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# Does public ownership and accountability increase diversity? Evidence from IPOs

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## Abstract

Does public ownership improve employment diversity? Organizational researchers theorize that increased transparency to regulators and the public should lead firms to conform to legal and social norms; but also that social closure and decoupling should preserve the *status quo*. Empirical research has been difficult because we lack data on comparable private firms and because firms likely self-select into going public. We construct a new, nationally representative dataset that links firms' filings for initial public offerings to longitudinal data on employment composition from the Equal Employment Opportunity Commission. We construct a set of comparable firms by looking at companies that filed and then withdrew a plan for an IPO. To account for selection bias in withdrawal and IPO success, we instrument the transition to public ownership using market returns in the book-building phase of the firms' IPO attempts. We find no evidence that moving from private to public ownership increases the representation of women or nonwhite workers or managers. We discuss the implications of this finding for our ability to generalize findings in organizational research.

## Introduction

Researchers and policymakers debate whether we can improve organizational outcomes by making internal practices visible and accountable to outsiders. In the wake of the civil-rights movement, a critical outcome has been the diversity of organizations' workforces and leadership. Thus social scientists have studied how formal reporting requirements and information disclosure affects workforce composition. Such research takes two main forms. One form considers internal rules and policies that directly target diversity and often involve disclosure to regulators, if not to the public (Kalev et al., 2006; Cohen et al., 2009; Stainback and Tomaskovic-Devey, 2012; Ferguson, 2015). The other form considers the indirect effects of public ownership through equity shares. In this second case, diversity is not what drives firm owners' decision to go public, but the process of the initial public offering (IPO) itself requires formalizing internal procedures and revealing information to interested outsiders. Diversity scholars have theorized that such formalization should alter

workforce composition (Reskin et al., 1999; Farrell and Hersch, 2005; Perrault, 2015). In such cases we can ask, does public ownership improve employment diversity?

In this paper, we examine private firms' IPOs on the stock market. This is a salient event for several reasons. First, securities regulation in the United States imposes a raft of reporting and disclosure requirements on firms that seek investment on the exchanges. Researchers know this well: data from those reporting requirements are a reason why so much research focuses on publicly traded firms. Second, many efforts to reform corporate practices try to leverage public firms'<sup>1</sup> reputations to prod them to change (Soule, 2009; Vasi and King, 2012). Thus activists track the gender composition of corporate boards to see whether the newly public recruit more women than older firms (2020 Women on Boards, 2017), and lobby firms in shareholder meetings and other venues to release detailed employment data (Miller, 2014; Leswing, 2017). Third, publicly traded companies are some of the largest employers in the United States,<sup>2</sup> so any efforts to improve their workforce diversity could have a disproportionate impact. Fourth, in addition to theoretical priors, data on corporate board diversity suggests that publicly traded firms might be more diverse than privately held ones (Dezső et al., 2016; Zarya, 2017). It would be useful to know how much of that difference, if any, reflects the causal impact of the transition to public ownership.

Despite the setting's salience for this research and policy question, we know little about how going public affects the composition of firms' workforces. At best, we have contradictory evidence on the relative diversity of public and private firms. Public firms are older and larger than private ones, on average, and operate in a different mix of industries, which might increase their observed diversity (Davis et al., 2007). But public firms also tend to be more profitable and have larger internal labor markets, which make them more attractive workplaces; social closure by the dominant group might limit opportunities for diversifying such firms (Weeden, 2001). For any of these reasons, a cross-sectional relationship could be spurious. What little evidence we have on the within-firm, before/after effects of going public is mixed. Most such studies necessarily limit themselves to the composition of firms' boards (Bhagat and Black, 1999; Miller and Del Carmen Triana, 2009; Walters et al., 2017), and they lack the counterfactual prediction of boards' compositions had

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<sup>1</sup>Throughout, we must distinguish between *publicly traded* or *publicly owned* firms and *public-sector* firms, i.e., those owned and/or operated by the government. For clarity, throughout this paper we use "public firms" to mean publicly traded, private-sector companies, and explicitly refer to "public-sector" firms and organizations when that comparison is relevant.

<sup>2</sup>Though this trend has declined somewhat in recent years (Editorial Board, 2018).

the firms remained private. Even if we had reliable before/after information for a representative population of firms, we would still need to deal with firms' self-selecting into going public.

We construct the first dataset to reliably examine the effect of transitioning to public ownership on employment composition in US firms. We match information from firms' initial registration statements with the Securities and Exchange Commission (SEC) to data on workforce composition from the annual EEO-1 establishment surveys that those firms file with the Equal Employment Opportunity Commission (EEOC). Doing so gives us longitudinal information on firm's employment diversity before, during, and after the period when they either complete or withdraw their filing for an IPO.

Such a dataset increases generalizability and lets us examine within-firm changes rather than cross-sectional differences. However, it is insufficient to deal with endogeneity concerns. Firms may withdraw their IPO filings for reasons that also affect employment (most obviously, sudden performance declines) and thus confound the estimated affect of the IPO itself. To address this issue, we use stock-market fluctuations in the two months following the IPO filing date as an instrument for IPO completion. Several recent studies that try to estimate the causal impact of going public on other outcomes, such as innovation (Bernstein, 2015), have used this type of abnormal market returns as an instrument (See also Busaba et al. (2001)). This instrument relies on the sensitivity of filers to stock-market movements during the book-building phase of their IPO. Such movements have large implications for the focal firm's valuation but, crucially, are beyond the focal firm's control. Furthermore, while these market movements directly affect firms' valuation they have much less impact on long term performance. Thus, they are unlikely to be systematically related to the longer-run trends in the firm's employment composition. This instrumental variable approach gives us a way to construct *de facto* treatment and control groups of firms and thus draw causal inferences about the impact of IPO on workforce diversity. Furthermore, because we have information about employment composition for *all* large U.S. employers in the time period, not just those who attempted to go public, we can compare the endogenous effects, if any, of planning to go public with the treatment effect of the status transition itself.

We find, essentially, well-estimated zero effects of the transition to public ownership on employment diversity. Publicly traded firms have less diverse workforces than the economy as a whole, and we find no evidence that said diversity happens in the wake of, or as an affect of, an IPO.

This holds whether we consider the nonwhite share of the firm’s workforce, or its female share; the nonwhite share of managerial and supervisory share of the firm’s personnel, or the female share of the same. While we find no *treatment* effects associated with the IPO, we do find that some salient differences across firms who *intend* to go public. Compliance with filing EEO-1 surveys is higher among those firms that submit initial IPO filings, for example, which is consistent with research on formal rules and policies that involve regulatory disclosure (Dobbin and Sutton, 1998; Bielby, 2000). Because we do not have composition data for non-complying firms, we cannot rule in or out an endogenous effect of formalization on diversity in anticipation of going public. Nonetheless, those hoping for a substantial impact on employment diversity from the greater accountability and transparency associated with public ownership will find these results distressing.

These findings make three contributions. First, they lend support to the idea that diffuse accountability is insufficient to produce substantive change in employment diversity, and that a specific focus on diversity is required (Reskin, 2000; Kalev et al., 2006). Second, they imply that stakeholder activism and private politics, by themselves, have not shifted the general norms around public corporations Briscoe and Safford (2008); Soule (2009); Briscoe and Gupta (2016). Third, they suggest that even substantial changes in firm governance may not affect composition in well-established firms, and that a focus on newer firms, and the turnover of firms, may offer greater rewards Ferguson and Koning (2018).

