms, or governments.

[36] MSA stands for Metropolitan Statistical Area.

stock options are commonly used by information-technology firms [Oyer and Schaefer, 2005], the Glassdoor stock bonus variable does not distinguish option from share grants. Note again that Glassdoor does not require responses to bonus questions, and the sample is a potentially selected subset of the one from Table 1. I observe non-zero supplemental compensation for 51 percent of reports across all firms, while according to Leamer [2012] 93 percent of defendant employee-years included supplemental compensation.[37] The following results should therefore be interpreted with caution. For each compensation type I estimate a linear probability model using an indicator for positive compensation, a linear model with log compensation as the dependent variable, and a Poisson fixed-effects model that subsumes extensive and intensive margins in one equation [Correia et al., 2020].[38] Accordingly, estimates in column three are semi-elasticities. The probability of a positive stock bonus declines by 7.0 percentage points per no-poaching agreement. Conditional on a positive stock bonus, the amount declines by 30 percent (36 log points). The combined effect is an average decline in stock bonus of 48 percent.[39] The estimate from the linear probability model is statistically significant at the five percent level and the other two estimates are statistically significant at the one percent level. The estimate for cash bonuses is very small and positive in the linear probability model. Conditional on a positive cash bonus, the amount declines, but the estimate is not statistically significant. In the Poisson model the combined effect is positive, but the standard error is again large and the 95 percent confidence interval includes practically important values with both positive and negative signs. The pattern of results in Table 2 is consistent with employee retention as one of the motives for stock-option grants [Core and Guay, 2001, Oyer and Schaefer, 2005]. Retention may be desirable to reduce recruiting costs, or to avoid holdup problems with human capital investments (discussed in Section 5.2). A firm engaged in no-poaching agreements has less need to offer employees incentives to stay, but the firm's need to offer cash bonuses may be unchanged.

Last among my primary results, Figure 3 presents estimated effects on job satisfaction ratings from the difference-in-differences design of equation (1). Exact point estimates appear in Table A7. As one might expect, the largest negative estimate is for compensation and benefits: -.20 stars, or -5.7 percent of the sample mean, statistically significant at the one percent level.[40] In proportional terms the magnitude is similar to the salary effect from

[37]In my sample the fraction of defendant employee-years with positive supplemental compensation is also 51 percent.

[38]Observation counts in column 3 differ from those in column 1 because the PPML estimator drops observations perfectly predicted by the fixed effects.

[39]The event study corresponding to the estimated effect on stock bonuses is Figure A3 and robustness checks are in Table A11.

[40]The event study corresponding to the estimated effect on compensation ratings is Figure A4 and robustness checks are in Table A12.

column one of Table 1. This estimate is consistent with employees being aware their salaries were depressed relative to their own counterfactuals or reference points. Standard search models would predict increased search effort in response to such awareness [Chade et al., 2017]. If employees indeed increased search effort, that was an additional loss. In contrast to the salary loss, which was transferred to other factors of production, a loss from additional search would have been a social (deadweight) loss. Figure 3 also shows a negative effect on ratings of opportunities, -.10 stars (-3.0 percent of the mean), statistically significant at the five percent level. This could reflect both decreased internal opportunities, e.g. reduced promotion opportunities from senior employees leaving less frequently, and decreased external opportunities caused directly by the no-poaching agreements. The estimate for senior leadership is small (-.016 stars) and not statistically distinguishable from zero. This is consistent with most employees remaining ignorant of the no-poaching agreements; it is difficult to imagine that leadership ratings would not have suffered, had the agreements been widely known. Similarly, the estimate for work-life balance is somewhat small (-.068 stars) and not statistically distinguishable from zero. In light of the negative effects on ratings of opportunities and compensation, the small negative estimate for overall job rating is striking: -.034 stars, or .1 percent of the mean. There are several possible explanations for this contrast. The simplest is that salary losses from the no-poaching agreements had a small effect on job satisfaction for this sample of highly paid technology workers. It is also possible that colluding firms compensated workers for salary losses with increased non-pecuniary amenities [Clemens et al., 2018, Clemens and Strain, 2020, Clemens, 2021].[41] This hypothesis requires that the firm's optimal bundle of non-pecuniary amenities improves under no-poaching agreements, e.g. because the pool of competing bundles in the effective labor market has changed.[42] Finally, workers affected by the no-poaching agreements might have expected greater returns to job tenure than did control-group workers, perhaps because of anticipated investments in firm-specific human capital [Acemoglu and Pischke, 1999].[43]

4.2 Further robustness

This section supplements the robustness checks of Table 1 with several additional analyses. First and most important, Table 3 conducts empirical tests for spillovers (previously discussed in Section 3). For these tests I compute Herfindahl-Hirschman indices (HHI) using counts

[41] Relevant amenities in this setting might include, for example, more pleasant offices or free drinks on Friday afternoons.

[42] This hypothesis additionally requires that the amenity improvements are not captured by any dimension of the Glassdoor ratings, as none show increases.

[43] Future returns could be pecuniary or non-pecuniary. For additional discussion of firm-specific human capital in this context, see Section 5.2.

of Glassdoor reports at the major occupation-MSA level, treating all colluding firms as a single entity. Column one limits the sample to labor markets below the DOJ's threshold for high concentration (HHI<2500), where equilibrium salary spillovers are less likely.[44] The resulting estimate, -4.6 percent, is strongly similar to my primary estimate (-4.8 percent). Column two limits the sample to markets with HHI less than 2000, and the estimate is -4.8 percent. Column three forms a control group from all industries outside the primary sample–industries other than "Computer Hardware & Software," "Internet", and "Motion Picture Production & Distribution." Similarly column four forms a control group from selected industries, chosen *ad hoc* for their dissimilarity to treated-firm industries. Examples include "Health Care Services & Hospitals," "Department, Clothing, & Shoe Stores," and "Grocery Stores & Supermarkets."[45] In both column three and column four, the aim is to form a control group that is distant from the treated group in the output market. Insofar as jobs involve human capital specific to that output market, distance in the output market will imply distance in the labor market and reduced likelihood of spillovers. Both of the resulting estimates, -5.3 percent in column three and -4.4 percent in column four, are similar to my primary estimate. Finally, excluding workers who switch across treatment and control firms increases the magnitude of the estimate slightly to -4.9 percent, suggesting workforce composition effects do not introduce substantial bias in my specifications. Broadly the results in Table 3 are inconsistent with large spillovers.

Next I evaluate robustness to minor sample and specification changes (see Table A8 for full results). Adding user fixed effects, which rule out endogeneity from time-invariant individual characteristics, yields a larger estimate of -9.2 percent. This is not my preferred specification because it limits the sample to multiple reporters, who may be less representative than than single reporters. The larger magnitude, relative to my primary estimate, comes from the change in sample, not specification; dropping the user fixed effects yields a highly similar point estimate (-8.9 percent) in the multiple-reporter sample. Limiting the sample to reports with annual salaries gives an estimate of -4.6 percent, similar to my primary estimate. Using the level of salary as the dependent variable results in an estimate of -$5925.5. Returning to log salaries, constructing the treatment indicator without duration

[44]The correlation between HHI and labor markdowns is theoretically ambiguous in sign, but a large literature finds negative relationships between concentration and wages, consistent with greater employer market power under high concentration [Card, 2022].

[45]The full list of control industries in column four of Table 3 is as follows: "Health Care Services & Hospitals," "Department, Clothing, & Shoe Stores," "Colleges & Universities," "Banks & Credit Unions," "Investment Banking & Asset Management," "Consulting," "Fast-Food & Quick-Service Restaurants," "Advertising & Marketing," "Accounting," "Insurance Carriers," "Grocery Stores & Supermarkets," "Casual Restaurants," "Biotech & Pharmaceuticals," "General Merchandise & Superstores," "Aerospace & Defense," "Staffing & Outsourcing," "Consumer Products Manufacturing," "Hotels, Motels, & Resorts," "Real Estate," "Other Retail Stores," "Logistics & Supply Chain," "Food & Beverage Manufacturing," and "Industrial Manufacturing."

weighting produces no change from my primary estimate. Lastly, modeling treatment as a duration-weighted agreement count yields an estimate of -2.5 percent per full-year agreement. As the number of agreements was not randomly assigned to firms, this estimate should be interpreted cautiously, but it is consistent with larger salary impacts on workers covered by more agreements.

