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## Employer market power in Silicon Valley

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## Employer Market Power in Silicon Valley

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## Employer Market Power in Silicon Valley

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### ABSTRACT

Adam Smith alleged that employers often secretly combine 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 U.S. Department of Justice investigation, I estimate the effects of these agreements using a difference-in-difference 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 5.6 percent, consistent with considerable employer market power. Stock bonuses and job satisfaction were also negatively affected.

**JEL Classification Codes:** J42, K42, L41, K21

**Key Words:** No-poach agreement, employer market power, Silicon Valley, tech companies, Glassdoor, compensation

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# 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 each other’s employees. Managers informed recruiters which potential hires were off-limits and some human resources departments maintained written lists. Some agreements included additional anticompetitive restraints, such as prohibition of bidding wars. Enforcement was 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, 2010c, 2012). This use of market power was remarkably simple and cheap, relying on well-defined commitments from a small number of individuals. No elaborate salary schedules were required. 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.

The Silicon Valley no-poaching scheme is noteworthy in several additional respects. Colluding firms paid high average salaries, and high-wage firms commit fewer violations of labor rights (Marinescu et al., 2021).<sup>1</sup> Colluding firms were large, and large firms may self-enforce regulations more stringently than small (Pomeranz, 2015), in part because of greater whistleblowing risk (Kleven et al., 2016). Moreover many competitors remained outside the agreements, including large firms like Amazon and Microsoft. This competitive fringe could

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<sup>1</sup>The mean salary at a colluding firm outside the collusive period was \$98,015 (2009 US\$; Table A5).

have reduced the returns to labor-market coordination.

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, 2010c). This was followed by a civil class action in 2011, with settlements in 2015 and 2018.<sup>2</sup> 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. 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). To date the DOJ has filed at least five indictments in accordance with this guidance (Reichel, 2023).<sup>3</sup> These cases attest not only to the criminality of no-poaching agreements among firms, but also to their existence in multiple large US sectors, including health care and aerospace.<sup>4</sup> European antitrust authorities have also discovered illegal no-poaching agreements, for example in Portuguese football and Lithuanian real estate (Rodrigues et al., 2024).

Using difference-in-differences designs, I estimate the effect of the Silicon Valley 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 5.6 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 reviews.

These results are important because the information technology sector is a large and

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<sup>2</sup>Appendix A presents a more detailed timeline.

<sup>3</sup>The cases in question were: *United States v. DaVita Inc.*, *United States v. Hee & VDA OC LLC*, *United States v. Manahe*, *United States v. Patel*, and *United States v. Surgical Care Affiliates, LLC*.

<sup>4</sup>The DOJ has also brought a civil no-poaching case in the railroad equipment industry and filed statements of interest in private civil suits involving medical school faculty and fast food workers (US Department of Justice, 2019).

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).<sup>5</sup> Today the colluding firms comprise approximately 14 percent of the market capitalization of the S&P 500 index.<sup>6</sup> 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 Silicon Valley no-poaching agreements (US Department of Justice, 2012).<sup>7</sup> From 1997 to 2012, the revenue share of the 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 power in the labor market (US CEA, 2016).

This paper contributes to the empirical literature on employer market power.<sup>8</sup> 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.<sup>9</sup> 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. Rather than studying markdowns directly, a second group of empirical papers diagnoses employer market power using labor supply elasticities.<sup>10</sup> Azar et al. (2022) 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. The empirical literature on no-poaching agreements is relatively younger and smaller. Krueger and Ashenfelter (2017) study the prevalence of no-poaching clauses in franchise employment contracts, but do not evaluate their consequences. In recent working papers, Callaci et al. (2023) evaluate the wage and salary effects of explicit franchise no-poaching clauses across a variety of US industries, from

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<sup>5</sup>Value-added figures are for “Information-communications-technology-producing industries.”

<sup>6</sup>As of mid 2022, all colluding firms except Lucasfilm and Pixar were part of the S&P 500 Index. Total market capitalization of the index was \$31.9tn. Market capitalizations for colluding firms were: Adobe \$175.83bn, Apple \$2.38tn, eBay \$24.33bn, Google \$1.52tn, Intel \$152.74bn, and Intuit \$109.34bn.

<sup>7</sup>Smith (1790) commented in similar spirit, “The masters, being fewer in number, can combine much more easily [than the workmen].”

<sup>8</sup>For surveys see Boal and Ransom (1997), Bhaskar et al. (2002), Ashenfelter et al. (2010), Manning (2011), and Manning (2021).

<sup>9</sup>Nonsolicitation clauses prohibit former employees of a firm from soliciting its clients.

<sup>10</sup>Another set of papers infers labor supply elasticities from recruitment and separation elasticities (Manning, 2021).

tax preparation to automotive maintenance, while Lafontaine et al. (2023) conduct a similar evaluation for franchise restaurants.

Relative to the existing literature, this paper differs along several dimensions. First, to the best of my knowledge, it is the first empirical work on the labor market effects of no-poaching agreements.<sup>11</sup> Such agreements may affect workers even in jurisdictions where non-compete 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, 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, this study is related to the literature on market concentration and workers' outside options. In recent work Caldwell and Danieli (2024) derive a novel outside options index, which may be thought of as a worker-specific concentration index, and estimate its effect on pay using a shift-share instrument. Similarly Schubert et al. (2024) construct an outside-options index at the location-occupation level and use variation from the national hiring patterns of large firms to identify effects on pay. Dodini et al. (2023) employ a measure of labor-market concentration in markets defined by tasks. Other recent work on concentration and pay includes: Brooks et al. (2021), Azar et al. (2022), Berger et al. (2022), Felix (2022), and Dodini et al. (2023).<sup>12</sup> In the context of this broader literature, my paper contributes by studying a natural experiment involving concealed variation in outside options. Prior work has not achieved consensus on whether highly educated technology workers are likely to be vulnerable to employer market power (Naidu et al., 2018, Dodini et al., 2023), and my results suggest they are. This paper also speaks to firm incentives to manipulate outside options and policy aimed at deterring such behavior. My estimated effects on salary alone imply that the no-poaching agreements reallocated at least \$3.1 billion from labor to other factors of production, but colluding firms settled the class action for just \$435 million

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<sup>11</sup>This study predates Callaci et al. (2023) and Lafontaine et al. (2023).

<sup>12</sup>More distantly related papers on imperfect competition and labor markets include Card et al. (2018), Lamadon et al. (2022), and Rubens (2023).

(Elder, 2015, Settlement Website, 2018). This implies that existing civil antitrust penalties likely will not act as effective deterrents to employer coordination, complementing the qualitatively similar findings of Stansbury (2021) on the Fair Labor Standards Act and the National Labor Relations Act. My setting also allows for investigation of the mechanisms linking outside options to pay. I estimate negative salary effects for both new and incumbent workers, increasing in magnitude with the duration of exposure to the no-poaching agreements. These results indicate that reductions in outside options can reduce both initial pay and pay growth over time.

Finally, my results contribute to the economic literature on white-collar crime descended from Sutherland (1940).<sup>13</sup> 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.<sup>14</sup> 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).<sup>15</sup> 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).<sup>16</sup> The remaining literature is rather idiosyncratic. Levitt (2006) studies non-payment for donuts and bagels in office settings where individual payments are unobserved. 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).

The rest of the paper proceeds as follows. Section 2 provides additional description of the agreements and evaluates them in light of the cartel literature. Section 3 describes my data, Section 4 presents estimating equations, and Section 5 discusses empirical results. Section 6 explores policy implications and concludes.

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<sup>13</sup>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).

<sup>14</sup>LIBOR stands for “London Interbank Overnight Rate.” Interest rates on many debt instruments are indexed to LIBOR.

<sup>15</sup>TRAC data begin in 1986.

<sup>16</sup>See Slemrod (2007) for a review.



## 2 Institutional background

### 2.1 The nature of the 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 of directors” (Saveri, 2011). Google CEO Eric Schmidt, Intel CEO Paul Otellini, Intuit Chairman Bill Campbell, and Pixar President Ed Catmull were also important to the scheme (Koh, 2013b). The executives operating the cartel knew it was illegal and endeavored not to put details in writing. Schmidt worried, for example, “I don’t want to create a paper trail over which we can be sued later? Not sure about this” (Koh, 2013b).

By “agreement,” then, this paper denotes an evolving set of secret commitments worked out between cartel-firm executives in private communications, not something analogous to a written contract. All agreements prohibited cartel firms from recruiting (“cold calling”) each other’s workers (US Department of Justice, 2010c, 2012). The DOJ argued that cold-calling is an important feature of labor markets for technology workers:

“Although Defendants employ a variety of recruiting techniques, cold calling another firm’s employees is a particularly effective method of competing for computer engineers and computer scientists. ...Defendants frequently recruit employees by cold calling because other firms’ employees have the specialized skills necessary for the vacant position and may be unresponsive to other methods of recruiting. For example, several Defendants at times have received an extraordinary number of job applications per year. Yet these companies still cold called engineers and scientists at other high tech companies to fill certain positions” (US Department of Justice, 2010a).

Employees made similar points in Glassdoor reviews, e.g. “The only way to go is to either a) start your own company ... b) get poached (very common),” (Google 2014) and “It’s a well-known secret that the best way to get a large raise and promotion at Apple is to leave and be hired back a few years later into the new position” (Apple 2008).

At least two agreements required that if a worker of one party applied to another and an offer resulted, the prospective new employer would inform the current one (US Department of Justice, 2010d,b, 2013). At least one also prohibited the prospective new employer from hiring such an applicant without permission of the current employer (US Department of Justice, 2013). At least one prohibited bidding wars (US Department of Justice, 2010d,b). The

terms changed over time (US Department of Justice, 2013) and in some cases evidence suggests, but does not definitively establish, provisions beyond non-recruitment (Koh, 2013b). Agreements were not limited by geography or employee role (US Department of Justice, 2010c,a, Leamer, 2012), but there is some evidence that they were enforced more rigorously in cases of highly educated, highly paid employees (Leamer, 2012, Koh, 2013b).<sup>17</sup> Implementation relied on internal lists of firms not subject to recruiting (“Do Not Call” lists) and/or instructions to human resources departments (US Department of Justice, 2010a). Additional details appear in Appendix B.

## 2.2 How the no-poaching cartel functioned

Porter (2005) catalogs five problems that a cartel must address: “1) Detection by Antitrust Authorities or the Victims; 2) Secret Price Cutting (Unilateral Defection); 3) Entry; 4) Reconciliation of Disparate Interests; 5) Responding to New Circumstances.”<sup>18</sup> Drawing on evidence from the DOJ investigations and the civil class action, this section discusses how the Silicon Valley no-poaching cartel approached these problems.

### 2.2.1 Detection

Affected employees were not informed of the no-poaching agreements and cartel firms endeavored to keep them secret (US Department of Justice, 2010a). Intel CEO Paul Otellini said, “[W]e have a handshake ‘no recruit’ between [Google CEO] Eric [Schmidt] and myself. I would not like this broadly known” (Koh, 2013b). For an individual worker with limited information, inferring the existence of the agreements would have been nearly impossible. Non-receipt of a recruiting call from another firm could have reflected that firm’s lack of interest. A job application might have failed because the hiring firm preferred another candidate. Even a highly sociable worker has a limited circle of friends and colleagues, whose experiences are unlikely to facilitate inference about worker flows across large heterogeneous firms.

The possibility remained that the cartel might have been revealed from within. As Porter (2005) observes, “A time-honored method of detecting collusion is finking by a dissident cartel member or an ex-employee, or the complaints of customers.” To mitigate against such potential betrayal, the number of people aware of the cartel was limited to top executives and the human-resources employees who implemented the agreements (US Department of Justice, 2010a). The simplicity of the no-poaching agreements may also have helped maintain

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<sup>17</sup>In court, attorneys and Judge Lucy Koh discussed a document revealing concern about the loss of a sou chef (United States District Court, 2013).

<sup>18</sup>Similar taxonomies appear in Levenstein and Suslow (2006) and Asker and Nocke (2021).

secrecy. A more complex scheme, e.g. salary fixing, would have required more frequent communications, and each communication would have been an opportunity for detection (Asker and Nocke, 2021).

### 2.2.2 Unilateral defection

**Incentives** Masur and Posner (2023) argue that downward nominal wage rigidity and the pay equity norm make defection from a labor-market cartel riskier than defection from a product-market cartel. If a firm defects, raising wages to attract workers from other cartel members, the equity norm may require the defector to raise wages for its existing workers as well. Should the attempted defection fail, or should circumstances change, downward nominal rigidity may make wage cuts very costly.

**Concealment** Concealing defection would have been nearly impossible in this setting. An employee receiving a job offer from another cartel firm would naturally have asked her current employer whether it would like to match or beat the offer. Firms commonly conduct “exit interviews” and ask departing workers where they are going (Spain and Groysberg, 2016). Finally, LinkedIn would have quickly revealed a defection to the cartel.<sup>19</sup> LinkedIn launched its website in 2003 and the no-poaching cartel began largely in the 2005-7 period. It is possible that LinkedIn makes no-poaching cartels more sustainable. More generally, increases in labor-market transparency (e.g. from pay transparency laws and websites like Glassdoor) could sometimes make workers worse off by facilitating employer cartels (Masur and Posner, 2023).

**Response** Agreements were “managed and enforced” through direct communications between executives (US Department of Justice, 2010a). In one instance Jobs complained to Schmidt, “I would be very pleased if your recruiting department would stop doing this [cold-calling Apple employees].” Schmidt replied: “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.” Jobs’ response has become infamous: “ :) ” (a smiley face; Edwards, 2014). Additional examples are in Appendix B.

