Between-Firm Inequality and Informal Social Relations[1]

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Employer investment, social closure, peer networks: substantial research highlights differences in informal social structure across workplaces. Yet studies of pay inequality between firms have largely neglected these differences in favor of more easily measurable features like firm size or ownership structure. We show how three types of workplace social relations shape firm pay setting: employer relational investment that supports higher wages, social closure as a source of bargaining power, and amenity ties that lock workers into jobs despite low pay. To operationalize these concepts, we draw on text data from a large archive of job reviews. Variance decomposition analyses show that differences in social relations account for up to 20% of overall inequality in between-firm pay premiums and 7% of residual inequality. Differences in informal social organization, and not just formal organization, predict pay differences between firms.

INTRODUCTION

In the 1970s, a series of crises rocked the vertically integrated, managerial corporation that had dominated American capitalism for a half century

(Davis 2016; Langlois 2023). At the level of work organization, the Fordist division of labor was attacked both for its cumbersome inefficiency (Juravich 1988; Womack, Jones, and Roos 2007) and for growing job dissatisfaction among even highly paid production workers (Kanter 1978; Aglietta 2000). Between chaos on the shop floor and the blue-collar blues, bureaucratic workplace organization—which defined the postwar era of egalitarian economic growth—appeared oppressive, outdated, and brittle.

These crises were the prelude to a historic rise in inequality between firms that began in 1980 (Barth et al. 2016; Song et al. 2018). Today, much economic advantage is due to working at a firm that is consistently high paying: pay varies between firms employing even similar workers in similar jobs, and it varies consistently, such that firms that pay managers generously also pay high wages to customer service representatives (Groshen 1991). These firm pay premiums account for between a third (Wilmers and Aeppli 2021) and a fifth (Song et al. 2018) of total US earnings inequality. Yet they are hard to explain with reference to either competitive wage setting or differences in formal organization across firms.

In this article, we propose that informal workplace social structures shape earnings inequality. In doing so, we return to the workplace reorganization that followed the crises of the 1970s: One managerial response was to embrace informal social ordering in the workplace. Managerial rhetoric shifted from hierarchy to collaboration (Boltanski and Chiapello 2005) and from scientific management to organizational culture (Barley and Kunda 1992). Proposals for team-based production work (Vallas 2003; DeVaro 2008), concertive control (Barker 1993), and behavioral research–inflected human resources management (Kochan, Katz, and McKersie 1994) sought to use peer relationships to motivate frontline workers without the grinding boot of direct supervision. Other initiatives promised commitment between employers and empowered workers, in which efficiency wages would be policed not by bureaucratic personnel rules but by relational contracts (Appelbaum and Batt 1994; Osterman 2006; Vidal 2022). Postbureaucratic management also opened novel opportunities for social closure, as in finance and the professions, where privileged workers found new scope to exercise informal power and enforce symbolic boundaries (Weeden and Grusky 2014; Godechot 2016; Neely 2022). Indeed, notwithstanding the collaborative and even egalitarian rhetoric that accompanied these initiatives, the uneven and varied socialization of the workplace presided over two generations of rising inequality.

Insofar as various types of social relations emerge unevenly across organizations, inequality in pay between firms could reflect such differences. The unequal allocation of informal social structure could itself be a source of inequality, alongside better-studied differences in formal organization (like firm size and collective bargaining agreements) and market position (like market

power). To assess this idea, we first clarify several types of social relations that could contribute to between-firm inequality through distinct mechanisms. We then measure the incidence and associated earnings effects of these social relations using millions of worker reports from online job reviews.

Specifically, we distinguish three types of social relations, each emphasized in qualitative research on post-1970s reorganization of the workplace. First, employer investment can exchange generous pay for higher effort from workers (Arthur 1992; Appelbaum et al. 2000). This gift-exchange process, consistent with the high-commitment work models noted above, initiates a reciprocity relationship between workers and their employer (Akerlof 1982; Bellemare and Shearer 2009). Insofar as some employers pursue a high-commitment employment relationship, while others stick to the low road, this could exacerbate inequality between firms. Second, social closure among employees can increase their bargaining power against their employer (Wright 2000). When privileged groups coordinate to exclude outsiders, they can extract more surplus than would be delivered by competitive labor market processes (Sayles 1958; Neely 2022). If the propensity for successful employee social closure varies across firms, then exposed employers will be forced to increase pay more than others. Third, peer social relations can deliver amenity ties, in which employers substitute efficiency wages with peer monitoring (Barker 1993). Meaningful relationships with coworkers predict job satisfaction (Roscigno, Sauer, and Valet 2018) and reduce turnover (Jiang et al. 2012), effectively locking workers into their job.

These types of social relations emerge from substantial qualitative research on organizations, discussed further below, and mark distinct channels through which differences in pay can persist between firms. Recent research on between-firm earnings inequality by sociologists has begun developing these ideas, even if not in exactly the terms proposed here: Increased employer social distance across supply chains can weaken gift-exchange obligations (Wilmers 2018), outsourcing can erode informal group bargaining (Cobb and Lin 2017), and demographic minority group status can undermine wage claims (Tomaskovic-Devey, Hällsten, and Avent-Holt 2015). Yet this extant research has inferred informal social relations from observable measures of formal organization and worker composition—like buyer market power, firm size, and immigrant worker share—rather than by measuring social relations directly. Measuring these social relations at scale, across multiple firms, presents a serious methodological challenge, but doing so is critical for determining whether informal social structure in the workplace affects labor market inequality.

To address this challenge, we draw on economy-wide job review data from the company Glassdoor to develop dictionary-based measures of worker-reported workplace social relations. Specifically, employer investment is identified by worker reports of employer relational investment, caring for

employees, and supervisor support for subordinates. Social closure is measured with review discussions of group loyalty and boundaries. Social amenity ties are identified by coworker friendliness, kindness, and helpfulness. We validate these text-based measures by surveying employees and comparing direct survey questions to text-based job review responses.

We then use these measures to augment standard wage models with firm-level variation in social relations. Employer investment and social closure are both associated with higher pay, while positive social interaction with coworkers is associated with lower pay. These patterns persist conditional on standard Mincer human capital controls, with fixed effects for detailed labor markets (city by year by occupation) and with fixed effects for employer industry and formal organization type. We corroborate these findings using subsamples of the data that allow us to follow the same worker-reviewer across multiple jobs and to use firm fixed effects to study within-firm, over-time changes in social relations and pay. We also conduct tests for reverse causality and technical work task confounders and study the effects of the COVID-19 shock to workplace social structure.

Finally, we quantify the contribution of varying social relations across firms to overall earnings inequality. Differences in workplace social relations across firms account for up to 20% of the variance in firm pay premiums (conditional on occupational composition). Social relations also explain around 7% of residual variability in firm premiums, conditional on occupation, city, industry, organizational type, firm size, and firm revenue (observable measures that account for nearly half of the overall variability in premiums in our data).

These results suggest that between-firm differences in the nature of social relations in the workplace can help explain residual differences in firm pay premiums. In an era marked by varied and uneven initiatives to socialize the workplace, we document strong associations between types of workplace social structures and pay levels. Just as social network ties can sort workers across firms, and just as workplace social relations affect attainment within organizations, so too can the uneven allocation of workplace social structures account for inequality in pay between firms.

INEQUALITY IN FIRM PAY PREMIUMS

The empirical regularity that some firms consistently pay more has motivated research spanning over 70 years (Slichter 1950; Groshen 1991; Card et al. 2016). Specifically, there is substantial variation in pay among firms employing similar workers in similar jobs. Moreover, this variation is not idiosyncratic to one or another job; instead, some firms consistently pay more across job titles, while other firms consistently pay less (Dickens and Katz 1987). So pay variation is not simply random or one-off miscalibration. Different firms pursue different wage policies across multiple jobs. Indeed,

these two patterns violate simple models of competitive labor markets that predict that firms should converge on a single price for labor at a given skill level. So why is pay not just a function of a worker's skills but also of the firm at which they are employed?

While proposed prior explanations for firm pay premiums emerge from multiple disciplines, and often presuppose different models of wage bargaining and different specific operationalizations of firm pay premiums, they can be summarized as emphasizing cross-firm variation either in formal organization and bureaucratic employment rules or in product market position and rent sharing. Differences in formal organization are empirically important for accounting for firm premiums (Rosenfeld 2021): Large firms (Cobb and Lin 2017) and unionized firms (Wilmers 2019) pay more than small and non-union firms. Likewise, decisions about job design and the boundaries of the firm can affect pay premiums, as when outsourcing blunts pay comparison restraints (Weil 2014; Cobb and Stevens 2017). More broadly, production process differences like capital equipment and R&D intensity also account for variation in pay among similar firms (Barth, Davis, and Freeman 2018). Similarly, structured management practices and high-performance work organization are both associated with higher firm-wide pay (Osterman 2006; Bender et al. 2018). Differences in pay between firms are in part due to differences in formal structure and formal management practices.

Beyond these aspects of formal organization, differences in product market position can be a source of firm premiums, inasmuch as more profitable firms may pay more to workers. This intuition has spanned from older structuralist labor market sociology (Kalleberg, Wallace, and Althauser 1981) to modern empirical labor economics research on rent sharing (Card et al. 2016). If some firms have particularly successful technology (Autor et al. 2020), hold valuable patents (Kline et al. 2019), or face weak and demand-inelastic buyers (Wilmers 2018), they pay more. Across these empirical settings, more profitable (and typically larger) firms pay a larger rent to their advantaged employees.

While we do not contest the importance of these two broad sources of firm pay premiums, we see several reasons to widen the scope of explanations. First, these formal organization and product market position explanations do not fully account for observed variability in firm pay premiums. For example, one careful US study that includes measures for many of these explanations finds considerable residual variation in firm premiums (Barth, Davis, and Freeman 2018). Consistent with organizational sociology that has long emphasized the importance of informal organization, we expect that differences in social relations across firms account for part of the resilience of inequality in firm pay premiums. Even if firms converge toward similar formal organizational practices, subtler differences in social relations and informal organization can persist.

More importantly, the standard approaches to explaining firm premiums include several puzzles. The decline of unions and a broader increase in employment flexibility blunt the causal importance of formal organization in pay setting (Wilmers 2020; Rosenfeld 2021). If there is no formal collective bargaining agreement to contend with, why would profitable firms share rents with their workers? Indeed, absent strong bureaucratic pay rules, explanations for firm premiums based on product market position end up begging the question of why firms would share rents at all (Fligstein and Fernandez 1988). Recent research on profit sharing and incentives has emphasized the importance of relational contracts for effectively translating rents into sustained motivation (Gibbons and Henderson 2012). Alternatively, informal social closure among incumbent employees can be powerful, even absent formal union bargaining (Neely 2022). Concerted attention to variation in social relations could thus supplement explanations for firm premiums that focus on product market position and profit sharing.

