

# Values and Inequality: Prosocial Jobs and the College Wage Premium\*

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## Abstract

Employers often recruit workers by invoking corporate social responsibility, organizational purpose, or other claims to a prosocial mission. In an era of substantial labor market inequality, commentators typically dismiss these claims as hypocritical: prosocial employers often turn out to be no more generous with low-wage workers than their competitors are. In this paper, we argue that prosocial commitments in fact inadvertently reduce earnings inequality, but through a different channel than generosity. Building on research on job values, we hypothesize that college graduates are more willing than nongraduates to sacrifice pay for prosocial impact. So when employers appeal to prosocial values, they can disproportionately reduce pay for higher-educated workers. We test this theory with data on online US job postings. We find that prosocial jobs requiring a college degree post lower pay than standard postings with exactly the same job requirements, whereas pay at prosocial jobs not requiring a college degree pay no differently from other low-education jobs. This gap reduces the aggregate college wage premium by around 5 percent. We present a variety of supplementary evidence using labor market data, worker survey responses, and a vignette experiment with hiring managers. The findings reveal an unintended consequence of employers' embrace of prosocial values: it offsets macro-level inequality.

**Keywords:** College premium, prosocial jobs, workplace inequality, wage gap, corporate social responsibility

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The pay gap between workers with and without a college degree has doubled over the last 40 years. Most research on rising earnings inequality assumes that workers seek only the highest possible pay, constrained variously by skill level, bargaining power, or institutional constraints (Araki, 2020; VanHeuvelen, 2018; Wilmers, 2020). However, a long tradition in the sociology of work challenges this assumption. From Durkheim and Parsons on the professions (Durkheim, 2014; Parsons, 1951) and Elton Mayo on human relations (Mayo, 2014), up to ethnographies of normative control (Kunda, 1992) and experiments on social incentives (Ashraf and Bandiera, 2018), researchers find that many workers toil at least partly in service of others.

Recently, employers have actively stoked these prosocial values among their employees by advertising corporate social responsibility (Lim and Tsutsui, 2012), organizational purpose (Adler and Heckscher, 2018), corporate mission (Gartenberg et al., 2019), and nonprofit status (Johnston and Johnston, 2020) or B Corp certification (Marquis, 2020). Field experiments confirm that workers indeed seek jobs advertised as prosocial and even sacrifice pay to take them (Burbano, 2016; Hedblom et al., 2019). In this paper, we ask how employers' claims of prosocial commitment affect overall labor market inequality.

We build on prior research that finds that employer-side practices have exacerbated pay inequality and, specifically, have heightened pay gaps between college and non-college workers. Part of increased inequality is due to a lagging supply of new college graduates, insufficient to keep up with technological change (Autor et al., 2020). But employer-driven changes have exacerbated these labor market fundamentals: in various ways, the weakening of bureaucratic employment policies at large employers loosened prior constraints on pay inequality (Bidwell et al., 2013; Cobb, 2016). The transformation of pay- and benefit-setting (Dencker and Fang, 2016; Kristal et al., 2020), outsourcing and downsizing (Cobb and Lin, 2017; Jung, 2016), decline of internal labor markets (Cappelli, 2001; Wilmers and Kimball, 2022), and weakened collective bargaining agreements (Brady et al., 2013; Western and Rosenfeld, 2011; Wilmers, 2019) all heightened pay gaps between college and non-college

workers. This growing body of research demonstrates that employer-side changes exacerbated rising inequality.

Yet, at the same time as employers have shifted away from bureaucratic employment rules, many have also embraced the discourse of prosocial values. Examples range from corporate responsibility commitments (Henderson, 2020; McDonnell and King, 2013) to a new emphasis on company mission and purpose (Gartenberg et al., 2019). These commitments, while common and indeed characteristic of post-bureaucratic organizations (Adler and Heckscher, 2018), stand in tension with the new, flexible employment practices typically linked to increased inequality: prosocial commitments do not reduce organizational protections or necessarily impose harsher market discipline on workers. We ask then, when employers embrace prosocial values, does this increase inequality—like other post-bureaucratic employer practices—or does it offset it?

Insofar as research has considered the impact of prosocial or socially responsible employers on inequality, it has skeptically assessed such employers’ claims to being exemplary employers. If a prosocial employer raises pay for low-wage employees, it could be competed out of the market by lower-cost competitors (Henderson, 2020; Osterman, 2018). Moral licensing and sharpened budget constraints—due to high costs of non-employee social responsibility programs—may also lead prosocial organizations to squeeze their employees (Ormiston and Wong, 2013). Perhaps the starkest examples come from progressive nonprofits that seem to betray their stated ideals by burning through low-paid, idealistic employees (Fisher, 2006). Together, these skeptical responses imply that employers’ invocations of prosocial mission either have null or negative effects on inequality.

We start instead from the perspective, prominent in recent organizational research, that prosocial mission and social responsibility are job amenities that allow lower pay (Burbano, 2016; Hedblom et al., 2019). A consistent finding from research on normative control is that employer-side prosocial rhetoric frequently affects different groups of workers differently (Alvesson and Willmott, 2002; Chreim, 2006; Kunda, 1992; Vallas, 2003). We draw on the

sociology of job values to argue that willingness to accept lower pay for a prosocial job is more prevalent among higher-educated workers—who can better afford it—than among lower-educated workers (Johnson et al., 2007; Kalleberg and Marsden, 2019). Prosocial job values thus function as a luxury good, which higher-paid workers are more willing to trade for as the urgency of pecuniary income recedes (Kalleberg and Marsden, 2013). As a result, when employers emphasize prosocial work, this allows reduced pay for some college graduates, but not for workers with less education. Because prosocial work is a job amenity particularly sought by college graduates, its expansion partially offsets the college wage premium. This process is consistent with the micro-level findings of the skeptical research cited above, but offers opposite predictions for macro-level inequality effects.

We test these ideas by drawing on data from online job postings. We code postings according to whether they advertise prosocial features of a job, specifically like (a) helping a community or group, rather than an individual consumer, (b) engaging in non-market-mediated, non-commercial activity, or (c) emphasizing non-profit-maximizing organizational goals. To obtain estimates of the effect of these prosocial job characteristics, we compare prosocial and standard job postings that list exactly the same subsets of detailed skill requirements (out of 27,000 in total). We also obtain panel estimates of the effect of the same job being advertised with or without prosocial language. We then aggregate these data to MSA-level college wage premiums to assess the impact of prosocial job amenities on the skill pay gap. This approach to analysis offers a middle course between field experiments, with high internal validity but no way to aggregate up to quantify labor market-wide effects, and large survey and administrative data analysis, which lack direct measures of prosocial job characteristics. We supplement this core source of evidence with worker-sourced pay data, household survey responses, analysis of job applicants’ resumes, and an online experiment with hiring managers, all of which accumulate evidence for the job amenity inequality mechanism we theorize.

Together, these results uncover an important exception to research indicating that new

employer-side, post-bureaucratic employment practices exacerbate skill-based inequality. We identify a countervailing force, in which new emphasis on mission and broader prosocial values allows reduced pay for college workers. Because the prosocial pay discount is only present for jobs that require a college degree, the spread of prosocial jobs has the unintended consequence of reducing skill-based inequality. More broadly, this skill-varying tradeoff between pecuniary and non-pecuniary benefits invokes a class of leveling mechanisms rarely discussed in research on contemporary earnings inequality. The exchange of wealth for status is a staple of research ranging from the Kwakiutl potlatch (Piddocke, 1965), to patronage and deference in rural India (Breman, 1979) and of sociological theory about exchange across different forms of capital (Bourdieu, 1986). Our findings suggest renewed relevance for these ideas in the contemporary labor market: inequality can be offset when higher potential-earning workers trade material for nonmaterial benefits.

## 1 Inequality and worker motives

Rising earnings inequality over the last 40 years has spurred a substantial research agenda, spanning multiple disciplines, to account for inequality between workers of different skill levels (Autor et al., 2020; Weeden, 2002; Wilmers and Aepli, 2021). Much of this research can be summarized by the idea that earnings inequality rests on labor market supply and demand for skill, as modified by organizational and institutional constraints. When skill-biased technological change raises demand for college graduates but the supply of such workers does not keep pace, wages for college graduates increase (Acemoglu and Autor, 2011). Increases in the college wage premium accounts for a substantial portion of the overall increase in earnings inequality since the early 1980s (Autor et al., 2020). At the same time as underlying market forces heightened inequality, the post-World War II institutional bulwarks against market inequality fractured and faded. Collective bargaining coverage receded, widening pay gaps between skill groups (Farber et al., 2018; Western and Rosenfeld, 2011). Large corporations

shed low-wage workers through outsourcing and reduced pay for those who remained directly employed (Cobb and Lin, 2017; Weil, 2014; Wilmers, 2018). The result of these labor market and organizational changes was a rising and then persistently elevated college wage premium.

Both sociological and labor economics theories of these wage dynamics—and by extension, explanations for the college wage premium—share strong assumptions about the motivations of workers and employers. Workers seek high pay and employers try to minimize labor costs, conditional on employing workers skilled enough to be productive. Employers are then additionally constrained by union threat and government intervention and by invidious pay comparisons among employees. Workers are constrained by their individual skills and by their capacity for coordination and collective action. While sociologists have emphasized this latter, social feature of labor market dynamics, even this collective action is primarily directed toward acquisitive purposes (Weeden, 2002; Wilmers, 2019), as many radical critics of labor unions have recognized (La Botz, 2013). Research on inequality thus commonly assumes primarily acquisitive motives for labor market activity. Accordingly, it focuses on supply and demand and on the organizational or institutional constraints which guide these simple motives toward more- or less-unequal outcomes.