Despite considerable interest from researchers in the early years of this century (e.g., Bielby (2000), Reskin (2000)), the bloom has somewhat faded from the rose of formal bureaucratic procedures as a means to improve public welfare. In part, this is because managers’ resistance to such policies can be greater than first anticipated (compare Kalev et al. (2006) with Dobbin et al. (2015), for example). But organizational research in this area suffers from a subtler problem as well. Firms have the discretion to collect and share certain types of data with researchers. For workforce-diversity information, this is almost always so. Firms have similar discretion to implement policies meant to move the needle on diversity. Such discretion is not a problem in its own right. For those of us who want to know the causal impact of such policies, though, this self-selection constantly threatens bias. Worse still, it is a bias that puts a thumb on the scale in favor of results we might prefer (Ferguson, 2015). This is why events like the IPO are so important for us to study: where we have uniform reporting requirements and exogenous sources of variation, we can weigh with more

balanced pans. And we can reevaluate whether our current policy regime does indeed contribute enough to tip the balance.

## **Publicly traded firms and workforce composition**

Two streams of organizational thought bruit the idea that changing from private to public ownership could affect employment diversity. One flows from research on the role of bureaucracy in personnel policies. The other flows from research on social movement activism against private firms. In each case, researchers theorize that a combination of outsider control through an ownership stake and formalization for compliance and reporting will discourage firms from persisting in bad practices.

The replacement of managers' personal discretion with formal personnel rules and procedures is well studied. The roots of such formalization are varied, ranging from trade unions' desires for employment security (Jacoby, 1985) through scientific managers' attempts to optimize labor allocation (Baron et al., 1988) and firms' needs to comply with external regulation (Edelman, 1990; Dobbin and Sutton, 1998). In each case, instituting objective criteria and explicit decision rules for allocation is supposed to reduce the scope for "pre-organizational" characteristics like race and sex to influence the firm's choices (Perrow, 1986). A key argument in such work is that, absent widespread commitment by firms themselves to change their employment practices, it has been the growth of the regulatory state and its intrusion into economic life that has convinced, or compelled, managers to change (Dobbin and Sutton, 1998; Kelly and Dobbin, 1999; Edelman et al., 1999). New reporting requirements often expose firms' internal practices to public scrutiny, which puts pressure on laggard firms to improve their performance, even if they already have formal practices in place (Fung et al., 2007; Chatterji and Toffel, 2010).

Applying this reasoning to a firm's IPO is straightforward. Securities regulations in the United States impose a host of reporting requirements on publicly traded firms. Investors also compare publicly traded firms to each other more often, which encourages standardization on certain dimensions to facilitate apples-to-apples comparisons (Zuckerman, 2000). Personnel policies are frequently targeted for rationalization in the run-up to IPO: top management teams know that they will be subjected to greater scrutiny under public ownership and take actions to "get out from behind the eight ball" (Hastings, 2018; Irvin, 2016). Such pressures, and management's attempts to conform to

them, are why public ownership is widely argued to improve firm governance (Hart, 2003; Nelson, 2003; Hochberg, 2012).

Activists and investors care at least as much about public firms' practices as the government does. Activists of various stripes have increasingly targeted corporations to pursue social goals (Soule, 2009; King and Pearce, 2010; Briscoe and Gupta, 2016). Activism specifically around race and gender discrimination in employment was a hallmark of the Civil Rights and women's movements, particularly as the focus of those movements shifted from legal reform to enforcing and complying with new laws (Minkoff, 1999; Briscoe and Safford, 2008; Ferguson et al., 2018). Many of the tactics used by social movements that target private firms leverage those firms' reputations among their shareholders, either directly or indirectly through the effect that the firms' broader reputations have on their share prices (King and Soule, 2007; McDonnell et al., 2015).

Privately held firms are less exposed to stakeholder activism than their publicly traded counterparts, for at least two reasons. First, gathering basic information about the firm is often more difficult, because such firms need not produce annual reports for shareholders or hold public meetings. Second, activists have fewer levers with which to move private firms. Direct actions like boycotts (McDonnell and King, 2013) can still make a dent, but indirect actions that might affect equity prices are not an option. Thus to the extent that outside activism can affect a firm's hiring and promotion practices, we would expect it to bite more in firms once they have gone public, and thus again to expect within-firm change around the IPO.

In short, organizational research suggests at least two broad reasons why we might expect publicly traded firms to be more diverse than privately held ones. Yet there are also at least two theoretical reasons why we might not see such effects. The first has to do with social closure, the practice by which "social groups formed around positions in the technical division of labor create social and legal barriers that restrict access" to members of that social group's choosing (Weeden, 2001, p. 57). Historically, publicly traded firms have been among the most successful in the American economy: they are larger, more profitable, more innovative, and faster growing. In part because of their size and scope, they ramified internal labor markets that offered comparatively good, long-term employment (Kochan et al., 1986; Davis, 2009). Precisely because publicly traded firms offer better jobs, we might expect that powerful social groups—in this context, white males—would try harder to preserve positions in such organizations for themselves. While social closure



might lead us to suspect that public firms would be less diverse than private ones, on average, it is less clear what effect it might have on a firm's transition to public ownership. Social closure could account for a "levels" difference between firms; more successful firms might be less diverse, not because whiteness or maleness drives success, but because success encourages powerful actors like white males to try to monopolize opportunities. Yet social closure need not also imply a "trend" difference during IPO. Successful firms that go public might be less diverse than the rest of the economy yet see growth in their relative diversity after IPO, for the reasons discussed already.

The second reason we might expect little impact of IPOs on workforce diversity is decoupling. Virtually no employment regulations mandate specific compositions for any workforce. Instead, they emphasize unbiased policies and procedures. Even the EEOC's establishment surveys, from which we get some of our primary data, are intended to establish audit trails of firms' employment records, not to be *prima facie* evidence of probity or malfeasance (Stainback and Tomaskovic-Devey, 2012). Thus American legal doctrine looks for compliance with anti-discrimination legislation in firms' adoption of diversity policies rather than in who, precisely, they hire. Such an approach only works insofar as the policies and procedures adopted actually do affect employment diversity. Unfortunately, the evidence for which practices do and do not encourage diversity is mixed at best (Kalev et al., 2006; Sørensen and Sharkey, 2011; Ferguson, 2015). Ceremonial compliance is a risk in this setting: firms adopt policies that protect them from legal liability but do not move the needle on diversity. Pressure from outside activists need not abate just because firms have complied with legal requirements. Yet activists have no privileged certainty about which policies will work. Furthermore, recent attempts to replicate the salutary effects of simple transparency and simple information disclosure on other organizational practices have frequently failed to replicate earlier findings (e.g., Loewenstein et al. (2014), Ho et al. (2017)).

In sum, our prior beliefs about how going public might affect firm diversity are ambiguous. There are well-founded theoretical reasons to predict effects from the change to public ownership, but also well-motivated null expectations. Thus we think it is reasonable to summarize our predictions in terms of testable hypotheses. Given the historical dominance of employment and particularly leadership in the United States's large firms by white males, we operationalize diversity with shares of female and nonwhite employees and managers. Thus we offer four hypotheses:

**Hypothesis 1** *Going public via an IPO should increase the share of female employees in a firm.*

**Hypothesis 2** *Going public via an IPO should increase the share of female managers in a firm.*

**Hypothesis 3** *Going public via an IPO should increase the share of nonwhite employees in a firm.*

**Hypothesis 4** *Going public via an IPO should increase the share of nonwhite managers in a firm.*

It is in situations like these that we are typically most interested in sound empirical research, because ultimately we need data and agreement with it to adjudicate theoretical claims. Yet reliable data on the employment effects of going public has been hard to come by.

### **Research designs for studying ownership**

Estimating the effects of ownership types requires clearing two empirical hurdles. Both relate to whether we can construct a valid counterfactual. First, many types of information are *only* gathered on or reported systematically by publicly-traded firms. Thus, even for firms that receive the “treatment” of going public, we may lack the data to do a before-and-after test. Second, the decision to take a firm public is not random, so there is every chance that the decision is correlated with other actions that would affect employment composition. Thus, even if we did have before-and-after data, we could not treat the estimated effect as causal.