I also consider alternative definitions of the treated group. Qualitative evidence from the class action suggests the no-poaching agreements may have been enforced more vigorously for technical employees [Leamer, 2012]. This implies a triple-difference specification, using non-technical employees at colluding firms to help estimate counterfactual salaries. Table A9 presents estimates from such a specification. The marginal effect of a full-year no-poaching agreement on non-technical employees is -.1 percent and one cannot reject a zero null hypothesis at conventional test sizes. The additional effect on technical employees is -4.9 percent (statistically significant at the five percent level), consistent with stronger enforcement against this group. The triple-difference specification is not my preferred one because defining the group of technical employees requires either researcher discretion or reliance on the class action plaintiffs. Context implies another group of employees at colluding firms who may not have been treated: recruiters and interviewers, who had detailed knowledge of the no-poaching agreements. I estimate a variant of my primary double-difference specification in which the agreement indicator interacts with a recruiter indicator. The effect on non-recruiters is -4.5 percent, but the coefficient on the recruiter interaction is +5.4 percent, statistically significant at the five percent level.[46] This indicates recruiters were less affected than other employees by the no-poaching agreements. There are multiple potential mechanisms for such a difference, including increased job-search effort by recruiters and compensation for participation in an illegal scheme.

4.3 Mechanisms

How exactly did the no-poaching agreements reduce labor compensation at colluding firms, relative to other firms? All agreements prohibited parties from "cold calling" (recruiting) each other's employees. Many agreements required that if an employee of one party applied to another, the prospective new employer would inform the current one. Many also prohibited the prospective new employer from hiring such an applicant without permission of the current employer. In a search model, these types of provisions would reduce the arrival rate of job offers for incumbent employees. In the event of an offer, bidding wars were generally prohibited [US Department of Justice, 2010b, Saveri, 2011, US Department of Justice, 2012,

[46] In a test of the sum of these estimates, one cannot reject a zero null hypothesis at conventional sizes.

Ashenfelter et al., 2022]. In a search model, a no-bidding-wars provision would reduce the offer arrival rate and shift the distribution of wage offers downward for both incumbent and new employees.

Glassdoor data do not permit me to assess each dimension of the agreements in isolation. Analyzing heterogeneity by experience is modestly illuminating, but first an important caveat is required: the meaning of the Glassdoor experience variable is quite unclear. Users are confronted with the word "Experience" above a pull-down menu, from which they select a value from "Less than a year" and the integers 1 through 60. Whether users interpret this as a question about job tenure, occupational experience, or total work experience is uncertain. Table A13 reports results from a regression in which the indicator for having one or more agreements in force interacts with experience.[47] The estimate for workers with less than a year of experience is -2.9 percent and is not statistically significant ($p = .32$). Whether these workers were new to the job or the occupation, as new employees they should have been largely unaffected by prohibitions on cold-calling. Prohibitions on bidding wars, however, might have reduced salaries for new employees. More experienced employees are grouped into two-year bins to improve precision, and all remaining estimates in Table A13 are statistically significant at the one percent level. Magnitudes grow from -4.1 percent at one to two years of experience to roughly -5 percent at three through eight years, before falling slightly to -4.2 percent at nine or more years of experience. This heterogeneity cannot be attributed to any single cause. It is, however, consistent with reduced salary growth over the course of a job or career, potentially reflecting both fewer renegotiations and fewer job-to-job transitions.

5 Sources of employer market power in Silicon Valley

As discussed in Section 4.1, the salary results in Table 1 are toward the larger end of the range of magnitudes from recent empirical work on employer market power. This is particularly striking given that many technology firms remained outside the no-poaching agreements. While Glassdoor data do not allow for well-identified empirical tests of the sources of employer market power, some discussion may nonetheless be fruitful.[48] Broadly, large salary effects from no-poaching agreements, despite a competitive fringe, are consistent with "thin" labor markets in the sense of Manning [2003b]. This thinness may arise from search costs and job differentiation (distance in a characteristic space) [Manning, 2003a, 2021], which are discussed in Sections 5.1 and 5.2.

[47] An exhaustive set of non-interacted experience indicators is also included.

[48] The definitive treatment is Manning [2003a].

5.1 Search costs

The notion of substantial search costs, as in Burdett and Mortensen [1998], in Silicon Valley might initially sound fanciful. Large information-technology firms are regularly discussed in the media and may be said to bear household names. They often recruit labor through online platforms with millions of users. Indeed search and information frictions between large technology firms might be quite small with respect to the existence of a vacancy. Full information about an vacancy, however, requires knowing not only of its existence, but of its terms. Large firms may be internally heterogeneous, leading workers to speak of being in a particular group or team within the firm. Some groups are descended from small, formerly autonomous firms acquired by the large firm (e.g. Google acquired YouTube in 2006) and may have distinctive cultures. Full information about this heterogeneity is not part of public job postings.[49] To learn about important non-wage amenities [Sorkin, 2018], workers must engage in costly application and interview processes.

Search frictions between large and small firms may be considerably greater. Many small firms in Silicon Valley are startups, whose expected lifespan may be quite brief. Some startups operate for years in "stealth" mode, deliberately avoiding publicity [Villano, 2013]. Simply learning that a firm exists may be costly for a worker. This is not sort of industry discussed by Manning [2003b], who argues, "It is not hard to find employers: just look them up in the yellow pages." Learning about a vacancy at a small firm may be costlier still. Perhaps the most compelling evidence of information frictions, though, comes from the venture capital (VC) industry. Technology-focused VCs can consistently achieve high returns, consistent with private information about small firms [Kaplan and Stromberg, 2001, Hochberg et al., 2007]. Indeed the very existence of VCs arguably testifies to the difficulty of aggregating and evaluating information about small, recently born technology firms. Finally, all the same frictions that exist between large firms apply with respect to the terms of job offers.

5.2 Job differentiation

Workers' human capital may contribute substantially to segmentation of the labor market in information technology. For example, consider the case of software engineers. These workers may arrive at the firm with general-purpose human capital, e.g. programming skills in Python. Large firms often require workers to invest in more specialized human capital. Many Apple programmers work in Swift, while many Google programmers work in Go

[49] It is hard to describe culture in a job advertisement. Even if a hiring group were to try, applicants might not regard the description as credible.

[Weinberger, 2015]. (Section 5.3 studies the Apple-Google agreement separately from others.) As job tenure lengthens, the composition of a software engineer's human capital stock changes. General-purpose human capital depreciates, while firm- or even project-specific human capital increases. The change in composition reduces the value of a worker to small firms that rely on general-purpose human capital. This is particularly true if the small firms have little market power themselves, and thus little willingness to finance general-purpose training. Mobility across large firms is unaffected, provided the large firms pay for firm-specific training [Becker, 1965]. The larger estimates from the triple-difference specification of Table A9, in which high-skill technical workers are the treated group, are consistent with "job lock" from human capital.

Some types of job differentiation may be correlated with distance, both within and across urban areas. All firms participating in the no-poaching agreements were headquartered in the San Francisco Bay Area and maintained large workforces there, particularly in high-skill occupations. In contrast, four of the five most frequently observed control-group firms (see Table A6) were headquartered elsewhere: Amazon and Microsoft in the Seattle area, Qualcomm in San Diego, and Epic Systems near Madison, Wisconsin. The lone exception was Cisco, headquartered in San Jose, California. One should not overstate this point. The no-poaching agreements were not limited to the Bay Area, and there were many small information-technology firms competing in the Bay Area labor market during the time period studied by this paper. On the other hand, migration across US states has declined in recent decades [Molloy et al., 2011] and that suggests that a programming job in Madison, Wisconsin is not a perfect substitute for a programming job in Cupertino, California.

Not only were treated firms all headquartered in the Bay Area, their headquarters were near each other within the Bay Area. The Lucasfilm-Pixar agreement was something of a special case, not only because these two firms are in motion picture production, but geographically. Lucasfilm was then headquartered at Skywalker Ranch in relatively rural Marin County, more than 30 miles north of San Francisco and roughly 65 miles from most information technology firms in the Santa Clara Valley ("Silicon Valley"). At the time of the agreements Pixar was also in the northern Bay Area, specifically Point Richmond. Thanks to the Richmond Bridge, the two firms were just 19 miles apart by car, and they were far closer to each other than to plausible labor-market competitors. The other colluding firms all clustered in the Santa Clara Valley, and the average headquarters-to-headquarters driving distance in this group was only 9.4 miles (see Table A14). Insofar as intra- or inter-urban distances removed some firms or vacancies from the choice sets of Silicon Valley workers, they contributed to the labor market power of the colluding firms.