Moreover, the explanations noted above typically focus on explaining firm pay premiums, rather than firm pay penalties. However, understanding between-firm inequality requires explaining both why some firms pay above the apparent market rate and how some manage to pay below that rate. A simple explanation is that nonwage job amenities attract workers to low-paying jobs (Sorkin 2018; Wilmers and Zhang 2022). But recent research shows that many job amenities are strongly positively correlated with pay: High-paying jobs also offer better health and retirement benefits, for example (Kristal, Cohen, and Navot 2020). We build on prior qualitative research to propose that friendship and camaraderie among coworkers work differently, as amenity ties. In this way, workplace social relations offer an explanation for why some workers stick with lower-paying jobs, despite apparently more lucrative outside options (Jäger et al. 2024).

PAY AND INFORMAL SOCIAL RELATIONS

Researchers have long studied how informal social processes affect rewards within organizations (Kanter 1977; Tilly 1998). Members of a powerful faction can hoard opportunities to boost their earnings (Pfeffer 1989; Hultin and Szulkin 1999; Abendroth et al. 2017). Advantageous network positions can deliver promotions and higher income (Burt 1992; Podolny and Baron 1997). Status hierarchies, activated in informal interaction, reinforce inequality (Ridgeway 2014; Sauer et al. 2021). Likewise, informal norms can distort performance expectations and penalize historically disadvantaged workers (Acker 1990; Kelly et al. 2010; Correll et al. 2020). This research paradigm has demonstrated that informal social relations, broadly construed, shape inequality and attainment within organizations.

Yet comparative organizational ethnographies consistently emphasize substantial heterogeneity across workplaces in informal social relations (Gerald and Nicod 1984; Kellogg 2009; Dörflinger, Pulignano, and Vallas 2021; Griesbach 2022). There are not only differences among coworkers in their positioning across a fixed social structure but also differences between firms in the shape and nature of these structures. Some workplaces involve substantial investment and commitment between employers and workers (Ton 2014; Roscigno et al. 2018). Others externalize and marketize the employment relation (Kalleberg 2011; Weil 2014) or indulge in low-trust and despotic surveillance (Crowley et al. 2010; Hatton 2020). Some workplaces are webs of dense social ties and networks (Newman 2000; Mears 2015), while others are anomic (Goldthorpe 1969; Sallaz 2015). Some have tight cliques and closure (Dalton 1959; Neely 2022); others are entirely open (Cameron 2022), porous (Powell et al. 2005), or participatory (Sobering 2019).

Can attention to these differences in informal social relations within workplaces help resolve dilemmas in prior research on between-firm inequality? Drawing on prior research in organizational sociology, we suggest that social relations can be arrayed across two dimensions, as depicted in figure 1. First, we distinguish whether relations link workers horizontally to each other or vertically to managers (Roscigno et al. 2018). This is an important difference that generates distinct predictions about pay effects, which we discuss in more detail below. Vertical commitment, or employer investment relations, drives employers to share rents with workers by making employee effort central to increasing productivity. In contrast, horizontal amenity ties can substitute for, and thereby lower, pay. The key difference here lies in control over pay setting. A purportedly worker-invested employer who pays low wages and does not share rents risks charges of hypocrisy: If an employer really cares about her workers, why does she refuse to share her profits? In contrast, a friendly peer coworker, even one who tacitly extends concertive control and thereby serves managerial interests, has no responsibility for wage setting and betrays no horizontal relationship.

Both employer investment and amenity ties are essentially those of consent (Burawoy 1979), which engages workers in the hegemony of a production process. A second dimension spans from these consent relations to more bounded or oppositional relations. Here social closure, whether defined narrowly for a work group or team or more broadly among all workers within a firm, bolsters bargaining power against an employer. Closure can be expressed vertically, say, a solidaristic team against a rapacious senior leadership, or horizontally, as teams, departments, and units sharpen their boundaries. Either way, these closure structures provide the oppositional identity and social capital necessary for acquisitive collective action in pursuit of higher pay. As such, we do not emphasize the vertical/horizontal closure distinction in our theorizing or analysis.
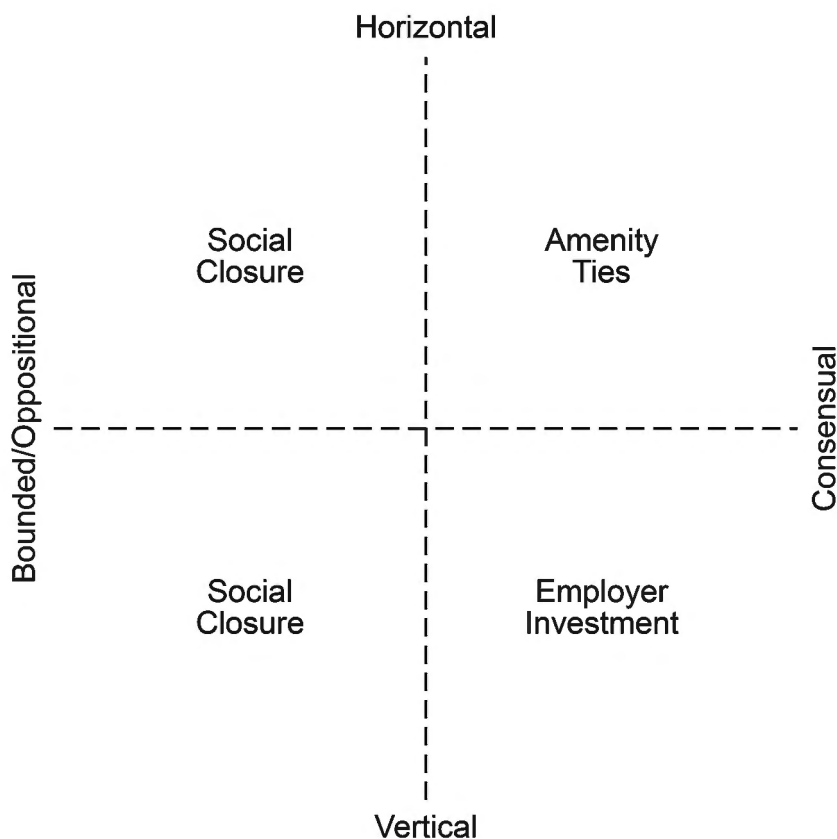
Fig. 1.—Types of workplace social relations for predicting pay premiums. Conceptual schema, arraying types of social relations across horizontal to vertical and bounded/oppositional to consensual axes. Horizontal to vertical distinguishes social relations primarily focused on peers from those between supervisors and subordinates. Bounded/oppositional to consensual distinguishes the content of social relations, as either essentially collaborative with alters or combative with an outgroup.

In the following, we define these types of social relations and their effects on pay in more detail before turning to their operationalization.

Employer Investment and Gift-Exchange Relations

A long line of research contrasts "high-road" and "low-road" firms on the basis of both wage levels and distinct approaches to worker management (Tsui et al. 1997; Osterman 2018). Research in management and industrial relations identifies firms that elicit worker commitment by building trust between

workers and employers (Arthur 1992; Appelbaum et al. 2000). These high-commitment or high-investment firms are in turn able to deploy a series of management practices that would be impossible absent worker trust (Cappelli 2004; Gibbons and Henderson 2012).

Consistent with these ideas, gift-exchange and efficiency wage models in economics also derive high wages from a social exchange, between employer and worker, of higher pay for effort (Akerlof 1982; Raff and Summers 1987). Both the management and labor economics versions of this argument emphasize that employer commitment to workers supports higher pay.

Common to these complementary lines of research is the idea that some employers pay more as a way to elicit superior worker performance. Moreover, this increased pay is contextualized in a thick, ongoing employment relationship also marked by commitment from the worker (whether through better performance, less shirking, or less frequent turnover). In these approaches, workers' perception of employer investment is a key, often unmeasured, determinant of higher pay and improved performance.

Despite its prominence in theories spanning multiple disciplines, prior research has not empirically tested the specific claim that a thick, relational exchange undergirds the wage benefits for workers in high-commitment firms. Survey-based research finds measures of social capital, like worker trust in employer and perceived investment, correlated with firm productivity (Tsui et al. 1997; Leana and Pil 2006; Wu 2008; Andrews 2010). One paper finds a related worker outcome, of retraining for retention, to be predicted by high-empowerment work practices (Cappelli 2004). We expect that employer investment supports higher pay as part of a relational exchange for worker commitment.

Social Closure

In contrast to committed relations between workers and employer, substantial research emphasizes ways that coworkers can force up their wages via social closure. Classic industrial relations research tracks solidaristic work groups (Kuhn 1961; Crozier 1963), and the sociology of labor emphasizes the efficacy of associational power in informal labor activism (Fantasia 1989; Wright 2000). Another line of theory (Parkin 1983; Murphy 1988) and recent empirical work (Weeden and Grusky 2014; Rosenfeld 2021) show how social closure among privileged workers can also allow them to capture firm resources from owners.

For example, hedge fund employees form tight cliques of same-race and same-gender colleagues, which allows them to concentrate financial benefits (Neely 2022). More broadly, financial managers extract large bonuses by holding up their employers (Godechot 2016), and managers extract rents through restructuring (Dencker and Fang 2016; Rosenfeld 2021). Likewise,

cultural matching in law and elite professional services, insofar as cultural fit is defined by incumbents, can restrict entry of outsiders (Rivera 2012). As one influential account puts it, "The exercise of power to exclude categorically distinct others from resources must happen in interaction and typically in some organizational context" (Tomaskovic-Devey and Avent-Holt 2019, p. 138). These forms of workplace social closure can force up pay for powerful incumbents, particularly in resource-rich firms, analogously to the way that occupational licensing and related barriers can inflate pay within a protected occupation (Weeden 2002).

A canonical example in organizational research comes from Crozier's (1963) maintenance workers, who banded together to destroy machine manuals. This allowed their clique to have a monopoly on crucial knowledge and provided the basis of their bargaining power. Another example comes from Sayles's (1958) classic study of work groups in auto manufacturing: The wire drawers implemented a work slowdown, solidaristically sacrificing incentive earnings for two months, to defeat a union–management agreement to reduce pay for overtime work. Outside of these classic manufacturing bargaining settings, social closure can improve working conditions via wage floor norms (Breza, Kaur, and Krishnaswamy 2019), wildcat strikes and protests (Korschun and Welker 2015), informal workplace mobilization (Kellogg 2009), output restriction norms (Homans 1950), nonunion advocacy involvement (Reich and Bearman 2018), or even strategic collusion among gig workers (Tassinari and Maccarrone 2020). Across these disparate channels, social closure facilitates workers' collective bargaining power.