Existing research on ownership changes reflects these limitations. Many studies focus on boards or top management teams, because information on such people is more readily available. Yet large organizations might make titular or token changes at the most visible levels of management without significantly altering their larger composition (Dezsó et al., 2016). More fundamentally, privately-held firms do not have to disclose data on their senior leadership, so efforts to study public ownership almost always have to track changes to boards or top management teams *since* going public (e.g., Farrell and Hersch (2005); 2020 Women on Boards (2017)). In such work’s defense, we have little reason to presume that most companies are exemplars of diversity before they go public. But firms might try to change their composition somewhat before an IPO, to better conform to prevailing opinions. In such cases, any “effect” of public ownership could predate the IPO itself and be missed by *post hoc* analyses.

A study that can estimate the causal effect of an IPO requires data that clear these empirical hurdles. First, we need data on employment composition in firms whose availability is not conditional on ownership status. The survey data from the EEOC that we use here relies on an establishment-*size* threshold but, crucially, is collected both for privately held and publicly traded firms. Second, we need an exogenous source of variation in ownership type. This is why we apply a technique developed in financial research to instrument IPOs with abnormal market returns during the book-building phase of the IPO process, which we describe below.

## Method

### Data Sources

We draw on three sources for our data. Information on IPO filings comes from Thompson Financial. Market returns draw on NASDAQ data from Compustat. We build firm diversity measures using establishment surveys filed with the EEOC.

When a firm decides to try to go public, it must file an initial registration statement with the SEC. The chief record of this registration is form S-1, which describes the filer's business and gives information about the firm's finances. After submitting the S-1, firms enter the book-building phase, where they market the IPO to potential investors. During this phase, companies have the option to withdraw their IPO by submitting form RW to the SEC. Roughly 25 percent of all initial registrations are withdrawn before IPO. Withdrawing firms tend to remain private; only 18 of withdrawing firms filed a second registration within five years of their withdrawal (Bernstein, 2015).<sup>3</sup>

We gather S-1 data using Thompson Financial's SDC New Issues database. SDC's coverage of withdrawn IPOs begins in 1985. We therefore collected all S-1s filed from 1985 to 2014, when our data series from the EEOC ends. Following the IPO literature, we exclude financial vehicles like Real Estate Investment Trusts (REITs) that have few or no employees. Since distinguishing between financial firms and these vehicles is difficult, we exclude financial firms from our analysis. Specifically, we exclude unit offers, American depository receipts (ADRs), limited partnerships, special acquisition vehicles, spin-offs, closed-end funds (including REITs), and financial firms (SICs

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<sup>3</sup>Including or excluding firms that file more than one S-1 does not affect our pattern of results.

6000 through 6999). We find 8,475 initial registrations filed between 1985 and 2014. Of these, 2,238 were subsequently withdrawn. This 26 percent withdrawal rate is consistent with prior research.

For our abnormal-returns instrument, we follow Bernstein (2015) and use fluctuations in the stock market in the months after a firm's S-1 filing as an instrument for going public. We pulled data on the daily value of the NASDAQ from 1 January 1985 through 31 December 2014 from Compustat. For each filing, we then calculate the NASDAQ return for the three-month period before the S-1 filing and for the two-month period after the filing. The first measure allows us to control for general trends in the stock market. The second reflects changes in market conditions that are exogenous to the firm's filing decision. If returns are positive, the firm is more likely to complete its IPO. If the market drops, the firm is more likely to withdraw its filing.

To monitor compliance with the Civil Rights Act, the EEOC collects data on the sexual and racial composition of workforces in establishments with 100 or more employees.<sup>4</sup> The EEO-1 survey form gathers identifying information for the establishment, such as its location, industry, and (when relevant) its parent firm. The parent-firm field is quite useful for us because it lets us construct firm-level composition from establishment-level data. The bulk of the survey form is a matrix of occupational classifications and race/sex combinations, into which employers enter counts of employees. Both the occupational and race/ethnic categories have remained fairly stable over the years. Stainback and Tomaskovic-Devey (2012) discuss the structure of and trends within these data in detail. Ferguson and Koning (2018) explain changes particularly to the 2007 cohort of EEO-1 surveys that must be taken into account to build comparable series beyond 2006. Following their lead, we exclude the cohort of firms that enter the data in 2007 as well as the 2007 observations for firms that appeared earlier.

We matched firms between the EEOC and SDC datasets. These two data sources do not share a universal common identifier, nor are all S-1 firms necessarily in the EEOC's data. We think it is useful to consider what match rate we can reasonably expect in these data. Only firms above the EEOC's size threshold are required to file EEO-1 surveys, and various studies have suggested that the compliance rate for reporting is between 75 and 80 percent (Perman, 1988; Robinson et al., 2005; Stainback and Tomaskovic-Devey, 2012). While the S-1 filings do not include employee counts, we can measure how many of the 6,237 completed IPOs say they have more than 100 employees in

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<sup>4</sup>The threshold is 50 employees if the establishment performs significant federal-contract work.

the firm’s first annual report. 5,162 annual reports have this information and 3,613, or 70 percent, have more than 100 employees. Assuming that the firms that do and do not go public have similar employee count distributions, then of the 8,475 S-1 filings we can expect 5,933 to have more than 100 employees before going public. Taking into account the EEOC’s reporting compliance rate, we would expect a “perfect” matching algorithm to match 4,450, or 53 percent, of the S-1 filings to an EEO-1 record.

We use a two-stage matching algorithm. In the first stage, we match records based on their having the exact same standardized firm name and 5-digit ZIP code, the same Dun & Bradstreet number, or the same Employer Identification Number (for IRS filings). Matching solely on EEO-1 reports from the same year as the S-1 filing, we match 1,297 S-1 records. Matching on EEO-1 reports from any year, we match an additional 2,540 S-1 records. In the second stage, we set aside the records matched in the first stage and perform a fuzzy match on the remainder. Using Stata’s `reclink` command, we first block by state and then generate match scores based on the Levenshtein distance between firm names, ZIP codes, and street addresses. We had a research assistant manually review the highest-scored matches for each S-1 record and select the best one, or no match if none of the candidates looked appropriate. Restricting our match to EEO-1 reports from the same year of the S-1 filing, we matched 351 S-1 records. Expanding our match to EEO-1 reports from any year, we matched an additional 231.

In total, we matched 4,419 of the 8,475 S-1 records to at least one EEO-1 report, for a match rate of 52 percent. Given that our expectation was 53 percent, we are satisfied with the matching procedure. The final matched sample includes 672, or 15 percent, withdrawn IPOs. The withdrawal rate in the unmatched S-1 population is 39 percent. Most of this difference appears to stem from smaller firms’ being less likely in general to complete IPOs than larger firms. Because we want to measure changes in composition in the five years after IPO and because our EEOC data series ends in 2014, we concentrate on all matched firms in our data that produced an S-1 filing between 1985 and 2009. Figure 1 gives a sense of the clustering of IPO filings over time, as well as the IPO withdrawal rate and the success of our matching process for different years.

[Figure 1 about here.]

Several trends are noticeable in Figure 1. The annual number of IPOs dropped off sharply

after the tech bubble burst in 2001 (left-hand panels). After rebounding (though not to near their previous levels), IPOs dropped again after the housing bubble burst in 2008. The success rate for IPOs has declined over time (panel B). In our data, the success rate bottoms out at less than 40 percent in 2009. Given that our instrument leverages abnormal market returns, this secular swing is actually reassuring. We should *hope* that, amidst the greatest market turmoil since the Great Depression, many firms would rethink their decision to go public. While the withdrawal rate of IPO filings has risen over time, this is not strongly correlated with our ability to match filings with EEOC data, which is roughly constant (panel D).