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<sup>19</sup>LinkedIn is a website where workers and job seekers post resumes (CVs).

### **2.2.3 Entry**

There is a substantial literature in economics pointing out barriers to entry in the information-technology sector. For example, Varian et al. (2004) discuss how bundling of applications into suites decreases expected revenues and increases costs for potential entrants. The authors also argue that the network effects and increasing returns to scale common in information technology make entry more difficult. A complete review of relevant entry barriers is beyond the scope of this study.

### **2.2.4 Reconciliation of disparate interests**

Unlike a typical product-market allocation scheme, the no-poaching agreements did not explicitly divide the surplus they created, which may have papered over disparate interests and promoted cartel stability. When conflicts did arise, the publicly available evidence does not fully reveal how they were resolved. An interaction between Jobs and Palm CEO Edward Colligan may provide a clue, however. According to a declaration Colligan filed with the court during the class action, in August 2007 Jobs called Colligan and suggested a no-poaching agreement. Colligan responded by email, “Your proposal that we agree that neither company will hire the other’s employees, regardless of the individual’s desires, is not only wrong, it is likely illegal.” Jobs wrote back, “I’m sure you realize the asymmetry in the financial resources of our respective companies. . . My advice is to take a look at our patent portfolio before you make a final decision here” (Colligan, 2013). That is, Jobs threatened to punish Palm with expensive patent litigation if it did not enter a no-poaching agreement with Apple.

Such punishment can be viewed as a deterrent to unilateral defection, but it can also be viewed as an instrument to suppress discontent over the division of surplus. This is consistent with the idea, discussed in Asker and Nocke (2021), that cartel behavior is easier to sustain when firms interact in multiple markets. The discussion between Jobs and Colligan included a product market (mobile phones and accompanying software) and two input markets (labor, intellectual property).

### **2.2.5 Responding to new circumstances.**

New circumstances were addressed via communications among high-level executives. In some cases these exchanges led to modified cartel commitments. For example, the DOJ complaints allege that the eBay-Intuit agreement initially required notification even if an employee of one firm applied to the other, and later “metastasized into a no-hire agreement” (US Department of Justice, 2013). Empirical analysis of such mechanisms appears in Section 5.4.

## 3 Data

### 3.1 Description

My primary data come from Glassdoor, an online aggregator of wage and salary reports contributed by workers. All reports cover employer, work location, job, salary, and tenure. Glassdoor data suit the present investigation particularly well. Unlike US government data sets, they identify employers by name. Without such identification it is difficult to study firm-level anticompetitive behaviors like no-poaching agreements. Many US administrative data sets do not include occupations, and occupations in nationally representative US surveys like the Current Population Survey are relatively coarse. Glassdoor uses machine-learning models to classify users’ jobs at three increasingly granular levels: general occupation, specific occupation, and job.<sup>20</sup> As described by the company, the machine-learning model groups jobs using job search and clicking behavior on the Glassdoor website. 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.<sup>21</sup> The Glassdoor survey asks about non-salary compensation, including stock and cash bonuses and profit sharing, but responses to these questions are not required. 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, and firm-level cross-sectional measures of revenue and headcount as of 2020. 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 selection in Section 3.3 below.

My estimation sample comprises all Glassdoor reports by regular full- and part-time employees<sup>22</sup> 2007-2018 in US industries containing at least one cartel firm: “Computer Hardware & Software,” “Internet,” and “Motion Picture Production & Distribution.”<sup>23</sup> This paper’s title notwithstanding, the sample is not limited to Silicon Valley or the broader San Francisco Bay Area.<sup>24</sup> All cartel firms—that is, all firms named as defendants in DOJ complaints—are represented. All non-cartel firms for which Glassdoor reports exist are included. Table A3

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<sup>20</sup>The ten most frequent categories in my sample under each classification are in Table A1.

<sup>21</sup>Excluding these observations does not meaningfully change my estimates; see Appendix F.

<sup>22</sup>Interns and contract workers are excluded.

<sup>23</sup>Reports from outside the US are not used in my analysis, nor are reports from US industries other than “Computer Hardware & Software,” “Internet,” and “Motion Picture Production & Distribution.”

<sup>24</sup>The most frequently observed MSAs are listed in Table A2.

provides descriptive statistics.<sup>25</sup>

A second Glassdoor data set contains user ratings of jobs overall and job attributes: career opportunities, compensation and benefits, senior leadership, and work-life balance. Ratings range from one to five stars. Text reviews are also included. These data begin a year later, in 2008. Users are a subset of those who contribute salary reports. Table A4 provides descriptive statistics for the full sample of ratings.<sup>26</sup> 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 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.

### 3.2 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 (2022) 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 statis-

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<sup>25</sup>Table A5 compares colluding and non-colluding firms 2015-2018, by which time effects of the no-poaching agreements had likely dissipated. Figure A1 shows mean salaries for colluding firms over time.

<sup>26</sup>Figure A2 is a histogram of compensation ratings. Half-stars were permitted for attributes (but not for overall ratings) 2008-2012. Table A6 reports means for colluding and non-colluding firms 2015-2018.

tics 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 workers with undergraduate and graduate degrees, who comprise 93 percent of my sample (Table A3).

### 3.3 Sample selection

The Occupational Employment Statistics from the Bureau of Labor Statistics permit evaluation of my Glassdoor estimation sample. Figure 1 presents a scatter plot of occupation-years, where occupations are defined by year-2010 Standard Occupational Classification (SOC) codes.<sup>27</sup> To limit the influence of noise, the sample is limited to occupation-years with at least 10 Glassdoor reports. Vertical coordinates are nominal mean salaries from Glassdoor. 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. 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.<sup>28</sup> While the Glassdoor sample is not randomly drawn, Figure 1 provides evidence that it nonetheless reasonably represents mean salaries. The plaintiff’s expert report from the class action (Leamer, 2012) allows limited comparisons with firm administrative data, which are discussed in Appendix C.

## 4 Empirical strategy

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

$$\ln(\text{Salary}_{iejlt}) = \alpha_{ej} + \beta_{jt} + \gamma_{lt} + \delta \text{Agreement}_{et} + \zeta \text{Spillover}_{et} + \varepsilon_{iejlt} \quad (1)$$

Indices are  $i$  for individual (Glassdoor user),  $e$  for employer,  $j$  for job,  $l$  for location (state), and  $t$  for time (year). The parameters  $\alpha_{ej}$  control for cross-sectional differences across employer-job groups. The parameters  $\beta_{jt}$  control for arbitrary job-year time trends,  $\gamma_{lt}$  for arbitrary location-year time trends. The treatment variable  $\text{Agreement}_{et}$  is a duration-

<sup>27</sup>Glassdoor provided a mapping between SOC codes and Glassdoor’s specific occupation variable.

<sup>28</sup>Beyond \$145,600 some OES means are top-coded, making agreement between the two data sources much less likely.

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,  $Agreement_{et} = \left(\frac{4}{12}\right) 1 + \left(\frac{8}{12}\right) 0$ .<sup>29</sup> It follows that  $\delta$  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  $\delta$  as the average of heterogeneous effects from different agreement counts.<sup>30</sup> The variable  $Spillover_{et}$  is the product of an indicator for the 2007-9 period and an indicator for being a “connected” control-group firm. Connected firms are defined based on users who file multiple reports over the entire sample period: if any worker is observed at a given control firm and a treated firm, that control firm is considered connected. Accordingly  $\zeta$  is the spillover effect of the no-poaching agreements on cartel firms’ closest labor-market competitors.

The parameters of equation (1) are estimated 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 interacts with year indicators, and the 2014 treatment-control difference is normalized to zero.<sup>31</sup> The treatment group is comprised of Adobe, Apple, eBay, Google, Intel, Intuit, Lucasfilm and Pixar (Table A7 gives frequencies). Table A8 lists the most frequently observed control-group firms, starting with Amazon, Microsoft, and Cisco. Well-known consumer-facing firms like Uber and Facebook are represented, as are 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.<sup>32</sup> 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 six and a half percent, with little variation 2007-2009. 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

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<sup>29</sup>Details for each treated firm are in Appendix B. Duration weighting is relevant only for Intel in 2007, and Table A13 shows that eliminating duration weighting does not change estimates.

<sup>30</sup>Firm-level agreement counts range from 1 to 3, and the mean among colluding firms is very close to 2.

<sup>31</sup>In a more typical difference-in-differences event study where untreated observations of ever-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. Figure 2 reveals that this choice is not consequential, however: normalizing relative to any year 2014-2018 would not meaningfully change the results.

<sup>32</sup>In Figure 2 Intel observations are coded as treated throughout the period 2007-2009.



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 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. While there is some variance in the post-treatment point estimates 2012-2018, there is no evidence of different trends in untreated potential outcomes across the treatment and control groups. 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 could introduce bias. In the context of Figure 2 it is also important to note that exit from the no-poaching agreements might have differed from entry. For example, if any degree of tacit collusion persisted throughout the post-treatment period, then my estimates would be biased downward in magnitude.

The second important identifying assumption for my research design is the Stable Unit Treatment Value Assumption (SUTVA), or more colloquially the “no spillovers” assumption. Equation (1) captures potential spillovers with the term  $\zeta Spillover_{et}$ . Intuitively, conditioning on this term causes the counterfactual for the treated firms to be based on control firms that are distant in the labor market, but subject to similar product-market shocks. This makes it more likely that the SUTVA holds, but my measure of connectedness plausibly contains error. A single idiosyncratic worker transition would cause me to code a control firm as connected, even if it does not actually compete against treated firms for any other worker. On the other hand, my measure misses competing firms that do not participate in a worker transition within the Glassdoor multiple-reporter sample, and could miss firms that provide an outside option in salary negotiations that is not exercised. Appendix D shows that using an alternative measure of connectedness based on online job-search behavior yields similar results. Empirical estimates in Section 5.1 suggest that spillovers may be negative: the no-poaching agreements reduced salaries at competing non-cartel firms. If equation (1) failed to capture some such negative spillovers, that would bias estimated effects of the no-poaching agreements downward in magnitude.

Figure 2 also speaks to several potential sources of bias. The first is employer-driven misreporting. Treated firms might have wished to artificially increase their Glassdoor salary means after the no-poaching agreements were publicly revealed in 2009. It is possible 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, any one-year 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 six and a half percent. Given my empirical strategy, only a campaign of misreporting sustained over the entire post-treatment period could create large bias. Additional discussion is in Appendix E. The second potential source of bias relevant to Figure 2 is post-treatment changes in firm-specific labor supply. For example, workers might have required a compensating differential to work at a firm with a diminished reputation. But the only mentions of the no-poaching agreements in Glassdoor reviews are 2014-2018, and Figure 2 shows no evidence that treated-firm wages rose relative to control-firm wages in this period.<sup>33</sup> In addition, treatment-control differences in overall job satisfaction did not decline 2014-2018 (Figure A5), as one would anticipate if diminished firm reputation weighed on the minds of workers. Finally, if “superstar” cartel firms like Apple and Google were more sheltered from the recession of 2008-2009 than control firms, that would bias magnitudes upward (Autor et al., 2020); Section 5.2 evaluates this concern.

## 5 Empirical results

### 5.1 Primary results & robustness

#### 5.1.1 Salary

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 column-one sample ( $n = 249922$ ) is the basis of all compensation regressions except where otherwise noted. In some cases the observation count reported by an estimator is smaller because of how singleton or separated observations are treated. All sample differences are described in the table notes. The estimated no-poaching effect on annual salary is approximately -5.8 percent. This estimate is statisti-

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<sup>33</sup>It is possible these 2014-2018 mentions resulted from the press coverage of the class-action settlement, which was initially agreed in 2014 and finalized in 2015 (see Appendix A).

cally significant at the one percent level ( $p = .006$ ), and the 95 percent confidence interval runs from -10 percent to -1.7 percent.<sup>34</sup> 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\$; see Table A3). One might expect such characteristics to make these workers less vulnerable than others to employer market power. 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 even eliminate salary effects.

The reports of expert witnesses from the class action against the cartel firms provide initial benchmarks for the estimates in Table 1. Said experts had complete 2001-2012 administrative labor compensation data from defendant firms, but no data from other firms, making construction of counterfactuals difficult.<sup>35</sup> 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 from -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, 2013a). 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, 2013b). My primary estimate is inconsistent with an average null effect or “overcompensation.”

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. (2021) estimate a .5 percent causal decrease from a similar concentration change. Benmelech et al. (2022) find that a one-standard-deviation increase in HHI is associated with a 1 to 2 percent decrease in wages,

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<sup>34</sup>Alternative approaches to statistical inference are presented in Table A12 and discussed in Appendix F.

<sup>35</sup>In addition, the event study of Figure 2 suggests that 2010-2011 salaries at colluding firms were depressed by lingering effects of the no-poaching agreements, limiting their usefulness as part of a counterfactual.

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. The smaller magnitude of these estimates, relative to Table 1, is potentially consistent with a positive compensating differential for a contractually agreed noncompete clause.<sup>36</sup> Prager and Schmitt (2021) find that hospital mergers in the upper quartile of concentration increases reduced the wages of skilled workers by 4 to 6.8 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; Appendix H discusses the sources of such power in the setting of this study.