In more general terms, social closure bolsters a threat of decreased effective labor supply for jobs within an organization. Solidaristic groups of workers can refuse managerial directives, slow down work, or demand concessions in a collective effort to bargain. This raises bargaining power for incumbent workers against owners over and above what would be available on an open, competitive labor market. For firms marked by substantial social closure, pay should be forced up. Note, however, that these social closure dynamics need not function along axes of race, gender, or other traditional categorical distinctions. Insofar as a group or team within a workplace forges effective solidarity, it can wield increased bargaining power (Freund 2022). Strong emphasis and celebration of team or departmental identity may not correspond to ascriptive characteristics but nonetheless support workers' collective power against their employer.

At some firms, workers effectively organize and achieve their goals, while at others they flounder in division and are sidelined (Sayles 1958; Kuhn 1961). Employers bargaining with effective, closed groups will be forced to pay more on average than employers unconstrained by social closure. Social closure solidarity thus gives an additional explanation for why large and profitable firms might share rents with their workers: not because it is necessary to

maintain a social exchange, but rather because solidaristic groups of workers can effectively threaten informal collective action.

Amenity Ties

While employer investment and social closure are both associated with higher pay, positive interactions and strong friendship ties, or amenity ties, can have the opposite effect. Substantial case study research shows how concertive control (Barker 1993), peer network reciprocity (Mears 2015), and peer control (Loughry 2010) can lock workers into compliance and consent. Obligations to peers can loom as more powerful and elusive constraints than commands from a supervisor.

While this ethnographic research focuses on peer influence as a source of control and discipline, rather than on wage effects per se, economics and organizational behavior research has surfaced evidence for this possibility. A large body of organizational behavior and human resources research has linked embeddedness in relationships with coworkers to lower turnover (Jiang et al. 2012). Workers forego pay to work in a team (Hamilton, Nickerson, and Owan 2003), and aggregate sectoral comparisons show that industries with high rates of sexual harassment may compensate with higher pay (Hersch 2011). Most directly, survey research finds that trust in coworkers and additional income are similarly correlated with job satisfaction (Helliwell and Huang 2010). Beyond these specific empirical findings, an older tradition of management scholarship from Elton Mayo and the social relations school emphasizes how positive social relations at work are crucial motivators over and above wages (Mayo 2004).

Consistent with these ideas, we argue that friendship ties and positive interactions at work can function as concertive control that allows some firms to pay less for comparable jobs. No research has directly assessed whether amenity ties in the workplace are associated with lower firm-wide pay. We further bring this idea to the puzzle of relatively low-wage firms contributing to between-firm inequality: Positive social relations among coworkers can allow employers to sustain low pay. Note the crucial difference here from employer investment: While employer investment requires a wage premium to be credible, team-based and networked control does not. Peers have no direct pay-setting authority, so a relationship can develop without any pay premium at all.

Taken together, these processes show how differences in informal social relations can account for between-firm inequality. We extend research on firm pay premiums by identifying clear mechanisms that would lead employers to share rents (employer investment and social closure) and that could account for workers accepting low-paying jobs (amenity ties). On the latter, we emphasize that social capital need not pay off immediately

in the form of higher wages, as in employer investment and social closure, but might actually lead workers to accept lower wages as they invest in the nonwage amenity of strong workplace-based ties.

Overall, two key insights emerge from our framework. First, relations marked by consent have divergent predictions for wages depending on whether a participant in the relationship has authority over wage setting. In vertical relations, low wages can betray purported employer investment. In horizontal relations, amenity ties can abet low wages by strengthening concertive control. Second, social closure and amenity ties, albeit both marked by a version of solidarity, can have starkly different effects. While social closure supports acquisitive instrumental action on behalf of some group, amenity ties may be limited to dyadic and affective exchange. There is no automatic conversion of friendships into collusive action. As Reich and Bearman (2018) conclude in their study of work and union organizing at Walmart, "Friendship, versus other kinds of instrumental relations, is a weak foundation upon which to build a movement" (p. 251).

While these types of social relations do not offer an exhaustive accounting of social structure at work, they cover key features of workplace socialization that have been foregrounded by decades of postbureaucratic management. Employer investment, social closure, and amenity ties all loom large in prior qualitative research on organizations and also hold plausible connections to pay determination. The uneven distribution of these social structures across workplaces could account for between-firm differences in pay.

DATA AND MEASURES

Despite the prominence of these ideas across several disciplines, variation in social relations across firms has not been used to model firm pay premiums or economic inequality more broadly. We attribute this to data limitations. The linked employer-employee administrative data used to study firm pay premiums include no measures of informal social organization (Song et al. 2018; Wilmers and Aeppli 2021). On the other hand, small, industry-specific surveys that ask about organizational social capital—the main prior method used to study between-firm differences in social relations—do not have economy-wide scope or the high dimensional occupational controls necessary to estimate firm premiums net of job composition (Leana and Pil 2006; Wu 2008; Andrews 2010).

To develop measures of social relations in large, linked employer-employee data, we use a massive source of worker reports about their workplaces. Specifically, we draw on two large datasets from Glassdoor, an online job review company (Marinescu et al. 2018; Corritore, Goldberg, and Srivastava 2020; Sockin and Sojourner 2023). The first dataset includes 7 million text reviews. In these reviews, respondents report pros, cons, and feedback for their current

or former employers. The second dataset includes detailed information on job title and pay for a mostly separate set of reviewers. Together, these text reviews and pay data allow us to study how worker-reported features of informal organization relate to between-firm pay differences.[2] We include only Glassdoor reviewers who report working in the United States. Our data sharing agreement with Glassdoor prohibits posting these proprietary data publicly, but all code and auxiliary data for this project can be found in Wilmers, Tong, and Zhang (2024).[3]

In our analysis, we use dictionary methods to measure social relations discussed in the text firm reviews (Grimmer and Stewart 2013). We then aggregate these to the firm-year level and match them to the pay reviews data. This allows us to estimate wage models using the pay data with the firm-aggregated, text-based information from the reviews. In the following, we first discuss the representativeness of the Glassdoor data and then describe how we measure social relations in the text reviews data.

Interpreting Online Review Data

Before proceeding to a detailed analysis of these data, a cautionary note is in order. The Glassdoor reviews are a long way from the direct ethnographic observation of workplaces that we draw on to theorize workplace social relations. All of the problems endemic in the move from ethnography to interview or survey therefore emerge here. Without any ground truth access to workplace social relations, we cannot adjust away workers' sense-making and interpretive work. Workers may complain about workplace politics and factions to justify their own lack of advancement. They may celebrate an indifferent manager as caring only in comparison to an abusive boss in their prior job.

Ultimately, however, the use of review data here invokes deeper problems of research method and interpretation that go beyond the typical distortions of self-reported data. Reviews are first and foremost a strategic context of worker voice. Workers complain, cajole, and brag in ways that are less likely for survey respondents, who are assured that only aggregated results will be shared with the public. Workers use these reviews to communicate to coworkers and prospective coworkers and to celebrate or publicly sanction

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[2] Glassdoor job reviews are submitted by workers, who must provide a verifiable email address or social media handle to post a review. Reviews are then screened by Glassdoor and discarded if they violate the site's terms and conditions (Sockin and Sojourner 2023). Reviewers typically fill out reviews because they are required to do so to access the rating and pay information on the website (Marinescu et al. 2018). Some share of these reviews is likely employer solicited or fake. We discuss tests to identify and exclude these reviews in "Dropping Employer-Elicited Reviews."

[3] In the Harvard Dataverse, https://doi.org/10.7910/DVN/CCMF4W.

their employers. This aspect of strategic communication may lead workers to emphasize solidarity and conflict, even in relative quiescent workplaces, as a way to cow an employer into concessions. The reviews may play up workplace friendships in hopes of attracting cooperative types to their workplace. The literature on talk at work (Drew and Heritage 1992), and the broader field of conversational analysis, emphasizes how these activity sequences and goals shape communication and, applied to our particular case, filter measures of social structure.

It is of particular concern if the use of online reviews not only varies by register and strategic intent but does so in a way that is correlated with wages. Perhaps low-wage, dissatisfied workers use job reviews to casually fire off complaints, while high-wage, long-seniority workers use them to carefully analyze and depict the key features of a workplace. Occupation, industry, and tenure controls, discussed below, should address some of these general differences, but systematic variation in style of review use across dimensions not observable to us could still be a source of bias.

We are also likely to disproportionately pick up social relations when they occur in organizational cultures marked by reflexivity about social structure. For example, it may be normative in a workplace to frequently discuss how the organization is like a family, or how coworkers are best friends. Indeed, it may be normative to do so even if, invoked ritualistically, such discussions have little connection to the content of actual ties between employer and employee or among coworkers. In these cases, even critical reports from hostile employees may strategically partake in the language characteristic of the organization's culture (Steinberg 1999).

We assume in our analysis that, first, such strictly discursive invocations would not support wage premiums or meaningful nonwage amenity. Second, we assume that this discourse, along with strategic distortions, is uncorrelated, conditional on covariates, with pay. It therefore ends up as measurement error that biases estimate coefficients on our measures of social relations toward zero.

These are strong assumptions, particularly in cases where rhetoric and culture have independent effects on wages, separate from social structural effects. In the following, we formulate multiple tests addressing issues of selection into reviewing and discussing social relations. However, we emphasize throughout the meaningful gap between our aggregate proxies and what could be achieved through direct, ethnographic observation of workplaces.

Representativeness of the Job Review Data

A more tractable data quality question lies in whether participants in the online review platform are representative of workers more broadly. We use two strategies to address this issue. To address representativity at the industry by occupation level, we matched 2019 Occupational Employment

Statistics (OES) two-digit industry by occupation employment shares to the data. We first coded the jobs and firms in the Glassdoor data into standard census codes, North American Industry Classification System (NAICS), and Standard Occupational Classification (SOC), as discussed in appendix B (all appendices are online only). Comparing these consistent SOC codes in appendix B shows that Glassdoor data substantially overrepresent managerial (18% vs. OES 6%), professional (like computer and math at 15% vs. OES 3%), and service (like retail sales at 15% vs. OES 10%) jobs. In contrast, production, construction, and other traditional blue-collar jobs are underrepresented.

To correct for this occupation and industry-wide distortion, we calculate weights that make the Glassdoor data equivalent to the OES in its distribution across these 451 broad industry and occupation cells. This procedure allows weighted estimates from a Glassdoor sample that has roughly the same occupational skill and production context as the overall labor market (see tables A3 and A4 in app. B).

However, this reweighting strategy still leaves the problem that within-industry-by-occupation reviewer selection could bias results. For example, Glassdoor reviews include a disproportionate share of employment at large firms, as workers at small firms may be nervous about accidentally revealing their identity to their employer (Sockin and Sojourner 2023). This is exacerbated after we drop firms with fewer than five total reviews. To assess this, we conducted the first, to our knowledge, online survey that compares workers who have participated in job reviewing to those who have not. Specifically, we recruited a sample of 1,000 US workers using Prolific, an online participant recruitment platform (see app. C for more details on our survey approach). We asked each respondent whether they had previously filled out an online job review or not. We then compared characteristics across reviewer and nonreviewer respondents.