These physical distances may cause or be correlated with several labor-market frictions.

Distance may reduce information diffusion among workers, or between workers and firms [Keller, 2002, Belenzon and Schankerman, 2013, Rosenthal and Strange, 2020]. Across markets, social ties and moving costs plausibly reduce worker movement [Molloy et al., 2011]. This may be particularly true in my sample of technology workers because of housing markets in the cities where they live. Table A15 shows that the five most frequently observed metropolitan areas are San Jose, Seattle, San Francisco, New York, and Los Angeles. Housing markets in these cities generally exhibit inelastic supply, sometimes in tandem with distortions from rent control, e.g. in San Francisco [Diamond et al., 2019], and property tax policies, e.g. California's Proposition 13 [Ferreira, 2010]. While there are a few cities in Table A15, such as Detroit, with more permissive regulatory environments [Gyourko et al., 2021], the large majority are restrictive. This often leads to high prices and high search costs, which reduce worker mobility across cities.

Even within a broader area like a Census Core-based Statistical Area or Commuting Zone, physical distance may partition the labor market to some extent [Brueckner et al., 2002, Manning, 2003b, Marinescu and Rathelot, 2018]. High traffic congestion in areas like Seattle and the Bay Area, coupled with limited public-transit substitutes, implies that commuting costs may increase rapidly with distance [Manning and Petrongolo, 2017]. Large information-technology firms commonly offer bus service with internet access, important in an unpleasant urban commute, while small firms are unable to do so because they do not enjoy economies of scale in transportation. The same housing-market attributes mentioned previously make it difficult to move house within an area. High prices may require multiple full-time jobs within the household, creating coordination problems over moves [Naidu et al., 2018]. Because eligibility for a given public school typically is based on residence in its catchment area, moving more than a small number of miles may force children into a new school, with attendant adjustment costs and loss of social connection. Together these circumstances can make it quite costly for a technology worker to switch from a job in San Jose to one in San Francisco, or from Redmond to Seattle.

Other types of job differentiation may be related not to geography, but to firm size [Bhaskar et al., 2002]. Excepting Lucasfilm and Pixar, the colluding firms were large.[50] From the worker perspective, small firms in the competitive fringe are imperfect substitutes for large firms for a variety of reasons [Green et al., 1996]. Employment risk at small firms is plausibly greater [Winter-Ebmer, 2001]. For a risk averse worker, a given salary offer at a small firm is less appealing than the same salary at a large firm, holding non-pecuniary amenities fixed. Large firms may be highly productive or enjoy rents [Burdett and Mortensen,

[50] As of this writing in 2022, the lowest market capitalization among other colluding firms was eBay's $32bn, and the second lowest was Intuit's $130bn.

1998, Autor et al., 2020], e.g. from patents [Kline et al., 2019].[51] Diseconomies of scale in monitoring may cause larger firms to pay more from efficiency-wage motives [Boal and Ransom, 1997].

5.3 A summary measure of labor-market thinness

There are many ways to define closeness in the labor market, but Glassdoor has studied what is arguably a particularly informative one: user search behavior [Chen-Zion, 2015]. If a user looks at job listings for both firm A and firm B, but not for firm C, that may be considered a revealed-preference "vote" for A and B being close in the labor market, with C farther away.[52] While the graphs in Chen-Zion [2015] do not completely cover the setting of the no-poaching agreements, the graph of Amazon's revealed labor market in June 2015 is informative. Amazon and Microsoft, both control-group firms in the research design of Section 3, are each other's closest competitors.[53] The two Seattle-based firms compete far more with each other than with Google. Between treated firms Apple and Google the situation is quite different. For Apple, Google is the most important labor-market competitor. For Google, Apple is the second most important labor-market competitor, after Hewlett-Packard. This labor-market closeness is consistent with the large estimated salary decreases from no-poaching agreements (Table 1) and suggestive of why the Apple-Google agreement was originated and enforced. Table A16 presents results from a specification that yields separate no-poaching effects for Apple, Google, and other colluding firms. Estimates for Apple and Google are -5.6 and -11 percent, respectively, substantially larger in magnitude than the -2.4 percent average for others. This heterogeneity potentially reflects labor market power from multiple sources, including firm-specific human capital like skill in Swift or Go programming (see Section 5.2). The relatively large estimates of Table A16 are consistent with the evidence from Chen-Zion [2015] that Apple and Google are close labor-market competitors in the absence of a no-poaching agreement. The Google estimate (-11 percent) is very close in magnitude to the 10 percent pay increase Google announced after the DOJ investigation became public [Ashenfelter et al., 2022].[54] This comparison suggests that the estimates of Table 1 do not suffer from substantial bias, and fails to falsify the identifying assumptions required by the difference-in-differences approach of equation 1.

[51] Autor et al. [2020] list Apple and Google as examples of "superstar" firms.

[52] This assumes that looking at a listing is costly in terms of time and effort. Closeness in the market plausibly reflects both low job differentiation and low search costs.

[53] In Chen-Zion [2015], a thicker connecting line between two firms denotes greater closeness and competition in the labor market. That study draws on Glassdoor click-behavior data to which I do not currently have access.

[54] More formally, one cannot reject a null hypothesis of a -10 percent Google effect at any conventional size; the 95 percent confidence interval is $[-.135, -.085]$.

Broadly the descriptive patterns in Sections 5.1-5.3 are consistent with relatively stronger labor-market competition among colluding firms than between colluding and non-colluding firms. If such was indeed the case, that would predict large effects of no-poaching agreements on labor earnings at colluding firms (see Section 4.1), without large spillovers into the broader labor market (see Section 4.2).

6 Policy discussion & conclusion

Economists have long been interested in employer market power [Smith, 1790, Robinson, 1933, Reynolds, 1946a,b], but opportunities to study its clandestine, criminal use have been understandably rare. Using labor compensation data from Glassdoor, this paper estimates the effects of secret no-poaching agreements among Silicon Valley technology companies. Difference-in-differences regressions return negative, statistically significant estimates for both salaries and stock bonuses. They suggest the high market concentration in many US labor markets creates scope for increased use of oligopsony power, with potential negative impacts on workers and broad social welfare. In addition, these estimates are consistent with the possibility that low labor shares at "superstar" firms arise in part from oligopsony [Autor et al., 2020].[55]

My analysis lends weight to calls for greater research and policy scrutiny of employer market power and its sources, including mergers, mobility constraints, and information frictions [Krueger and Posner, 2018, Marinescu, 2018, Posner, 2021, US Department of the Treasury, 2022]. Rich data sources like Glassdoor have made it much easier to study the exercise of employer market power after the fact, but the problem of real-time detection remains. Antitrust authorities could combine market structure analyses like Chen-Zion [2015] with firm-to-firm job flows from tax data in order to identify potential anticompetitive restraints in the labor market. That is, if job seekers behave like firms A and B are close competitors but few workers ever move from one firm to the other, there might be a secret labor-market restraint in place. One might worry that an anticompetitive restraint could influence search by covered workers, and so alter revealed-preference measures of labor market closeness. While this cannot be ruled out, if a restraint is secret then online search behavior (as opposed to actual transitions) might be minimally affected. Looking at search behavior in entry-level

[55] Autor et al. [2020] discuss this possibility but argue against its importance. The authors regress average payroll per worker on concentration changes and find a "slightly positive" relationship. There are at least three objections to this exercise: 1) the ideal counterfactual wage is the perfectly competitive wage (marginal revenue product), which may increase with labor flows into superstar firms, rather than the previous average wage under lower concentration; 2) more generally, concentration changes are endogenous [Langella and Manning, 2021]; 3) as discussed in Section 5, concentration is not the only source of labor market power for large firms.

jobs would avoid this potential problem, though of course it is possible that firms compete differently in markets for entry-level and non-entry-level labor. While the absence of labor flows among competing firms does not necessarily imply antitrust violations, such patterns in the data could be used to efficiently target investigative resources like lawyer-hours and subpoenas.