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  $e^{-.058} - 1 = -.056$ , or 5.6 percent. Earnings in the absence of the agreements would then have been  $\frac{\$52bn}{1-.056} = \$55.1bn$  and employee losses were approximately \$3.1bn, or \$28,428 per class member.<sup>37</sup> This estimate should be viewed as a lower bound on transfers from labor to owners of other factors (Shy and Stenbacka, 2019). It excludes non-salary compensation, spillover effects on workers at non-cartel firms, and additional job search costs incurred by affected workers. Even ignoring these omissions, my damage estimate is roughly seven times greater than the \$435 million the defendants paid to settle the case (Elder, 2015, Settlement Website, 2018).<sup>38</sup> This gap suggests that existing civil penalties may not deter future no-poaching cartels.<sup>39</sup>

In column 1 of Table 1 the estimated spillover effect, corresponding to  $\zeta$  in equation 1, is approximately -1.2 percent. Taken at face value, this indicates that the no-poaching agreements reduced salaries at the closest labor market competitors of the colluding firms. The associated 95 percent confidence interval, however, runs from -3.3 percent to +.9 percent. Estimated spillovers in the other columns of Table 1 are all within this interval. Because available precision is poor and spillovers are not the focus of this paper, estimated spillovers are not reported in subsequent tables, but all analyses control for spillovers as specified in equation (1) and discussed in Section 4. Alternative empirical approaches to spillovers are

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<sup>36</sup>That is, contractual noncompetes may both limit outside options and require a compensating differential, with the net effect of these two forces being negative.

<sup>37</sup>Alternatively one can assume that only technical and creative workers were affected (59,550 employees). From the technical-worker estimate of Table 4, the exact percentage effect of the agreements is  $e^{-.069} - 1 = -.067$ . Earnings in the absence of the agreements would have been  $\frac{\$33bn}{1-.067} = \$35.4bn$  and salary losses were roughly \$2.4bn.

<sup>38</sup>Apple, Google, Intel and Adobe settled together for \$415 million in 2015. The other defendants settled for \$20 million.

<sup>39</sup>This remains true even if one allows for considerable uncertainty in my estimate, non-settlement losses, and overstatement of affected earnings by the plaintiffs.

presented in Appendix D.

The remaining columns in Table 1 evaluate the robustness of the primary no-poaching 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 indicators, and educational attainment indicators. The resulting estimate is -6.2 percent. Column three presents a specification with industry-year fixed effects, ruling out bias from divergent industry-level trends. The resulting estimate, -5.5 percent, is slightly smaller in magnitude than the estimate from my primary specification. Selection on time-varying unobservables remains a potential threat to identification. As a check of this concern, column four estimates a specification in which treatment and spillovers interact with a “give to get” indicator (described in Section 3.1). Previous research has found that “give to get” mitigates selection of employees with highly positive or negative views of their jobs (Marinescu et al., 2021). If intense feelings about one’s job are correlated with determinants of earnings or misreporting, then “give to get” reports may differ systematically from others. Such reports are arguably also less susceptible to bias from employer-driven misreporting. 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 -6 percent. Coefficients on the interactions with the “give to get” indicator are small and not statistically distinguishable from zero at any conventional level. Additional robustness checks appear in Appendix F.

### 5.1.2 Non-salary compensation

Table 2 examines non-salary compensation, including cash and stock bonuses. While 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 questions about non-salary compensation, and the sample is a potentially selected subset of the one from Table 1. I observe positive supplemental compensation for 51 percent of reports across all firms, while according to Leamer (2012) 93 percent of defendant employee-years included supplemental compensation.<sup>40</sup> To evaluate selection indirectly, I estimate heterogeneous effects of the no-poaching agreements on salary by whether a report includes positive stock compensation.<sup>41</sup> The salary estimate for users reporting positive stock compensation (Table A15) is four percentage points larger than the estimate for other users. The following results should therefore be interpreted with

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<sup>40</sup>In my sample the fraction of defendant employee-years with positive supplemental compensation is also 51 percent.

<sup>41</sup>Note that positive stock compensation is potentially endogenous.

caution, particularly when considering magnitudes.

For each compensation type in Table 2 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).<sup>42</sup> The estimated effect of the no-poaching agreements on the probability of a positive stock bonus is -8.3 percentage points. Conditional on a positive stock bonus, the amount declines by 65 percent (105 log points). From column three, the combined effect is an average decline in stock bonus of  $1 - e^{-1.59} = -.796$ , or 79.6 percent. The corresponding event study is Figure 3 and robustness checks are in Table A16. All three stock-related estimates are statistically significant at the one percent level. Estimated effects on cash bonuses are negative, but substantially smaller than estimated effects on stock bonuses, and are not statistically significant at conventional levels.

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 Appendix H.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. Text reviews are consistent with this explanation. In 2008 an Intel employee noticed, “Their package for compensation is changing over the years to more ‘cash based’ and less retention (stock) oriented.”

### 5.1.3 Job satisfaction

Last among my primary results, Figure 4 presents estimated effects on job satisfaction ratings from the difference-in-differences design of equation (1). Exact point estimates appear in Table A11. As one might expect, the estimated effect on ratings of compensation and benefits is negative: -.25 stars, or -7.2 percent of the sample mean, statistically significant at the one percent level. The corresponding event study is Figure 5 and robustness checks are in Table A17. In proportional terms the magnitude is similar to the salary effect from 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. Text reviews also reveal employee awareness of reduced salaries, both in levels and in changes. One 2008 Apple review claimed, “Also slightly lower pay relative to other high tech companies. Benefits are average.” Another 2008 Apple review observed, “At least one division was giving less than 3% in average yearly salary increases to critical senior engineers, while the company’s profits

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<sup>42</sup>Observation counts in column three differ from those in column one because the PPML estimator drops observations perfectly predicted by the fixed effects.

soared.”<sup>43</sup>

Figure 4 also shows a negative effect on ratings of opportunities, -.27 stars (-8.2 percent of the mean), statistically significant at the one percent level. The corresponding event study is Figure 6 and robustness checks are in Table A18. 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 smaller (-.09 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.<sup>44</sup> Analysis of text reviews reveals no contemporaneous mention of the no-poaching agreements, and fewer than ten mentions after the 2009 DOJ investigation, all 2014-2018.<sup>45</sup>

Similarly in Figure 4, the estimates for work-life balance and overall job satisfaction are somewhat small (-.088 stars and -.11 stars, respectively) and not statistically distinguishable from zero. The latter estimate is potentially consistent with the importance of non-pecuniary amenities (Akerlof et al., 1988, Sockin, 2022). If overall job satisfaction co-varies positively with intention to leave a job, then the small no-poaching effect on this outcome in Figure 4 could reflect a low separations elasticity, which would be consistent with substantial employer market power (Manning, 2003, 2011).

## 5.2 Potential confounding from the 2008-9 recession

As mentioned in Section 4, if treated firms fared better than control firms during the recession of 2008-9, that could bias the magnitudes of estimated effects upward. This concern is motivated partly by Autor et al. (2020), which mentions treated firms Apple and Google as high-productivity “superstars.” Two factors mitigate this potential bias. First, the control group also contains superstars. Autor et al. (2020) mentions control firms Facebook, Amazon,

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<sup>43</sup>Google reviews from the collusive period are qualitatively similar. “The base salary and lack of promotion opportunities really start to wear down employee morale after the first few years” (2008). “Google base compensation is on the low side, and is supposed to be more than made up for by incentive bonuses, but these are largely illusionary [sic] because few employees receive the necessary ‘exceeds expectations’ performance evaluation” (2008).

<sup>44</sup>Perhaps surprisingly, Figure A3 shows no obvious decline in ratings of senior leadership after the DOJ investigation was revealed, or during the class action lawsuit. One possible explanation is leadership turnover: both Steve Jobs of Apple and Eric Schmidt of Google resigned their CEO positions in 2011.

<sup>45</sup>Reviews were searched for the following strings: poach, agreement, secret, illegal, recruit, cold, antitrust, anticompetitive, conspiracy, lawsuit, prosecution, subpoena, investigation, scandal, fixing, justice, and crim. Matching reviews were read. Example mentions include “You guys scare me. All the corrupt wage fixing and boy’s club at the top, very disturbing” (Pixar 2017) and “And let’s not forget their participation in the wage fixing scandal that robbed employees for over a decade” (Lucasfilm 2018).

Airbnb, and Uber. Appendix Table A.5 in Autor et al. (2020) additionally lists control firms Microsoft and Hewlett Packard. Second, by averaging over a nine-year post-treatment period my research design would reduce—but not eliminate—bias from the hypothesized confounding in 2008-9.

To investigate this concern empirically, I merge the Glassdoor sample with stock price, revenue and headcount measures for North American publicly traded firms from Compustat (Compustat Daily Updates - Fundamentals Annual, 2023).<sup>46</sup> Column one of Table 3 re-estimates my preferred specification using the subsample for which these Compustat variables are available. Compared to my primary result (column one of Table 1), the resulting point estimate is slightly larger in magnitude: -6.3 percent. Column two additionally controls for average stock price. This is potentially a bad control. If stock price is related to the discounted present value of profits, then it is plausibly a function of both the class-action settlements and labor costs, both of which responded to the end of the no-poaching agreements. However this control does partial out firm-specific shocks to profits and so reduces the scope for confounding from firm-specific effects of the 2008-9 recession. In column two the estimated no-poaching effect is -6.4 percent. Columns three through five control for functions of real revenue: log revenue, indicators for quintiles of log revenue, and log of revenue per worker. Again these controls may be functions of treatment, but they reduce the scope for confounding from firm-specific revenue shocks. Estimates range from -5.9 to -6.3 percent. The quintile-indicator specification of column four may be thought of as “matching” on real revenue in the sense of Angrist and Pischke (2009). Column six controls for the interaction of year-2003 revenue per worker with year-of-salary indicators. This flexibly controls for differences in shocks to firms with different pre-treatment performance, and avoids conditioning on plausibly endogenous post-treatment variables. It relies, however, on the selected sample of firms that were both public and active in 2003. The resulting estimate is -6.7 percent. Finally column seven uses a different subset of the primary sample, including both public and private firms, based on Glassdoor measures of revenue and headcount: firms with real revenue per worker greater than or equal to the minimum among treated firms. The Glassdoor revenue and headcount measures are from a 2020 cross section, so this subsample is defined based on potentially endogenous variables. Informally, though, this subsample requires that control firms possess “star power” similar to treated firms. The column-seven estimate is -6.6 percent. Taken together, the analyses in Table 3 are inconsistent with the hypothesis that differentially strong treatment-group performance through the 2008-9 business cycle generates large upward bias in the magnitude of my primary estimate.

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<sup>46</sup>Nominal revenue is converted to 2009 US dollars using the chained personal consumption expenditures deflator from the US Bureau of Economic Analysis.



### 5.3 Heterogeneity

I first consider treatment heterogeneity based on job. These analyses are possible because unlike US administrative data (e.g. LEHD<sup>47</sup> data), Glassdoor data include both firm names and job classifications. Qualitative evidence from the class action suggests the no-poaching agreements might have been enforced more vigorously for technical employees (Koh, 2013b).<sup>48</sup> To investigate, Table 4 estimates a regression in which a no-poaching indicator interacts with non-technical and technical job indicators. The marginal effect of a full-year no-poaching agreement on non-technical employees is -2 percent and one cannot reject a zero null hypothesis at conventional test sizes. The marginal effect on technical employees is -6.9 percent (statistically significant at the one percent level), and one can reject a null hypothesis of equality with the effect on non-technical workers ( $p = .034$ ). The larger effect on technical workers is consistent with stronger enforcement against this group, but could also arise if bargaining is more important for workers in high-skill positions (Caldwell and Harmon, 2019).<sup>49</sup>

Qualitative evidence from the DOJ investigation implies two other groups of employees at colluding firms who might have been treated differently. Recruiters required detailed knowledge of the no-poaching agreements in order to comply with them, and managers may also have been exposed to information about the agreements through their participation in the hiring process. I estimate a regression in which the agreement indicator interacts with other-worker, recruiter, and manager indicators. The marginal effect on recruiters is -1.2 percent and on managers -1.9 percent, but the marginal effect on other workers is -7.5 percent (Table 4).<sup>50</sup> These results are consistent with the hypothesis that employees who knew about the no-poaching agreements were less affected by them. There are multiple potential explanations for such a difference, including increased job-search effort and compensation for participation in an illegal scheme.<sup>51</sup> Additional analysis of heterogeneous treatment effects is in Appendix G.

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<sup>47</sup>Longitudinal Employer Household Dynamics

<sup>48</sup>While no-poaching enforcement might have been stronger for technical employees, the DOJ concluded that the agreements were not limited by geography or employee role (US Department of Justice, 2010c, 2012).

<sup>49</sup>The greater importance of bargaining for high-skill workers could arise from greater match surplus, greater willingness of the firm to negotiate, or both. Bargaining and other mechanisms are discussed in Section 5.4.

<sup>50</sup>Table 4 reports marginal effects. In a test against the null hypothesis that the manager and recruiter coefficients equal the coefficient on other workers,  $p = .0001$  for managers and  $p = .12$  for recruiters.

<sup>51</sup>The DOJ found no evidence of weaker enforcement against recruiters or managers, so that is not a likely explanation (US Department of Justice, 2010c).