One concern that Figure 1 raises is that, as the withdrawal rate of IPO filings grows, a gap opens up between our match rates for firms that go through with IPOs and that for firms that pull out (Panel F). The most obvious reason why we would not find matched data for withdrawn firms is because those firms remain small (i.e., under the 100 employee EEO-1 reporting threshold) or go out of business. A subtler reason could be that firms that withdraw are less likely to fulfill their various reporting requirements, since they have fewer eyes on them. If such firms were also less likely to diversify, then we would effectively be comparing firms that went public to firms that withdrew and kept up their reporting requirements—the “good” end of the privately-held spectrum, as it were. This could bias our analysis against finding effects from IPOs.

These hypotheticals raise any number of questions about how changes in firm performance might simultaneously affect ownership type and workforce composition (through growth or layoffs, for example); and it underscores why it could be misleading to compare *all* successful IPO filings to *all* failed ones, as in Figure 1. Concerns like these motivate our choice of an instrumental-variable approach in our main analyses. As we discuss below, we supplement OLS results (which would be confounded by the reporting issue suggested by panel F in Figure 1) with an instrument that causes firms to exogenously withdraw their filing due to changes. If our instrument is unrelated to whether the firm complies with EEOC reporting requirements, then the results from our instrumental variable analysis will also be free of non-reporting bias. As we demonstrate in the results section, we find that firms which experience high and low market returns, our instrument, during the book building phase are equally likely to complete EEOC surveys in the years after their initial filings.

Using the 4,008 matched firms, we construct two datasets for our analyses. The first dataset

looks at the impact of going public on firm diversity exactly five years after the S-1 filing date. For 2,508 matched firms, we observe an EEO-1 report exactly five years after initial filing. For 1,195 of these firms, we also have an EEO-1 report at the time of filing.<sup>5</sup> The second dataset retains the longitudinal structure of the EEO-1 data. In this case, we include all matched firms where we have at least one filing after the S-1 filing date. This yields 3,974 firms and 50,827 firm-year observations.

## Variables

Our dependent variable is the diversity of a firm’s workforce. We use groups’ employment shares to measure diversity. For both datasets, we calculate the percentage of female and of nonwhite employees in the firm. To test the idea that the effects of going public might be restricted to the more visible (and often more highly demanded) managerial positions, we also calculate the percentage of female and of nonwhite employees in the “Officials and Managers” occupational category of the EEO-1 survey matrix.<sup>6</sup> In 2007, the EEOC split the “Officials and Managers” category into “First/Mid-Level Officials and Managers” and “Executive/Senior-Level Officials and Managers,” to study whether protected groups were genuinely rising into leadership positions in firms or plateauing at lower supervisory levels. Because our data series only includes a few years with this distinction, we combined the two to maintain comparability with earlier years. Doing so is a more conservative test, precisely because it gives the same weight to diversity among low-level managers as it does to diversity among more senior ones.

Our independent variable is an indicator of whether a firm is publicly traded. We code a firm as publicly traded beginning in the calendar year that they complete their IPO. In principle, a firm that withdraws a filing could attempt an IPO at a later date, but this is quite rare in practice (Busaba et al., 2001; Dunbar and Foerster, 2008). We focus on firms’ first attempts at going public and treat any first-round failure as permanent. Doing so treats any firm that *did* withdraw and later go public as classical measurement error, biasing our estimates toward zero. Excluding these firms does not alter the pattern of our results. Taking a publicly traded firm private is an even more rare event. Per the construction of our sample, in some analyses we measure the dependent

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<sup>5</sup>This drop from 2,508 to 1,195 firms does not necessarily indicate a lack of compliance. Firms that are under one-hundred employees at the time of filing need not report an EEO-1 form in that year.

<sup>6</sup>A sample EEO-1 form can be viewed at <https://www.eeoc.gov/employers/eeo1survey/upload/eeo1-2-2.pdf>.

variables five calendar years after the IPO; in others we include our dependent variable for all post-IPO years for which we have data and then cluster our standard errors at the firm level.

Identifying the effects of going public on firm diversity is a challenge because of the self-selection of firms into IPO. Indeed the greater effect the transition to public ownership may be, the larger a concern selection bias is. This is because firms that will face greater public scrutiny of their internal operations are likely to make alterations to things like their hiring practices before they go public. Furthermore, if selection into IPO is correlated with higher growth or expansion then composition could become spuriously correlated with public offerings. Addressing these selection concerns requires a source of variation in completing an IPO that is uncorrelated with the initial decision to attempt an IPO and with wider firm performance. For such an instrument, we use cumulative market returns during the book-building phase of the firms' IPOs. Short-run fluctuations in the equity markets in the wake of S-1 filings strongly predict IPO completion by the filing firms; the effect is most pronounced around market declines (Busaba et al., 2001; Benveniste et al., 2003). We use returns on the NASDAQ exchange because prior work with this instrument (e.g., Bernstein (2015)) has explored its validity, but movements in the NASDAQ and other exchanges are highly correlated. We measure cumulative percentage returns in the 60 days after a firm's S-1 filing.

Table 1 presents summary statistics for the sample, broken out by completed and withdrawn IPO filings. The right-most column of Table 1 reports differences in means across the two subsamples, along with significance levels of a two-tailed  $t$ -test of means with separate variances. The two samples are indistinguishable in size or composition at the time of their S-1 filings. Five years after filing, firms that ultimately went public have comparable representation of women employees and managers, but have significantly *fewer* nonwhite employees and managers than firms that remained private, by about three percentage points. Table 1 also shows that firms that completed their IPOs encountered considerably better market performance in the two months after their S-1 filing, which is line with the logic of our identification strategy.

[Table 1 about here.]

## **Analytic strategy**

The baseline specification for workforce diversity takes the following form:



$$D_i^{post} = \alpha + \beta IPO_i + \gamma D_i^{pre} + \nu_k + \mu_t + \epsilon_i \quad (1)$$

where  $D_i^{post}$  is the employment share of interest (female managers, nonwhite employees etc.) five years after the S-1 filing,  $D_i^{pre}$  is the equivalent measure in the year before filing, and  $IPO_i$  is the indicator variable for publicly traded status. Under each of our null hypotheses,  $\beta$  would equal zero. Such models include industry ( $\nu_k$ ) and year ( $\mu_t$ ) fixed effects, and  $\epsilon_i$  represents the remaining error. We also test the effect of going public using all post-filing year observations. In this alternative panel specification we also include report-year fixed effects to account for trends in firm diversity.

If the decision to file or withdraw an IPO filing is correlated with workforce composition and therefore with  $\epsilon_i$ , then  $\beta$  may be biased. Thus, we compare firms that filed and completed IPOs in a given year to firms that filed and withdrew, and we instrument for IPO completion using cumulative NASDAQ returns. We follow Bernstein (2015) in using a two-month window of the book-building phase because we find his argument compelling: while one could use NASDAQ returns over the entire phase (which averages four months), the length of that phase is correlated with the likelihood of withdrawing. Using longer windows yields an instrument that is stronger in a statistical sense but less plausibly orthogonal to firm performance. We thus use a fixed window that is considerably shorter than the average book-building period but still correlated with completion or withdrawal.