Yet a substantial research program in organizational theory demonstrates that workers care not just about bread and pay, but about roses too. Indeed, many workers are willing to accept lower pay to work for prosocial and socially responsible employers. Recent field experiments provide the strongest evidence: socially responsible employers are able to post lower wages and attract more productive workers (Burbano, 2016; Hedblom et al., 2019). Studies with observational data find that firms with strong corporate social responsibility reputations can pay lower wages (Nyborg and Zhang, 2013) and that workers' wages decline when they switch to nonprofit employment (Johnston and Johnston, 2020). Together, these findings challenge a core assumption of much research on macro-level inequality generally and on the college wage premium specifically.

One exception to the assumption of strictly financial motivation in inequality research

comes from tracing revealed preferences of workers for different firms (Sorkin, 2018). Using labor-market-wide earnings data, Sorkin finds that compensating differentials in general can account for more than half of the inequality that arises from different pay levels across firms. However, Sorkin is unable to observe job amenities directly: compensating differentials could be due to non-wage benefits, firm status, or work intensity. Moreover, recent research suggests that non-wage compensation such as health and retirement benefits are actually more unequally distributed than pay (Kristal et al., 2020). We therefore focus in on theorizing and measuring the inequality effects of a specific amenity: prosocial job characteristics. And we argue that, beyond differential access to this amenity, the interaction between heterogeneity in workers’ desire for prosocial work and employer endorsement of prosocial messages can affect earnings inequality.

## 2 Inequality-reducing prosocial wage effects

Workers’ willingness to trade pay for the job amenity of prosocial meaning can affect between-skill-level inequality if that willingness is itself unequally distributed. Specifically, we expect that college-educated workers will place a higher value on a job’s prosocial characteristics, while non-college workers will be less likely to do so. By extension, prosocial jobs that require a college degree can post a lower wage than similar jobs that do not have a prosocial orientation, but prosocial jobs that do not require a college degree cannot get away with this.

In developing this idea, we build on a longstanding finding in the sociology of job values: workers who are advantaged, highly educated, and of high socio-economic status place less emphasis on instrumental and extrinsic job characteristics (Johnson and Mortimer, 2011; Kalleberg and Marsden, 2013, 2019; Kohn and Schooler, 1969). This research proposes a prioritized hierarchy of job values, in which the presence of uncertain or low extrinsic benefits takes higher priority relative to intrinsic features of work, like meaning, achievement

and inherent interest (Kalleberg and Marsden, 2013). Inherent work rewards are sought as a relative luxury, elastic with respect to income and education (Benditt, 2015). Within the broad umbrella of intrinsic work rewards, researchers distinguish task mastery and inherent interest from prosocial or altruistic rewards (Johnson et al., 2007; Marini et al., 1996). When employment security and decent pay cannot be taken for granted, they outrank the ineffable benefits of an inspiring social mission. Put differently, because college-educated workers receive higher pay and material security than non-college workers, they are more willing to trade pay for prosocial work.

Note that this difference need not be a conscious prioritization by an individual worker. Research on the origins of job values frequently attributes them to socialization in class-distinct families, schools and—ultimately—workplaces and occupations (Stephens et al., 2014). For example, a long research tradition in psychology and education suggests that socialization in college makes students adopt a more prosocial orientation (Brandenberger and Bowman, 2015; Padilla-Walker and Carlo, 2015). Beyond the formative years of college, experience in particular occupations and organizations reinforce value commitments (Grant, 2007; Johnson, 2001; Kohn and Schooler, 1969). Research on callings and professional identity provides vivid examples of how work experience socializes workers to value non-material job amenities (Bunderson and Thompson, 2009; Dobrow, 2013).

We do not aim to quantify the distinct contributions to workers’ values from childhood background and higher education, occupation, and organizations; we leave that rich debate to the job values and work orientations literature (Halaby, 2003). Moreover, we do not claim that these work values reflect true altruism or generalized prosocial dispositions (Grant and Berg, 2012). The underlying psychology of these values may be a function of reputation and signaling considerations, group-belonging or habituated, unexamined commitments (along the lines of the early sociological work on this topic (Kohn and Schooler, 1969)). For our theory of earnings inequality, we draw from the job values research only the robust finding of differences in self-reported work values between college and non-college workers.



It is less well-established whether workers actually follow through on their self-reported values and differentially sacrifice pay in response to employer evocation of prosocial job meaning. The more recent research on compensating differentials and job meaning, dominated by case studies and experiments, has not focused on the classic distinction between high- and low-education workers that dominates the survey-based job values research. But recent studies still offer reason to expect that higher-educated workers are particularly responsive to the prosocial job amenity: negative pay tradeoffs are identified for elite college graduates (Frank, 2010), salaried workers (Gicheva, 2020), lawyers (Carnahan et al., 2017) and consultants (Bode et al., 2015). These consistent findings across different types of highly educated, salaried and professional workers provide warrant to expect that such workers, at least, will respond to a prosocial job amenity. Fewer studies assess non-college workers’ willingness to make a similar trade-off, but one field experiment finds that high-performers are more responsive than low-performers to prosocial signals (Burbano, 2016). Another experiment finds that higher educated workers report willingness to give up more salary for a meaningful job than less educated workers report (Hu and Hirsh, 2017).

We therefore expect that employers will respond to college workers’ heterogeneous willingness to trade prosocial job amenity for lower pay. Specifically, we hypothesize that:

**Hypothesis 1.** *The prosocial pay discount will be stronger for jobs requiring a college degree.*

As a macro-level corollary of this prediction, we also expect that when employers advertise prosocial job characteristics, they reduce the college wage gap:

**Hypothesis 2.** *More prosocial jobs within a labor market will reduce the college pay gap.*

Before introducing our data, it is important to elaborate on this second hypothesis. First, we do not take a strong position on employers’ motives. In a context of historically high pay premiums for college-educated workers, employers could use prosocial messaging and commitments strategically, to attract such workers at lower pay. Or employers could be genuinely prosocial, seek to attract mission-oriented employees to a given job, yet simply set

pay levels in line with labor supply available for that job. In any case, when this differentially valued job amenity is credibly offered, it will aggregate up to a reduction in the college pay premium within a local labor market.

Second, we do not see prosocial compensating differentials as an alternative explanation to the supply-and-demand and institutional theories of labor market inequality. Instead, considering workers’ nonpecuniary motives to work—and employers’ engagement with these motives—reveals an understudied facet of recent labor market dynamics. Increased demand for college-educated workers could actually drive employers toward more reliance on prosocial messaging. Moreover, employers’ prosocial commitments could increasingly shape worker mobilization and labor market institutions: protecting the reputational amenities associated with a given job could be a goal of worker collective action. The standard dynamics emphasized in inequality research could thus amplify or be shaped by the prosocial jobs we consider here. We discuss these possibilities further in the conclusion. But, as these issues of job values have been assumed away in prior macro-inequality research, we see our task as first assessing whether the distribution of these values indeed matters to between–education-level inequality.

### 3 Data

There are two key challenges to testing these ideas. First, we need to measure whether or not a job is advertised as prosocial. Neither large household surveys of workers nor proprietary firm data include information on the prosocial framing or characteristics of a job. Thus, standard data used to study pay inequality cannot address our research question. Second, we need to compare similar jobs to distinguish the prosocial job amenity from differences in work requirements. Even jobs within the same industry and occupation could differ in their specific work and skill requirements in ways correlated with both pay and prosocial framing. Detailed job characteristics data is thus necessary to insure that differences in pay

are indeed due to prosocial framing.

We address both challenges by drawing on job postings collected by Burning Glass Technologies (BGT). BGT examines some 40,000 online job boards and company websites to source job postings, removes duplicates and parses the postings into a systematic, machine-readable form. It captures the near-universe of jobs posted online (Deming and Kahn, 2018; Hershbein and Kahn, 2018). As in prior BGT-based research, we obtained data from 2007 and 2010–2019. While these data are proprietary, we have posted all code and supplementary data used in our analysis at [TK URL post-review].

We restrict our attention to the 36 million job postings in the BGT data that list wage or salary information, accounting for about 17 percent of all job postings. Besides pay, each posting includes information on education and experience requirements, detailed industry and occupation information (predicted by BGT from the job-posting text), work location, posting date, employer name (when available), and skill requirements. Since education level is important for our study, we further restricted our sample to the 9 million postings that explicitly mention degree requirement and are non-missing on other variables used in the analysis. Some jobs clearly expect a college degree, but do not specify this explicitly. Others have no explicit or implicit education requirements and would consider applications from non-college applicants. By excluding these ambiguous jobs, we ensure that we accurately distinguish jobs by their required level of education.

Although online job postings do not capture all job openings, past studies suggest that 60 to 70 percent of job openings in recent years have been posted online (Carnevale et al., 2014). Previous work comparing industry-level data from the BGT to the Bureau of Labor Statistics’ Occupational Employment Statistics (OES) finds that the BGT data is broadly representative of the US labor market at the industry level (Hershbein and Kahn, 2018). However, management and professional positions are overrepresented and low-wage occupations are underrepresented. Moreover, we severely limit the sample via the restrictions noted above. Therefore, in all analyses, we weight the BGT data by occupation industry in the

OES. This upweights jobs that are rare in the BGT, relative to their prevalence in the BLS data. Appendix Table A.1 summarizes the weighting adjustments to the BGT data at the level of broad occupation categories.

### 3.1 BGT as a source of pay data

Our key dependent variable is pay listed in the BGT job posting. For postings listing annual salary, we use log salary. For postings listing hourly or weekly wages, we convert to full-time equivalent logged salary. Using the pay data listed in online job postings poses two potential concerns. In this section, we discuss these concerns in some detail, as they constitute a key downside to using BGT data rather than standard labor market surveys to study inequality and determinants of pay.

First, pay listed in the job posting may not accurately reflect the actual pay offered to new hires. To assess this, a recent paper compared the pay in BGT’s job postings to wages for new hires in the Current Population Survey (CPS) and found the two sources to be highly consistent (Hazell and Taska, 2019). Using the average new-hire wage at the state-quarter level, the authors showed that the two datasets have a correlation of almost 1 (0.97), suggesting that pay data in BGT closely reflect those in representative surveys. Additional tests comparing BGT pay data to the Quarterly Workforce Indicators data also show a close match (Hazell and Taska, 2019). Thus, on this aggregate level, BGT pay closely tracks pay in other datasets.