## 5.4 Mechanisms

How did the no-poaching agreements reduce labor compensation at colluding firms, relative to other firms? In general, theory predicts that by limiting workers’ outside options, no-poaching agreements shift salary bargaining outcomes in favor of employers. Bargaining is likely to be particularly important for the highly educated technology workers in my sample. Hall and Krueger (2012) find that 60 percent of US workers with advanced degrees and 87 percent of “knowledge workers” report bargaining with their employer.<sup>52</sup>

In particular, all of the Silicon Valley no-poaching agreements prohibited parties from “cold calling” (recruiting) each other’s workers. As described in Section 2, some agreements made additional provisions: 1) notification of the incumbent employer in the event a worker tried to move from one cartel firm to another on her own initiative; 2) prohibition on hiring such a worker; 3) no bidding wars. In a search model, no-recruitment and no-hire provisions reduce the arrival rate of job offers for incumbent workers at colluding firms. In a search model with bargaining, a no-bidding-wars provision reduces the offer arrival rate, and may shift the mean wage offer downward for both incumbent and new workers at colluding firms. Such adverse changes in the offer distribution will reduce the frequency of renegotiations and job-to-job transitions for incumbents, slowing salary growth (Bagger et al., 2014, Caldwell and Harmon, 2019). Three hypotheses follow.

1. If no-bidding-wars provisions affect firm behavior, then one expects to see negative salary effects for new workers at colluding firms.
2. If no-cold-calling and/or no-hiring provisions affect firm behavior, then one expects to see more negative salary effects for incumbent workers than for new workers at colluding firms.
3. If the frequency of renegotiations and job-to-job transitions is reduced, then one expects each additional year of exposure to no-poaching agreements to further reduce salary, relative to a counterfactual.

Heterogeneity of no-poaching effects across workers sheds some light on these hypotheses. As an initial test of hypotheses (1) and (2), I manually create an indicator for entry-level jobs and estimate a wage regression in which it interacts with a no-poaching indicator.<sup>53</sup> In column one of Table 5 the effect on entry-level workers is -3.8 percent, consistent with hypothesis (1), but the estimate is not statistically significant. The estimate for non-entry-level workers (-5.9 percent) is larger, consistent with hypothesis (2). The difference in the

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<sup>52</sup>The survey of Hall and Krueger (2012) was conducted in 2008, within the period spanned by my Glassdoor sample.

<sup>53</sup>For example, in the technology sector many “Specialist” and “Analyst” positions are entry-level.

point estimates is not statistically significant, however, and could derive from other factors, as entry-level status is not plausibly quasi-random. Nonetheless the pattern of results is consistent with both no-bidding-wars and no-cold-calling provisions reducing salaries.

A more direct test of all three hypotheses is made possible by Glassdoor’s measure of job tenure.<sup>54</sup> Based on the year of the salary report and this tenure variable, one can calculate the number of years during which a worker was exposed to one or more no-poaching agreements.<sup>55</sup> Column two of Table 5 reports results from a regression in which the no-poaching indicator interacts with exposure duration and experience indicators are added to the set of controls. Because the no-poaching agreements were secret and ended in response to a DOJ investigation, exposure duration is *potentially* quasi-random.<sup>56</sup> The no-poaching effect on workers with one year of exposure, who are new to the colluding firms, is -4.1 percent. While this estimate is not statistically significant, it is consistent with hypothesis (1), that the prohibition on bidding wars reduced salaries. Estimated no-poaching effects are larger for incumbent workers with two or more years of no-poaching exposure, consistent with hypothesis (2), that no-cold-calling and/or no-hiring provisions affect firm behavior. These estimates are statistically significant. Finally and most strikingly, estimate magnitudes grow weakly monotonically with exposure, consistent with hypothesis (3), that the no-poaching agreements reduced the frequency of renegotiations and job-to-job transitions.<sup>57</sup> In summary, the pattern of estimates in Table 5 is what one expects to see if no-poaching agreements reduce worker salaries when joining a cartel firm, and additionally suppress salary growth as job tenure increases.

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<sup>54</sup>In completing a salary report about a particular job, users select “Experience” from a pull-down menu that includes “Less than a year” and the integers 1 through 60. Glassdoor codes “Less than a year” responses as zero. All other questions in the Glassdoor survey pertain to a particular job, rather than a career, and so I interpret experience as largely reflective of job tenure. It is possible that some users report experience in a given field, or even years of labor force participation. Such responses would tend to flatten the estimated relationship between no-poaching exposure duration and salary.

<sup>55</sup>This calculation assumes all agreements began in 2005, which is reasonable given the evidence of Appendix B. For example, a worker at a colluding firm who reported a 2008 salary and seven years of experience was exposed for four years (2005-2008). The calculation also assumes that workers with fewer than five years of exposure were not employed at a colluding firm in the years from 2005 through the starting year implied by their job tenure response.

<sup>56</sup>Table A19 evaluates selection on observables into no-poaching exposure duration. While one can reject a zero null hypothesis in a joint F test over coefficients on observables, the imbalances are extremely small, with five of six coefficients zero to at least two decimal places. Because demographic variables are available for a subset of observations, this test does not speak to balance in the full sample. Imbalance on unobservables remains possible.

<sup>57</sup>Note that one cannot reject a null hypothesis of equality for any pair of duration-specific no-poaching estimates.

## 6 Policy discussion & conclusion

Economists have long been interested in employer market power (Smith, 1790, Robinson, 1933, Reynolds, 1946b,a), 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 estimates for both salaries and stock bonuses. Disaggregated estimates for recruiters and managers, who plausibly knew about the no-poaching scheme, are much smaller. This is potentially consistent with compensation for participation in the illegal agreements. My findings suggest the high market concentration in many US labor markets creates scope for increased use of oligopsony power, with potential negative impacts on workers. 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).<sup>58</sup>

This 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 and Burning Glass have made it much easier to study the exercise of labor market power after the fact, but the problem of real-time detection remains. Antitrust authorities could combine market structure analyses based on job search behavior (e.g. 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 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. Alternatively, regulators could follow the approach of Delabastita and Rubens (2023), estimating wage

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<sup>58</sup>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 counterfactual perfectly competitive wage (marginal revenue product) could increase simultaneously with labor flows into superstar firms; 2) more generally, concentration changes are endogenous (Langella and Manning, 2021); 3) as discussed in Appendix H, concentration is not the only source of labor market power for large firms.

markdowns from firm production and cost data.

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 \$3.1 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. Other recent, prominent, white-collar crimes provide benchmarks for my damage estimates. The salary losses from the no-poaching agreements are smaller than estimates of losses from the Madoff investment fraud, which range from \$13.2bn (Lewis, 2010) to \$17.5bn (Peterson-Withorn, 2021). The difference in magnitudes is partly attributable to the longer thirteen-year duration of the Madoff fraud, relative to the roughly five-year duration of the no-poaching agreements. The salary damages in Silicon Valley are an order of magnitude greater than the estimated \$93mn in illegal profits generated by insider trading at the Galleon Group (U.S. Securities and Exchange Commission, 2012).

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 role of such social ties in the exercise of employer market power is a promising subject for future research.

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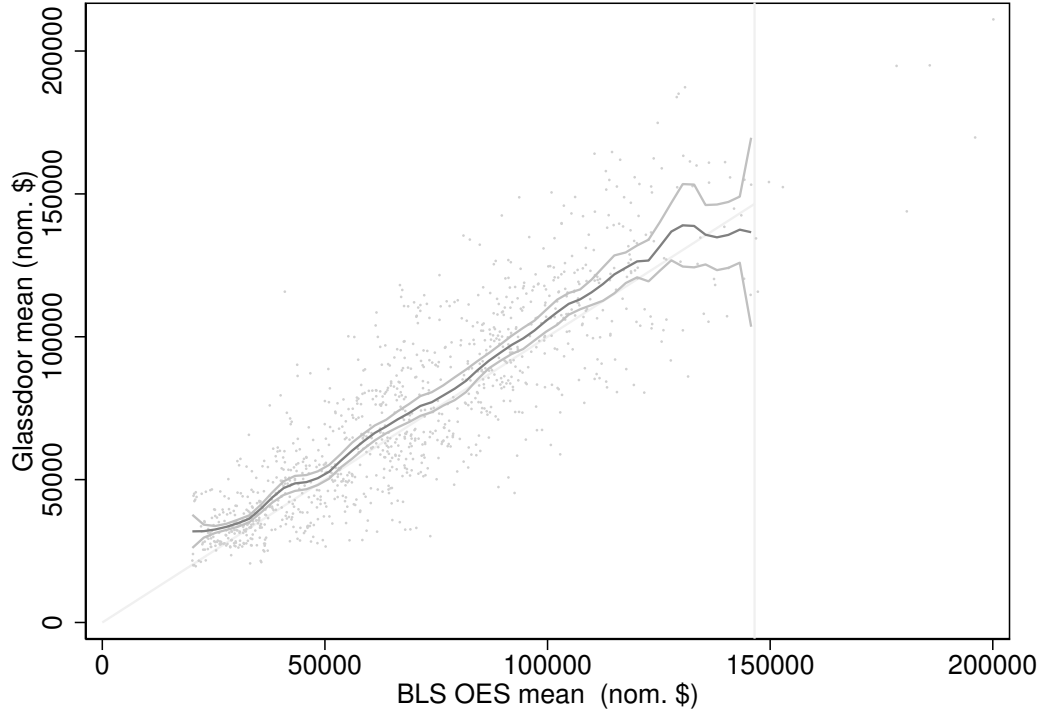
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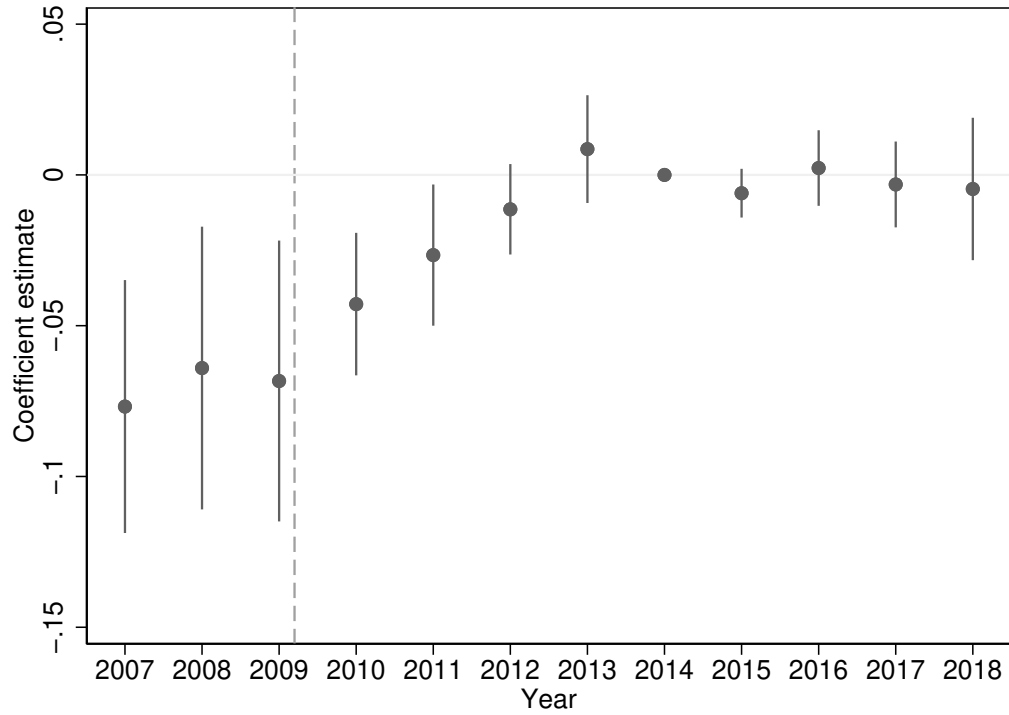
## 7 Figures

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



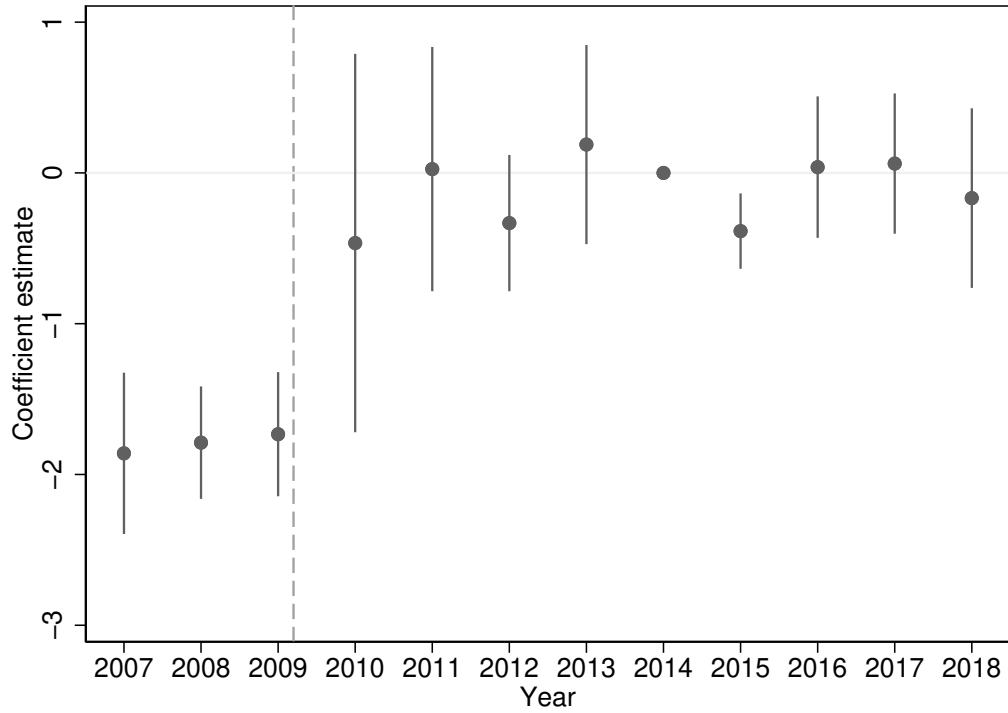
The Glassdoor average salary for an occupation corresponds well to the Bureau of Labor Statistics (BLS) OES average annual wage. Each point on the scatter plot is an occupation-year (minimum 10 Glassdoor reports), where occupations are defined by year-2010 SOC codes. Vertical coordinates are nominal 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 Epanechnikov 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