Table A5 in appendix C shows that overall, survey respondents who report previously writing an online job review are similar to nonreviewers. Consistent with prior work, reviewers have slightly higher pay, are more likely to have a college degree, and are more likely to work in a large firm or a high-status industry. However, in this sample, these differences are small (e.g., 6 percentage points more BA degrees) and would largely be corrected via our industry by occupation weights. In the Glassdoor data, we also find that higher earners and lower earners within the same firm have relatively consistent reports of social relations in their reviews (see app. D). Additionally, we asked reviewers a battery of direct survey questions about social relations in their workplace. We discuss these questions in detail in appendix C. They capture survey-based versions of the employer investment, social closure, and amenity ties workplace relation types introduced above. Appendix table A5 shows that workplace social relations are quite similar between reviewers and nonreviewers.

Overall, this descriptive assessment reassures us that the weighted Glassdoor data are similar to those of the overall US workforce. While we cannot rule out some unobserved differences between job reviewers and nonreviewers, the two groups are quite similar across standard demographic characteristics. Crucially, they also report similar experiences of the workplace social relations that we study in this project. We return to these and other sample selection issues in robustness tests reported below.

Measuring Social Relations

A second challenge in using these data to study inequality and social relations lies in deriving measures of social relations from the unstructured text available in the text review data. To do this, we assembled dictionaries that track worker reports of employer investment, social closure, and amenity ties. Note that unlike prior work in organizational theory using text analysis of job reviews (Corritore et al. 2020), we proceed primarily deductively and generate our categories from prior research rather than from an inductive approach, like topic modeling or clustering, in which categories emerge from the text itself. This fits our research question, which focuses on testing the relationship between specific types of social relations and pay.

Specifically, employer investment covers mentions in reviews of employers or supervisors caring for, supporting, or listening to employees. These are explicitly vertical relations, in which a superior (or the company or employer writ large) is perceived to be emotionally invested in or otherwise committed to a subordinate. Alongside emotional investment, the employer investment category covers intimacy, recognition, deference, and respect from superior to subordinate.

In contrast, social closure involves group loyalty within organizations, which can be coupled with hostility to the employer or to other workgroups. This includes discussion of factions and cliques in the workplace alongside bounded loyalty to one's particular team, department, or immediate colleagues. These various aspects of social closure measure the kind of bounded group cohesion that can undergird zero-sum bargaining over an organization's surplus. Crucially, for this category, which is about boundaries and opposition rather than exclusively positive affect, we draw in part from the "cons" comments included in Glassdoor.

Finally, we operationalize amenity ties as reports of coworker friendliness, kindness, and helpfulness. Like social closure, these ties often refer to group cohesion. But they are shorn of boundedness and negative valence. Instead, reports of amenity ties are explicitly generalized throughout an organization, rather than concentrated within a specific group. They also focus on the amenity value of ties specifically, noting how fun or nice coworkers are, rather than professing loyalty or advertising group rank. For ambiguous terms,

the key pros/cons distinction in the reviews allows us to carefully distinguish characterizations of hegemonic (amenity tie) from bounded (social closure) relations. Amenity ties are also distinct from employer investment insofar as, while they are both consistently positive, amenity ties refer mainly to horizontal friendships among coworkers.

To operationalize these general definitions, we identified unigram, bigram, and trigram phrases for each type of social relation out of the most common 2,000 phrases that appeared in the reviews. These phrases were compiled after stemming the whole review corpus and removing common stop words (articles, prepositions, etc.). Stemming the text allowed us to avoid the onerous task of manually identifying all variation forms of each root phrase in our dictionary. Next, we began cycles of iterative manual coding in which we first searched for representative exclude phrases and additional key phrases,[4] and then checked dictionary classification performance based on manually coded samples. In developing these dictionaries, we were careful to exclude explicit mentions of pay or compensation (our dependent variable in the analysis); of formal organizational structure, policies, or practices; and of firm performance (the key firm characteristics emphasized in prior studies of firm pay premiums). Instead, we focused on explicit mentions of informal social relations and organization in the workplace. Appendix D1 describes our approach to these coding cycles in more detail.

After seven rounds of checking and verification, the final versions of each dictionary reach above 0.9 on accuracy and above 0.7 on F1 score for all three categories of social relations based on the manual codes of two coders for a random 500-review sample.[5] Table A8 in appendix D1 presents four widely used measures of fits to assess dictionary performance: the accuracy, precision, recall, and F1 score for each category based on the comparison between dictionary classifications and manual codes (Nelson et al. 2021).

This internal validation exercise cannot rule out measurement error that could come from differences between reviews and directly surveyed experience. Therefore, we included a module on our online survey (introduced

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[4] We use exclude phrases to identify false-positive hits for dictionary words and remove those misclassified reviews from the given social relation category counts. E.g., "friendli" is a keyword in the amenity ties dictionary, and "dog friendli" is one exclude phrase for "friendli": we won't classify a review as suggesting amenity ties for the occurrence of "friendl" when "friendly" is embedded in the bigram "dog friendly."

[5] Accuracy rate refers to the proportion of reviews for which dictionary and human codes fit (Grimmer and Stewart 2013). Despite its prevalent use in classification evaluation, this metric could be misleading, especially for imbalanced data. Precision, the proportion of true dictionary hits according to human codes among all dictionary hits, and recall, the proportion of true dictionary hits among all true hits according to human codes, can more accurately diagnose the two types of error. F1, the harmonic mean of precision and recall, is generally considered the best overall metric for classification evaluation. In the classification literature, 0.70 is the rule of thumb for good fit (Nelson et al. 2021).

above and discussed in app. C) in which we asked respondents to fill in a text review of their current or most recent employer. In designing this module, we copied the wording of the Glassdoor review portal exactly and copied the formatting as closely as possible. This module came prior to the direct survey questions mentioned above.

We use these simulated job reviews to compare text-based measures to the direct survey questions about social relations that we also included on the survey. We were particularly focused in this exercise on identifying false positives, or the presence of a social relations keyword when the opposite social relation was felt. For instance, if a respondent wrote "supervisor cares" in the review section, and we found that the survey response indicated that the individual did not think their supervisor cared about them, we would be concerned that we were detecting a false sentiment with our dictionary.

Appendix table A9 shows the results of this exercise, which reveals two points. First, our text-based measures have high precision and rarely include false positives. When the dictionary measure picks up a social relation, the direct survey responses usually indicate that social relation too. Second, however, our measures have a high rate of false negatives. This occurs because many reviewers do not discuss social relations, unprompted, in their reviews. This issue is ubiquitous, although underdiscussed, in research approaches that use unstructured text data to measure topics that are covered but unprompted by direct survey questions. In the robustness tests below, we propose a simple approach to testing for bias from these false negatives in which we compare wage models fit on the direct survey responses to our main models fit using the text-based measures.

More broadly, this comparison implies that when we do measure social relations, we pick up social relations that were particularly salient for workers. Respondents who do believe various types of social structure are present in their workplace, and report as much when asked directly, frequently do not think to mention social relations in an unstructured job review. This may mean that implicit comparisons shape our results. For example, workers who move from atomized to friendly workplaces may be more likely to comment on their friendly coworkers. Likewise, it may mean that workplaces that emphasize social interaction and social skill increase the salience of social relations. We test for bias from this source below by controlling for the social tasks and skills listed in firms' job postings. Nonetheless, the asymmetry between false positives and false negatives in these kind of, increasingly available, text data should be a target for future methods research.

Table 1 summarizes the resulting top words and phrases for the social relation categories. The full lists of words (see app. D4) for each category vary in length, but all have detailed sets of exclude phrases to focus on real instances of social relation discussion in the reviews. Appendix D2 explains the rules used to code reviews according to the dictionary phrases for each

TABLE 1
Key Phrases for Review-Based Codes

Category	Top Phrases
Employer Investment	support, care employe, respect, access, genuin, assist, realli care, transpar, feedback, mentor
Amenity Ties	friendli, famili, nice peopl, friend, welcom, environ, peopl nice, fun place, new peopl, pleasant
Social Closure (Pros)	depart, great team, network, connect, peer, good team, group peopl, inclus, team great, union
Social Closure (Cons)	depart, polit, side, drama, gossip, poor communicate, cliqu, club, high school, nepot

category. We present example reviews for each social relation category in appendix D5. These classifications are not exclusive. For example, a review could indicate both employer investment, when discussing a caring manager, and amenity ties, when discussing friendships among horizontal coworkers.

Finally, to validate that our concepts were distinct, we conducted a principal component analysis of the survey questions we asked the survey respondents. The analysis revealed two main principal components across which our 12 survey questions varied (see app. D3). The first component distinguishes consensual from oppositional relations and explains 57% of the variance in responses. This component distinguishes social closure from the more collaborative relations of amenity ties and employer investment. The second principal component distinguishes vertical relations and horizontal relations and explains 24% of the variance. This analysis suggests that the social relations concepts should not be further collapsed, as they pick up distinctive differences in how respondents answered survey questions. Moreover, the principal component analysis results roughly correspond to our theoretical framework in figure 1. We construct our final firm by year-level measures of social relations by aggregating these coded reviews up to the firm by year level. We do this for each social relation type by taking the share of reviews that discuss that social relation in each firm by year. We then drop any firm by year with fewer than five text reviews to ensure that we have more than a handful of reports on social relations for each firm by year (table A22 shows similar results dropping firm-years with fewer than 50 text reviews). We include reports from both current and former employees (table A23 shows similar results keeping only current employees from both the text and pay reviews).

Additional Measures

Our key dependent variable, pay, is drawn from the Glassdoor pay reviews data. Reviewers are given the option of entering pay on an hourly, weekly,

monthly, or annual basis. Reviewers can also enter several types of supplementary compensation: cash bonus, stock bonus, profit sharing, sales commission, and tips. We convert all pay types to an hourly basis, assuming full-time-equivalent hours and full-year employment. Unfortunately, the data do not include hours or weeks worked, which introduces measurement error for annual reporting respondents who do not work full-time. For our main pay measure, we sum hourly base pay and hourly-level versions of the supplementary compensation types. This gives us a broadly comparable pay measure, which includes types of compensation typically undercovered in standard labor market survey data.

In addition, we construct several firm-level control variables. We include logged revenue to control for differences in firms' resource availability. We also include logged employment to adjust for organization size. We will discuss how we implement other types of control with fixed effects in the next section.

We also incorporate individual-level human capital controls into some of the models, including standard Mincer controls for age, age squared, education, years of relevant experience, and gender (Mincer 1974). These fields are not required for Glassdoor users, so they are missing for around three-quarters of the total sample. As such, we use these only in supplementary models, which we explain in more detail below. Table A1 provides descriptive statistics for the independent, dependent, and control variables discussed above. Table A2 presents a correlation matrix for variables in the main sample.