Consistent with this prior work, Figure 1 shows that the overall college pay premium in the BGT data analytical sample is very similar to that in the Current Population Survey–Outgoing Rotation Group (CPS-ORG) for equivalent years. The CPS-ORG shows that the long rise in the college pay premium has slowed since the late 1990s and flattened entirely since 2010. The BGT series also show no rise and even indicates a small decline since 2010.

[Figure 1 about here.]

A second—and more difficult—problem with using BGT pay data is selection on listing the job’s starting pay. Since only a fraction of job postings list pay information, they may be systematically different from those that do not. Most concerning for us, it is possible that high-paying, prosocial employers seeking college job applicants are less likely to post starting pay information than are low-paying prosocial employers seeking college job applicants. In this case, missing data on our dependent variable could negatively bias our estimates of the prosocial penalty for college workers.

We address this problem in two ways. First, in a robustness test discussed further below, we match the BGT data to an independent source of pay data and re-run our models using that alternative source of pay data. Specifically, we match all BGT job postings (at an occupation–employer–name–county–year level and including both postings that report pay and those that do not) to 10 million workers’ pay reviews posted in the job review website Glassdoor. These reviews, obtained directly from Glassdoor, provide a source of pay that avoids any selective reporting on the part of employers. By obtaining pay estimates directly from worker self-reports, we provide a strong test of whether pay posting selection affects our estimates. We again re-weight the resulting analytical sample with the OES to upweight rare jobs in the BGT-Glassdoor matched data. Because most jobs in Glassdoor are not actively hiring and most BGT job postings do not result in a hire who posts on Glassdoor, the intersection between these two data sources substantially reduces our sample size (to 1 million). We therefore use it as a robustness test rather than the main sample.

Second, we aggregate data up to the Metropolitan Statistical Area (MSA) level to study variation in the college wage premium across local labor markets. This aggregation allows us to use a college wage premium for employed workers calculated from the CPS-ORG and to compare that measure to one derived from the BG postings. These two supplementary sources of wage data, at the job and the MSA level, respectively, avoid the selection issues involved in relying only on posted pay.

## 3.2 Measuring prosocial jobs

We predict wages using whether or not a job posting indicates commitment to prosocial values. We conceptualize prosocial values broadly. We aim to include traditional corporate social responsibility, in which organizations engage in noncommercial activities such as community service and sustainability. We include organizations with explicitly nonprofit goals, such as “saving the world” or “giving back.” Beyond explicit CSR, we include broader commitments to corporate purpose and mission, over and above profit maximization. We also include explicit discussion of helping a community or group or of making a large positive impact on an individual (“changing a life”). Of course, the work content of many jobs can be characterized as helping consumers or clients, but we exclude postings that do not emphasize a large impact on an individual or targeting a community. Together, these different dimensions of nonprofit organizational goals, noncommercial activity, and prosocial work content define prosocial values in the job postings.

To operationalize this broad concept of prosocial values, we search for words and phrases that capture it. We do this with a mix of manual coding and word embedding. We first randomly drew 15 million job postings in the BGT database and parsed them to get rid of stop words and punctuation. We then manually coded 500 job postings to identify key phrases commonly associated with prosocial meanings, including “making a difference,” “giving back,” “positively impact,” “improving the lives,” and “people’s lives.” Using these seed phrases, we used the word-embedding model to identify the 100 closest words and phrases that share a similar contextual meaning as these phrases in these job postings.

Word embedding is increasingly used in the computational social sciences; its mechanics are relatively simple. It relies on neural networks to process unstructured text data. Specifically, two words are considered neighbors if they are no farther apart than five words in a document and, for each word, the algorithm uses a neural network to summarize its common neighbors in job postings. The information about common neighbors is condensed into a vector of fixed dimension (we use 100). That is, word embedding converts a word

to a 100-dimensional vector that represents the meaning of that word (see Appendix A for more details). We can then quantify the association between any two word-vectors using their cosine similarity. A high degree of similarity between two vectors indicates that the two words are semantically close in the context. For example, the phrase “making a difference” has a high-cosine-similar score with the word “mission” in the job postings, suggesting that the two express similar meanings in job postings. Once we have the cosine similarity between any two words, we can construct a dictionary of prosocial words by iteratively associating a set of words and phrases in the job postings to a group of seed words defining prosocial values. Such a procedure, known as bootstrapping, is common in the information retrieval literature for learning new semantic lexicons (Riloff and Jones, 1999).

After running the word-embedding model, we manually checked the resulting words and phrases to see how they are used in the job postings and eliminated those that do not consistently refer to prosocial values. We also excluded false positive phrases, like “mission critical” and “mission system” for “mission.” Using this method, we created a context-specific dictionary that captures prosocial meaning in job postings.<sup>1</sup> We designate a job posting as advertising prosocial values if it includes any of the words and phrases in this dictionary; we found that 37 percent of the job postings in our sample mention prosocial values.

To validate this measure, we aggregate it to the firm level and compared it to the Kinder, Lydenberg, Domini Research and Analytics (KLD) score (Kang, 2015). Developed by a for-profit company, KLD scores are similar to credit ratings. KLD uses a team of analysts to measure a firm’s social responsibility. We used a combined KLD score for each firm by aggregating its scores on environmental and social dimensions (and excluding scores on corporate governance dimensions). KLD scores are only available for the publicly traded firms in our sample, so we focused our validation exercise on those firms. As Appendix Figure A.1 shows, the proportion of job postings showing prosocial meaning is positively associated with a firm’s KLD score. We also compare our job postings measure to a non-firm measure of prosocial behavior, using the share of respondents in an MSA who report in the Current

Population Survey Volunteer Supplement doing some volunteer work. We find that the MSA-level proportion of prosocial job postings also predicts volunteerism. Together, these comparisons with outside data increase our confidence that we have identified job postings advertising prosocial values.

Figure 2 summarizes the levels of prosocial language across occupations and industries. Consistent with intuition, the highest levels are found in caring occupations such as community and social services, healthcare support, and healthcare practitioners and in industries such as public administration and healthcare. In contrast, work deep in supply chains, such as mining and production occupations, and in strictly consumer services, such as hotels and restaurants, has low levels of prosocial language. These patterns further increase our confidence that our dictionary method captures meaningful variation across jobs. Note, however, that even in occupations such as management and industries such as manufacturing, more than a quarter of the job postings include prosocial language. Consistent with our discussion above, employers use prosocial language for a wide variety of jobs.

[Figure 2 about here.]

We also study how prosocial language varies across college and non-college jobs: 50 percent of college jobs and 42 percent of non-college jobs use prosocial language. This 8-percentage-point gap is not mainly due to differences in employer skill composition: a 6-percentage point gap in the level of prosocial postings by education persists conditional on employer fixed effects. Moreover, these employer fixed effects only account for 38 percent of the overall variation in prosocial language. These basic descriptive results suggest that employers do indeed frame jobs differently for college and non-college workers. While we do not take a position on whether employers do this strategically to appeal to particular values among types of workers, these patterns do demonstrate that prosocial language is not strictly a function of company-wide boilerplate language in job postings.



## 4 Analytical approach

Using the BGT data, we test Hypothesis 1 by predicting pay associated with a job advertisement  $w_i$  with our measure of whether a job posting  $i$  uses prosocial language, *SOCIAL*:

$$\log w_i = \beta_1 \text{SOCIAL}_i + \beta_2 \text{COLLEGE}_i + \beta_3 \text{SOCIAL}_i * \text{COLLEGE}_i + X_i' \gamma + \alpha_i + \epsilon_i, \quad (1)$$

To capture the predicted variation in the effect of prosocial values by skill level, we interact *SOCIAL* with an indicator for whether a job requires a college degree. We then interpret the resulting estimates as the college pay premium and prosocial pay penalty across job postings.  $\beta_1$  is the effect of prosocial framing for non-college jobs.  $\beta_2$  shows the college earnings premium for regular jobs.  $\beta_3$  indicates the additional prosocial penalty associated with college jobs, over and above that for non-college jobs.

As noted above, prosocial jobs may be systematically different than regular jobs. We therefore control, in vector  $X_i'$ , for a series of observable job characteristics available in the job postings: posting word count (logged), years of experience required, performance pay (bonus or commissions), and part-time work hours. We also include a series of increasingly stringent fixed effects in  $\alpha_i$ : year of posting, occupation and labor market. We define labor market as a year by industry by MSA by college-degree-requirement job. This labor market fixed effect absorbs any local labor market-specific supply and demand changes that are either general across the labor market; specific to college compared to non-college workers; or specific to workers with industry-specific skills. This tight comparison allows us to control for the key labor market dynamics that typically affect wages (and might also affect employers' prosocial job posting or messaging).

Finally, we add a fixed effect for occupation by job requirements. This last fixed effect requires explanation. The BGT data include granular indicators for each job requirement listed in a given job posting. These indicators are lightly harmonized but still cover some 27,000

unique skill measures, ranging from stem cell preparation to siding carpentry. We summarize each unique combination of job requirements as a fixed effect category (of which there are 4.3 million unique combinations of requirements in the data and 4.6 million total job requirement by occupation categories). The models that include occupation–job-requirement fixed effects thus compare job postings in the same 6-digit Standard Occupational Classification (SOC) occupation code that include exactly the same detailed job requirements. Because of these strenuous data requirements, we retain only 1 million occupation–job-requirement fixed-effects categories with multiple postings and drop the 3.6 million singletons. In those models, the prosocial wage difference is estimated by comparing jobs in the same occupation and with the same requirements, but which either do or do not include prosocial language in the posting. This tight comparison between observably similar jobs improves on any matched comparison possible with household surveys of workers, which do not include granular data on job requirements. The detailed BGT data make possible this new method of identifying the compensating differential associated with the prosocial job amenity.