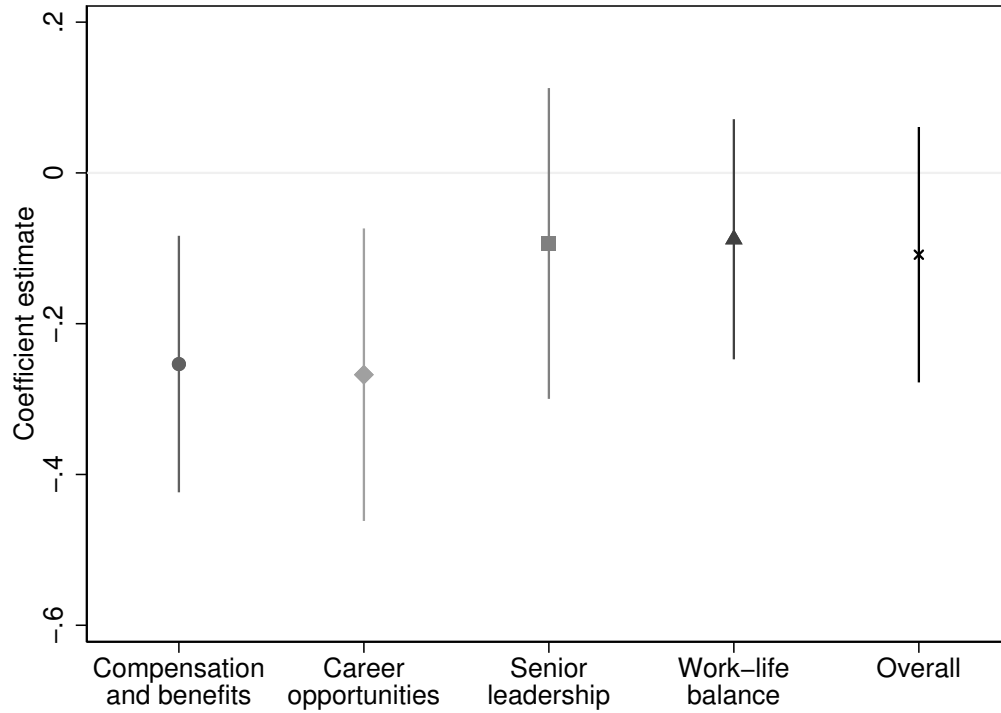
The no-poaching agreements reduced salaries 2007-9. After the effects dissipated, salaries at cartel and non-cartel firms moved in parallel. Coefficient estimates are from a variant of equation (1), in which the duration-weighted no-poaching indicator is replaced by interactions of a firm-level ever-treated 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, plus a spillover control: the interaction of a connected-firm indicator with a 2007-9 indicator. 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 stock bonuses



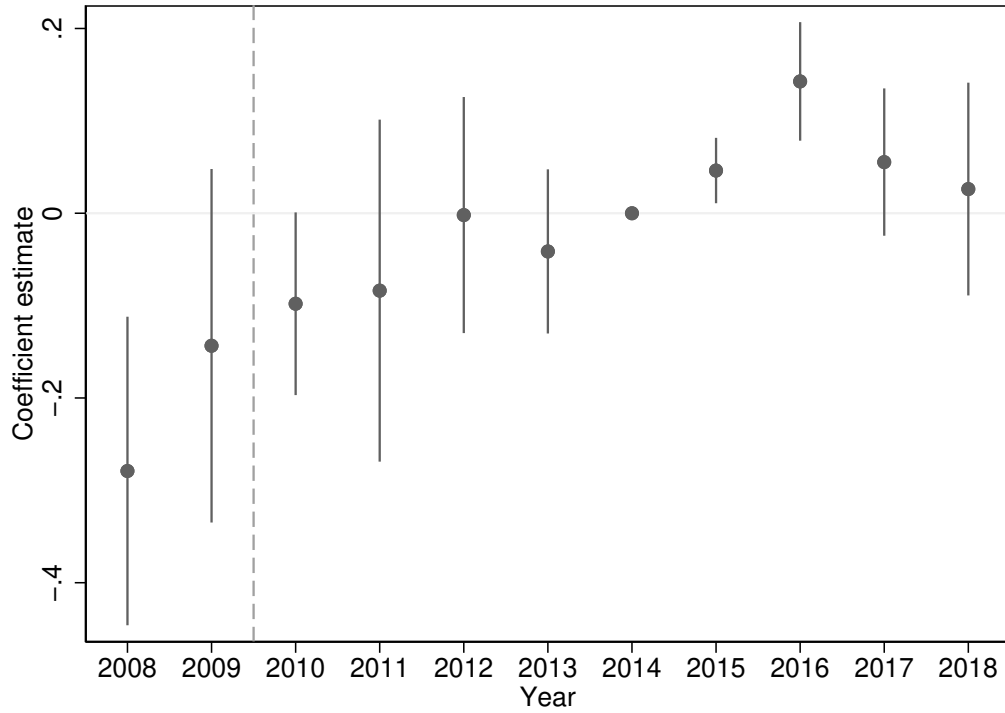
The no-poaching agreements reduced stock compensation 2007-9. After the effects dissipated, stock compensation at cartel and non-cartel firms moved in parallel. Coefficient estimates are from a variant of equation (1), in which the duration-weighted no-poaching indicator is replaced by interactions of a firm-level ever-treated indicator with year indicators, estimated using the Poisson pseudo-maximum-likelihood estimator of Correia et al. (2020). The dependent variable is stock bonuses (2009 US\$). Controls are job-employer, job-year, and state-year fixed effects, plus a spillover control: the interaction of a connected-firm indicator with a 2007-9 indicator. 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 4: Effect of no-poaching agreements on job reviews



The no-poaching agreements reduced ratings of compensation and career opportunities. 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, plus a spillover control: the interaction of a connected-firm indicator with a 2008-9 indicator. Standard errors are two-way clustered on general occupation and employer, and whiskers represent 95 percent confidence intervals. Table A11 presents the exact point estimates and standard errors corresponding to this figure. Figures 5 and 6 present event studies for compensation and career opportunities, respectively. Event studies for remaining categories are in Figures A3 through A5.

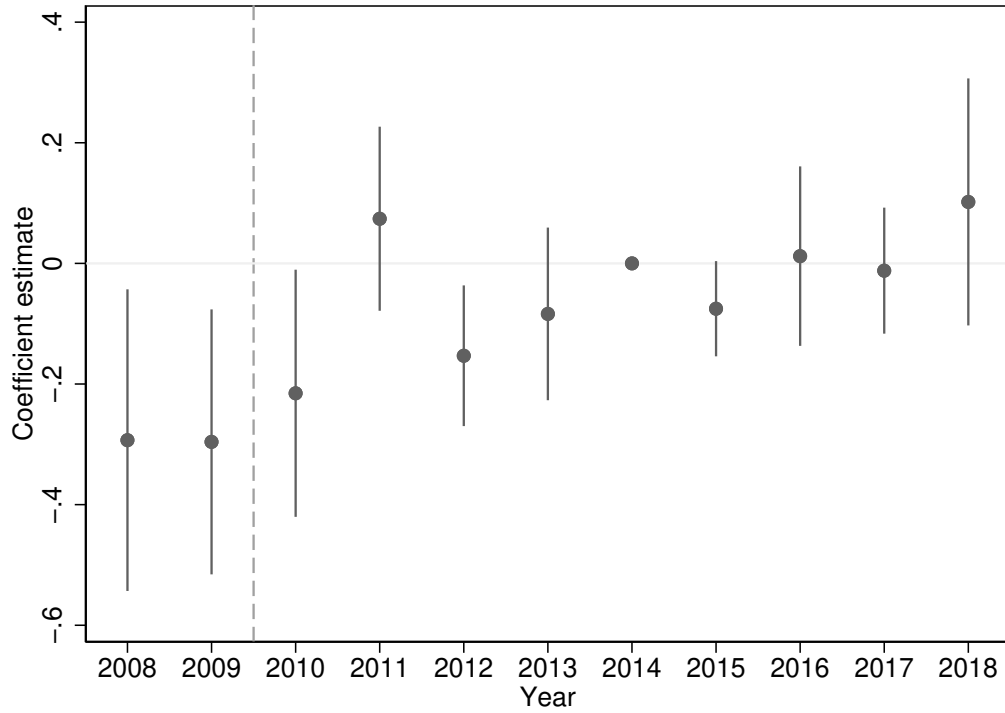
Figure 5: Event study, compensation & benefits ratings



The no-poaching agreements reduced ratings of compensation and benefits 2008-9. After the effects dissipated, salaries at cartel and non-cartel firms moved largely in parallel. 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 a rating of compensation and benefits from one to five stars. Controls are job-employer, job-year, and state-year fixed effects, plus a spillover control: the interaction of a connected-firm indicator with a 2008-9 indicator. 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. This event study corresponds to the pooled estimate for compensation and benefits in Figure 4.



Figure 6: Event study, career opportunities



The no-poaching agreements reduced ratings of career opportunities 2008-9. After the effects dissipated, salaries at cartel and non-cartel firms moved in parallel. 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 a rating of career opportunities from one to five stars. Controls are job-employer, job-year, and state-year fixed effects, plus a spillover control: the interaction of a connected-firm indicator with a 2008-9 indicator. 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. This event study corresponds to the pooled estimate for career opportunities in Figure 4.

## 8 Tables

Table 1: Effect of no-poaching agreements on salary

	Primary	Demographics	Industry- year FE	Give-to-get
Agreement in force	-0.058 (0.021)	-0.062 (0.019)	-0.055 (0.019)	-0.060 (0.020)
Spillover	-0.012 (0.011)	-0.016 (0.013)	-0.011 (0.010)	-0.012 (0.0094)
GTG $\times$ Agreement in force				0.012 (0.019)
GTG $\times$ Spillover				0.0013 (0.023)
Observations	249922	70249	249922	249922

The no-poaching agreements reduced salaries by  $e^{-.058} - 1 = -.056$ , or 5.6 percent. 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, plus a spillover control: the interaction of a connected-firm indicator with a 2007-9 indicator. Subsequent columns present variants of this primary specification, always including the fixed effects previously mentioned. Column two adds demographic controls: a female indicator, age indicators, and educational attainment indicators. The sample is smaller because Glassdoor does not require users to disclose demographic information. Table A13 presents results from equation (1) in the sample for which demographic variables are available. Column three includes industry-year fixed effects. Column four interacts variables of interest with an indicator for 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	-0.083 (0.028)	-1.05 (0.22)	-1.59 (0.22)
Observations	249922	43775	128588
	Cash bonus - LPM	ln(Cash bonus)	Cash bonus - PPML
Agreement in force	-0.012 (0.020)	-0.24 (0.18)	-0.20 (0.20)
Observations	249922	85482	191900

The no-poaching agreements reduced stock bonuses, but there is no strong evidence they reduced cash bonuses. 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, plus a spillover control: the interaction of a connected-firm indicator with a 2007-9 indicator. Column two excludes reports with zero compensation of a given type, as the log is undefined. Column three employs the Poisson pseudo-maximum-likelihood estimator of Correia et al. (2020). 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, 2008-9 recession checks

	Public firms	Stock price	ln(Revenue)	ln(Rev.) binned	ln(Rev.) /worker	ln(2003 Rev. /worker)*yrFE	(Rev./worker) ≥min. treated
Agreement in force	-0.063 (0.024)	-0.064 (0.023)	-0.059 (0.023)	-0.063 (0.025)	-0.059 (0.024)	-0.067 (0.022)	-0.066 (0.023)
Observations	149714	149714	149714	149714	149714	132660	192920

Negative estimated effects of the no-poaching agreements do not arise from differences in the impact of the 2008-9 recession on cartel and non-cartel firms. Estimates are from variants of equation (1). The dependent variable is log real annual salary (2009 US\$). Controls included in all columns are job-employer, job-year, and state-year fixed effects, plus a spillover control: the interaction of a connected-firm indicator with a 2007-9 indicator. Subsequent columns present variants of this primary specification. Column headings denote either the sample or the key additional control variable(s). Column one re-estimates my preferred specification (as in column one of Table 1) using a subset of my primary sample: publicly traded firms. This establishes a baseline against which columns two through six may be compared. Additional controls in columns two through six are from Compustat Daily Updates - Fundamentals Annual (2023). Column two controls for stock price. Column three controls for log real revenue (2009 US\$). Column four controls more flexibly for log real revenue using a set of five indicators for quintiles. Column five controls for log real revenue per worker. Column six controls for the interaction of (pre-treatment) 2003 log revenue per worker with year-of-salary indicators. The sample is smaller because not all firms were operating and publicly traded in 2003. Column seven uses a different subset of my primary sample, containing both public and private firms, based on Glassdoor measures of revenue and headcount: firms with real revenue per worker greater than or equal to the minimum among treated firms. Standard errors are two-way clustered on general occupation and employer.