METHODS

We use these measures to study how pay varies with workplace social structure. In our main analysis, we estimate this pay effect, conditional on a series of controls:

$$\ln y_{i,t} = \alpha_{o,k,t} + \alpha_s + \beta SC_{f,t} + x'_{f,t}\gamma + \epsilon_{i,t,} \tag{1}$$

where $SC_{f,t}$ is the set of social relations, measured with a dictionary classification of review text and averaged across reviewers within firm f by year t groupings. Showing how pay, $\ln y_{(i,t)}$, for worker i in year t varies with these firm-level measures of social relations provides an initial test of the idea that variation across firms in workplace social relations can account for pay inequality. We cluster standard errors at the firm level in all models to account for our firm-year level social relations measures and for autocorrelation within firms over time.

Variability in firm pay could arise from a variety of sources that could also affect the distribution of social relations across firms. First, some firms employ higher-level occupation workers, which will clearly be associated with

higher pay. These workers could also be more likely to report employer investment, which would lead to a spurious positive association between that social relation and pay. Second, different firms could be located in different labor markets. Some labor markets pay more for certain types of workers and could also be associated with certain types of social relations, as in the political science literature on geography and social capital (Putnam 2000). We address both these occupational composition and local labor market confounders with an occupation by city by year fixed effect, $\alpha_{o,k,t}$, which restricts comparison to social relations among similar jobs in the same local labor markets. Specifically, we use six-digit SOC codes to capture considerable granularity in occupation, and the Glassdoor metro area variable, which includes both metropolitan and micropolitan statistical areas. These fixed effects net out average differences in pay across local labor markets for specific occupations.

Second, consistent with prior research on firm pay premiums, firms in different product markets and with different formal organizational structure could pay differently. Likewise, these firms could have different social relations: Perhaps a small firm is more likely than a large one to facilitate employer investment and commitment, or a service firm more likely than a manufacturing firm to be marked by amenity ties. We adjust for these differences by including α_s, which we define as three-digit NAICS industry categories crossed with organization type. Organization type is a Glassdoor classification that distinguishes publicly traded, privately held, nonprofit, and other broad organizational types. We also include a vector $x'_{f,t}$ of firm-year-level controls for log(revenue) and log(employment).

Taken together, these fixed effects and controls allow us to estimate the effect of firm-level social capital on pay, conditional on differences in labor market tightness (via the occupation by city by year fixed effects), occupational skill, firm size, and industry. However, these models, which can be estimated on our main pay reviews sample, still suffer from substantial unobserved worker-level heterogeneity. Higher-paying firms may be higher paying because they select for higher-skill workers, even within occupations. These workers could then bring different social relations to the firm.

We follow two strategies to check for this issue, both of which require shifting to subsample analyses. First, we add the human capital controls noted above: age, age squared, highest degree completed (high school, associate's, BA, BA+), gender, and years of relevant experience. These controls are standard measures of individual worker skill, which allow us to adjust for differences in work experience and formal education. Coupled with the occupation and local labor market fixed effect, this specification controls for a rich set of observable differences among workers.

However, there could still be unobserved differences among workers, even in the same occupation and with the same education level. To test for this, we draw on the subsample of reviewers who fill out reviews for more

than one employer. Glassdoor provides a consistent user ID that allows these workers to be tracked across each of their employers. As such, we can test the change in workers' pay when they switch across firms characterized by different types of social relations. This worker fixed-effects model adjusts for time-invariant unobserved heterogeneity among workers at different firms.

Similar to workers, there could also be unobserved differences among firms that are not accounted for by the industry, organization type, and occupational composition controls included in the main models. As such, in a final model, we add firm fixed effects to adjust for time-invariant, unobserved differences in pay and social relations between firms. This final specification estimates the association between year-to-year pay changes and year-to-year changes in measured social relations. Unfortunately, as in other settings with panel data and imperfectly measured independent variables (Freeman 1984), this firm fixed-effect approach substantially exacerbates measurement error in our social relations measures. The reason for this is that the year-to-year fluctuations in measured social relations likely have a larger share of noise than does the fixed component of the measure, which is stable across time. This measurement error issue is likely to bias the social relations effect toward zero.

But we include this firm fixed-effects model in supplementary analysis because the unobserved firm-wide heterogeneity it adjusts for could bias our wage effects estimates in the main sample models. This approach also harnesses the repeated observations of firms across years in the Glassdoor data. Prior research linking specific firm characteristics to firm pay premiums has often relied on cross-sectional measures of those characteristics, as in analyses of pay premiums as a function of structured management practices (Bloom et al. 2021) or high-performance work systems (Osterman 2006). We see this firm fixed-effects approach, while imperfect, as an improvement on these prior studies of firm pay premiums.

DESCRIPTIVE FINDINGS

We motivate this study as an attempt to account for inequality in firm pay premiums. The study of firm premiums presupposes two empirical regularities: differences between firms in pay for similar jobs and a substantial correlation in relative pay between different job types in the same firm. Does this pattern hold in our data?

In the first panel of figure 2, we divide all jobs into high- and low-pay occupations, splitting at the median average occupation pay level. We then compute firm-level averages for high- and low-pay occupations separately. Figure 2 plots those firm-level averages. Three points emerge. First, unsurprisingly, high-pay occupations are consistently paid more than low-pay occupations at the firm level: The vast bulk of the data is above the identity
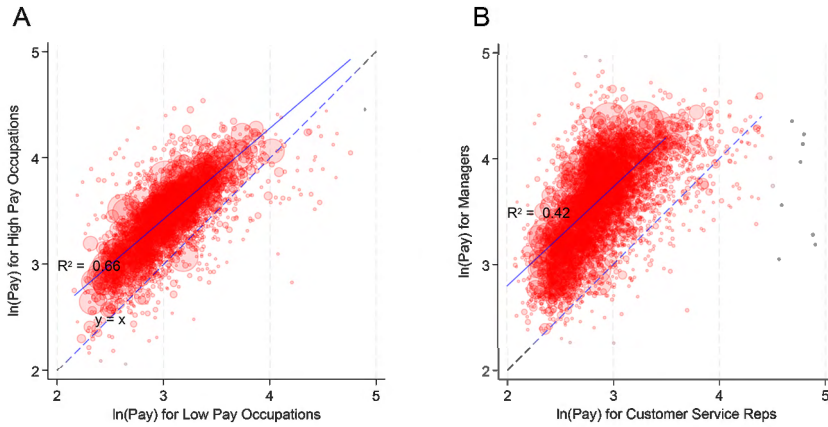
Fig. 2.—Defining firm pay premiums. Data from Glassdoor. For plotting, we exclude observations below 2 and above 4.5 logged wages and plot a 10% random sample of the total data. R^2 is based on full sample.

line. Second, there is substantial variation across firms in pay for both high- and low-pay occupations. Third, there is a strong correlation ($\rho = 0.66$) in firm-level relative pay for high- and low-pay occupations. In other words, the firms that pay high wages to low-pay occupational jobs also pay relatively well for jobs in high-pay occupations.

In the second panel of figure 2, we conduct a similar exercise for two of the most common jobs found in the Glassdoor data: managers and customer service representatives. This comparison again shows that workers in the higher-pay occupation (managers) are consistently paid more than cus- tomer service representatives in the same firm. In this specific comparison, managers have a wider range of pay across different firms, while customer service representatives have a narrower range, in which even the highest- paying firms are not paying representatives more than around $30 per hour. However, even here there is a strong correlation between pay for the higher- and lower-pay occupations across firms ($\rho = 0.42$). These charts show that, beyond occupation, where you work matters: Working at a high-paying firm brings higher pay across job titles.

Next, figure 3 turns from pay to firm premiums specifically. These firm premiums are estimated conditional on occupation fixed effects to track the firms that pay relatively more or less than expected given their occupa- tional composition. Figure 3 shows binned scatter plots illustrating the bivar- iate correlation between social relations and firm pay premiums. Discussion of both employer investment and social closure are positively correlated with pay ($\rho = 0.15$ and $\rho = 0.13$, respectively). In contrast, discussion of friend- ship and other amenity ties is negatively correlated with pay ($\rho = -0.33$).
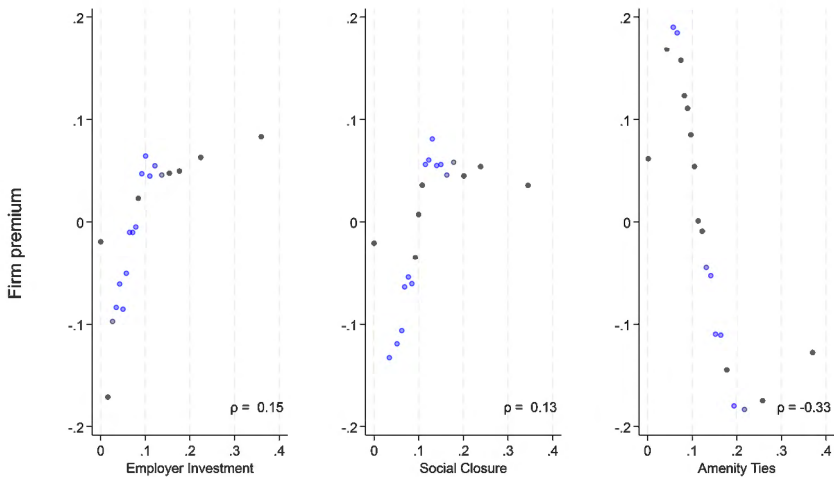
F<small>IG</small>. 3.—Firm pay premiums by review discussion of social elations. Data from Glassdoor. Firm pay premiums are estimated conditional on firms' occupational composition. Social relations are measured at the firm-year level and binned into 20 equal-sized groups. OES weights are applied.

The social relations variables all have outliers near the 0 value, consistent with measurement error from false negatives suggested by the comparison of text to survey measures above. Despite this, the measures capture meaningful variation in firm premiums.

SOCIAL RELATIONS WAGE MODELS

Table 2 shows a series of regression results demonstrating how pay varies with workplace social relations. Model 1 shows that, consistent with figure 3, both employer investment and social closure are positively associated with worker pay, while amenity ties are negatively associated. Together these three text-based variables by themselves account for 8% of the variation in pay in the Glassdoor sample.