Because prosocial language is substantially correlated within employers and within industries, we cluster standard errors at the 3-digit NAICS industry level. We summarize the key variables used in the analysis in Table 1.

[Table 1 about here.]

## 5 Job-level results

Is the prosocial pay discount greater for jobs that require a college degree? Table 2 presents our main results. Model 1 shows that the overall college earnings premium, including both prosocial and regular jobs, is 0.53 log points. Exponentiating, jobs that require a college degree offer around 65-percent-higher pay than non-college jobs. This pay gap echoes that shown in Figure 1 and its magnitude is consistent with the large literature on Mincer wage equations and skill-based inequality. Model 2 interacts the college degree requirement with

prosocial job characteristics; prosocial jobs that require a college degree have an earnings penalty 0.09 log points larger than that for jobs that do not. Indeed, in this baseline model (controlling only for posting word count, experience requirements, performance pay, part-time status, and year of posting), non-college jobs—but not college jobs—are actually paid more when they reference prosocial values.

[Table 2 about here.]

The results in Model 2 indicate that the college earnings premium is around 0.09 log points smaller among prosocial relative to regular jobs. Comparing the overall college earnings premium from Model 1 to the non-prosocial premium in Model 2 indicates how much this prosocial penalty reduces the overall college premium. Without prosocial jobs, the overall college earnings premium would be around 5 percent larger. This is a substantial quantitative difference in macro-level inequality, equivalent to around half of the observed increase in the college premium during the 1990s. The prosocial contribution to offsetting the college pay premium is a function of both the larger penalty for prosocial job characteristics faced by college-educated workers and, secondarily, the higher share of prosocial jobs for college-educated workers (42 percent, compared to 33 percent for non-college jobs from Table 1).

While these overall estimates indicate the quantitative importance of prosocial jobs to the college earnings premium, they are subject to several sources of omitted-variable bias. The remaining models in Table 2 fit increasingly stringent models to mitigate this risk of bias. These additional controls reduce the interpretability of the overall college pay gap: the college wage premium conditional on occupation, for example, is not very meaningful. But they allow tight identification of the prosocial pay penalty across very similar jobs. Model 3 adds a fixed effect for year by 3-digit NAICS industry by MSA by college degree requirement. If prosocial jobs are more likely to appear for college-educated workers in regions and industries with lower overall demand for college workers, that could account for the education-based difference in prosocial effects. Estimates in Model 3 show that after adjusting for labor-market- and industry-specific skill premiums, the difference persists.

Next, Model 4 adds controls for 6-digit SOC occupation codes. Prosocial jobs may be more likely to appear in relatively lower-paying occupations for college relative to non-college workers. However, Model 4 again shows that the prosocial penalty gap persists.

Finally, Model 5 presents our strongest test of the earnings effects of prosocial job language. Even comparing among jobs in the same occupation, as in Model 4, one job may have different work requirements than another. These differences in turn affect pay. For example, a nurse at a nonprofit hospital may be given less responsibility than a nurse at a for-profit hospital. To address this concern, we compare only jobs posting the same occupation and the exact same set of skills and job requirements. Including this fixed effect reduces our sample size by around half: many job postings have no exactly comparable equivalent in the sample. However, this model specification provides a very tight comparison between similar jobs that nonetheless differ on their prosocial framing. The results in Model 5 show that in this comparison, the college/non-college prosocial penalty gap increases to -0.07 log points. In this tightest model, prosocial language has no effect on earnings in similar non-college jobs, but it lowers earnings for college jobs by around 9 percent. When jobs require a college degree, prosocial framing reduces pay relative to regular jobs with identical tasks, skills, and requirements. There is no such difference for non-college jobs.

## 6 MSA-level results

Next, we assess whether the micro-level results presented in Table 2 aggregate up to the macro, labor-market level at which the college earnings premium is often quantified. In Figure 3, we chart the MSA-level college wage premium against the MSA-level proportion of prosocial jobs. MSAs with a larger share of prosocial jobs have a lower wage premium ( $\rho = -0.12$ ). Shifting from an MSA with around 20 percent prosocial jobs to one with 40 percent is associated with a decline in the posted wage premium for college jobs from 0.56 to 0.51.

That first association only predicts posted wages, as in the job posting models in Table 2. In the second chart in Figure 3, we take the college wage premium from the Current Population Survey–Outgoing Rotation Group (CPS-ORG) data. We use the CPS hourly wage measure with a pareto imputation for top-coding, along with other adjustments outlined in Schmitt (2003). This gives a college wage premium for currently employed workers, rather than for posted jobs. It is therefore a more meaningful inequality metric than the BGT-postings–based premium, though also more distant from the share of posted prosocial jobs. Nonetheless, the chart shows a similar negative correlation between the MSA-level college wage premium and the share of posted prosocial jobs, which is around half as strong as the job-posting–based negative correlation.

[Figure 3 about here.]

However, these univariate associations could be explained by other differences across labor markets, such as supply and demand for skill. We next model this macro-level inequality explicitly as a college wage premium that varies across MSA-years. We also include controls, drawn from the CPS-ORG and calculated at the MSA-year level, to adjust for: unemployment rates, calculated separately for college and non-college workers; the share of college-educated workers out of the total labor force; average age of the workforce (as a proxy for total workforce experience); the share women; union density; share public sector employees; and calendar year. Together these controls adjust for various labor market supply and demand, and for institutional effects, which could influence both the level of college wage premium and the observed share of prosocial job postings.

Table 3 shows the results of these models, predicting both the college wage premium for the currently employed, from the CPS-ORG, and the posted college wage premium from BGT. Models 1 and 6 offer a baseline association, consistent with Figure 3. Models 2 and 5 add controls for the observable labor market characteristics mentioned above. Even conditional on those labor market characteristics, more prosocial job postings are associated with a smaller college pay gap. Models 3 and 6 add MSA fixed effects to adjust for any

time-invariant unobserved heterogeneity across MSAs. These models show that increases in prosocial job postings are associated with reductions in the college pay gap. Taken together, these models suggest that the negative association between prosocial job postings and the college pay gap is not accounted for by observable labor market characteristics or by unobserved, time-invariant features of local labor markets.

[Table 3 about here.]

## 7 Supplementary Evidence

### 7.1 Selective pay posting in job ads

As noted above, most BGT postings do not explicitly post a starting wage. Because this missing data is unlikely to be random, it could bias the results presented in Table 2. To test whether this selective reporting drives our results, we next predict worker-reported pay using Glassdoor job reviews. While we change the source of our pay dependent variable, we retain the BGT-based measure of prosocial values in the job posting and retain all control variables and model specifications from Table 2.

Table 4 shows the results. Despite drawing on a very different sample (of BGT jobs that do and do not post pay information, and only those that match to Glassdoor) and an entirely different dependent variable (pay reported by workers in Glassdoor, rather than starting pay posted by an employer), the results are remarkably similar. In each model, the prosocial pay penalty is larger for jobs that require a college degree than for those that do not. The analytical sample is an order of magnitude smaller than the BGT-only estimates and the results are therefore noisier and slightly attenuated toward zero. However, they remain qualitatively consistent and similar in magnitude to the other estimates.

This exercise substantially raises our confidence that selective reporting in the BGT data is not driving our estimate of the education-varying prosocial pay penalty. Even when using

pay that circumvents employer reporting entirely, and relies only on worker reports, the patterns are similar.

[Table 4 about here.]

## 7.2 Panel analysis of job postings

While the tightly controlled results in Model 5 of Tables 2 and 4 condition on a battery of job characteristics, there could still be unobserved differences between prosocial and regular jobs. To test for these unobserved differences, we identify a subset of BGT job postings that appear multiple times over the period and which switch between discussing or not discussing prosocial status. We initially identify job postings by occupation, employer, and MSA and subsequently add the granular job requirements interaction noted above. This requires dropping all job postings with missing employer names (25% of observations), which introduces a different analytical sample. This panel strategy also includes substantial measurement error, as a job could appear to be treated by prosocial framing with only a single word change between postings. This measurement error likely biases our panel estimates toward zero.

However, the results in Table 5 are qualitatively consistent with the findings discussed above. The effect of prosocial language on posted pay is consistently negative for jobs requiring a college degree. For non-college jobs, the effect is smaller and not consistently negative across specifications. Even for college jobs, the magnitude of the prosocial penalty attenuates relative to the Table 2 models, consistent with substantial measurement error in the panel comparisons. Nonetheless, this panel analysis provides additional supplementary evidence that prosocial framing allows a stronger pay discount for college than for non-college jobs.

[Table 5 about here.]

### 7.3 Evidence on demand side of prosocial job inequality

Employers are more likely to emphasize prosocial messages and offer lower pay when targeting college graduates than non-college graduates. However, even with the tightly controlled data presented above, we are limited in inference to observational comparisons. We also do not observe actual decision-making about the postings in the BGT data.

To demonstrate this process causally, we conducted a survey experiment in September 2020. Using Prolific, an internet-based survey company, we recruited 400 individuals in US firms who make hiring decisions for their companies. Each respondent was first randomly given one of two versions of an example job posting for a medical assistant position in a medium-sized healthcare firm. The two versions differ only in targeting workers of different education levels. The control version specifies that the position requires a high school degree and is mainly targeting those without a college degree. The treatment version requires a college degree. Note that the job posting is based on a real job posting and the position of medical assistant in practice does vary in college degree requirement. In our BGT data, about 5 percent of medical assistant positions require a post-secondary degree, 50 percent require only a high school degree, and 45 percent do not specify.

After reading through the job posting, each respondent is shown a brief description of the job’s associated prosocial values: “Our company’s mission is to improve people’s lives through meaningful innovation. We design products that help improve the lives of low-income Americans. Every year, we also donate 5 percent of our profits to local communities.” They are then asked to put themselves in the shoes of a hiring manager and to respond to two questions on a five-point Likert scale: “Do you think including this prosocial message would help to attract more qualified candidates for this position?” and “Do you think that the prosocial message may encourage job applicants to take this position despite offering a slightly lower pay relative to the market?” The goal of this simple survey experiment is to observe whether employers perceive the value of a prosocial message differently depending on its target population.