Our first-stage regression takes the following form:

$$IPO_i = \alpha_1 + \beta_1 NSDQ_i^{60-post} + \gamma_1 NSDQ_i^{90-pre} + \nu_k + \mu_t + \epsilon_{2i} \quad (2)$$

where  $NSDQ_i^{60-post}$  is the instrumental variable. In this first stage, we also control for  $NSDQ_i^{90-pre}$ , the market performance in the three months *before* the S-1 filing. This control is important to isolate market performance after filing from performance beforehand, which almost certainly influenced the decision to file. The second-stage equation estimates the impact of IPO on workforce diversity:

$$D_i^{post} = \alpha_2 + \beta_2 \widehat{IPO}_i + \gamma_2 D_i^{pre} + \gamma_2 NSDQ_i^{90-pre} + \nu_k + \mu_t + \epsilon_{3i} \quad (3)$$

where  $\widehat{IPO}_i$  is the predicted value from equation 2. If the IV assumptions are met, then  $\beta_2$  estimates the causal effect of an IPO on diversity.

Separately estimating several types of fixed effects in the first stage of a two-stage least squares regression can greatly exaggerate the standard errors. This is a particular concern when, as here, we find null effects in the reduced-form model. We therefore fit these models using Stata’s `ivreghdfe` package (Correia, 2018), which in this case separates variable values from their industry and year means before estimation.

## Results

Table 2 presents a balance test for our instrument. Our instrument should be unrelated to our dependent variables except through its effect on our independent variable of interest. Accordingly, Table 2 presents regressions of firm size, share of female employees, and share of nonwhite employees on NASDAQ returns in the 60 days after S-1 filing, along with fixed effects for S-1 filing year and (where outcomes are averaged over years) fixed effects for years. Because these models include repeated observations of firms, we cluster robust standard errors by firm. In each case, we find no correlation between our instrument and workforce-composition measures in any of the years before S-1 filings. There is no reason to think that trends in workforce composition are confounded with market performance in firms’ book-building periods.

[Table 2 about here.]

Table 3 presents estimates of the parameters in our first-stage model, from equation 2. Models 1 through 3 present linear probability results with several types of fixed effects; we fit these with Stata’s `reghdfe` package to obtain the same standard errors that are used in `ivreghdfe`’s two-stage least squares routine. Model 1 shows that bullish market performance in the two months after filing is positively correlated with completing an IPO, as theorized and shown in prior research. Models 2 and 3 show that this association is robust to fixed effects by industry and year, and to controlling for market performance in the three months before the S-1 filing. Crucially, there does not appear to be a weak-instruments problem. The F-statistics in models 1-3 range from 15.2 to 65.9, well above the rule-of-thumb weak-instrument cutoff of 10.

[Table 3 about here.]

Models 4 and 5 of Table 3 address the concern over non-reporting bias. To reiterate, a potential concern with these data is that firms that do not complete their IPOs are also less likely to comply with reporting requirements like the EEOC's. If workforce diversity is correlated with EEOC reporting compliance (and it is quite reasonable to think it is), then this sample selection would bias our results to zero. In these data, though, this concern seems unfounded. Model 4 shows that firms that withdraw are no less likely to have an EEO-1 survey on file five years after filing than are firms that complete their IPO. Model 5 shows a similar result: withdrawing firms tend to have as many EEO-1 surveys recorded as completing firms. Non-reporting bias does not appear to be a problem in the sample used here.

We present estimates of the parameters in our second-stage model in Tables 4 through 7. These tables comprise tests of hypotheses 1 through 4, respectively. The structure of these tables is identical, with only the dependent variable changing. Accordingly, we discuss Table 4 in detail and then review any differences in Tables 5 through 7.

[Table 4 about here.]

Table 4 examines the effect of IPO on the share of female employees in firms' workforces. As in the first-stage models of table 3, models in table 4 cluster robust standard errors at the firm level and include fixed effects for industry and filing year. Models that include all post-filing years also include fixed effects for the reporting year. Model 1 shows OLS results. Here, an IPO has no effect on the share of female employees five years later. Models 2 then shows results from the second stage of our IV regression. The results conform to those of the OLS model: IPO is unrelated to women's share of employment.<sup>7</sup> In Model 3, we add in the control for the female share of the firm's workforce before filing. This variable is strongly and directly correlated with female shares post-file, as one would expect. In this, the fully specified second-stage model, the coefficient on IPO is similar in size to that in Model 1, and similarly non-significant. In Models 2 and 3 we see strong first-stage F-statistics of 28.8 and 21.8, respectively. Our null results are not the result of a weak instrument.

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<sup>7</sup>We do estimate both larger coefficients and larger standard errors for IPO in Model 2, which might suggest that the non-significance in this model has more to do with the inefficiency of two-stage least squares or a weak instrument than a true null effect. We cannot fully explain the jump in this coefficient, though it is worth noting that we see much smaller movement in the coefficients for share nonwhite employees and managers in Tables 6 and 7.

Models 4 through 6 tell the same story as Models 1 through 3, but rather than focus on a point in time five years after filing, we include all post-filing years for which we have data. The two approaches each have strengths and weaknesses: picking a single point in time avoids various types of survivor bias, but is necessarily arbitrary and risks dropping firms whose report for that specific year is unavailable. Using all available post-filing reports takes advantage of more data, reducing the possibility that we get non-significance due to insufficient statistical power, but at the cost of potential survivor bias. In this case the trade-offs are immaterial, because the estimated coefficients for IPO are once again non-significant and close to zero.

It is important to note that our null findings are not merely the result of noisy data, but appear consistent with going public’s having a negligible effect on diversity. The standard errors for the OLS estimates are small, just under 1 percentage point. After five years, our OLS models suggest that the largest expected effect at the 95-percent level would be a 2 percentage point increase in the share female. While the standard errors for the instrumental-variable models are larger, the point estimates are more negative. After five years, our IV results suggest that, at the 95-percent level, the largest gain is 4.2 percentage points, but that the the effect could also be -9 percentage points.

Tables 5 through 7 show nearly identical results. We find no evidence that going public has any effect on the share of women in management, the share of nonwhite employees, or the share of nonwhite managers. The only consistently significant result across all four tables is the predictive power of group composition prior to S-1 filing—but this is just path dependence.<sup>8</sup> Nor does this appear to be an issue of statistical power. In virtually all models, the coefficients on IPO are close to zero, the standard errors in the OLS results are no larger than 1 percent, and the IV standard errors range from 3 to 5 percent. Given that the magnitude and direction of our IV estimates reveal little in the way of selection bias, we think the tighter standard errors from our OLS models better reflect our uncertainty in the effect of going public.

<sup>8</sup>Model 6 of table 4 and Models 4 and 5 in table 6 suggest that there is some relationship between market performance before filing and average group shares in public firms in the years after IPO (each at  $p < .05$ ), but we strongly caution against reading too much into these coefficients. Tables 4 through 7 are a classic example of the multiple-comparisons problem (Gelman and Loken, 2014), where multiple runs of similar models are biased toward yielding false positives. In this case the probability that we would get three results significant at  $p < .05$  in 24 models when there is no real relationship is  $\binom{24}{3} .05^3 .95^{24-3} = .086$ . By comparison, if going public “really” affected workforce composition even half of the time, the probability of obtaining 24 false negatives would be  $5.96 \times 10^{-8}$ .

[Table 5 about here.]

[Table 6 about here.]

[Table 7 about here.]

Such points notwithstanding, the results in Tables 4 through 7 do represent compromises between comparability (examining a single point in time equally long after each filing) and statistical power (pooling all post-filing observations). We want to rule out the possibility that we find null effects because of a poor choice about how to parameterize the dependent variable. We therefore chose to study the effects of IPO non-parametrically with respect to time. We did this by regressing our dependent variable on indicator variables for years before and after the S-1 filing, interacted with an indicator variable for whether the firm ultimately completes its IPO. This allows us to directly compare the distributions for successful and failed IPO firms over time. The results of this exercise, shown in Figure 2, lend further support to the null results from our instrumental-variable models. Not only do the confidence intervals from the two populations routinely overlap but also, in all cases, the means trend in the same direction.<sup>9</sup>

[Figure 2 about here.]