Table 4: Effect of no-poaching agreements on salary, by job type

	ln(Salary)	ln(Salary)
Non-technical workers	-0.020 (0.019)	
Technical workers	-0.069 (0.020)	
Recruiters		-0.012 (0.028)
Managers		-0.019 (0.018)
Other workers		-0.075 (0.018)
Observations	249856	249857

Non-technical workers, recruiters, and managers were less affected by the no-poaching agreements. Estimates correspond to a variant of equation (1) in which the treatment indicator interacts with: non-technical and technical worker indicators (column one); or other worker, manager and recruiter indicators (column two). Estimates are marginal effects. In both columns the dependent variable is log real annual salary (2009 US\$). Controls are job-employer, job-year, and state-year-job-type fixed effects, plus a spillover control: the interaction of a connected-firm indicator with a 2007-9 indicator. Standard errors are two-way clustered on general occupation and employer. In a test against the null hypothesis of equal marginal effects in column one,  $p = .034$ . In a test against the null hypothesis of equality with the marginal effect on other workers in column two,  $p = .0001$  for managers and  $p = .12$  for recruiters. The estimation sample is identical to that in column one of Table 1, but the reported observation counts are slightly smaller because the additional controls create more singletons.

Table 5: Effect of no-poaching agreements on salary, by entry-level status and no-poaching exposure duration

	ln(Salary)	ln(Salary)
Entry-level	-0.038 (0.028)	
Non-entry-level	-0.059 (0.020)	
1 yr exposure		-0.041 (0.034)
2 yrs exposure		-0.052 (0.024)
3 yrs exposure		-0.057 (0.015)
4 yrs exposure		-0.057 (0.018)
5 yrs exposure		-0.063 (0.023)
Observations	249847	249913

The no-poaching agreements reduced salaries for both new and experienced workers, but effects grew larger as workers were subjected to the agreements for longer periods of time. Estimates in column one correspond to a variant of equation (1) in which the treatment indicator interacts with indicators for entry-level and non-entry-level jobs. In column two the treatment indicator interacts with a set of indicators for the duration of no-poaching exposure, which ranges from one to five years. This specification also includes a non-interacted set of experience indicators. Estimates are marginal effects. In both columns the dependent variable is log real annual salary (2009 US\$). Controls are job-employer, job-year, and state-year-job type (column one) or state-year (column two) fixed effects, plus a spillover control: the interaction of a connected-firm indicator with a 2007-9 indicator. Standard errors are two-way clustered on general occupation and employer. The estimation sample is identical to that in column one of Table 1, but the reported observation counts are slightly smaller because the additional controls create more singletons.

# Online appendix

## Appendix A 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, 2010c).
- Dec. 21, 2010. Complaint filed in US v. Lucasfilm (US Department of Justice, 2010d).
- 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).
- May 23, 2014. Initial class action settlement involving Adobe, Apple, Google, and Intel.
- September 2, 2015. Final class action settlement approved.

## Appendix B Details of no-poaching agreements

Further detail, including quotations from cartel-firm documents, is available in Leamer (2012).

- Apple-Google. The agreement began no later than 2006 (US Department of Justice, 2010c). 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. Each firm placed the other on an internal “Do Not Call” list. On at least two occasions Apple complained of violations to Google, which responded by conducting internal investigations and reporting back to Apple (US Department of Justice, 2010c,a).
- Apple-Adobe. The agreement began no later than May 2005 (US Department of Justice, 2010c). Each firm placed the other on an internal “Do Not Call” list (US Department of Justice, 2010a).

- Apple-Pixar. The agreement began no later than April 2007. Apple placed Pixar on its internal “Do Not Call” list. According to the DOJ, “. . . senior executives at Pixar instructed human resources personnel to adhere to the agreement and maintain a paper trail in the event Apple accused Pixar of violating the agreement” (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). Violations were handled among top executives. For example, in 2007 eBay CEO Meg Whitman complained of poaching to Scott Cook, who served simultaneously on the boards of eBay and Intuit. Cook responded, “#@!%\$#&!!! Meg my apologies. I’ll find out how this slip up occurred again. . .” (US Department of Justice, 2013).
- Google-Intel. The agreement began no later than September 2007 (US Department of Justice, 2010c). 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 placed Intel on its internal “Do Not Call” list and Intel informed its HR department of the agreement (US Department of Justice, 2010a).
- Google-Intuit. The agreement began no later than June 2007 (US Department of Justice, 2010a). Google committed to not poach Intuit employees, but there is no evidence that Intuit made an analogous commitment. Intuit more than once complained of violations. Google responded by conducting internal investigations and reporting back to Intuit. In every case Google determined that a violation had not occurred (US Department of Justice, 2010a).
- Lucasfilm-Pixar. The agreement began no later than January 2005 (US Department of Justice, 2010d). 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. Twice in 2007 Pixar complained to Lucasfilm about the latter’s recruiting efforts. Such complaints led the firms to resume cooperation (US Department of Justice, 2010b).

## Appendix C Comparisons to administrative data

The plaintiffs’ expert report from the civil class action relied on complete administrative data from defendant firms 2001-2012 (Leamer, 2012). Experts in the case did not have access to data from other (control-group) firms. While most data in the public version of



the Leamer (2012) report are heavily redacted, some comparisons with my Glassdoor sample are possible.

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.

Leamer (2012) Fig. 5 gives firms, jobs, years, and nominal compensation for the named plaintiffs. 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.<sup>59</sup> 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; named plaintiffs are similar to firm-level conditional means (fitted values), and Glassdoor means are similar to named plaintiffs. The Glassdoor measures of non-salary compensation are noisier, at minimum, and potentially less representative.<sup>60</sup>

## Appendix D Alternative approaches to spillovers

A different approach to spillovers is made possible by Glassdoor research on user job-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 indication that A and B are close

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<sup>59</sup>This observation is far from the corresponding total-compensation fitted value of roughly \$110,000 from the Leamer model, perhaps because of the large non-salary compensation.

<sup>60</sup>For non-salary compensation, the mean difference between administrative and Glassdoor data is -\$13,170.

in the labor market, with C farther away.<sup>61</sup> 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 4, are each other’s closest competitors.<sup>62</sup> 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 and suggestive of why the Apple-Google agreement was originated and enforced. To assess spillovers I define an indicator that equals one for 2007-2009 reports from firms linked to Apple or Google by Chen-Zion (2015): Accenture, Amazon, Cisco, Hewlett-Packard, IBM, Microsoft, Oracle and Salesforce. Adding this spillover indicator to Equation 1 (in place of my preferred spillover indicator based on worker flows) returns an estimated direct no-poaching effect of approximately -4.9 percent (column one of Table A10). This approach to spillovers has a substantial limitation: the networks of Chen-Zion (2015) include only two of eight colluding firms, albeit the two largest by market capitalization. The firms defined as “connected” under this approach are a subset of those defined as connected (based on worker flows) in my primary specification.

Remaining columns in Table A10 present further approaches to potential spillover effects of the no-poaching agreements. Column two 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 three 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.”<sup>63</sup> In both column two and column three, 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, -5.1 percent in column two and -4.4 percent in column three, are coarsely similar to my primary estimate. Excluding workers who switch across treatment and control firms returns an estimate of -5.8 percent (column four), suggesting workforce composition effects

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<sup>61</sup>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.

<sup>62</sup>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 have access.

<sup>63</sup>The full list of control industries in column four of Table A10 appears in the table note.

do not introduce substantial bias. In column five connected firms are dropped (instead of controlling for spillovers) and the estimate is -5.6 percent. Similarly column six drops MSAs with at least one treated firm from the control group and the estimate is -6.7 percent. Finally column seven interacts a treated-MSA indicator with a 2007-9 indicator, controlling for spillovers, and the estimate is -4.8 percent. While multiple approaches to spillovers are defensible in this context, Table A10 shows that all of them yield estimated no-poaching effects of roughly -5 to -6 percent.

## Appendix E Firm-driven misreporting

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 Glassdoor 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).<sup>64</sup> Following the public disclosure of the DOJ investigation in June 2009, the firms that participated in the no-poaching agreements might have wished to falsely increase Glassdoor salary reports and job ratings, but 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. The employer campaigns documented by Winkler and Fuller (2019) lasted just one to two months. As discussed in Section 4, such brief interventions would exert minimal influence on my estimator, which pools over nine post-treatment years. 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

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<sup>64</sup>Guaranteed Rate is not in my Glassdoor sample because it is not in the same industry as one of the colluding firms.

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. As a check of these concerns, I estimate a panel model of the share of employees reporting to Glassdoor as a function of a no-poaching indicator and find no relationship (Table A9).

## Appendix F Additional robustness checks

### F.1 Inference

Table A12 presents alternative approaches to statistical inference for estimated effects on salary. I begin with a jackknife estimate of the standard error, clustering on employer. According to recent guidance from Cameron and Miller (2023), this is equivalent to CV3 (sometimes denoted CR3) standard errors. MacKinnon et al. (2023) make a number of favorable comments about CV3 standard errors, including “...inference based on CV3 tends to be more reliable, sometimes much more reliable, than inference based on other CRVEs.” They point out that in simulations from several different authors, tests based on CV3 standard errors do not over-reject, though they may under-reject. Along similar lines, Hansen (2023) shows that the cluster jackknife estimator of the standard error is never biased downward. In summary, CV3 standard errors are a good choice if one’s primary concern is false positives. Column one of Table A12 presents my primary estimate with a jackknife standard error, clustered on employer. The jackknife standard error is .02, very similar to the estimate of .021 obtained from two-way (CV1) clustering on employer and occupation. This suggests that the t test in my primary results table is not likely to over-reject the zero null hypothesis. Column two presents results from a wild bootstrap procedure, again clustered on employer. The resulting p-value is substantially less than .01, and the values contained in the 95 percent confidence interval are all large enough to be economically significant.

### F.2 Specification and sample

The remainder of this appendix evaluates robustness to minor sample and specification changes; see Table A13 for full results. Limiting the sample to reports with demographics gives an estimate of -5.1 percent. Limiting the sample to reports with annual salaries gives an estimate of -5.8 percent, identical to my primary estimate. Using the level of salary as the dependent variable results in an estimate of -\$6352. Returning to log salaries, constructing the treatment indicator without duration weighting produces no change from my primary estimate. Modeling treatment as a duration-weighted agreement count returns an estimate

of -2.8 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. Limiting the sample to large firms yields an estimate of -8.3 percent, while limiting the sample to firms headquartered in the San Francisco Bay Area yields an estimate of -5.5 percent.<sup>65</sup> Finally, including indicators for transition effects in 2010 and 2011 gives an estimated no-poaching effect of -6.5 percent, consistent with Figure 2.<sup>66</sup>

## Appendix G Additional heterogeneity

To examine heterogeneity by labor market concentration, Herfindahl-Hirschman indices (HHI) are computed using counts of Glassdoor reports at the major occupation-MSA level, treating all colluding firms as a single entity. This HHI calculation requires an additional, untestable assumption: that Glassdoor report counts are proportional to firm headcounts at the major-occupation-MSA level. 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). Table A14 presents specifications in which the no-poaching treatment interacts with HHI. In column one the HHI enters continuously. The coefficient on the interaction of the HHI and the no-poaching agreements is negative, consistent with larger-magnitude effects in more concentrated labor markets, but not statistically significant. Column two uses indicators for unconcentrated, moderately concentrated, and highly concentrated markets, with HHI thresholds taken from the 2010 DOJ Horizontal Merger Guidelines (US Department of Justice, 2010e). Again estimates are consistent with larger effects in more concentrated markets. Differences between the coefficients for unconcentrated and moderately concentrated, and between unconcentrated and highly concentrated, are statistically significant at the one percent level. Column three interacts the agreement indicator with the partial HHI of the cartel firms. The estimated coefficient on the interaction is negative and statistically significant at the one percent level. That is, the no-poaching cartel was more effective when there were fewer outside options in the labor market.

Table A20 presents results from a specification that yields a separate no-poaching effect on salary for each cartel firm. Five of eight firm-level point estimates are negative. Of these, four are statistically significant at the one percent level. For Intuit  $p=.064$  in a test against

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<sup>65</sup>All colluding firms were headquartered in the San Francisco Bay Area, and one could argue than other firms headquartered in the Bay Area provide a better counterfactual.

<sup>66</sup>More specifically, the transition indicators are interactions of a firm-level ever-treated dummy with a 2010 dummy and a 2011 dummy.

a zero null hypothesis. Effects on Intel (-.048) and eBay (-.058) are roughly similar to the effect on Apple (-.066). All are smaller than the effect on Google (-.12). This heterogeneity potentially reflects labor market power from multiple sources, including firm-specific human capital like skill in Swift or Go programming (see Section H.2). The relatively large estimates for Apple and Google are consistent with the evidence from Chen-Zion (2015) that these two firms would have been particularly close labor-market competitors in the absence of a no-poaching agreement.

The three positive estimates in Table A20 are not statistically significant, and all three of the associated 95 percent confidence intervals contain practically meaningful negative values, e.g. -.01. Table A7 shows that the three firms associated with positive point estimates are those with the smallest sample sizes. It is possible that the positive point estimate for Adobe arises from termination or reduced enforcement of the Apple-Adobe agreement in November 2007. The DOJ alleged that this agreement continued through 2009 (US Department of Justice, 2010c) and Leamer (2012) assumes the same in his analysis. However Leamer (2012) does cite one document (which is not part of the public record) indicating the Apple-Adobe agreement ended in November 2007. It is also possible that the positive point estimate for Pixar is influenced by the 2006 acquisition of that firm by Walt Disney Studios.