As Figure 4 shows, respondents answer more affirmatively to both of these questions when they are considering a college-requiring (treatment) than a non-college (control) job. Respondents are 10 percent more likely to believe that prosocial messages would attract more qualified candidates when targeting those with college degrees than those without and 17 percent are more likely to believe that candidates would sacrifice lower pay for prosocial values. These results support our proposed mechanism that employers (accurately) perceive higher-skill workers to be more receptive to prosocial values and therefore offer differentially lower compensation for prosocial positions targeting those workers.

[Figure 4 about here.]

## 7.4 Evidence on supply side of prosocial job inequality

While our analysis thus far focuses on the employer-side use of prosocial job meaning, our theory presupposes, on the supply side, real differences among workers in desire for prosocial jobs. In this section, we consider two sources of supplementary evidence for the idea that college-educated workers differentially value prosocial meaning: survey response evidence from the General Social Survey (GSS) and revealed preference data from resume records.

First, we use the GSS Work Orientations module (from the International Social Survey Programme) to ask whether workers with a college degree are more likely to value prosocial features of a job. Specifically, we predict responses to two questions about how important various aspects of jobs are: “A job that allows someone to help other people?” and “A job that is useful to society?” Both questions are asked on a 5-point Likert scale, which we code from 1 as “Very important” to 5 as “Not important at all”. Table 6 shows in Models 1 and 2 that college-educated respondents are more likely to value prosocial job characteristics than are less educated respondents. Models 3 and 4 specify that college-educated respondents are consistently more likely to consider usefulness to society as an important job characteristic, even conditional on controls for age, gender and region. In contrast, Models 5 and 6 show

that college-educated respondents are also more likely to consider helping others important, but the effect is less than half the size of that for helping society. These results provide direct evidence on an education gap in preference for prosocial job characteristics. They also show that this gap is mainly driven by the status-laden claim of usefulness to society, rather than a mundane kind of interpersonal helping behavior.

[Table 6 about here.]

Second, to provide more direct evidence on the link between workers’ prosocial values and their actual choice of job, we conduct a supplementary study based on resumes of one million workers. Specifically, we ask whether individuals who attended more service-oriented colleges have a higher likelihood of taking prosocial jobs. By measuring workers’ prosocial values with their attendance at a service-oriented college, we can link these values directly to the kinds of jobs they subsequently take (using the same job-posting–based prosocial job text measure as we use above). Note that, as above, we do not take a position on whether prosocial values are due to selection based on pre-college socialization or to a treatment effect of attendance at a service-oriented college. Instead, we simply use service-oriented college attendance as an indicator of graduates’ prosocial values.

From 2014 to 2018, BGT pulled resumes of labor market participants from various recruitment and staffing agencies, workforce agencies, and job boards, making it the most extensive resume database to date (Shubert et al., 2020). In cleaning the data, BGT’s research team anonymized all resume information and did not use any personally identifiable information. When a resume enters the system, the name, address, and other identifying details are encrypted. For this study, we do not have access to the actual resumes; we have data on each resume compiled by BGT’s research team.

The resume data provide individuals’ job histories and education up until the point at which they submit their resume, effectively making it a longitudinal dataset. We reconstructed the data into worker-year format, so that each worker-year constitutes a unique observation, and we kept those worker-year observations for which the individual is in the

labor force. Instead of comparing between college and non-college graduates, here we focus on only those with US college degrees. Our goal is to compare the job choice of workers who attended more service-oriented colleges and those who attended less service-oriented ones.

Using the resume data, we identified each worker’s bachelor institution. For each institution, we obtained its community service scores from *Washington Monthly*’s college ranking, which ranks all US colleges on this prosocial metric. The community score is based on the percentage of federal work-study grant money spent on community service projects, the number of alumni who serve in the Peace Corps, the size of the college’s ROTC programs, and the proportion of students who voted in previous elections. One limitation is that *Washington Monthly*’s college ranking is only available for recent years, from 2017 to 2019. Since the year-to-year changes in these rankings is relatively small, we simply take the three-year average value for each college and use it for all the years in the sample. To make this time-constrained measure more accurate, we study only workers who obtained a college degree after 2000 (2010 in an additional analysis).

We used firm name and location to merge the resume data with the job posting data discussed above. We used a fuzzy match algorithm to create a similarity score for every combination of firm name across the two datasets. We first identified exact matches on firm name and location. We then manually read through firm name combinations with a similarity score above a predetermined threshold (0.7 on a scale of 0 to 1) to identify additional matches. This procedure left 5,457 unique firms, 800,000 unique individuals who have worked in these firms, and 2.8 million individual-year observations.

We then use these matched data to test whether workers who attended more service-oriented colleges are more likely to work for more prosocial employers. We used the same measure of prosocial job posting data described above, but aggregated to the employer level. Table 7 shows that those who attended more service-oriented colleges are more likely to subsequently work for prosocial employers. This holds conditional on controls for years of experience and for gender in Model 1. The effect persists but attenuates somewhat with fixed

effects for MSA and occupation. This is consistent with the substantial portion of variation in prosocial language attributable to occupation. The effect also holds (and strengthens) in the subset of more recent graduates (2010–2018), for whom the university service score should be most accurate. Graduates of high-service universities are more likely to subsequently work in prosocial jobs. Taking high-service university attendance as a signal of graduates’ values, this pattern suggests that differences in self-reported values, displayed in Table 6, do affect employment choices.

[Table 7 about here.]

Together, these supplementary analyses provide evidence on both employers’ and workers’ motivations in our prosocial signaling and inequality theory. Figure 5 summarizes the full set of claims in our argument and the place of each piece of our supporting evidence. College-educated workers are more likely to value the inherent importance of a job. Workers who graduated from a service-oriented college are more likely than other college graduates to end up working at an organization using prosocial framing in its job postings. On the demand side, respondents with hiring experience accurately perceive these differences on the supply side: they expect that college-educated workers are more likely to value prosocial jobs and more likely to accept those jobs for lower pay. These supplementary analyses fill in both the employers’ and workers’ sides of the labor market, which meet in the job postings we study in the main analysis.

[Figure 5 about here.]

## 8 Discussion

From corporate social responsibility to mission and purpose, employers frequently advertise their jobs’ prosocial characteristics. Recent research establishes that these prosocial signals

yield job applicants willing to accept lower pay in exchange for the job amenity of doing good (Burbano, 2016; Hedblom et al., 2019). In this paper, we ask how employers’ use of prosocial jobs affects earnings inequality. We draw on prior sociological research on heterogeneity in job values to predict that college-educated workers are more likely to accept a pay discount for working a prosocial job. Analyzing online job postings, we find a prosocial pay discount for jobs that require a college degree but not for non-college jobs.

Aggregating to the labor market level, we estimate that employers’ current level of prosocial jobs reduces the overall college wage premium by around 5 percent. This discount also appears in pay data reported by workers, suggesting that it is not driven by employers’ selective disclosure of pay in job postings. Supplementary evidence documents that hiring managers expect a larger prosocial discount for jobs requiring a college degree. We also find that college-educated workers report that having a job useful to society is more important than do non-college workers, and that workers who attended colleges more focused on community service are more likely to accept prosocial jobs. Together, these findings demonstrate that employers’ prosocial job amenities can actually offset the largest source of rising earnings inequality in the contemporary US labor market: the college pay premium.

These findings uncover a rare channel through which the shift toward post-bureaucratic organization actually reduces, rather than exacerbates, earnings inequality. A substantial line of research investigates new employer-side practices, like outsourcing and performance-based pay, that have increased inequality (Lemieux et al., 2009; Weil, 2014). In contrast, employers’ use of prosocial job amenities, as heterogeneously valued by workers at different skill levels, actually offsets inequality. This effect on inequality is likely unintended by employers. We agree with prior commentators who suggest that market pressure makes it unlikely prosocial employers will successfully and consistently offer workers *higher* pay (Henderson, 2020; Osterman, 2018): once we control for differences in job requirements, prosocial jobs pay no premium for non-college workers. Instead, we provide evidence on an employer practice that allows lower pay disproportionately for otherwise high-paid workers.

This channel of inequality reduction uncovers an organizational practice that can lead college workers to accept lower pay. Recent research has been skeptical of standard policy prescriptions for reducing inequality. Educational expansion increases the likelihood of underemployment for new graduates (Horowitz, 2018). Labor unions, which previously boosted pay for workers without a college degree, now represent nearly as many college as non-college workers (Farber et al., 2018). Minimum wages primarily affect inequality among less-educated workers, rather than closing the gap with college graduates (Autor et al., 2016). These beleaguered approaches focus either on changing the skill composition of the economy or on strengthening institutions that restrain inequality in pay setting.

We focus instead on how a prosocial job amenity can substitute for pay. This channel does not require restrictions on employers or uncompensated penalties for higher-educated workers. Instead, it shifts employment compensation for these workers from pecuniary to nonpecuniary forms. In this way, cost-minimizing employers can actually lower pay (for certain workers) while reducing inequality. This alignment between employer costs and incentives with inequality reduction could make this channel more stable and robust than other approaches to reducing inequality. Of course, this mechanism reduces inequality by lowering pay for those at the top, rather than raising pay for workers at the bottom. But more generally, our analysis points to a broader set of mechanisms, which reduce material inequality by trading pecuniary for nonpecuniary rewards such as status, job satisfaction, and intrinsic motivation. This broad class of tradeoff mechanisms defines an interesting area for future research.