Criminal antitrust cases are relatively rare, as are white-collar criminal cases against firms more generally [TRAC Reports, 2020]. Instead most white-collar cases focus on individuals involved in financial fraud, health care fraud, government procurement, and identity theft [TRAC Reports, 2021]. The magnitude of my damage estimate, roughly \$2.5 billion in lost salary alone, argues that labor-market antitrust violations are at least as economically consequential as other forms of white-collar crime. Reforming prosecutors' constraints and incentives to encourage prosecutions of firms for labor-market antitrust violations could benefit workers and yield large gains in social welfare.

Finally, both the DOJ and the class-action plaintiffs alleged that board interlocks facilitated the Silicon Valley no-poaching agreements: all colluding firms shared at least one board member with Apple [Saveri, 2011]. Prior work has found that board interlocks facilitate the diffusion of business practices [Davis, 1991]. While such interlocks have declined in the United States since 1997 [Chu and Davis, 2016], restricting them might make the future exercise of labor-market power by large employers more difficult. More broadly, social ties among management elites are plausibly important to the creation and sustenance of oligopsonistic agreements. Adam Smith observed, "To violate this combination [to reduce wages] is every where a most unpopular action, and a sort of reproach to a master among his neighbours and equals" [Smith, 1790]. The nature and use of social ties in exercising employer market power is a promising subject for future research.

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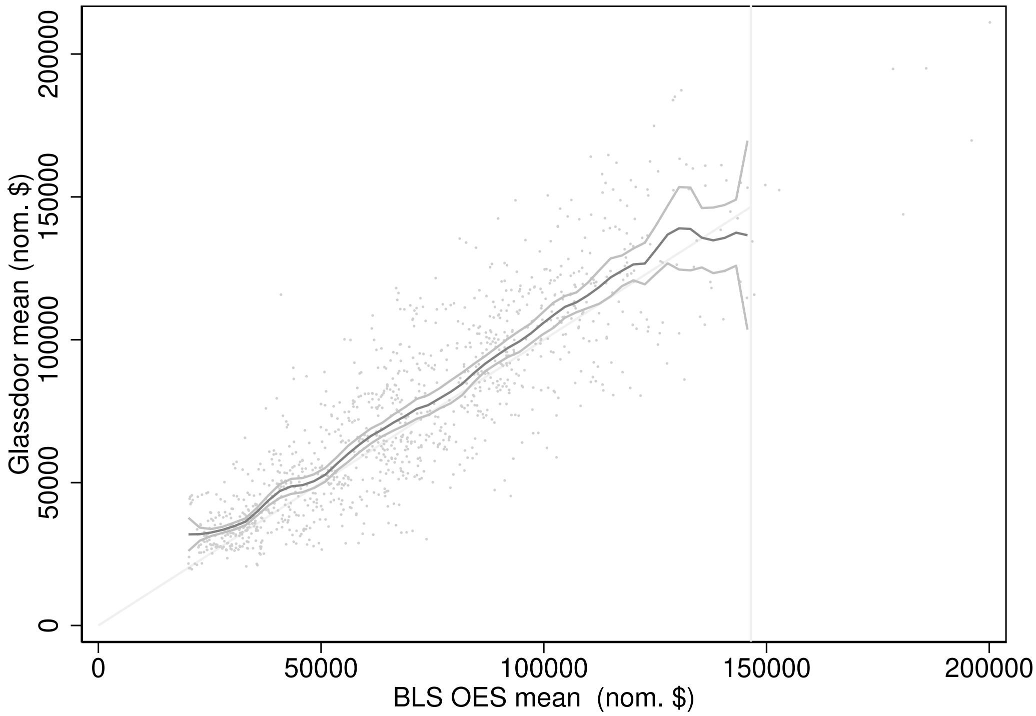
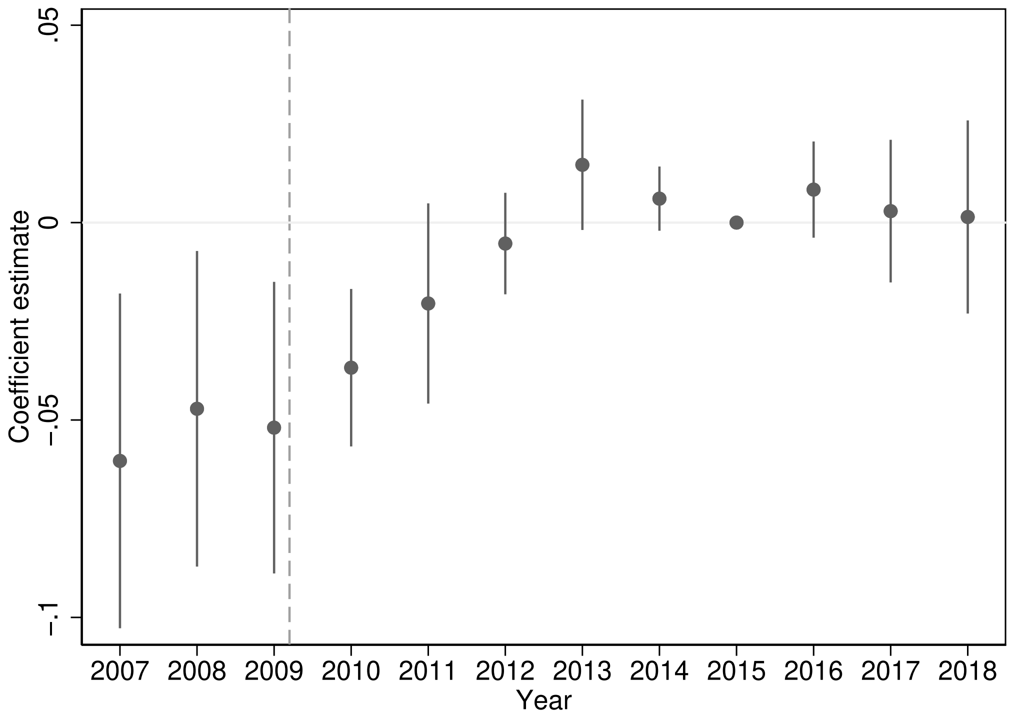
7 Figures

Figure 1: Average salary in Glassdoor and BLS OES data, 2007-2018



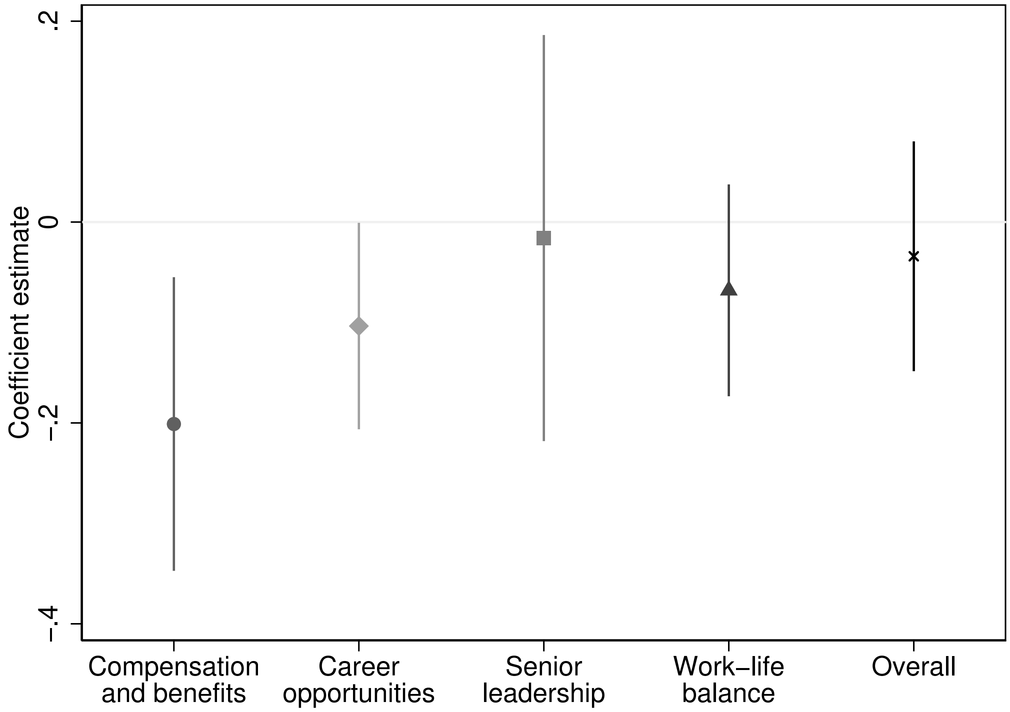
Each point on the scatter plot is an occupation-year (minimum ten Glassdoor reports), where occupations are defined by year-2010 SOC codes. Vertical coordinates are nominal mean salaries from Glassdoor data. Horizontal coordinates are nominal mean annual wages from BLS OES data. Included Glassdoor industries are "Computer Hardware & Software", "Internet", and "Motion Picture Production & Distribution." The dark gray fit through the scatter plot is from a local linear estimator, with an Epanechikov kernel and $3,000 bandwidth. The lighter gray lines around the local linear fit represent the 95% confidence interval. The sloped gray line is the function $y = x$. OES data are censored at high values, with thresholds from $145,600 to $208,000 depending on year. The vertical gray line represents the minimum censoring threshold, beyond which agreement of OES and Glassdoor data is much less likely.