## Appendix H Sources of employer market power in Silicon Valley

As discussed in Section 5.1, 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. 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). While Glassdoor data do not allow for well-identified empirical tests of the sources of employer market power, Sections H.1 and H.2 discuss descriptive evidence on search costs and differentiation, respectively.<sup>67</sup>

### H.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

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<sup>67</sup>The definitive treatment is Manning (2003a).

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 a 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.<sup>68</sup> To learn about important non-wage amenities (Sorkin, 2018), workers must often engage in costly application and interview processes. Online salary spreadsheets and salary transparency campaigns suggest that the wages offered by different Silicon Valley firms are not common knowledge (McGregor, 2015).

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 the 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.<sup>69</sup> Finally, all the same frictions that exist between large firms apply with respect to the terms of job offers.

## H.2 Job differentiation

### H.2.1 Human capital

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

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<sup>68</sup>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.

<sup>69</sup>This argument assumes a non-empty intersection of information valued by investors and information valued by job seekers. For example, the risk of business failure is plausibly important to both groups.

capital. Many Apple programmers work in Swift, while many Google programmers work in Go (Weinberger, 2015). 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. One 2015 review of Google made this point explicitly: “Almost everything that an engineer at Google uses on a daily basis is home-grown and maintained by other Googlers. Much of this tooling is incredibly complicated and takes months to warm up to. This could be a major brain drain to consider depending on your career path and/or whether you’re thinking of staying at Google long-term.” 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.<sup>70</sup> The larger estimates for technical workers in Table 4 are consistent with “job lock” from human capital.

## H.2.2 Geographic distance

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. In contrast, four of the five most frequently observed control-group firms (see Table A8) 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.<sup>71</sup>

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 in the Presidio of San Francisco, roughly 50 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 Bay Bridge, the two firms were just 13.5 miles apart by car, and they were far closer to each other than to plausible labor-market competitors. The other cartel firms all clustered in the Santa Clara Valley, and the average headquarters-to-

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<sup>70</sup>Mobility across large firms may be unaffected, provided the large firms pay for firm-specific training (Becker, 1965).

<sup>71</sup>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.



headquarters driving distance in this group was only 9.4 miles (see Table A21). 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.

Geographic distances may be associated with labor-market frictions and job differentiation. Distance may reduce information diffusion among workers, or between workers and firms (Keller, 2002, Belenzon and Schankerman, 2013, Caldwell and Harmon, 2019, 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 A2 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 A2, 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 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.

### H.2.3 Firm size

Other types of job differentiation may be related not to geography, but to firm size (Bhaskar et al., 2002). Excepting Lucasfilm and Pixar, the cartel firms were large.<sup>72</sup> 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, 1998, Autor et al., 2020), e.g. from patents (Kline et al., 2019).<sup>73</sup> Diseconomies of scale in monitoring may cause larger firms to pay more from efficiency-wage motives (Boal and Ransom, 1997).

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<sup>72</sup>As of this writing in 2022, the lowest market capitalization among other colluding firms was eBay’s \$24.33bn, and the second lowest was Intuit’s \$109.34bn.

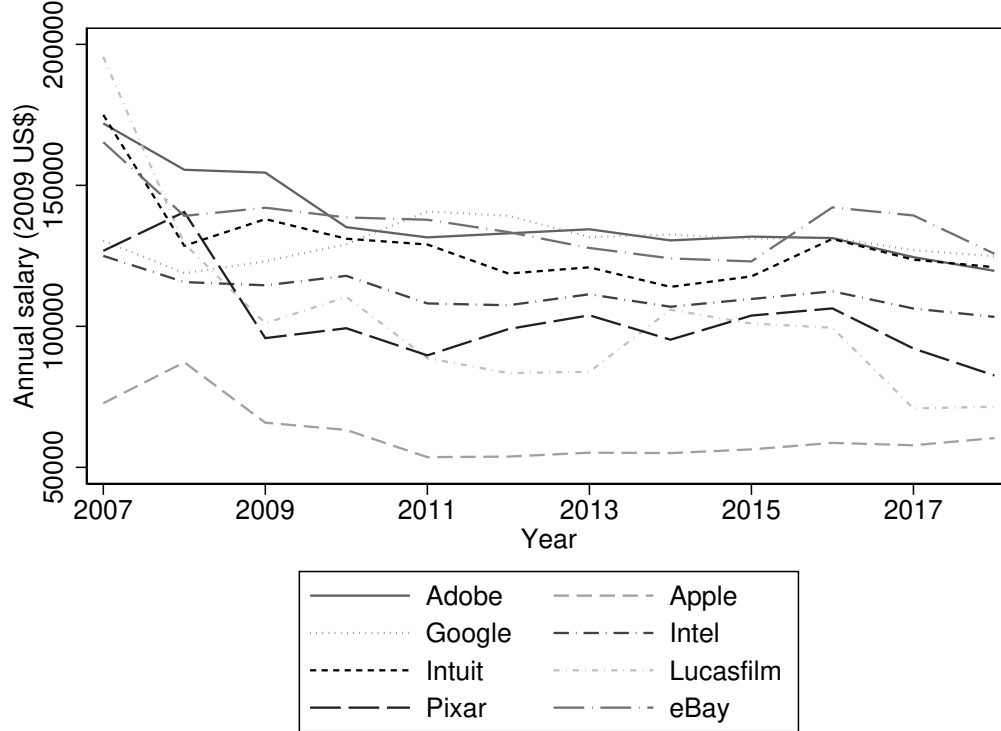
<sup>73</sup>Autor et al. (2020) list Apple and Google as examples of “superstar” firms.

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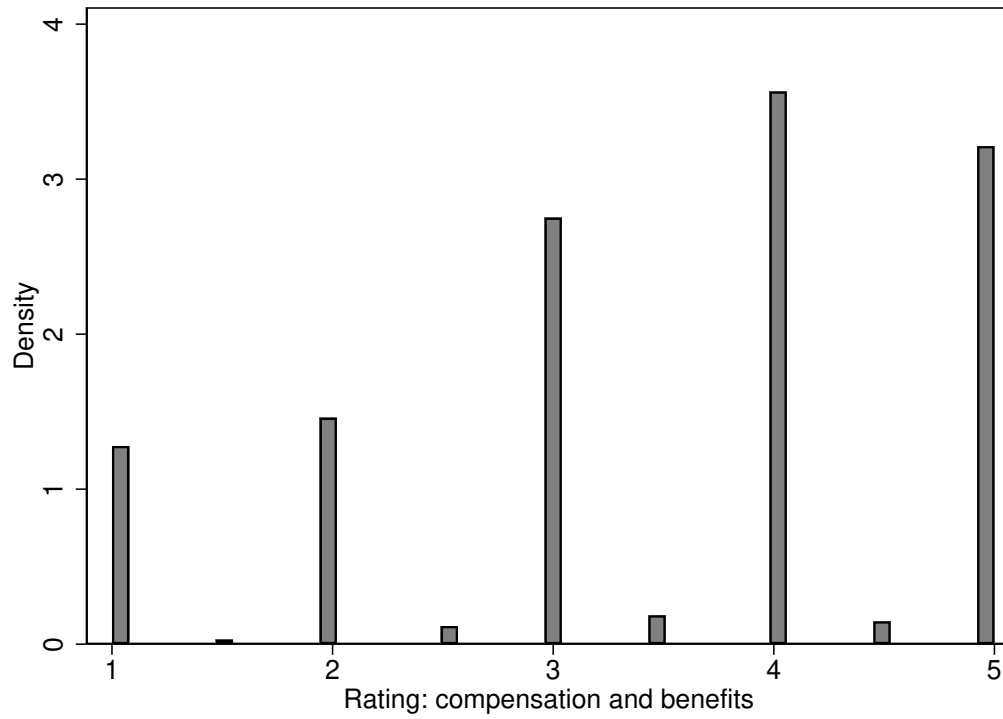
## Appendix I Additional figures

Figure A1: Unconditional mean salaries, colluding firms



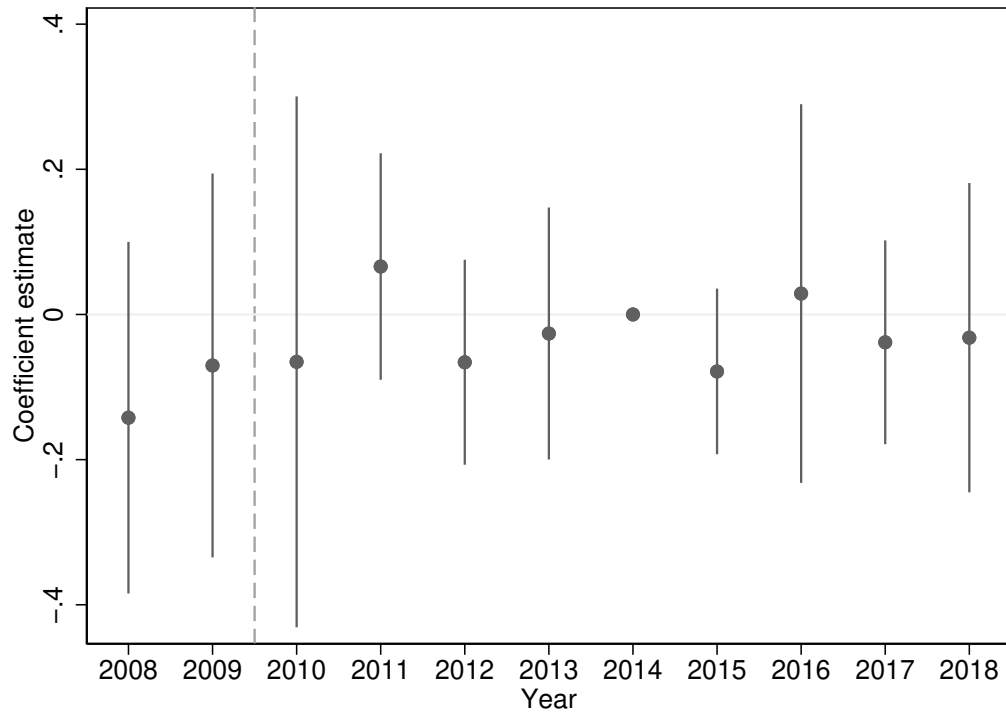
Plotted lines are mean real annual salaries (2009 US\$) for firms party to at least one no-poaching agreement. Mean salaries at Apple are lower due to the presence of retail employees, e.g. clerks working in Apple stores. The higher variance for Lucasfilm and Pixar stems, in part or entirely, from smaller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

Figure A2: Rating frequencies, compensation & benefits



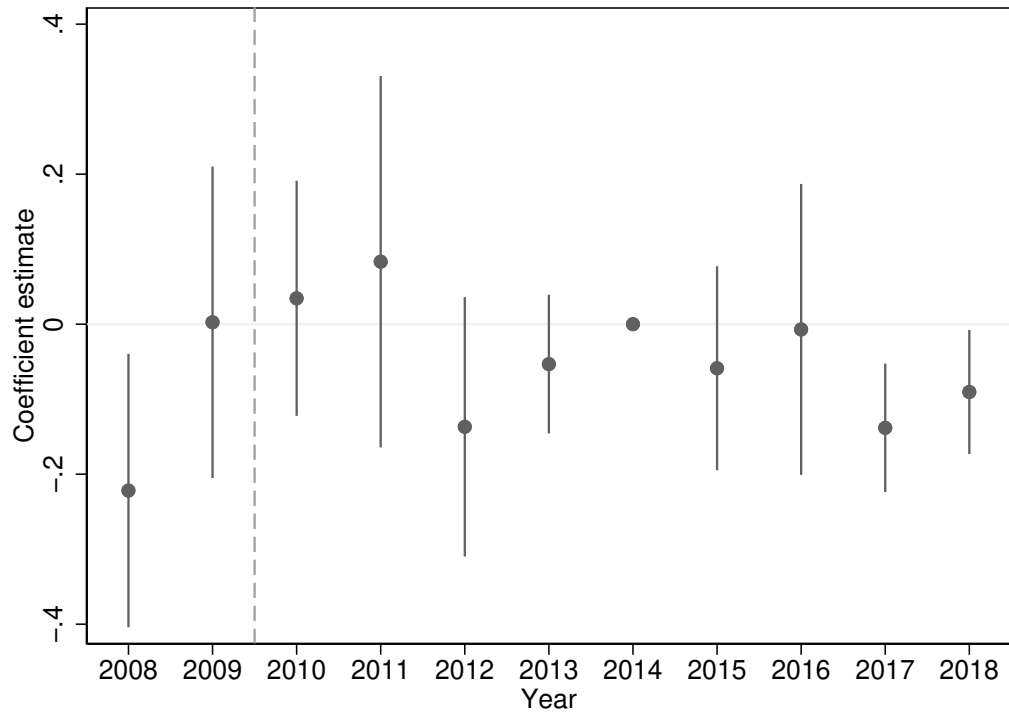
Illustrated are frequencies of star ratings for compensation and benefits. Data cover 2008-2018. Half-star ratings were permitted 2008-2012.

Figure A3: Event study, senior leadership ratings



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 a rating of senior leadership from one to five stars. Controls are job-employer, job-year, and state-year fixed effects, plus a spillover control: the interaction of a connected-firm indicator with a 2007-9 indicator. 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.