Several limitations of our study also suggest future research. First, we cannot determine whether marginal increases in prosocial job shares within a labor market will increase or decrease the magnitude of the effects estimated here. If prosocial job amenity is essentially a rivalrous status good—that is, the more people who have it, the less value it has—then further expansion of prosocial job framing should have diminishing returns. As more and more employers advertise prosociality, it will become less valuable as a basis for status

distinction. In contrast, if prosocial job amenity is more like a social norm, then the more widely employers embrace it, the more socially costly it will be for those who remain in regular jobs. In this latter case, increasingly widespread use of prosocial advertising will spur an increased gap between prosocial and regular jobs. Determining whether prosocial job amenity is more like a distinction good or a spreading norm is important for predicting the rate of change in inequality from further increases in employers' prosocial job framing.

Second, we focus in our analysis on a specific type of inequality, between workers with different levels of education. Rising inequality between education groups is a key contributor to rising overall labor market inequality (Autor et al., 2020), and exacerbates other important types of inequality, like the black/white wage gap (Bayer and Charles, 2018). However, reductions in between-education inequality that come from prosocial job meaning could exacerbate other forms of inequality. For example, a recent article finds that, alongside the positive association between education and prosocial work, there is also a negative correlation between class background and prosocial work, which mediates part of the class background pay gap (Fang and Tilcsik, forthcoming). Likewise, if women are more likely to value prosocial meaning at work, then prosocial jobs exacerbate gender inequality (Burbano et al., 2021). Future research should consider trade-offs across these different axes of inequality.

A third limitation of our analysis is that we cannot distinguish between job advertisement rhetoric and workplace reality. Presumably some firms advertise a prosociality belied by their actual strategies and work practices (Amengual and Apfelbaum, 2021). Future research should consider how workers' views of the meaning of their work changes over the course of job application, recruitment, and actual work. If an applicant accepts a job that an employer has falsely advertised as prosocial and at a wage that is lower than that for an equivalent regular job, he or she is likely to seek a new position upon discovering the deceit. This issue suggests that the distribution of employer credibility as a type of organizational social capital—workers' trust in the employer's advertised claims—could affect inequality in

between-firm pay differences.

Matching data like the job postings used in this paper, or other official company statements, to newly available data from workers' online job reviews could be a promising avenue for comparing rhetoric to reality. Do workers sanction employers who betray their prosocial advertisements? Many recent instances of labor organizing among highly educated workers have focused not on raising pay, but on punishing perceived employer violations of prosocial commitments. For example, engineers at Google have protested the company developing artificial intelligence for the Department of Defense and the company's handling of sexual harassment (Wakabayashi et al., 2018). If prosocial reputation becomes a significant part of some skilled workers' implicit compensation package, we could expect such conflicts and counterclaims to become more common. Understanding the conditions under which the hypocrisy constraint binds or can be avoided remains an important area of labor market research.

More broadly, our results suggest an understudied way in which non-price-mediated interdependence among workers affects inequality. The job characteristics that yield compensating differentials, in our theory, are valued neither as universal preferences nor as idiosyncratic whims of individual workers. Instead, a job's nonpecuniary rewards are valued differently according to group membership, class or school socialization, or extent of participation in certain status competitions. Our analysis provides the first evidence that an old idea in the study of work—that some workers value their work not just for its wages but as a contribution to society—has a bearing on macro-level inequality. This approach offers a promising area for future sociological research on earnings inequality.



# Notes

<sup>1</sup>The final list of phrases is: mission; compassion; core values; make a difference; compassionate; our values; making a difference; give back; positively impact; charitable; peoples lives; charity; people s lives; real difference; enriching; giving back; improve the lives; mission driven; life changing; embodies; lives of millions; humanity; tens of thousands; respect and dignity; live healthier; shape the future; purposeful; recognized and rewarded; core belief; improving the lives; lasting impact; meaningful ways; planet through; improve people; positively affect; tens of millions; values driven; hundreds of millions; someones life; positively impacts; people in need; gives back; transforming lives; donating; transforming peoples; purpose of improving; core beliefs; touches millions; improves the lives; tangible impact; donates; healthier and happier; accelerate the journey; positively affects; materially impact; improve mankind; missiondriven; materially affect; improves or saves; missionbased; lifechanging; renewing minds; differencemaker; strengthening communities; positive impact; your impact; climate change; the lives of; impact on the environment; impact on people; impact on youth; impact in the community; impact in someone's life; impact on society; positive impact; your impact; climate change; the lives of; impact on the environment; impact on people; impact on youth; impact in the community; impact in someone's life; impact on society; right thing; change the world; the life of; serving the community.

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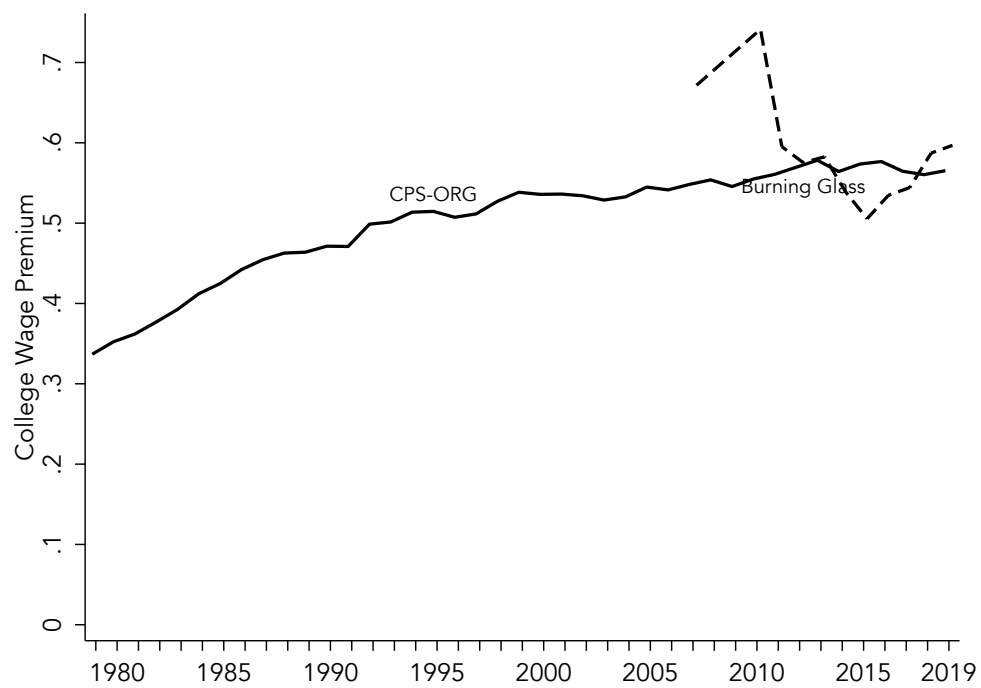
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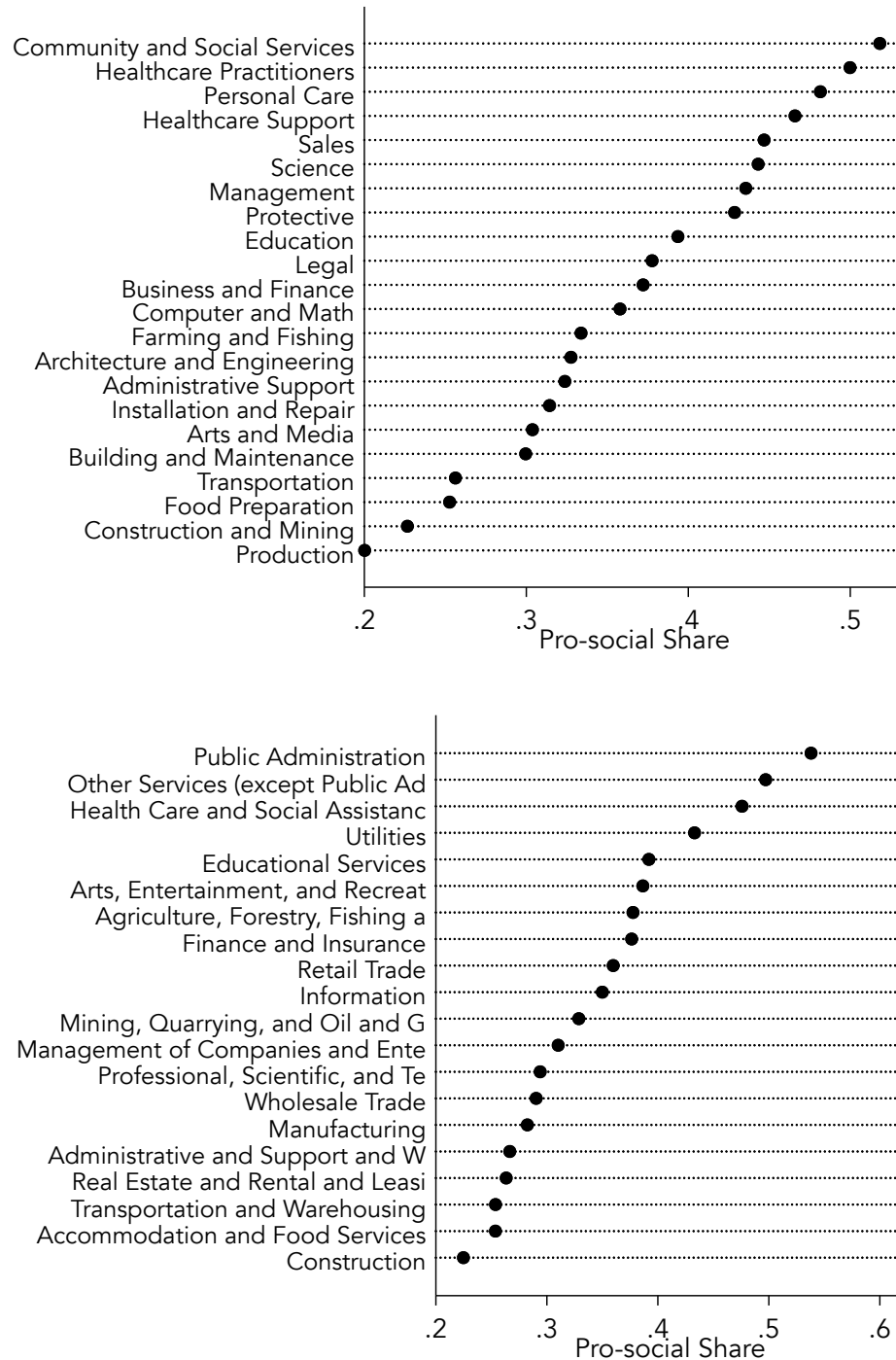
Figure 1: The Rising and Flattening College Wage Premium



Note: Data are from Current Population Survey—Outgoing Rotation Group and Burning Glass job postings. Less than high school are excluded, as job posting data does not include less than high school degree required.