Figure 2: Effect of no-poaching agreements on salary



Coefficient estimates are from a variant of equation (1), in which the duration-weighted no-poaching indicator is replaced by interactions of a firm-level ever-treated indicator with year indicators. The dependent variable is log real annual salary (2009 US$). Controls are job-employer, job-year, and state-year fixed effects. The vertical dashed line represents the end of the no-poaching agreements in response to the DOJ investigation. Standard errors are two-way clustered on general occupation and employer, and whiskers represent 95 percent confidence intervals.

Figure 3: Effect of no-poaching agreements on job reviews



Estimates correspond to variants of equation (1), with ratings of job satisfaction as dependent variables. Ratings range from one to five stars for compensation and benefits, career opportunities, senior leadership, work-life balance, and the job overall. Controls are job-employer, job-year, and state-year fixed effects. Standard errors are two-way clustered on general occupation and employer, and whiskers represent 95 percent confidence intervals. Table A7 presents the exact point estimates and standard errors corresponding to this figure.

8 Tables

Table 1: Effect of no-poaching agreements on salary

	Primary	Demographics	MSA FE	Give-to-get
Agreement in force	-0.048***	-0.048***	-0.057***	-0.071***
	(0.016)	(0.016)	(0.016)	(0.022)
Observations	249922	70249	176404	12844

Estimates in column one correspond to equation (1). The dependent variable is log real annual salary (2009 US$). Controls are job-employer, job-year, and state-year fixed effects. Subsequent columns present variants of this primary specification, always including the fixed effects previously mentioned. Column two adds demographic controls: a female indicator, age, age squared, and a set of educational attainment indicators. The sample is smaller because Glassdoor does not require users to disclose demographic information. Column three includes job-employer-MSA and job-year-MSA fixed effects (MSA FE). It uses the full Glassdoor sample from column one, but the reported observation count is reduced because the additional fixed effects create more singletons. Column four employs the same specification as column three, but restricts the sample to reports elicited by Glassdoor's "give to get" incentive. Standard errors are two-way clustered on general occupation and employer.

Table 2: Effect of no-poaching agreements on other labor compensation

	Stock bonus - LPM	ln(Stock bonus)	Stock bonus - PPML
Agreement in force	-0.070**	-0.36***	-0.48***
	(0.029)	(0.12)	(0.18)
Observations	249922	43775	128588
	Cash bonus - LPM	ln(Cash bonus)	Cash bonus - PPML
Agreement in force	0.014	-0.13	0.090
	(0.020)	(0.097)	(0.13)
Observations	249922	85482	191905

Estimates are from variants of equation (1). The dependent variable is an indicator for positive compensation of a given type in column one, log real compensation of a given type (2009 US$) in column two, and real compensation of a given type (2009 US$) in column three. Controls are job-employer, job-year, and state-year fixed effects. Column three employs the Poisson pseudo-maximum-likelihood estimator of Correia et al. [2020], and estimates are semi-elasticities. It uses the full Glassdoor sample from column one, but the reported observation count is reduced because the estimator drops separated observations [Correia et al., 2020]. Standard errors are two-way clustered on general occupation and employer.

Table 3: Effect of no-poaching agreements on salary, spillover checks

	HHI<2500	HHI<2000	Other industries	Selected industries	No switchers
Agreement in force	-0.046***	-0.048***	-0.053***	-0.044**	-0.049***
	(0.016)	(0.016)	(0.016)	(0.018)	(0.016)
Observations	143214	127440	2898341	1622029	247347

HHIs were computed for markets at the major occupation-MSA level, treating all colluding firms as a single decision-maker. Column one estimates equation (1) using only reports from markets with HHIs less than 2500, the DOJ threshold for high concentration. Column two uses only reports from markets with HHIs less than 2000. Column three forms a control group from all industries outside the primary sample–industries other than “Computer Hardware & Software,” “Internet,” and “Motion Picture Production & Distribution.” Column four forms a control group from selected industries, chosen for their dissimilarity to treated-firm industries: “Health Care Services & Hospitals,” “Department, Clothing, & Shoe Stores,” “Colleges & Universities,” “Banks & Credit Unions,” “Investment Banking & Asset Management,” “Consulting,” “Fast-Food & Quick-Service Restaurants,” “Advertising & Marketing,” “Accounting,” “Insurance Carriers,” “Grocery Stores & Supermarkets,” “Casual Restaurants,” “Biotech & Pharmaceuticals,” “General Merchandise & Superstores,” “Aerospace & Defense,” “Staffing & Outsourcing,” “Consumer Products Manufacturing,” “Hotels, Motels, & Resorts,” “Real Estate,” “Other Retail Stores,” “Logistics & Supply Chain,” “Food & Beverage Manufacturing,” and “Industrial Manufacturing." Column five excludes users observed at both treatment and control firms at any two points in time (“switchers”). The dependent variable is log real annual salary (2009 US$). All columns include job-employer, job-year, and state-year fixed effects. Standard errors are two-way clustered on general occupation and employer.

Appendix A Details of no-poaching agreements

According to the complaint in the civil class action, “Defendants’ conspiracy consisted of an interconnected web of. . . agreements, each with the active involvement and participation of a company under the control of Steven P. Jobs (“Steve Jobs”) and/or a company that shared at least one member of Apple’s board 16 of directors” [Saveri, 2011]. Agreements were not limited by geography or employee role [Leamer, 2012], but there is some evidence that they were enforced more rigorously in cases of highly educated, highly paid employees [Leamer, 2012, Koh, 2013a].

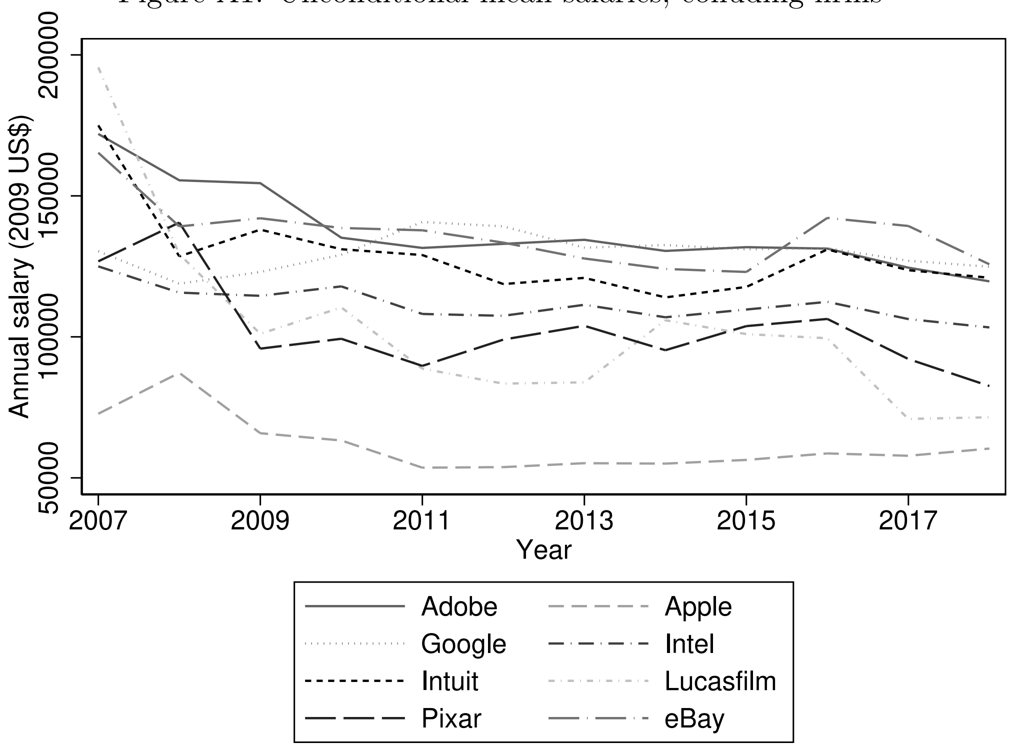
- Apple-Google. The agreement began no later than 2006 [US Department of Justice, 2010b]. The class action alleged that this agreement began in February 2005 [Leamer, 2012]. As my data begin in 2007, the difference is irrelevant to my analysis.
- Apple-Adobe. The agreement began no later than May 2005 [US Department of Justice, 2010b].
- Apple-Pixar. The agreement began no later than April 2007 [US Department of Justice, 2010a].
- eBay-Intuit. The agreement began no later than August 2006 and lasted until at least June 2009 [US Department of Justice, 2012].
- Google-Intel. The agreement began no later than September 2007 [US Department of Justice, 2010b]. The class action alleged that this agreement began in March 2005 [Leamer, 2012]. In Table 1, I conservatively adopt the DOJ start date of September 2007.
- Google-Intuit. The agreement began no later than June 2007 [US Department of Justice, 2010a].
- Lucasfilm-Pixar. The agreement began no later than January 2005 [US Department of Justice, 2010c]. The class action alleged that this agreement began before the year 2000 [Leamer, 2012]. As my data begin in 2007, the difference is irrelevant to my analysis.