Next, in model 2, we add fixed effects for city by year by occupation and for industry by organization type. These fixed effects explain around half of the remaining variation in pay and substantially attenuate the wage effects of the social relation measures. However, the coefficients on each type of social relation remain qualitatively consistent, even comparing across similar jobs and removing industry-specific pay premiums. In model 3, we add controls for number of employees and revenue, and the social relations coefficients remain consistent. For example, a 10-percentage-point higher rate of firm-year-level worker reports of employer investment is associated with wages that are 4% higher. Note that the sample underlying these models

TABLE 2
WAGE EFFECTS OF FIRMS' SOCIAL RELATIONS

	Main Sample			Alternative Controls		
	(1)	(2)	(3)	(4)	(5)	(6)
Employer investment	1.232***	.343***	.382***	.373***	.164***	.033***
	(.063)	(.013)	(.015)	(.018)	(.010)	(.005)
Social closure	.720***	.262***	.288***	.259***	.112***	.016***
	(.073)	(.013)	(.014)	(.017)	(.009)	(.005)
Amenity ties	−1.812***	−.381***	−.361***	−.309***	−.173***	−.042***
	(.069)	(.014)	(.016)	(.018)	(.011)	(.005)
log(number of employees)			−.000	−.000	−.000	
			(.002)	(.002)	(.001)	
log(revenue)			.008***	.006***	.002**	
			(.001)	(.001)	(.001)	
Age				.020***		
				(.001)		
Age2				−.000***		
				(.000)		
Associate's degree ...				−.010*		
				(.004)		
BA				.067***		
				(.003)		
BA+				.182***		
				(.004)		
Years of relevant experience				.020***		
				(.000)		
Female				−.076***		
				(.002)		
Constant	3.075***	3.037***	2.860***	2.281***	3.109***	3.044***
	(.019)	(.004)	(.017)	(.026)	(.013)	(.001)
R^2	.08	.56	.57	.66	.93	.61
Fixed effects:						
City × Year × Occupation		X	X	X	X	X
Industry × Organization Type		X	X	X	X	
Reviewer					X	
Firm						X
Within R^2	.082	.007	.009	.123	.002	.000
N	12,863,612	12,465,314	11,163,955	1,410,585	2,222,193	11,162,037

NOTE.—Data from Glassdoor. Standard errors clustered at the firm level. Sample size varies due to exclusion of singletons from fixed-effects models and exclusion of missing data from human capital controls model. OES weights included.

* $P < .05$.
** $P < .01$.
*** $P < .001$.

varies somewhat, due to singletons emerging from increasingly stringent fixed-effects inclusion. Table A24 shows similar differences in coefficients with a minimal consistent sample imposed.

While these core models show a consistent role for variation in firm-level social relations in predicting pay levels, they leave several sources of potential omitted variable bias. In the remaining models in table 2, we use subsamples of the Glassdoor reviews to address these issues. First, model 4 adds standard Mincer wage controls for age, age squared, education levels, gender, and years of relevant experience. These human capital controls are collected by Glassdoor but are not mandatory for reviewers to provide. As such, adding these controls leaves only a quarter of our sample from the main models. However, even conditional on worker-level human capital, social relations variables continue to predict pay.

Next, model 5 adds reviewer fixed effects. The reviewer fixed effects address the concern that the main estimates could be driven by unobserved worker heterogeneity: If higher-ability workers sort to firms with high employer investment or high social closure, then apparent wage premiums associated with workplace social relations may really reflect differences in worker ability. A small minority of Glassdoor reviewers fill out multiple reviews (around one-tenth of the main analytical sample). This allows us to compare pay for the same reviewer at high and low social relation jobs. Workplace social relations effects attenuate by between one-half and one-third but persist even in this switchers design.

Finally, in model 6, we add a firm fixed effect for each employer identified in the data. This model estimates pay variation based on within-firm, across-year changes in review-measured social relations at the firm level. It is therefore useful for assessing whether unobserved heterogeneity across firms is driving the apparent association between social relations and pay. Model 6 shows that while the estimates remain qualitatively consistent with the other models, they substantially attenuate. As noted above, we find this model difficult to interpret, as it will be particularly sensitive to the measurement error in the text-based social relations variables. Nonetheless, it provides a lower bound estimate of the true effect on pay of year-to-year changes in workplace social relations. As in the main models, these additional tests are estimated off of varying samples: table A25 shows the coefficients for model 3 with the relevant samples imposed for comparison purposes to models 4, 5, and 6.

SOCIAL RELATIONS AS A SOURCE OF INEQUALITY IN FIRM PAY PREMIUMS

The models in table 2 provide increasingly stringent tests of the idea that social relations, net of other relevant determinants of pay, affect wages. Our key question, however, is not how well social relations predict earnings in

general but whether they can account for firm pay premiums. To quantify the contribution of social relations to variability in firm pay premiums and to overall pay inequality, we next conduct a simple variance decomposition.

We first estimate a baseline two-way occupation and firm fixed-effects model, as discussed in Wilmers and Aeppli (2021) and analogous to a two-way worker-firm Abowd, Kramarz, and Margolis (AKM) model (Abowd et al. 1999):

$$\ln y_{i,t} = \alpha_{o,t} + \alpha_{f,t} + u_{i,t} \tag{2}$$

This model gives a vector of occupation premiums $\alpha_{o,t}$ conditional on the firms those occupations are employed by and a vector of firm premiums $\alpha_{f,t}$ conditional on each firm's occupation composition. While the operationalization of firm premiums varies in the prior empirical literature, it always controls for some measure of job type or worker skill level. This initial model estimates firm premiums that are interpretable as whether a firm pays relatively high or low wages for the kinds of jobs it employs. This model adjusts for the raw differences in job composition between, say, a large technology company and a small restaurant. If the restaurant pays its servers and cooks generously relative to that job type, then it could have a higher firm fixed effect than a technology company that employs many engineers but does so at the bottom of the labor market for that job. This operationalization of firm premiums thus captures the key idea that different firms' pay policies deviate from a single market price for a given type of job.

Next, we use the terms estimated in the two-way fixed-effects model to decompose pay inequality into components due to occupation, firm, their covariance, and a residual component:

$$V(\ln y_{j,t}) = V(\alpha_{o,t}) + V(\alpha_{f,t}) + 2\mathbf{Cov}(\alpha_{o,t}, \alpha_{f,t}) + V(u_{i,t}). \tag{3}$$

This variance decomposition distributes overall earnings inequality across pure between-occupation pay differences, net of effects from occupations appearing at higher- or lower-paying firms; inequality due to firm pay premiums; and the covariance, or sorting, component of inequality, which results from high-pay occupations being employed disproportionately at high-paying firms (Wilmers and Aeppli 2021). The decomposition also includes residual variance, which results from pay differences among coworkers in the same jobs and from firms that pay certain occupations more than would be expected from their firm and occupation premiums. For our purposes, inequality in the firm premium is our main interest.

We then compare this observed decomposition with a counterfactual decomposition, which shows inequality levels excluding wage variation due to the distribution of workplace social relations. To do this, we estimate a counterfactual version of the variance decomposition in which we first residualize earnings using our text-based social relations measures:

$$\ln y_{i,t} = \beta SC_{f,t} + \epsilon_{\{i,t\}}. \tag{4}$$

Note that because we include $SC_{f,t}$ prior to any other controls, this approach provides an upper-level estimate of the role of social relations in accounting for inequality in firm premiums. We then use the residualized earnings measure from equation 4 to estimate a second version of the two-way fixed-effects model and the variance decomposition, comparable to equations 2 and 3, but predicting pay already residualized by the social relations predictors, rather than unadjusted pay:

$$\epsilon_{i,t} = \alpha_{o,t}^{cf} + \alpha_{f,t}^{cf} + u_{i,t}^{cf}, \tag{5}$$

$$V(\epsilon_{i,t}) = V(\alpha_{o,t}^{cf}) + V(\alpha_{f,t}^{cf}) + 2\mathbf{Cov}(\alpha_{o,t}^{cf}, \alpha_{\{f,t\}}^{\{cf\}}) + V(u_{i,t}^{cf}). \tag{6}$$

Comparing the decompositions in equations 3 and 6 shows how the distribution of social relations across firms affects inequality.

The first panel in figure 4 gives the results of these decompositions. The left bars show that in the Glassdoor data, both occupation and firm premiums are quantitatively important: When entered simultaneously as predictors of wages, each account for around 10% of overall pay inequality. The right bars indicate the counterfactual residual pay variance. Comparing the firm premium in the real and counterfactual decompositions indicates that the distribution of social capital accounts for around 10% of inequality in firm pay premiums. Social relations also disproportionately characterize the firm pay premiums that higher-wage occupations benefit from. As such, it also contributes to inequality by increasing the covariance between occupation premiums and firm premiums.

This decomposition suggests an upper bound on the contribution of these social relations to accounting for inequality in firm pay premiums. However, it does not control for the formal organization and production differences between firms that have been emphasized in prior research. So, in the second panel of figure 4, we focus on firm premiums and incrementally layer in additional controls, each estimated prior to the social relations residualization. The first bar shows that the distribution of workplace social relations accounts for 20% of overall between-firm inequality, or the component of variance accounted for by firm premiums absent other controls. Adding the occupational composition adjustment described above reduces this role to 17%. Results are similar when we control for city, to account for differences in local labor demand. In the last bar, we show a fully saturated model, which controls for the industry, organization type, firm size, and firm revenue variables described above. These adjustments account for many of the formal organizational and product market differences between firms, as emphasized in prior research. Including these controls does substantially reduce the residual variance in the
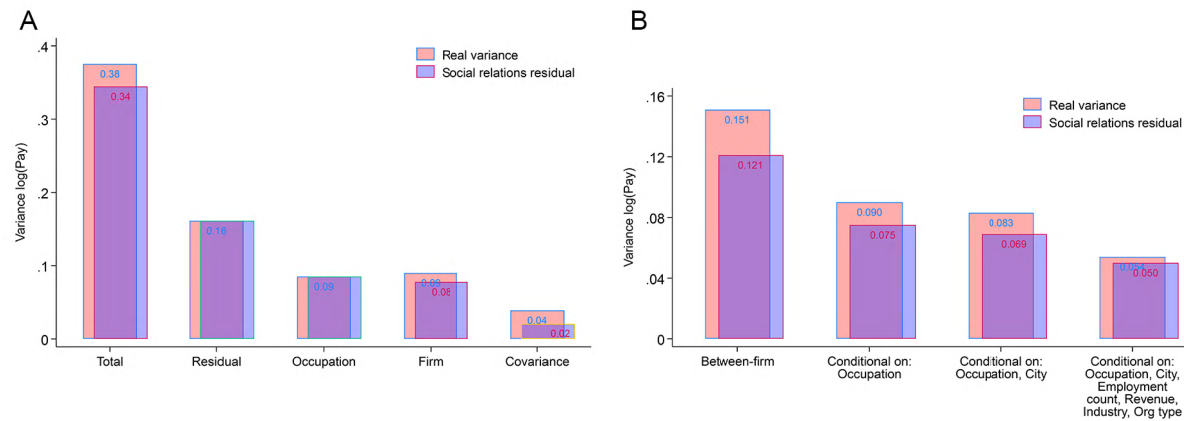
Fig. 4.—Counterfactual variance decomposition. Data from Glassdoor. The left panel shows variance decomposition across firm and occupation premiums, described in equation 2 for baseline or real variance and equation 3 for counterfactual or social capital residualized variance. The right panel shows results for between-firm variance only, with a series of additional controls for occupation, city, and firm characteristics. OES weights are applied.

firm premiums by nearly half. However, social relations continue to explain variation within this residual and account for around 7% of these within-industry and firm size residual premiums.