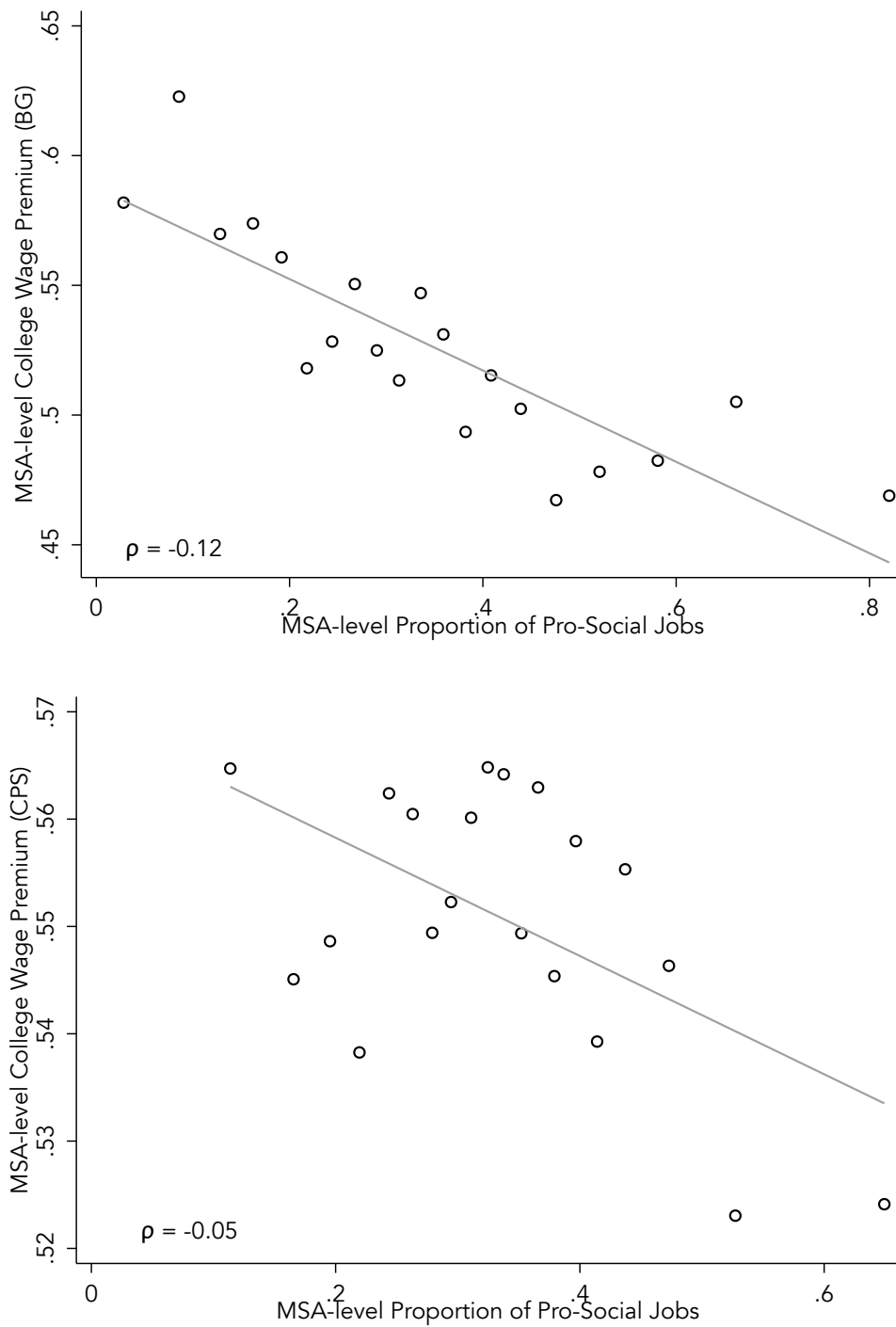


Figure 2: Prosocial References in Job Postings, by Occupation and Industry



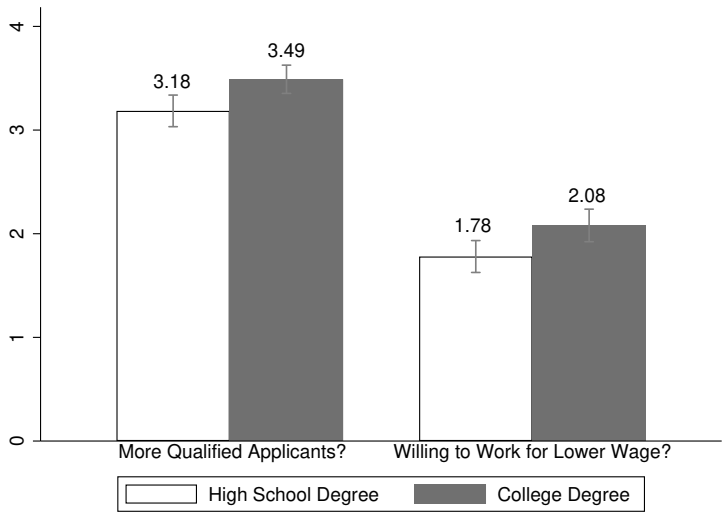
Note: Data are from word embedding analysis of BGT job postings.

Figure 3: MSA-level Proportions of Prosocial Jobs and College Wage Premium



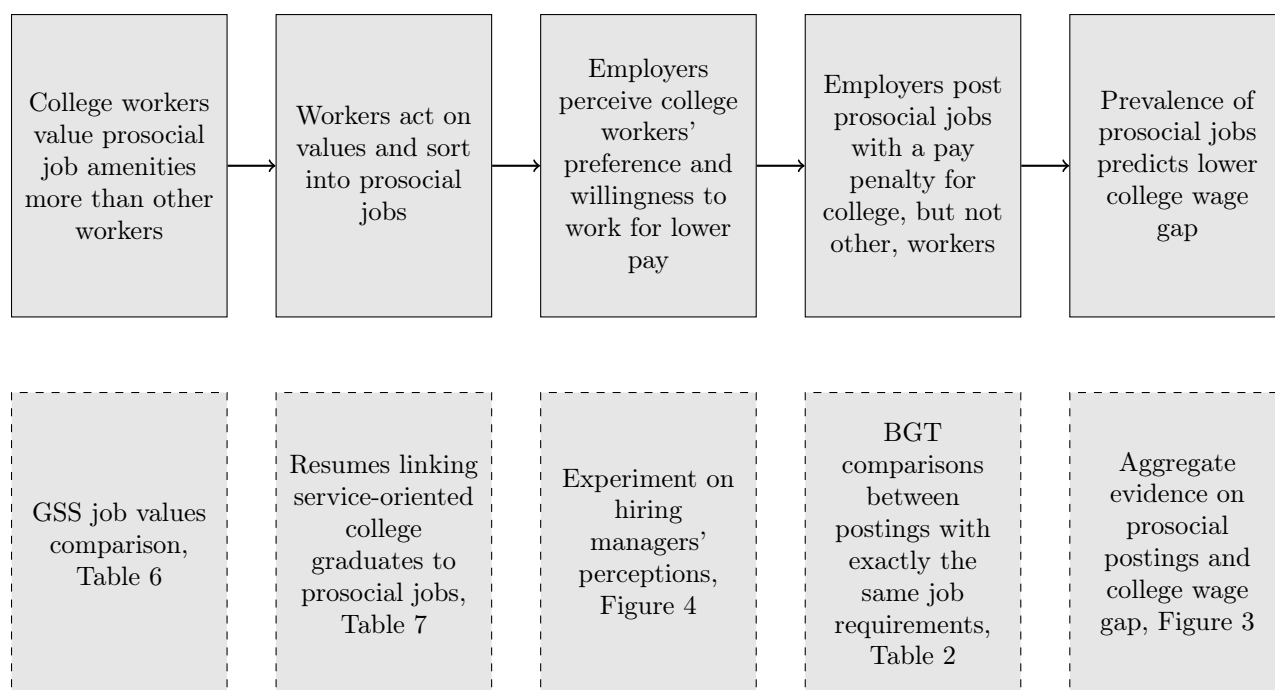
Note: Data are Burning Glass and CPS-ORG. Bins defined at 20 equal-sized quantile cuts of MSA X year observations.

Figure 4: Demand-side Evidence on Variation by Skill in Prosocial Effects



Note: Results based on 400 individuals with hiring experience considering a job requiring either a college or a high school degree. Plotted results are their responses to two questions: “Do you think including this prosocial message would help to attract more qualified candidates for this position?” and “Do you think that the prosocial message may encourage job applicants to take this position despite offering a slightly lower pay relative to the market?”

Figure 5: Theory and Evidence on Prosocial Jobs and Inequality



Note: Theoretical claims are in first row with solid boxes; corresponding evidence is in second row with dashed boxes.

Table 1: Descriptive Statistics

	Non-college		College	
	Mean	SD	Mean	SD
log(Annual Earnings)	10.42	0.46	11.05	0.50
Pro-social	0.33	0.47	0.42	0.49
log(Text Length)	7.80	0.61	7.99	0.64
log(Number of Skills Listed)	1.84	0.81	2.19	0.77
Experience Required	1.17	1.87	2.50	2.79
Bonus or commission	0.04	0.20	0.04	0.20
Part-time	0.16	0.36	0.07	0.25
Observations	4505733		4061998	

Note: Data are Burning Glass.