Appendix B Litigation timeline

- March 2009. DOJ sends civil investigative demands to technology firms.
- June 3, 2009. DOJ antitrust investigation becomes public [Helft, 2009].
- Sept. 24, 2010. Complaint filed in US v. Adobe [US Department of Justice, 2010b].
- Dec. 21, 2010. Complaint filed in US v. Lucasfilm [US Department of Justice, 2010c].
- March 18, 2011. Final judgment in US v. Adobe.
- May 4, 2011. Civil class action *In re: High-Tech Employee Antitrust Litigation* filed.
- Nov. 6, 2012. Complaint filed in US v. eBay [US Department of Justice, 2012].
- September 2, 2015. Remaining defendants Apple, Google, Intel and Adobe settle class action.

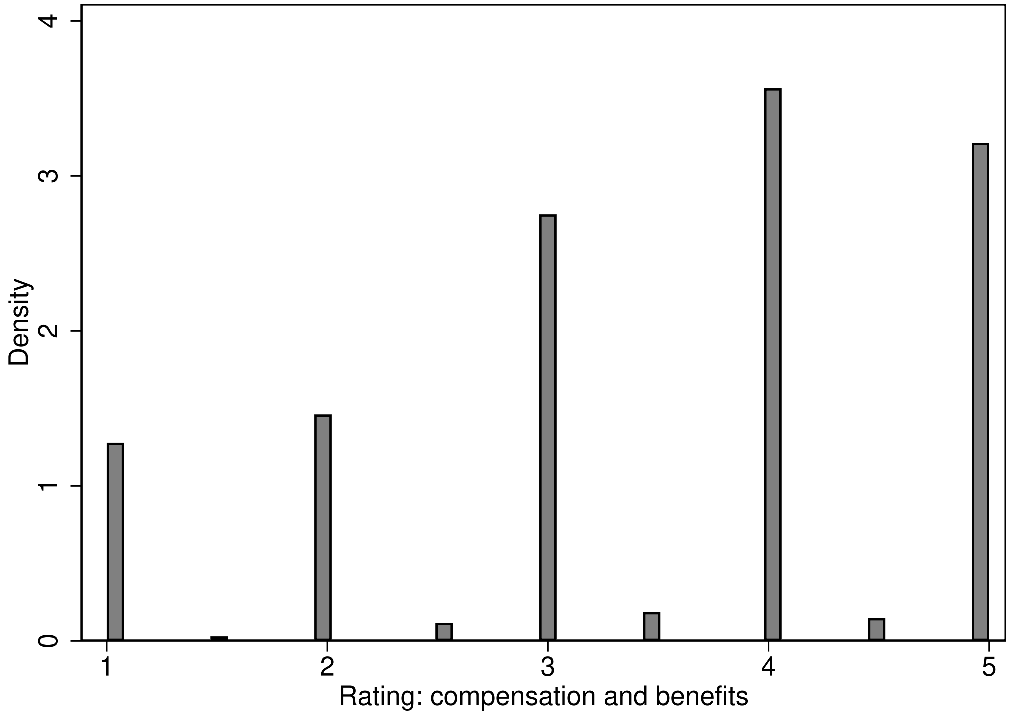
Appendix C Additional figures

Figure A1: Unconditional mean salaries, colluding firms



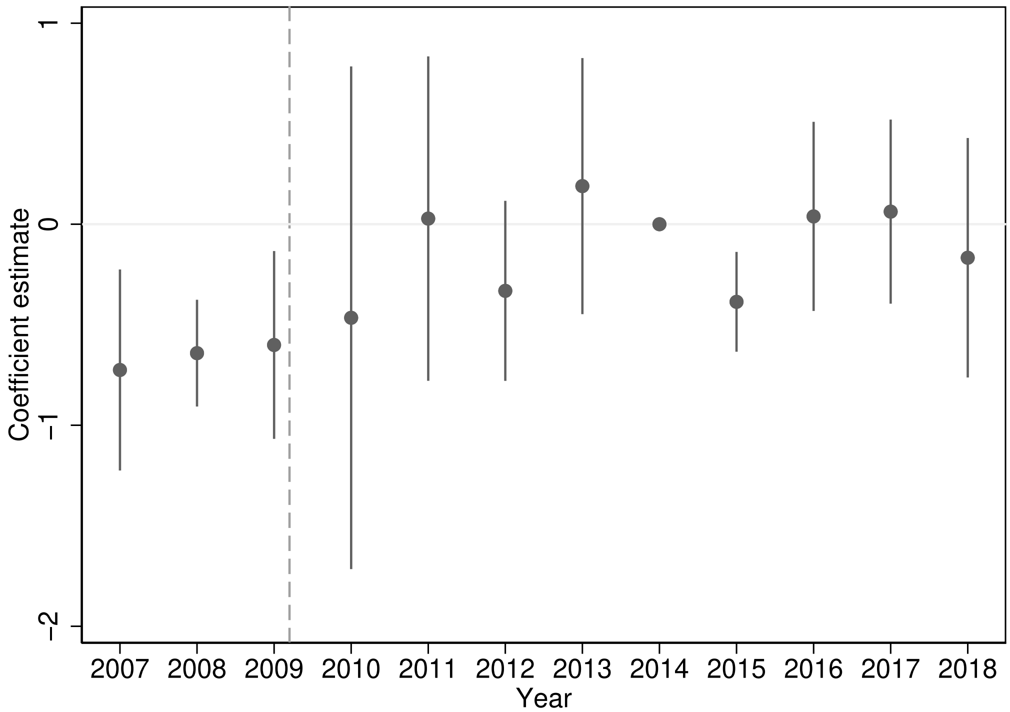
Plotted lines are mean real annual salaries (2009 US$) for firms party to at least one no-poaching agreement. Mean salaries at Apple are lower due to the presence of retail employees, e.g. clerks working in Apple stores. The higher variance for Lucasfilm and Pixar stems, in part or entirely, from smaller sample sizes at these two firms.