Figure 2 emphasizes a fact that Tables 4 through 7 do not: women’s representation in management, and nonwhites’ representation at all levels of employment, *has* tended to grow in our largest firms over time. Women’s total share of employment, already the highest of these four measures, has held nearly constant. Such progress is real and should be appreciated as such. But such progress has proceeded at nearly identical speed in firms that withdrew and in firms that completed their IPOs. The transition from private to public ownership, in and of itself, appears to have no effect.

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<sup>9</sup>The closest these confidence intervals come to non-overlap is when we consider nonwhite employees and managers. This parallels the significant difference in the summary statistics for those groups five years after filing seen in Table 1. Yet these differences can apparently be explained even in an OLS framework as spuriousness driven by heterogeneity in industries and years.

## Discussion

There is a tendency in the social sciences to say that you can learn nothing from a null result. This is inaccurate, or at least incomplete. You can learn nothing from a null result in observational data. The drive for better causal identification in recent years is often discussed in terms of how it helps us infer causation from significant correlations, but the converse is also true. If we agree on a theorized causal prediction, and if we agree that an experiment is well designed to demonstrate that prediction, then a null result in that experiment should reduce our confidence in that theory.

In this article, we have belabored our research design for precisely this reason. We have theoretical grounds to think that public ownership, and the greater visibility and accountability that it brings, should lead firms to increase their employment diversity when they go public. We also have theoretical grounds to think that going public might have no effect—privileged groups do not *like* to surrender privilege, and window dressing is not an empty metaphor. Such are the cases when we would like a clean experiment, or at least a natural one, to test between theories.

In the case of public ownership, the fundamental difficulty is the counterfactual. To which privately held firms should we compare publicly traded ones? There are myriad ways that publicly traded firms might differ from privately held ones—mix of industries, product markets, size, age—any of which could be correlated with workforce composition. Absent true random assignment of ownership type, we employ what we think is the next best alternative: compare publicly traded firms to privately held firms that started to go public but reconsidered. Of course, this comparison only makes sense if reconsideration is independent of things that would affect firms’ workforce composition. This in turn motivates our using an instrumental variable, market returns in the book-building phase, that prior research has demonstrated to be plausibly exogenous to firm strategy and behavior (Busaba et al., 2001; Bernstein, 2015).

We have constructed the first nationally representative dataset that links changes in firms’ ownership type with longitudinal data on employment diversity. We track firms through their attempts, failed and successful, to go public, and we employ a novel identification strategy to plausibly exogenize the change in ownership. If moving to public ownership affects workforce diversity, we would find evidence for it here. We find none.

We draw four implications from this study. The first is that a null result here supports theory

developed elsewhere. When they studied managerial “best practices” around diversity, Kalev et al. (2006) found that only organizational changes that created direct responsibility and accountability for particular individuals or groups had any effect on managerial diversity. They found no impact from programs that focused on reducing managers’ bias, or reducing workers’ social isolation. In general, diversity policies that leveraged broader opinion but did not directly target workforce composition underperformed. Our findings here can be interpreted in a similar light. It could be that integration, with which American society has struggled since before there was an America *per se*, is simply too intractable a problem for indirect pressure like public opinion to shift. General accountability, such as a firm experiences vis-à-vis the public when it conducts an IPO, may allow too much diffusion of responsibility to get traction.

The second implication is that social movement pressure, however large an effect it may have on specific firms, is not a general fallback. We want to be clear: our results do not speak against work on consumer boycotts (McDonnell and King, 2013), unions’ corporate campaigns (Perry, 1987; Wunnava, 2004), or other types of “private politics” (Soule, 2009). A consistent finding in much social-movement research is that various types of protest tactics *can* prod private firms to change their behavior (Soule and King, 2015; Briscoe and Gupta, 2016). The issue is generalizability from the cases studied. While public pressure has apparently led some targeted firms to diversify their workforces and to promote women and minorities, we see no general effect associated with greater exposure to public scrutiny.

Even if activists were to systematically target any and all firms—an already infeasible task—their work would never be done, because firms are founded and fail all of the time. The third implication here is that the initial diversity of firms seems quite important. Even large changes in the governance structure, as when a firm goes public, seems to have little effect on aggregate workforce composition. This builds on (Ferguson and Koning, 2018)’s argument that scholars of entrepreneurship and diversity need to consider ecological processes between firms, rather than just within-firm changes. Much scholarly and activist interest in workplace diversity focuses on large, public, successful firms. The reasons for this are understandable: these firms are where the available data comes from, and larger, more successful firms shape opinions about appropriate corporate conduct. Yet there are two problems with this approach. First, within-firm change itself is quite difficult. Second, off to the side of this sound and fury, new firms are born all the time,

relatively homogeneous, and free to grow quite large before they appear on the radar. When it comes to workforce diversity, the place to bend the curve is probably not after an initial public offering but rather in the early years of far more firms.

Our fourth implication has to do with a type of organizational research that, today, is relatively rare. Organizational research has largely ceded the field of nationally representative data and average effects to economics and sociology. To some extent this is unavoidable, as detailed intra-organizational data is rarely available across a large and comparable cross-section of firms. Yet in the absence of such research, we have tended to generalize from the studies we do have, and to take for granted larger social facts—such as that public ownership probably increases employment diversity—that in truth have little basis in prior research. And the deep dives within firms of which organizational research is fond are prone to the subtle source of bias we raised at the start of this article. Much of our research happens inside or at least requires information from organizations. Those organizations vary in how willing and able they are to share data. Over the past two decades, much of the best organizational research has paid increasing attention to settings where a plausibly exogenous treatment allows causal inference of an effect (e.g., Fernandez and Friedrich (2011); Cowgill (2015); Moen et al. (2017)). Such research designs have greatly increased the internal validity of much of our research (Cronbach and Meehl, 1955). But external validity is still an issue. Organizations self-select to cooperate with organizational researchers—and with activists, and even with regulators. Such cooperation need not hinge directly on an issue like employment diversity. If a firm cannot systematically document its hiring practices, then it can neither share data on its practices with outsiders nor use that data to reform its own procedures. In equilibrium, we could easily produce studies showing great improvement in studied firms, even as the larger population stagnates or regresses.

Larger datasets often lack a richness that motivates much thinking in our field. Yet they offer a definitiveness that we should treasure. If, as here, they militate against a widely held but thinly substantiated social fact, we should be inclined to ask how that affects our theorizing, and indeed why we were so eager to believe that fact in the first place. It would be nice if publicly traded firms tended to grow more diverse over time—if we had a *de facto* policy intervention that firms desired. To make real progress on integration, though, we have to look elsewhere.