ROBUSTNESS CHECKS ON CAUSAL INFERENCE

In the main results above, we rely on a series of increasingly stringent models to rule out potential confounds in our estimates of the effect of social relations on inequality. However, with these observational data, substantial uncertainty remains about whether our estimates can be interpreted as causal parameters. Ideally, we would identify exogenous variation in social relations that are excludable with respect to pay setting. Unfortunately, we have not been able to identify such variation. Instead, we conduct several tests to assess specific threats to inference.

Reverse Causality

First, reverse causality is a particularly plausible problem for the interpretation of our models. If an employer raises wages, that might lead workers to view their employer as invested in them. It might also generate rents that workers then organize solidaristically to capture. In these cases, changes in pay would drive social structure, rather than the reverse. Ideally, we would test for this possibility with an instrument for social relations that does not otherwise affect pay. However, as a second-best approach, we study whether sharp changes in pay affect social relations. Specifically, we study the effect of voluntary minimum wage increases on social relations. These voluntary minimums were adopted by large retailers in response to idiosyncratic operational and turnover problems, largely stemming from tightening labor markets, rather than workplace social structure (Weil and Derenoncourt 2024). We draw a list of voluntary minimum adoptions by companies from a working paper by Wilmers, Roh, and Tang (2024a), which includes Walmart, Target, Gap, Costco, Amazon, Bank of America, Wells Fargo, U.S. Bank, and Aetna.

Table A11 shows that workplace social relations are not highly responsive to voluntary minimum wage increases. Model 1 establishes that, consistent with Wilmers et al. (2024a), adoption of a corporate minimum wage is associated with a 6.5% increase in pay for covered low-wage workers, relative to workers in the same city, year, and occupation. However, in models 2, 3, and 4, we predict workplace social relations with minimum wage adoption and find precisely estimated null effects. Taking the top of the confidence intervals for the social closure and employer investment estimates and the bottom for amenity ties, we plug them into the coefficients estimated in our most stringent table 2, model 6, firm fixed-effects estimates. This allows us to estimate

upper bounds for the share of the social relations wage effects attributable to reverse causality: 1.7% (employer investment), 1.5% (social closure), 2.1% (amenity ties). Of course, social relations could be more responsive to other types of wage increases, perhaps targeting the top or the median worker. However, it is reassuring that these voluntary minimum changes—which are publicly announced and can be cleanly identified—do not yield large changes in social relations in the workplace.

COVID-19 Shock

Another imperfect approach to causality is offered by the COVID-19 pandemic. This sharp change plausibly reduced the importance of social interaction and social structure in the workplace. Unfortunately, it is clearly not excludable with respect to pay. The pandemic and policy responses unleashed many changes to labor markets and organizations that crucially affected pay. However, we still see it as a useful event, under the following identification assumption: Comparing within the same occupation, labor market, and year, general pandemic labor supply and demand effects should impact pay relatively evenly across workplace social structures. However, through its effect on social structure, it could have opposing effects on pay. By reducing amenity ties, it may force pay increases. By reducing employer investment and solidarity, it may allow pay decreases. This heterogeneity in firms' responses to the pandemic provides predictions over and above the broader labor market disruptions introduced by the pandemic.

In table A12, we first predict the rate of workplace social relations with an indicator for post-2019. Each type of social relation declined during the pandemic. Note that, per appendix figure A4, social relations are declining steadily over time, but the pandemic shock brought an additional level reduction. This survives a control for the overall calendar year trend. Next, we estimate how pay changed for firms that had high rates of each social relation type prior to the pandemic. Model 4 shows that, relative to other workers in the same city, year, and occupation, workers in firms that had high employer investment and high solidarity prior to the pandemic saw relative wage declines after 2019. In contrast, workers in firms with high amenity ties experienced relative wage increases after 2019. These results suggest that a large disruption to workplace social relations had divergent pay affects, depending on the predominant type of social relation at a firm.

Controls for Work Tasks

Beyond these general approaches to establishing causal direction, we also focus on a key alternative explanation: that the technical and task composition of work could affect both social structure and wages. Specifically, some jobs

are highly relational—like retail service work or management consulting—while others are quite isolated. In the limit case of the latter—lighthouse keepers, perhaps—there is no possibility for social structure in the workplace. If variation in these task-driven aspects of workplace social structure persist within industry and organization type, and are associated with wage differences, they would be an omitted variable in our models in table 2.

To test for this, we draw on Burning Glass online job postings, which include information about job requirements and tasks. We build on prior research by Nelson, Wilmers, and Zhang (2024), which survey-coded the most common 2,000 tasks listed in job postings. Nelson et al. (2024) asked respondents to code whether a task is complex and typically learned on the job or off the job (relative to simple tasks) and whether the task is high autonomy or relational/social tasks (relative to easily supervised tasks). We take these survey-coded tasks, aggregate to the firm-year level, and then match the resulting task scores into our Glassdoor data. In addition to our usual inclusion criteria, we include only Glassdoor firm-years that were matched to at least five Burning Glass job postings.

Table A12 adds these task controls to the full set of models in table 2. Complex tasks and high-autonomy tasks consistently predict higher pay, while relational tasks generally predict low pay. These controls attenuate some coefficients, particularly for the baseline model, but magnitudes and significance levels remain largely consistent with those presented in table 2. This suggests that social relations matter for pay setting over and above technical differences in job tasks.

Comparing Amenity Ties to Fairness Norms

A second specific objection lies in our interpretation of amenity tie negative effects on pay. We argue that these negative effects reflect compensating differentials for a valued job amenity. However, an alternative interpretation invokes fairness norms: Strong ties in the workplace plausibly make it harder for employers to pay dispersed wages. In such a circumstance, employers may reduce both the mean and variance of wages to avoid violating fairness norms.

To distinguish amenity ties from fairness norms, we draw on two competing predictions that each theory makes about workers' subjective satisfaction with their pay. If compensating differentials and amenity ties holds, workers should be unsatisfied with their low pay. They like other parts of the job (specifically, their relationships with their coworkers) but should notice that they are underpaid relative to the broader market and relative to their productivity. They stay at their job despite the pay, not because of it. In contrast, when employers respect fairness norms, workers should on average be no less satisfied with their compensation, Indeed, the whole point

for employers respecting these norms is to avoid worker dissatisfaction with their pay.

Second, under a fairness norms regime, there should be meaningful heterogeneity by wage rank in worker pay satisfaction. Due to variance reduction, high-pay workers are underpaid while low-pay workers are overpaid. So we should expect a gradient of satisfaction with fairness norms: High-ranked workers within a firm should be less satisfied relative to workers in similar positions in other firms, while low-ranked workers should be more satisfied. The compensating differentials mechanism of amenity ties, in contrast, implies no such gradient.

To test these ideas, we match our merged pay dataset back into the Glassdoor text reviews data, matching on reviewer user ID and employer ID. This allows us to link individual reviewers' subjective assessment of their own compensation to their within-firm pay rank. Models 1, 2, and 3 of table A14 swap out pay as a dependent variable from the setup in table 2, replacing it with individual workers' five-point Likert scale rating of "Compensation & Benefits." Consistent with the job amenities theory, amenity ties are consistently associated with lower satisfaction with pay, even controlling for city × year × occupation and industry × organization type fixed effects. Next, model 4 interacts amenity ties with quintile pay rankings calculated for each employer-year. Model 4 shows that amenity ties predict lower satisfaction for the bulk of the firm rank distribution, including the bottom. Contrary to the fairness norms interpretation, the highest quintile actually has the least negative coefficient relative to workers in the same earnings rank in firms with low amenity ties, and top-ranked workers in high amenity ties firms express the least dissatisfaction with compensation. Together, these results suggest that amenity ties is a more plausible interpretation of our results than fairness norms.

Causality and Self-Report Data

Another aspect of the causality problem is introduced from the potential gap between our text-based measurement of social relations and the underlying reality of workplace social structure. Perhaps wage increases lead to more discussion of employer investment and solidarity but do not actually create more of either. In a simple version of this story, the checks we conduct above should address this. For example, if a voluntary minimum wage increase spurred workers to talk more about solidarity, then we would pick that up in our analysis. But there are more complex possibilities too. Perhaps workers in low-wage jobs emphasize amenity ties as a way to psychologically compensate for the low status associated with low pay. The fact that employers can be expected to read reviews provides some incentive compatibility for accurate reporting in this case: A low-wage worker excusing her stingy

employer risks reinforcing low wages. That is a high price to pay for anonymous self-justification.

In addition to the generic risk of inaccurate reporting, it is worth considering two specific cases of self-report distortion that could bias our estimates of wage effects. First, workers could attempt to rationalize their low pay in a job review by highlighting the benefit of friendships and amenity ties in the organization. However, this is inconsistent with the finding that reports of employer investment are associated with higher pay: Workers could just as plausibly rationalize away lower wages by asserting that managers are caring and supportive. Second, workers could attempt to intimidate their employers into giving them higher wages by describing social closure and oppositional solidarity in the workplace. In this case, it may not be the underlying social relation but rather its exaggerated depiction that drives a pay increase.

In general, we expect that reviews that use more subjective and less descriptive language are more likely to exhibit rationalization or strategic use, consistent with prior research on narrative and moralization (Ewick and Silbey 1995). For instance, discussion of employer investment evidenced by workers' high average tenure is mainly descriptive, while stories of "employers caring" or stories about managers' character are more moralized. Subjective language in the review text may represent pleas for potential employees or current employers to take the reviewers' concerns seriously (Ahmed 2021). We attempt to demonstrate robustness to social relations subjectivity by controlling for review positivity (see "Adjusting for Review Positivity") and dropping phrases and reestimating our models (see "Sensitivity of Dictionary Measures").

But notwithstanding these checks, our entire analysis is limited to self-reported data. It could be that minimum wage increases do spur oppositional solidarity but that workers, for fear of appearing ungrateful for a pay increase, do not emphasize the growth of solidaristic factions in their reviews. Ultimately, in these checks on causality, as elsewhere in our analysis, we make the strong assumption that workers write reviews descriptively and that departures from this usage are uncorrelated with pay, conditional on controls.

ROBUSTNESS CHECKS ON GLASSDOOR DATA SELECTION

Our main results suggest that variation in the incidence of social relations accounts for a quantitatively substantial portion of inequality between firms. However, there are complex sample and measurement issues attendant to the job reviews used in the analysis. In the following subsection, we discuss some robustness checks that assess these issues, which we summarize in table 3.