Figure A2: Rating frequencies, compensation & benefits



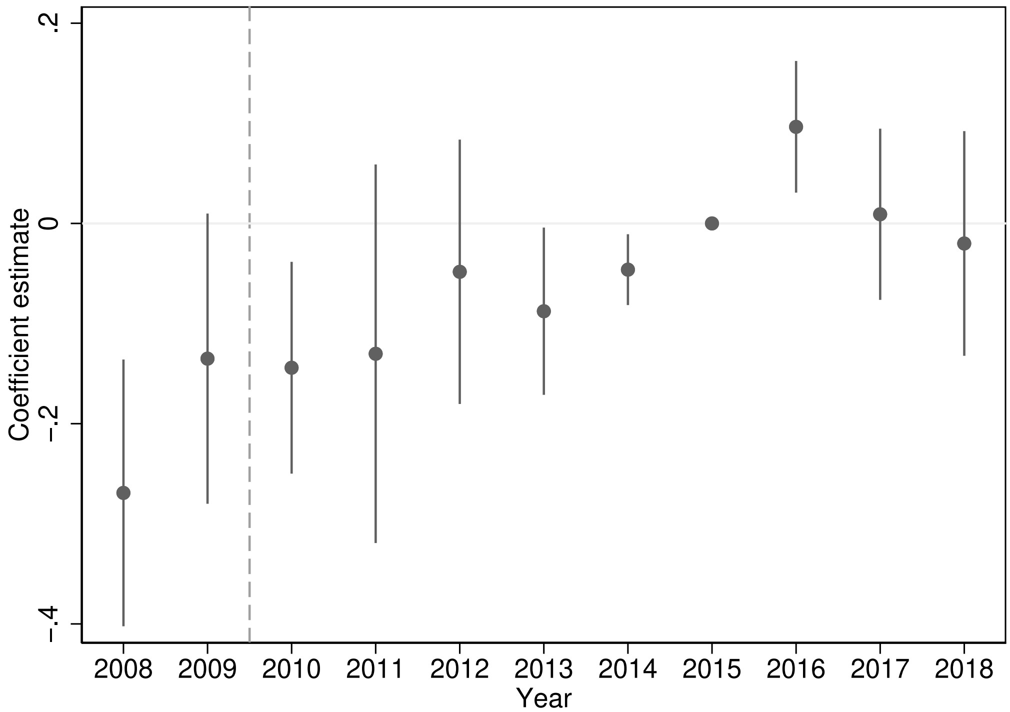
Illustrated are frequencies of star ratings for compensation and benefits. Data cover 2008-2018. Half-star ratings were permitted 2008-2012.

Figure A3: Event study, stock bonuses



Estimates are from a variant of equation (1), in which the number of no-poaching agreements is replaced by interactions of a firm-level ever-treated indicator with year indicators, estimated using the Poisson pseudo-maximum-likelihood estimator of Correia et al. [2020]. Estimates are semi-elasticities. The dependent variable is stock bonuses (2009 US$). Controls are job-employer, job-year, and state-year fixed effects. The vertical dashed line represents the end of the no-poaching agreements. Standard errors are two-way clustered on general occupation and employer, and whiskers represent 95 percent confidence intervals.

Figure A4: Event study, compensation & benefits ratings



Coefficient estimates are from a variant of equation (1), in which the number of no-poaching agreements is replaced by interactions of a firm-level ever-treated indicator with year indicators. The dependent variable is a rating of compensation and benefits from one to five stars. Controls are job-employer, job-year, and state-year fixed effects. The vertical dashed line represents the end of the no-poaching agreements. Note that Glassdoor ratings are not available prior to 2008. Standard errors are two-way clustered on general occupation and employer, and whiskers represent 95 percent confidence intervals.

Appendix D Additional tables

Table A1: Descriptive statistics, salary reports

	Mean	Std. dev.	Min	Max	Count
Base pay	93157.77	47766.49	13420.76	977254.12	259926
Cash bonus	20139.10	283409.89	0.00	36778968.00	259926
Stock bonus	16201.44	351455.61	0.00	47812660.00	259926
Female	0.29	0.45	0.00	1.00	174504
Age	32.73	8.55	16.00	70.00	98982
High school	0.06	0.23	0.00	1.00	96426
Some college	0.02	0.13	0.00	1.00	96426
College	0.63	0.48	0.00	1.00	96426
Graduate degree	0.30	0.46	0.00	1.00	96426

All forms of compensation in 2009 US$. Observation counts are weakly greater than in regression tables because the reghdfe command excludes singletons (observations perfectly predicted by the fixed effects). For example, the total observation count for base pay ($n = 259926$) is greater than the regression sample in column 1 of Table 1 ($n = 249922$).

Table A2: Descriptive statistics, salary reports, by group 2015-2018

	Control		Treatment	
	Mean	Std. dev.	Mean	Std. dev.
Base pay	98787.34	46734.46	98014.85	54374.81
Cash bonus	29244.02	361872.90	35108.10	364576.29
Stock bonus	19816.29	410461.84	43211.80	685853.97
Female	0.26	0.44	0.24	0.42
Age	33.40	8.56	31.92	8.39
High school	0.05	0.22	0.07	0.25
Some college	0.02	0.14	0.03	0.16
College	0.59	0.49	0.55	0.50
Graduate degree	0.34	0.47	0.35	0.48

All forms of compensation in 2009 US$. Treatment-control differences are not formally evaluated because the identification strategy of equation (1) allows for level differences in outcomes. For a discussion of identifying assumptions, see Section 3.

Table A3: Descriptive statistics, ratings of job satisfaction

	Mean	Std. dev.	Min	Max	Count
Overall	3.44	1.37	1.00	5.00	133337
Opportunities	3.30	1.39	1.00	5.00	133337
Compensation	3.49	1.27	1.00	5.00	133337
Leadership	3.07	1.47	1.00	5.00	133337
Work-life	3.45	1.36	1.00	5.00	133337

Job ratings data begin in 2008 and represent a subset of the users in the salary data. The observation count is slightly greater than in Table A7 because the reghdfe command excludes singletons (observations perfectly predicted by the fixed effects).

Table A4: Descriptive statistics, ratings of job satisfaction, by group 2015-2018

	Control		Treatment	
	Mean	Std. dev.	Mean	Std. dev.
Overall	3.44	1.42	3.95	1.11
Opportunities	3.32	1.45	3.59	1.23
Compensation	3.44	1.33	4.14	0.99
Leadership	3.09	1.53	3.43	1.31
Work-life	3.44	1.41	3.63	1.28

Job ratings data begin in 2008 and represent a subset of the users in the salary data. Treatment-control differences are not formally evaluated because the identification strategy of equation (1) allows for level differences in outcomes. For a discussion of identifying assumptions, see Section 3.

Table A5: Top 10 jobs in Glassdoor sample, by classification scheme

General occupation	Specific occupation	Job
software engineer	software engineer	software engineer
branch manager	manager	senior software engineer
engineer	software development engineer	account executive
account executive	account executive	account manager
product manager	program manager	project manager
program manager	product manager	director
sales representative	account manager	software development engineer
project manager	project manager	product manager
marketing manager	engineer	software developer
corporate account manager	software developer	program manager

Table A6: Most frequently observed control-group firms

	Count
Amazon.com, Inc.	19886
Microsoft Corporation	19053
Cisco Systems, Inc.	9699
Qualcomm Incorporated	4376
Epic Systems Corporation	4055
Cerner Corporation	3774
Tata Consultancy Services Limited	3275
Yahoo! Inc.	3189
Salesforce	2796
Honeywell International Inc.	2750
VMware, Inc.	2521
Yelp Inc.	2374
The Walt Disney Company	2175
Uber	2071
Facebook, Inc.	2028
Bloomberg L.P.	1996
Symantec Corporation	1989
SAP Aktiengesellschaft	1825
PayPal, Inc.	1612
Groupon, Inc.	1574
Expedia, Inc.	1549
CA Technologies, Inc.	1404
Citrix Systems, Inc.	1332
LinkedIn Corporation	1327
Advanced Micro Devices, Inc.	1249
Viacom Inc.	1099
HCL Technologies Ltd.	1094
NVIDIA Corporation	1065
NCR Corporation	1049
Total	104186

Above are report counts for control-group firms with more than 1000 reports. Because of this arbitrary truncation, the total observation count does not correspond to any other observation count in the paper. Two of the less familiar names, Epic and Cerner, are in health care IT. CA Technologies was formerly called Computer Associates. HCL Technologies is a large IT firm based in India.

Table A7: Effect of no-poaching agreements on job ratings

	Compensation	Opportunities	Leadership	Work-life	Overall
Agreement in force	-0.20***	-0.10**	-0.016	-0.068	-0.034
	(0.074)	(0.052)	(0.10)	(0.054)	(0.058)
Observations	133332	133332	133332	133332	133332

Estimates correspond to variants of equation (1), with ratings of job satisfaction as dependent variables. Ratings range from one to five stars for compensation and benefits, career opportunities, senior leadership, work-life balance, and the job overall. Controls are job-employer, job-year, and state-year fixed effects. Standard errors are two-way clustered on general occupation and employer. These estimates correspond exactly with Figure 3.