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**Table 1:** Summary Statistics

	Completed			Withdrawn			Difference
	Mean	Median	SD	Mean	Median	SD	
# Employees at S-1 filing (000s)	1.54	0.36	5.08	1.99	0.35	6.33	-0.45
% Female employees at S-1 filing	0.39	0.37	0.18	0.38	0.37	0.18	0.01
% Female managers at S-1 filing	0.25	0.22	0.16	0.26	0.23	0.16	-0.01
% Nonwhite employees at S-1 filing	0.24	0.19	0.18	0.25	0.21	0.17	-0.01
% Nonwhite managers at S-1 filing	0.11	0.08	0.12	0.11	0.09	0.12	-0.01
# Employees 5 years post (000s)	2.13	0.53	8.50	2.73	0.54	7.42	-0.60
% Female employees 5 years post	0.39	0.38	0.17	0.38	0.38	0.18	0.01
% Female managers 5 years post	0.27	0.25	0.15	0.27	0.23	0.16	0.01
% Nonwhite employees 5 years post	0.27	0.24	0.18	0.30	0.26	0.18	-0.03*
% Nonwhite managers 5 years post	0.13	0.10	0.12	0.15	0.11	0.13	-0.02*
60-day Post NASDAQ Returns	0.02	0.02	0.08	-0.03	-0.01	0.12	0.04***

\*  $p < .05$ , \*\*  $p < .01$ , \*\*\*  $p < .001$

**Table 2:** Balance Test

	Year before S-1 filing			All years before S-1 filing		
	(1) Log Employees	(2) % Employees Female	(3) Nonwhite	(4) Log Employees	(5) % Employees Female	(6) Nonwhite
60-day Post NASDAQ Returns	-0.197 (0.442)	0.070 (0.056)	-0.056 (0.056)	-0.492 (0.604)	-0.016 (0.074)	0.028 (0.064)
Observations	1,659	1,659	1,659	12,054	12,054	12,054
Number of firms	1,659	1,659	1,659	1,765	1,765	1,765
$R^2$	0.030	0.021	0.059	0.056	0.026	0.057

Robust standard errors, clustered at the firm level, in parenthesis. All models include fixed effects for S-1 filing year. Models using all years before filing (4–6) include report-year fixed effects.

\*  $p < .05$ , \*\*  $p < .01$ , \*\*\*  $p < .001$

**Table 3:** First-Stage Estimations

	(1)	(2)	(3)	(4)	(5)
	IPO	IPO	IPO	Any report 5 years post-file?	# of reports
60-day Post NASDAQ Returns	0.590*** (0.073)	0.374*** (0.075)	0.407*** (0.076)	-0.003 (0.083)	0.218 (1.189)
90-day Pre NASDAQ Returns			0.138* (0.060)		
Filing year FEs	No	Yes	Yes	Yes	Yes
Industry FEs	No	No	Yes	Yes	Yes
Observations	4,057	4,057	4,056	4,057	4,057
Number of firms	4,057	4,057	4,056	4,057	4,057
$R^2$	0.025	0.096	0.100	0.033	0.100
$F$	65.9	24.6	15.2		

Robust standard errors, clustered at the firm level, in parentheses.

\*  $p < .05$ , \*\*  $p < .01$ , \*\*\*  $p < .001$

**Table 4:** Does Going Public Increase the Share of Female Employees?

	Dependent variable: % Female employees					
	5 years post-file			All years post-file		
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	IV	IV	OLS	IV	IV
IPO	-0.003 (0.011)	0.134 (0.086)	-0.013 (0.044)	-0.011 (0.010)	0.061 (0.072)	-0.024 (0.033)
90-day Pre NASDAQ Returns	0.011 (0.034)	-0.000 (0.035)	-0.004 (0.022)	0.047 (0.033)	0.042 (0.033)	0.038* (0.017)
Pre-filing % Female			0.817*** (0.017)			0.852*** (0.015)
Observations	2,508	2,508	1,195	50,827	50,827	30,136
Number of firms	2,508	2,508	1,195	3,974	3,974	1,659
$R^2$	0.145	-0.066	0.792	0.156	-0.017	0.763
First-stage $F$		28.6	21.5		34.1	24.8

Robust standard errors, clustered at the firm level, in parentheses. All models include fixed effect for industry and S-1 filing year. Models using all post-filing years (4–6) include fixed effects for reporting year.

\*  $p < .05$ , \*\*  $p < .01$ , \*\*\*  $p < .001$

**Table 5:** Does Going Public Increase the Share of Female Managers?

	Dependent variable: % Female managers					
	5 years post-file			All years post-file		
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	IV	IV	OLS	IV	IV
IPO	0.006 (0.009)	0.112 (0.074)	0.019 (0.050)	-0.005 (0.008)	0.058 (0.057)	0.005 (0.032)
90-day Pre NASDAQ Returns	0.017 (0.030)	0.008 (0.031)	0.011 (0.028)	0.039 (0.026)	0.034 (0.026)	0.031 (0.022)
Pre-filing % Female mgrs			0.674*** (0.023)			0.715*** (0.019)
Observations	2,504	2,504	1,192	50,765	50,765	30,083
Number of firms	2,504	2,504	1,192	3,973	3,973	1,657
$R^2$	0.176	-0.053	0.533	0.226	-0.019	0.495
First-stage $F$		28.8	21.8		34.0	24.7

Robust standard errors, clustered at the firm level, in parentheses. All models include fixed effect for industry and S-1 filing year. Models using all post-filing years (4–6) include fixed effects for reporting year.

\*  $p < .05$ , \*\*  $p < .01$ , \*\*\*  $p < .001$



**Table 6:** Does Going Public Increase the Share of Nonwhite Employees?

	Dependent variable: % Nonwhite employees					
	5 years post-file			All years post-file		
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	IV	IV	OLS	IV	IV
IPO	-0.017 (0.011)	-0.079 (0.084)	0.005 (0.056)	-0.004 (0.009)	-0.041 (0.070)	0.000 (0.043)
90-day Pre NASDAQ Returns	0.028 (0.035)	0.033 (0.036)	0.025 (0.035)	0.061* (0.031)	0.064* (0.032)	0.027 (0.023)
Pre-filing % Nonwhite			0.832*** (0.020)			0.838*** (0.014)
Observations	2,508	2,508	1,195	50,827	50,827	30,136
Number of firms	2,508	2,508	1,195	3,974	3,974	1,659
$R^2$	0.078	-0.012	0.720	0.097	-0.003	0.666
First-stage $F$		28.6	21.7		34.1	24.7

Robust standard errors, clustered at the firm level, in parentheses. All models include fixed effect for industry and S-1 filing year. Models using all post-filing years (4–6) include fixed effects for reporting year.

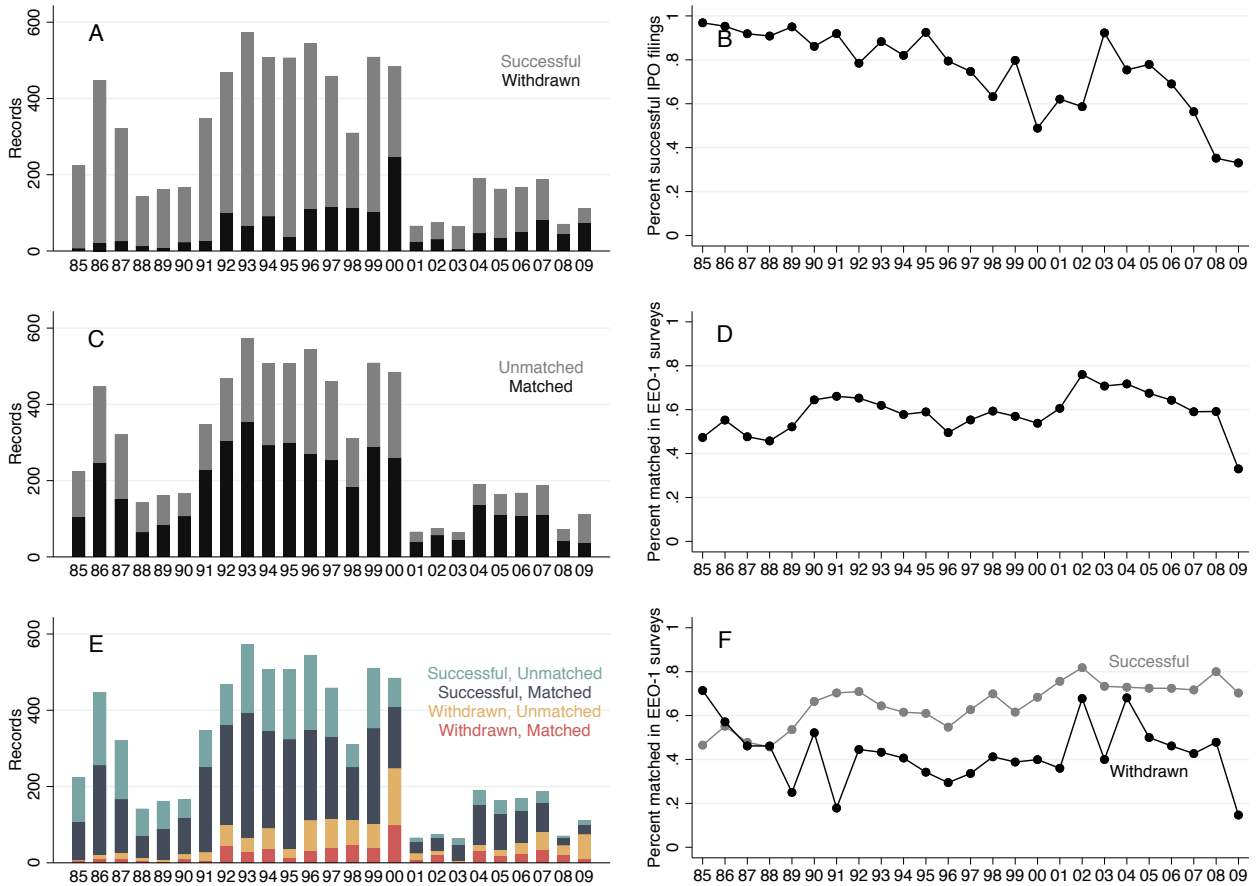
\*  $p < .05$ , \*\*  $p < .01$ , \*\*\*  $p < .001$