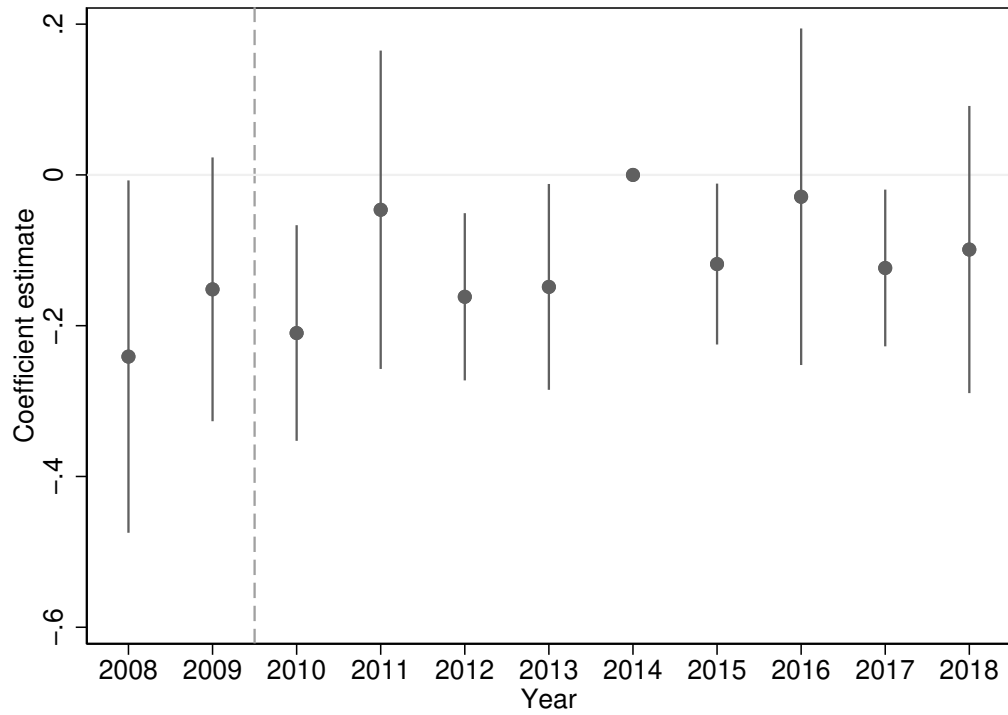
Figure A4: Event study, work-life balance ratings



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 a rating of work-life balance from one to five stars. Controls are job-employer, job-year, and state-year fixed effects, plus a spillover control: the interaction of a connected-firm indicator with a 2007-9 indicator. 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.



Figure A5: Event study, overall job satisfaction ratings



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 a rating of overall job satisfaction from one to five stars. Controls are job-employer, job-year, and state-year fixed effects, plus a spillover control: the interaction of a connected-firm indicator with a 2007-9 indicator. 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 J Additional tables

Table A1: 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 A2: 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 A3: 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 one of Table 1 ( $n = 249922$ ).

Table A4: 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 A11 because the `reghdfe` command excludes singletons (observations perfectly predicted by the fixed effects).

Table A5: 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 4.

Table A6: 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 4.

Table A7: Treated-firm frequencies, salary reports

	Count
Apple Inc.	14593
Intel Corporation	8050
Google Inc.	6830
Intuit Inc.	2546
eBay	1824
Adobe Systems Incorporated	1673
Pixar Animation Studios Inc.	144
Lucasfilm Ltd.	86
Total	35746

Table A8: 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 A9: Effect of no-poaching agreements on share reporting

	Share reporting
Agreement in force	-0.011 (0.0085)
Observations	17872

Glassdoor reports from the primary estimation sample (e.g. column one of Table 1) were aggregated to counts at the employer-year level. Zero counts were added to form a strongly balanced panel. Report counts were then divided by total headcount from Glassdoor to construct shares of employees reporting. Share reporting was modeled as a function of a duration-weighted no-poaching indicator, controlling for employer and year fixed effects and a spillover indicator. The standard error is clustered by employer.



Table A10: Effect of no-poaching agreements on salary, alternative approaches to spillovers

	Chen-Zion spillover control	Other industries	Selected industries	No switchers	No connected firms	No treated MSAs	MSA spillover control
Agreement in force	-0.049 (0.017)	-0.051 (0.016)	-0.044 (0.018)	-0.058 (0.021)	-0.056 (0.029)	-0.067 (0.044)	-0.048 (0.016)
Observations	249922	2898341	1622029	247347	93941	32986	249922

Column one replaces the spillover indicator of equation (1) with one based on the figures in Chen-Zion (2015). Column two forms a control group from all industries outside the primary sample—industries other than “Computer Hardware & Software,” “Internet,” and “Motion Picture Production & Distribution.” Column three 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 four excludes users observed at both treatment and control firms at any two points in time (“switchers”). Column five excludes connected firms, rather than controlling for spillovers as in Table 1 and elsewhere in the paper. Column six excludes MSAs in which at least one treated firm was present from the control group. Column seven includes a control for MSA-level spillovers: the interaction of a dummy for the 2007-9 period with a dummy for MSAs with a treated firm present. 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.

Table A11: Effect of no-poaching agreements on job ratings

	Compensation	Opportunities	Leadership	Work-life	Overall
Agreement in force	-0.25 (0.086)	-0.27 (0.098)	-0.094 (0.10)	-0.088 (0.081)	-0.11 (0.086)
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, plus a spillover control: the interaction of a connected-firm indicator with a 2007-9 indicator. Standard errors are two-way clustered on general occupation and employer. These estimates correspond exactly with Figure 4.

Table A12: Effects on salary, cluster jackknife and wild cluster bootstrap inference

	Cluster jackknife	Wild cluster bootstrap
Agreement in force	-0.058 (0.021)	-0.058 ( )
Wild bootstrap $p$		0.00080
Wild bootstrap 95% CI		[-.097, -.023]
Observations	249922	249922

The dependent variable is log real annual salary (2009 US\$). Controls are job-employer, job-year, and state-year fixed effects, plus a spillover control: the interaction of a connected-firm indicator with a 2007-9 indicator. In column one standard errors are from a jackknife procedure clustered on employer. In column two the p-value and 95% confidence interval are from a wild bootstrap procedure, clustered on employer, with 9999 replications. The bootstrap implementation is by Roodman et al. (2019).

Table A13: Effect of no-poaching agreements on salary, further robustness

		Demographic			
			Ann only	Salary (level)	ln(Salary)
Agreemen	in force	-0.051	-0.058 (0.022)	-6352.0 (2814.7)	
Unweighd	indicator				-0.058 (0.021)
Observ		70249	215757	249922	249922
		ln(Salary)	Large	Bay HQ	Transition indicators
Num. ts		-0.028 (0.0082)			
Agreemen	in force		-0.083 (0.018)	-0.055 (0.033)	-0.065 (0.021)
Yr=2010	treated firm				-0.041 (0.0100)
Yr=2011	treated firm				-0.025 (0.011)
Observ		249922	155600	86682	249922

Estimates are from variants of equation (1). Controls are job-employer, job-year, and state-year fixed effects, plus a spillover control: the interaction of a connected-firm indicator with a 2007-9 indicator. Upper panel: Column one estimates my preferred specification in the sample for which demographic variables are available. Column two limits the sample to users reporting an annual salary. Column three expresses salary in 2009 US dollars, instead of using the log transformation. Column four models treatment as an indicator, but without the duration weighting of Table 1. Lower panel: Column one models treatment as a duration-weighted count of no-poaching agreements. Column two limits the sample to firms with headcount greater than or equal to 7500, the size of the smallest treated firm. Column three limits the sample to firms headquartered in the San Francisco Bay Area, as all colluding firms were. Column four adds interactions of a treated firm indicator with 2010 and 2011 dummies, allowing for transition effects. Standard errors are two-way clustered on general occupation and employer.

Table A14: Effect of no-poaching agreements on salary, by market concentration

	ln(Salary)	ln(Salary)	ln(Salary)
Agreement in force	-0.053 (0.022)		-0.049 (0.022)
Agreement $\times$ HHI	-0.0000021 (0.0000017)		
Agreement $\times 1 \{HHI < 1500\}$		-0.036 (0.022)	
Agreement $\times 1 \{1500 \geq HHI < 2500\}$		-0.074 (0.022)	
Agreement $\times 1 \{HHI \geq 2500\}$		-0.065 (0.018)	
Agreement $\times$ cartel partial HHI			-0.0000068 (0.0000015)
Observations	249922	249922	249922

HHIs were computed for markets at the major occupation-MSA level, treating all colluding firms as a single decision-maker. Column one estimates a variant of equation (1), interacting the agreement variable with HHI. HHI also enters the equation in non-interacted form. Column two interacts the agreement variable with indicators for unconcentrated, moderately concentrated, and highly concentrated markets, with HHI thresholds taken from the 2010 DOJ Horizontal Merger Guidelines (US Department of Justice, 2010e). Non-interacted indicators also enter the equation. Column three interacts the agreement variable with the partial HHI of the cartel firms. Partial HHI also enters the equation in non-interacted form. The dependent variable is log real annual salary (2009 US\$). All columns include job-employer, job-year, and state-year fixed effects, plus a spillover control: the interaction of a connected-firm indicator with a 2007-9 indicator. Standard errors are two-way clustered on general occupation and employer.

Table A15: Effect of no-poaching agreements on salary, by stock reporting

	ln(Salary)
Agreement in force	-0.041 (0.020)
Stock > 0 × Agreement in force	-0.040 (0.0058)
Observations	249922

Estimates are from a variant of equation (1), interacting the agreement variable with an indicator for reporting stock compensation. The dependent variable is log real annual salary (2009 US\$). Controls are job-employer, job-year, and state-year fixed effects, plus a spillover control: the interaction of a connected-firm indicator with a 2007-9 indicator. Standard errors are two-way clustered on general occupation and employer.

Table A16: Effect of no-poaching agreements on stock bonuses, robustness checks

	Primary	Demographics	Industry-year FE	Give-to-get
Agreement in force	-1.59 (0.22)	-1.37 (0.45)	-1.64 (0.20)	-1.59 (0.36)
GTG × Agreement in force				0.21 (0.83)
Observations	128588	35659	128588	128588

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. The dependent variable is stock bonuses (2009 US\$). In column one controls are job-employer, job-year, and state-year fixed effects, plus a spillover control: the interaction of a connected-firm indicator with a 2007-9 indicator. Subsequent columns present variants of this primary specification, always including the fixed effects previously mentioned. Column two adds demographic controls: a female indicator, age indicators, and educational attainment indicators. The sample is smaller because Glassdoor does not require users to disclose demographic information. Column three includes industry-year fixed effects. Column four interacts the agreement variable with an indicator for reports elicited by Glassdoor’s “give to get” incentive. Standard errors are two-way clustered on general occupation and employer.

Table A17: Effect of no-poaching agreements on compensation ratings, robustness checks

	Primary	Demographics	Industry-year FE
Agreement in force	-0.25 (0.086)	-0.077 (0.15)	-0.25 (0.081)
Observations	133332	36624	133332

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, plus a spillover control: the interaction of a connected-firm indicator with a 2007-9 indicator. Subsequent columns present variants of this primary specification, always including the fixed effects previously mentioned. Column two adds demographic controls: a female indicator, age indicators, and educational attainment indicators. The sample is smaller because Glassdoor does not require users to disclose demographic information. Column three employs industry-year fixed effects. 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 A18: Effect of no-poaching agreements on career opportunities ratings, robustness checks

	Primary	Demographics	Industry-year FE
Agreement in force	-0.27 (0.098)	-0.21 (0.21)	-0.24 (0.093)
Observations	133332	36624	133332

Estimates correspond to variants of equation (1) with ratings of career opportunities as the dependent variable. Ratings range from one to five stars. Controls are job-employer, job-year, and state-year fixed effects, plus a spillover control: the interaction of a connected-firm indicator with a 2007-9 indicator. Subsequent columns present variants of this primary specification, always including the fixed effects previously mentioned. Column two adds demographic controls: a female indicator, age indicators, and educational attainment indicators. The sample is smaller because Glassdoor does not require users to disclose demographic information. Column three employs industry-year fixed effects. 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 A19: No-poaching exposure duration as a function of demographics

	Yrs exposure
Female	-0.0016 (0.0028)
Age	0.0044 (0.0015)
Age <sup>2</sup>	-0.000046 (0.000017)
Some college	0.022 (0.014)
College	0.0060 (0.0036)
Graduate degree	0.0012 (0.0054)
Joint F p-value	0.00030

Estimates correspond to a regression of no-poaching exposure duration (years) on demographic variables. Controls are job-employer, job-year, and state-year fixed effects, plus a spillover control: the interaction of a connected-firm indicator with a 2007-9 indicator. Standard errors are two-way clustered on general occupation and employer. “Joint F p-value” is the p-value from an F test against the null hypothesis that all coefficients on demographic variables are zero.

Table A20: Effect of no-poaching agreements on salary, by firm

	ln(Salary)
Adobe	0.023 (0.017)
Apple	-0.066 (0.021)
Google	-0.12 (0.020)
Intel	-0.048 (0.0097)
Intuit	-0.027 (0.014)
Lucasfilm	0.0073 (0.023)
Pixar	0.072 (0.062)
eBay	-0.058 (0.017)
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 cartel firms. The three positive estimates are not statistically significant, and the associated 95 percent confidence intervals contain practically meaningful negative values (e.g.  $-.01$ ). These are also the three firms with the smallest sample sizes (Table A7). For discussion see Appendix G. The dependent variable is log real annual salary (2009 US\$). Controls are job-employer, job-year, and state-year fixed effects, plus a spillover control: the interaction of a connected-firm indicator with a 2007-9 indicator. Standard errors are two-way clustered on general occupation and employer.



Table A21: 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							13.5	

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 in San Francisco and Pixar at Point Richmond. These two firms are treated as a separate group because: 1) they were the only two cartel 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.