TABLE 3
DISTINCT TYPES OF SELECTION IN THE GLASSDOOR DATA

Selection Problem	Testing Approach	Results
Selection into filling out Glassdoor reviews	Reweight data with OES weights	Tables A3, A4
	Compare demographics for survey respondents who do and do not report online job reviewing	Table A5
	Robustness check, pay model fit on survey responses, splitting sample by reviewers and nonreviewers	Table A6
	Robustness check, exclude reviews that are likely company solicited	Tables A18, A19, A20
Selection into discussing social relations in a review	Robustness check, pay model fit on direct survey responses	Table A6
	Comparing discussers to nondiscussers conditional on survey response	Table A7
Selection into discussing social relations using our dictionary words	Iterated, manual codings of dictionary-classified samples to identify false positives and false negatives	Table A8
	Comparison of direct survey responses to classification of mock job reviews	Table A9
	Robustness check, bootstrap dropping individual common words	Figure A5
	Robustness check, dropping uncommon words from dictionaries	Table A15

Comparing Text-Based to Survey-Based Measures

The text-based measures used in our main analysis have substantial measurement error: Many reviewers do not discuss social relations, even if they would report the presence of these relations if prompted with direct survey questions. Table A7 shows that among respondents who report social relations in survey questions, there is some selection by education and firm size into reporting on social relations in the text reviews. To test whether this biases our estimates, we draw again on the online survey introduced above and discussed in appendix C. We estimate additional wage models, relying on survey-based measures of workplace social relations instead of our text-based measures.

Our survey sample is not large enough to include the high dimensional fixed effects that we use in table 2. However, we estimate otherwise similar worker-level wage models, shown in table A6 in appendix C. In a first unconditional model 1, we predict hourly pay using social relations measures derived from averaging the Likert rating survey questions for each social relations type (discussed in more detail in app. C). Model 1 shows that the positive association between pay and both employer investment and social closure persists in the survey-based model. Likewise, amenity ties are

negatively associated with pay. Model 1 also shows that the overall variance in pay explained by social relations in the survey measures is similar to the main text measure models: around 6%. Model 2 adds human capital controls, and model 3 adds broad, two-digit industry fixed effects. Both have qualitatively similar results. These tests suggest that nonrandom measurement error in the text-based measures is not driving the patterns we see.

Sensitivity of Dictionary Measures

Our results could be sensitive to exactly which phrases are included in the dictionaries. To address this concern, we ran two robustness tests. First, we ran our main model with reconstructed social relation measurements based on three sets of shorter dictionaries that only retain the top 10%, 20%, and 50% phrases occurring most commonly. This check assesses whether our results are driven by rare dictionary words that might have a strong association with pay. Table A15 in appendix F1 shows that all coefficients on the social relations variables remain qualitatively consistent with the main results.

Second, we test whether our results are determined by any one of the core dictionary phrases that account for most of social relations occurrences. For this purpose, we reran our main model estimations for 30 rounds, in each of which we take out one of the top 10 dictionary phrases in building the three social relations measurements. Our results, in figure A5 in appendix F1, show that even leaving out these most common words, the coefficients are quite consistent.

Sensitivity of Analytical Unit

We aggregate social relations mentions to the firm level, disregarding meaningful workplace variation within large corporations that have geographically dispersed establishments. This decision is driven by a lack of establishment identifiers in the Glassdoor data. To address intrafirm disparities, we create firm-by-city-by-year measures of social relations, to approximate establishments. Table A16 in appendix F2 shows results that align with our firm-level analyses.

Adjusting for Review Positivity

Another concern about our text-based measures is that they may simply be picking up overall review positivity. Positivity could be associated with reports of any of the social relations types and also with high pay. To adjust for this, in table A17 in appendix F3, we control for a firm's overall rating and the average word count of its positive ("pro") review texts. Results in table A17, controlling for review positivity, are similar to those in our preferred models.

Assessing Selection into Reviewing

In addition to these issues with our text-based measures, our results could also be biased by selection into Glassdoor reviewing. The survey-based pay models introduced above are useful for assessing this issue. While we showed in table A5 that reviewers and nonreviewers had similar demographics and reported similar rates of social relations, the key question for our analysis is whether reviewers and nonreviewers exhibit the same relationship between pay and workplace social relations. We conduct a direct test of this issue by splitting the survey sample into respondents who reported online job reviewing and those who reported no reviewing. Models 5 and 6 in table A6 show that results for the subset of reviewers are somewhat stronger for employer investment and amenity ties but weaker for social closure relative to nonreviewers. The sample size for reviewers is small here, and estimates become noisy, but all results are qualitatively consistent across the samples and with the main analysis. The consistency in this test is reassuring here, and we see this split-sample approach as a promising method for assessing sample selection issues in online data more broadly.

Dropping Employer-Elicited Reviews

A more specific concern about selection into Glassdoor data is the potential for employers to elicit fake reviews from employees. We check our results for these concerns in three ways, detailed in appendix F4. First, we drop all five-star reviews for companies, assuming that employer-elicited reviews would be as positive as possible. Next, we use a crowdsourced measure of potential fake reviews by removing companies where the word "fake" comes in the text of the reviews. Real reviewers on Glassdoor sometimes accuse their employers of planting fake reviews. Finally, we exclude instances of suspicious review influxes, where the volume of reviews for a given month is higher than a third of a standard deviation from the previous month, as well as where the average review rating is over a third of a standard deviation from the previous month's average review rating. For smaller companies that had fewer than 12 reviews a year, we flagged months where there were five or more reviews as suspicious. We removed all reviews in years in which the firm had suspicious reviews. Our results are robust to each of these strategies for weeding out fake reviews.

DISCUSSION

There has long been interest among sociologists and organizational scholars in the ways that social organization of the workplace can contribute to inequality. Indeed, the postbureaucratic, post-Fordist workplace has been

characterized by various attempts to unevenly transform social relations in the workplace. We find that the distribution of social structures across firms tracks between-firm pay inequality, as some workers benefit from social closure and employer commitment, while others face concertive control via amenity ties. Specifically, these patterns account for around 20% of overall inequality in pay premiums between firms.

There are several limitations to our analysis. First, consistent with the literature on firm pay premiums (Barth, Davis, and Freeman 2018; Bender et al. 2018), we focus on quantifying the contribution of firm-level attributes to overall earnings inequality. We estimate a series of wage models, each of which presents different comparisons in pay between workers at firms with and without different types of social relations. However, the distribution of social relations across firms is likely correlated with other determinants of pay, beyond what we can adjust for effectively in our models. Future research in this area, and in the study of firm pay premiums more broadly, would benefit from compelling causal design. Unfortunately, most shocks to workplace social relations—like leadership changes, company mergers, or new work-from-home policies—are not excludable with respect to pay.

Second, we use text-based measures of social relations from online job reviews. This provides a unique opportunity, in large-scale wage data, to measure the rich workplace social relations concepts that we study here. However, throughout the article we emphasize the pitfalls and difficulties of the data that we use and summarize different species of selection issues in table 3. In our view, enthusiasm for these new data sources has often outpaced careful attention to these problems. We develop a series of checks on our approach, which could be useful techniques for other projects using similar online review data: iterated manual hand coding of samples of reviews to check the performance of each dictionary; gathering imitation reviews that allow us to compare direct survey questions to our dictionary-based measures; reweighting the data by industry and occupation composition in the overall US labor market; comparing surveyed demographics and our key variables for reviewers to nonreviewers; and fitting a similar wage model on survey-based data, splitting the sample into reviewers and nonreviewers. Overall, we are reassured by these tests that our reviews data are usable for this application.

Third, we focus on the extent to which differences in social relations across firms explain pay variation. We mainly leave aside the question of why firms are characterized by different types of workplace relations. This could be due to founding effects and imprinting, in which initial styles of interaction and commitment become durable social patterns (Baron, Hanna, and Burton 2001; Beckman and Burton 2008). Or it could be driven by homophilous sorting, in which workers with similar preferences for types of social interaction establish certain types of relations, but only when they are shielded from less prosocial free riders. Whether workplace social relations

are mainly determined by employer-side or worker-side supply and whether they are mostly fixed or mutable and fragile are critical questions. Our analysis simply demonstrates the importance of these types of social relations in understanding between-firm inequality. As such, it motivates further research on the determinants of employer investment, social closure, and amenity ties. Indeed, much research on these topics has either focused on single-company case studies or inferred informal social relations through rough, but available, proxies. Our work suggests that newly accessible free-text job reviews can be a useful source of data on social relations.

Notwithstanding these limitations, we make three contributions with this article. First, we extend prior research on social relations and inequality (Tomaskovic-Devey and Avent-Holt 2019). Organizational research on inequality and social relations has mainly focused on how informal social processes affect the distribution of rewards within organizations (Kanter 1977; Tilly 1998). More generally, sociologists have long argued that the distribution of social capital can drive inequality based on class (Bourdieu 1984) or race (Montgomery 1991; Pedulla and Pager 2019). However, these theories focus on how individual possession of social capital can sort privileged people into advantageous economic positions (like white workers relying on their weak ties to secure high-paying jobs). We extend this research to show how organization-level accumulations of particular types of social relations can create advantageous economic positions. In this way, social relations contribute to cross-sectional economic inequality, not just cumulative advantage or ascriptive inequality.

Our second contribution is methodological and involves substantial work to validate our approach to using job review data. We first document how these data compare to standard labor market survey data. We then develop a framework to measure potential selection on observable and unobservable workplace social relations by fielding a separate survey and asking about online review behavior as well as experiences of social relations at work. We then validate our dictionaries in multiple ways, both internally through comparing dictionary classification with human coding and externally through discussing how these dictionaries compare to traditional survey-based approaches. This method has the added benefit of allowing us to ask questions deductively, in comparison to more inductive, topic modeling approaches (Corritore et al. 2020). We hope this methodological validation work will be useful for researchers using job reviews (Zhang 2023) but also more broadly for the literature on novel labor market data sources, from social media–targeted surveys (Schneider and Harknett 2022) to survey-linked administrative data (Wilmers et al. 2024b).

Our final contribution is to bring these methods and ideas to debates about sources of between-firm inequality (Card et al. 2016; Wilmers and Aeppli 2021) and the sources of firm pay premiums. Dominant prior explanations

for this dimension of economic inequality have focused on formal organizational constraints on pay setting or on differences across firms in product market position. However, less attention has been paid to the way informal social structural constraints can create or require firm pay premiums. By bringing these ideas to large-scale labor market data, we uncover a novel source of between-firm inequality: differences in informal social structure.

Taken together, these contributions define a distinctively sociological contribution to labor market inequality research. Rather than consigning social relations to explaining within-organization inequality (Tilly 1998) or sorting processes (Pedulla and Pager 2019), we show how distinct social structures undergird organization-wide advantages. Future research should assess whether changing workplace social structures can explain macrolevel changes in labor market inequality. This direction would demonstrate novel mechanisms through which inequality emerges from local configurations of social relations.

DATA AVAILABILITY

Data supporting this article can be found in Wilmers, Tong, and Zhang (2024), in the Harvard Dataverse, https://doi.org/10.7910/DVN/CCMF4W.

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