Table A8: Effect of no-poaching agreements on salary, further robustness

	User FE	User FE sample	Annual only	Salary (level)	ln(Salary)	ln(Salary)
Agreement in force	-0.092***	-0.089***	-0.046***	-5925.5***		
	(0.0072)	(0.023)	(0.017)	(2172.0)		
Unweighted indicator					-0.048***	
					(0.016)	
Num. agreements						-0.025***
						(0.0065)
Observations	6867	6867	215757	249922	249922	249922

Estimates are from variants of equation (1). Controls are job-employer, job-year, and state-year fixed effects. Column one adds user fixed effects (User FE). The sample is comprised of users who submit two or more Glassdoor salary reports. Column two (User FE sample) is estimated from the same sample, but does not include user fixed effects; controls are exactly as in equation (1). Column three limits the sample to users reporting an annual salary. Column four expresses salary in dollars, instead of using the log transformation. Column five models treatment as an indicator, but without the duration weighting of Table 1. Lastly column six models treatment as a duration-weighted count of no-poaching agreements. Standard errors are two-way clustered on specific occupation and employer in columns one and two to obtain a sufficient number of clusters in the occupation dimension. In all other columns standard errors are two-way clustered on general occupation and employer.

Table A9: Effect of no-poaching agreements on salary, triple-difference specification

	ln(Salary)
Agreement in force	-0.010
	(0.016)
Agreement in force*technical class	-0.049**
	(0.023)
Observations	249856

This table modifies equation (1) by adding a third dimension of difference: technical vs. non-technical employees. The dependent variable is log real annual salary (2009 US$). Controls are job-employer, job-year, and technical class-state-year fixed effects. The sample is slightly smaller than in Table 1, column one, because the triple-difference regression leads to more singletons. Standard errors are two-way clustered on general occupation and employer.

Table A10: Effect of no-poaching agreements on salary, interacted with recruiter indicator

	ln(Salary)
Agreement in force	-0.045**
	(0.018)
Recruiter=1*Agreement in force	0.054**
	(0.023)
Observations	249749

Estimates correspond to equation (1), with the addition of an interaction between the number of agreements and a recruiter/interviewer indicator. Dependent variable is log real annual salary (2009 US$). Controls are job-employer, job-year, and state-year fixed effects. The sample is slightly smaller than in Table 1, column one, because the triple-difference regression leads to more singletons. Standard errors are two-way clustered on general occupation and employer.

Table A11: Effect of no-poaching agreements on stock bonuses, robustness checks

	Primary	Demographics	MSA FE	Give-to-get
Agreement in force	-0.48***	-0.74*	-0.72***	-1.27***
	(0.18)	(0.38)	(0.25)	(0.43)
Observations	128590	35658	83565	6712

Estimates are from variants of equation (1) based on the Poisson pseudo-maximum-likelihood estimator of Correia et al. [2020], which discards observations for which the outcome is perfectly predicted by the fixed effects. Estimates are semi-elasticities. The dependent variable is stock bonuses (2009 US$). Controls are job-employer, job-year, and state-year fixed effects. Subsequent columns present variants of this primary specification, always including the fixed effects previously mentioned. Column two adds demographic controls: a female indicator, age, age squared, and a set of educational attainment indicators. The sample is smaller because Glassdoor does not require users to disclose demographic information. Column three includes job-employer-MSA and job-year-MSA fixed effects (MSA FE). It uses the full Glassdoor sample from column one, but the reported observation count is reduced because the additional fixed effects create more singletons. Column four employs the same specification as column three, but restricts the sample to reports elicited by Glassdoor's "give to get" incentive. Standard errors are two-way clustered on general occupation and employer.

Table A12: Effect of no-poaching agreements on compensation ratings, robustness checks

	Primary	Demographics	MSA FE
Agreement in force	-0.20***	-0.16	-0.26**
	(0.074)	(0.14)	(0.10)
Observations	133332	36624	81821

Estimates correspond to variants of equation (1) with ratings of compensation & benefits as the dependent variable. Ratings range from one to five stars. Controls are job-employer, job-year, and state-year fixed effects. Subsequent columns present variants of this primary specification, always including the fixed effects previously mentioned. Column two adds demographic controls: a female indicator, age, age squared, and a set of educational attainment indicators. The sample is smaller because Glassdoor does not require users to disclose demographic information. Column three employs job-employer-MSA and job-year-MSA fixed effects (MSA FE). It uses the full Glassdoor sample from column one, but the reported observation count is reduced because the additional fixed effects create more singletons. Glassdoor's "give to get" incentive does not apply to ratings, so there is no specification analogous to column four of Table 1. Standard errors are two-way clustered on general occupation and employer.

Table A13: Effect of no-poaching agreements on salary, by experience

	Primary
<1 yr	-0.029
	(0.029)
1-2 yrs	-0.041***
	(0.015)
3-4 yrs	-0.050***
	(0.015)
5-6 yrs	-0.050**
	(0.019)
7-8 yrs	-0.052***
	(0.016)
9+ yrs	-0.042***
	(0.013)
Observations	249913

Estimates in column one correspond to a variant of equation (1) in which the treatment indicator interacts with a set of binned experience indicators. A full set of non-interacted experience indicators is also included. The dependent variable is log real annual salary (2009 US$). Controls are job-employer, job-year, and state-year fixed effects. Standard errors are two-way clustered on general occupation and employer. The observation count differs slightly from Table 1 because the additional experience variables create more singletons.

Table A14: Headquarters-to-headquarters distance (miles), treated firms

To / From	Adobe	Apple	eBay	Google	Intel	Intuit	Lucasfilm	Pixar
Adobe								
Apple	9.2							
eBay	3.7	6.7						
Google	13.7	10.4	16.3					
Intel	6.3	6.1	8.8	9.0				
Intuit	14.1	11.2	16.7	.6	8.8			
Lucasfilm								
Pixar							19.0	

Driving distances in miles obtained from Google Maps, March 7, 2022. Routes were requested for a headquarters-to-headquarters journey beginning at 11PM in order to minimize the influence of traffic congestion. Multiple routes were offered and the minimum distance was recorded. At the time of the no-poaching agreements, Lucasfilm was headquartered at Skywalker Ranch in Marin County and Pixar at Point Richmond. These two firms are treated as a separate group because: 1) they were the only two colluding firms in the northern Bay Area; and 2) they were the only two firms in motion picture production. Among remaining firms, the average headquarters-to-headquarters driving distance was 9.4 miles.

Table A15: Most frequently observed metropolitan areas

	Count
San Jose, CA	36182
Seattle, WA	32492
San Francisco, CA	22349
New York City, NY	19684
Los Angeles, CA	13546
Boston, MA	10124
Chicago, IL	9833
San Diego, CA	6487
Austin, TX	6317
Washington, DC	6241
Phoenix, AZ	6017
Dallas-Fort Worth, TX	5822
Atlanta, GA	5554
Portland, OR	5134
Kansas City, MO	4780
Madison, WI	4368
Raleigh-Durham, NC	4261
Minneapolis-St. Paul, MN	3312
Denver, CO	3076
Houston, TX	2618
Philadelphia, PA	2458
Provo, UT	2104
Miami-Fort Lauderdale, FL	2034
Orlando, FL	1967
Sacramento, CA	1699
Salt Lake City, UT	1631
Indianapolis, IN	1595
Detroit, MI	1303
Tampa, FL	1261
Baltimore, MD	1175
Riverside, CA	1163
Charlotte, NC	1080
Cincinnati, OH	1035
Boulder, CO	1010
Charleston, SC	1008
Total	230720

Above are report counts for metropolitan statistical areas (MSAs) with more than 1000 reports. Because of this arbitrary truncation, the total observation count does not correspond to any other observation count in the paper.

Table A16: Effect of no-poaching agreements on salary; Apple, Google & others

	Primary
Apple	-0.056***
	(0.015)
Google	-0.11***
	(0.013)
Others	-0.024**
	(0.010)
Observations	249922

Estimates in column one correspond to a variant of equation (1) in which the treatment indicator interacts with a set of indicators identifying Apple, Google, and other colluding firms. The dependent variable is log real annual salary (2009 US$). Controls are job-employer, job-year, and state-year fixed effects. Standard errors are two-way clustered on general occupation and employer.