**Table 7:** Does Going Public Increase the Share of Nonwhite Managers?

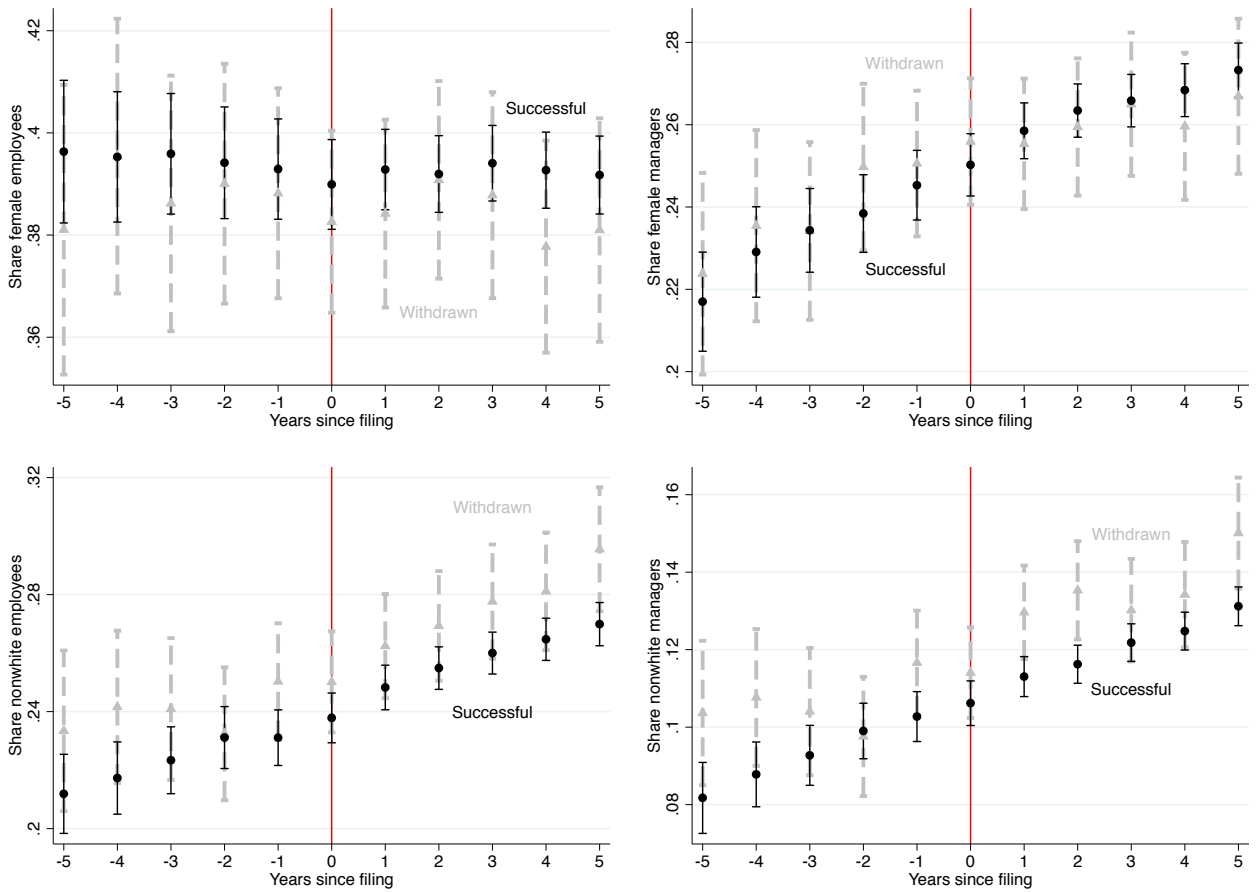
	Dependent variable: % Nonwhite managers					
	5 years post-file			All years post-file		
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	IV	IV	OLS	IV	IV
IPO	-0.003 (0.008)	-0.056 (0.058)	-0.038 (0.045)	0.001 (0.006)	0.016 (0.045)	0.017 (0.032)
90-day Pre NASDAQ Returns	0.018 (0.025)	0.023 (0.026)	0.024 (0.024)	0.034 (0.019)	0.033 (0.019)	0.007 (0.015)
Pre-filing % Nonwhite mgrs			0.765*** (0.030)			0.784*** (0.026)
Observations	2,504	2,504	1,192	50,765	50,765	30,083
Number of firms	2,504	2,504	1,192	3,973	3,973	1,657
$R^2$	0.100	-0.018	0.587	0.125	-0.001	0.530
First-stage $F$		28.8	22.2		34.0	24.6

Robust standard errors, clustered at the firm level, in parentheses. All models include fixed effect for industry and S-1 filing year. Models using all post-filing years (4–6) include fixed effects for reporting year.

\*  $p < .05$ , \*\*  $p < .01$ , \*\*\*  $p < .001$



**Figure 1:** Match properties for firm IPOs. Data consist of all S-1 filings in Thompson Financial’s SDC New Issues database between 1985 and 2009, linked to EEO-1 establishment surveys. Panel A shows the counts of S-1 filings that went to IPO and that withdrew beforehand; panel B plots the “success rate” of these filings. Panel C shows numbers of S-1 filings over time and the subset matched to EEO-1 records; panel D shows the corresponding match rate. Panel E slices the raw data into the four categories suggested by panels A and C. Panel F plots the match rate for S-1 filings, broken out by success of the filing. See the main text for further details on the data.



**Figure 2:** Marginal effects for the transition to public ownership on firm diversity, calculated by year. We generate each panel by regressing the dependent variable (Y-axis) on dummy variables for years before or after the S-1 filing, interacted with publicly-traded status. This allows us to see how the effect, if any, of public ownership varies over time without imposing parametric assumptions. Here, we show predicted means and variances for privately held and publicly traded firms for the five years before and after the S-1 filing. While shares of female and nonwhite employees and managers have risen in all firms, in virtually all cases in all years the confidence intervals between privately held and publicly traded firms overlap considerably. Nor is there any indication that the secular trend changes in publicly traded firms after IPO.