Table 2: Pro-social Jobs Earning Penalty if College Degree Required

	(1)	(2)	(3)	(4)	(5)
College	0.528*** (0.024)	0.562*** (0.025)			
Pro-social		0.059** (0.018)	0.058*** (0.012)	0.035*** (0.008)	-0.000 (0.004)
Pro-social * College		-0.090*** (0.019)	-0.054** (0.017)	-0.041** (0.013)	-0.067*** (0.017)
log(Text Length)	0.107*** (0.012)	0.102*** (0.013)	0.084*** (0.011)	0.074*** (0.007)	0.050*** (0.008)
log(Number of Skills Listed)	0.003 (0.014)	0.002 (0.014)	-0.007 (0.010)	-0.015*** (0.004)	
Experience Required	0.052*** (0.003)	0.052*** (0.003)	0.044*** (0.003)	0.031*** (0.002)	0.013*** (0.003)
Bonus or commission	0.106*** (0.022)	0.101*** (0.022)	0.090*** (0.021)	0.050** (0.017)	0.048*** (0.010)
Part-time	-0.188*** (0.034)	-0.187*** (0.034)	-0.133*** (0.021)	-0.080*** (0.013)	-0.022*** (0.003)
Constant	9.538*** (0.083)	9.566*** (0.081)	9.892*** (0.070)	10.014*** (0.048)	10.148*** (0.059)
$R^2$	0.36	0.36	0.56	0.65	0.91
Fixed effects:					
Year	×	×			
Year X Ind. X MSA X College			×	×	×
Occup.				×	
Occup. X Skill set					×
Observations	8253214	8253214	8132888	8132881	4679344

Note: Data are Burning Glass. All postings with non-missing salary, industry and education information included. Standard errors are clustered at the 3-digit industry level.

\*  $p < .05$ ; \*\*  $p < .01$ ; \*\*\*  $p < .001$  (two-tailed tests)

Table 3: MSA-level Effects of Prosocial Postings on College Pay Premium

	CPS College Premium			BG College Premium		
	(1)	(2)	(3)	(4)	(5)	(6)
Pro-social MSA share	-0.055** (0.021)	-0.057** (0.021)	-0.094*** (0.024)	-0.158*** (0.023)	-0.137*** (0.024)	-0.127*** (0.027)
$R^2$	0.00	0.04	0.31	0.02	0.03	0.27
Controls		×	×		×	×
MSA fixed effects			×			×
Observations	2787	2787	2786	2787	2787	2786

Note: Data are Burning Glass and CPS-ORG, aggregated to MSA-by-year level. Controls are from CPS-ORG: unemployment rates for college and non-college workers; share college workers out of total labor force; average age (as a proxy for workforce experience); share women; union density; share public sector; and year.

\*  $p < .05$ ; \*\*  $p < .01$ ; \*\*\*  $p < .001$  (two-tailed tests)

Table 4: Pro-social Jobs Earning Penalty (Glassdoor-based Pay Data)

	(1)	(2)	(3)	(4)	(5)
College	0.378*** (0.027)	0.388*** (0.027)	0.374*** (0.026)	0.057*** (0.013)	0.016 (0.028)
Pro-social		0.048 (0.025)	0.028 (0.017)	0.009 (0.010)	0.035 (0.033)
Pro-social * College		-0.076** (0.026)	-0.038* (0.018)	-0.024* (0.010)	-0.093* (0.044)
$R^2$	0.15	0.16	0.48	0.61	0.88
Controls	×	×	×	×	×
Fixed effects:					
Year	×	×			
Year X Ind. X MSA			×	×	×
Occup.				×	
Occup. X Skill set					×
Observations	1000118	1000118	930862	930849	744994

Note: Job postings are Burning Glass; dependent variable pay is from Glassdoor online reviews. All matched postings with non-missing salary, industry and education information included. Controls are log(text length), experience required, bonus paid and part-time job. Standard errors are clustered at the 3-digit industry level.

\*  $p < .05$ ; \*\*  $p < .01$ ; \*\*\*  $p < .001$  (two-tailed tests)



Table 5: Earnings Effects of a Job Switching to Pro-social

	Non-college			College		
	(1)	(2)	(3)	(4)	(5)	(6)
Pro-social	0.010*** (0.001)	0.010*** (0.001)	-0.001 (0.001)	-0.017*** (0.001)	-0.004*** (0.001)	-0.052*** (0.001)
$R^2$	0.82	0.85	0.95	0.76	0.79	0.94
Controls	×	×	×	×	×	×
Fixed effects:						
Emp. X MSA X Occ.	×	×		×	×	
Year	×			×		
Year X Ind. X MSA		×	×		×	×
Emp. X MSA X Occ. X Skill set			×			×
Observations	2731359	2693463	1740283	2146273	2122870	924697

Note: Data are Burning Glass. All postings with non-missing employer, salary, industry and education information included. Controls are log(text length), experience required, bonus paid and part-time job. Standard errors are clustered at the Industry X Year level.

\*  $p < .05$ ; \*\*  $p < .01$ ; \*\*\*  $p < .001$  (two-tailed tests)

Table 6: College-educated Workers Value Prosocial Job Characteristics

	Av. Prosocial		Useful to Society		Helps Others	
	(1)	(2)	(3)	(4)	(5)	(6)
College	0.076*** (0.019)	0.061** (0.019)	0.110*** (0.022)	0.092*** (0.021)	0.043* (0.021)	0.031 (0.021)
$R^2$	0.00	0.06	0.00	0.05	0.00	0.05
Controls for worker chars		×		×		×
Fixed effects:						
Year		×		×		×
Region		×		×		×
Observations	5499	5499	5499	5499	5499	5499

Note: Data are General Social Survey, years 1989, 1998, 2006 and 2016. Dependent variables are importance of a job that allows someone to help other people; that is useful to society; and the average of responses to both questions. Controls are age, age squared and female.

\*  $p < .05$ ; \*\*  $p < .01$ ; \*\*\*  $p < .001$  (two-tailed tests)

Table 7: High Service College Graduates Take Prosocial Employment

	2000-2018		2010-2018	
	(1)	(2)	(3)	(4)
University Community Service Score	0.032*** (0.001)	0.010*** (0.002)	0.050*** (0.002)	0.016*** (0.003)
$R^2$	0.02	0.06	0.01	0.05
Controls	×	×	×	×
Fixed effects:				
Year	×	×	×	×
MSA		×		×
Occupation		×		×
Observations	2591982	1829089	868733	540024

Note: The table shows the association between the graduating university's social service score and the employer's prosocial orientation. Data come from BGT's resume database. Graduating university's social service score comes from Washington Monthly's college ranking lists, and the employer's prosocial orientation is the employer's proportion of jobs in BGT job postings that mention prosocial values. Each individual worker-year is the unit of analysis. Controls are years in workforce and gender. Standard errors are clustered on individual.

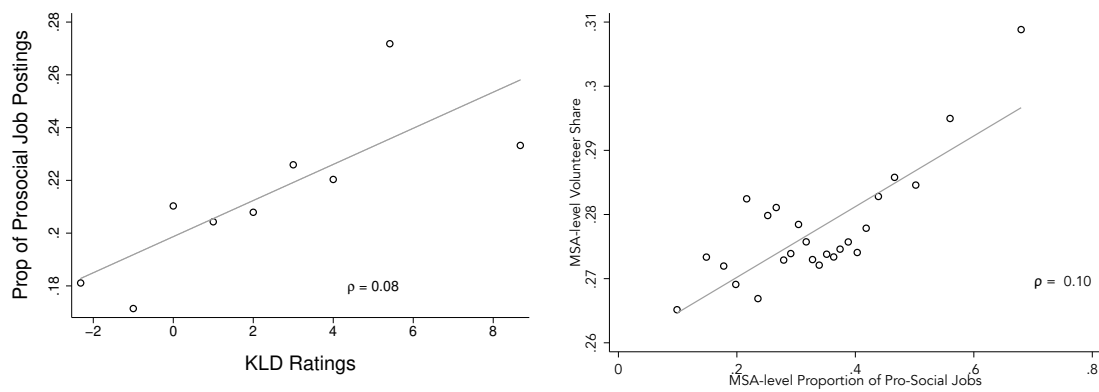
\*  $p < .05$ ; \*\*  $p < .01$ ; \*\*\*  $p < .001$  (two-tailed tests)

# A Appendices

[Table 8 about here.]

[Figure 6 about here.]

Figure A.1: Validating BGT Prosocial Values Measure



Note: Data are from Kinder, Lydenberg, & Domini (KLD) Social Performance Rating, Current Population Survey—Volunteer Supplement, and Burning Glass job postings.

Table A.1: Occupational Shares in Burning Glass and OES

	Burning Glass unweighted	Burning Glass weighted	OES 2019
Management	0.14	0.06	0.05
Business and Finance	0.09	0.05	0.06
Computer and Math	0.07	0.03	0.03
Architecture and Engineering	0.03	0.02	0.02
Science	0.02	0.01	0.01
Community and Social Services	0.03	0.02	0.02
Legal	0.01	0.01	0.01
Education	0.06	0.06	0.06
Arts and Media	0.02	0.01	0.01
Healthcare Practitioners	0.10	0.06	0.06
Healthcare Support	0.02	0.05	0.04
Protective	0.04	0.02	0.02
Food Preparation	0.02	0.10	0.09
Building and Maintenance	0.01	0.03	0.03
Personal Care	0.02	0.02	0.02
Sales	0.10	0.10	0.10
Administrative Support	0.15	0.14	0.13
Farming and Fishing	0.00	0.00	0.00
Construction and Mining	0.01	0.03	0.04
Installation and Repair	0.03	0.04	0.04
Production	0.02	0.05	0.06
Transportation	0.03	0.09	0.09

Note: Weighted Burning Glass series is weighted based on occupation, state and occupation employment totals from the 2019 Occupational Employment Statistics (OES).