Public Policy- Induced Changes in Human Capital Factor Market Imperfections: How Non-Compete Policy Affects Firm Performance

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ABSTRACT: We expand research on strategic factor markets and strategic human capital to incorporate public policy by exploring how public policy-induced changes in factor market imperfections affect firm performance. We exploit a quasi-natural experiment of a California Supreme Court decision ending enforcement of out-of-state non-compete agreements – agreements restricting inter-firm employee mobility – which gave California firms access to a previously protected factor market for human capital. We hypothesize and empirically confirm that this access increased California firms' market value and that firms with lower factor market rivalry and greater need for skilled human capital experienced greater increases. Our findings demonstrate that public policies regulating factor market access, not commonly considered in strategic factor market or human capital research, can have sizable effects on firms' market value.

Key Words: strategic factor market imperfections; human capital; public policy; employee non-compete agreements; event study

1. INTRODUCTION

Imperfections in strategic factor markets can be a source of competitive advantage because they provide some firms selective access to resources while excluding other firms (Barney, 1986). These imperfections, such as path dependency, causal ambiguity, and complexity, limit a firm's ability to access resources needed for strategy implementation (Barney, 1986, 1991; Chadwick, 2017). While research has explored how individual firms create factor market imperfections to their own benefit by establishing factor mobility barriers, such as by developing firm-specific human capital (Campbell, Coff, & Kryscynski, 2012), the question of how public policies impact the competitiveness of a subset of firms by creating, changing, or destroying factor market imperfections has, to the best of our knowledge, not yet been examined. Indeed, it has been noted that research at the intersection of market imperfections, public policy, and the creation of competitive advantage has been limited (Oberholzer-Gee & Yao, 2018), particularly in markets for human capital (Campbell et al., 2012). In this paper, we connect research on strategic factor market and strategic human capital with public policy research regarding the enforcement of employee non-compete agreements – agreements that restrict the movement of employees between firms and can thus be construed as factor mobility barriers – to explore how public policy-induced changes in imperfections in the factor market for human capital affect firm performance. Our empirical setting is a legal decision regarding the enforcement of non-compete agreements that provided a specific subset of firms access to a previously protected factor market for human capital by removing a factor market imperfection for these firms. We suggest, and find support, that this public policy-induced removal of an imperfection in the human capital factor market for in-jurisdiction firms increases the performance of these firms relative to other firms that remain subject to the imperfection. Furthermore, we identify factor market and firm characteristics – specifically, factor market rivalry and firm need for skilled human capital – that moderate this effect.

Human capital – the valuable knowledge, skills, and abilities of employees (Coff & Kryscynski, 2011) – is unique from other firm resources due to its mobility (Coff, 1997), and has been recognized as an asset critical to firm competitive advantage (Hitt, Bierman, Shimizu, & Kochhar, 2001). Human capital, unlike other firm resources, "depend[s] on the continued presence of people, who – unlike property, plant, and equipment – are not owned by the firm, but merely *employed*" (Younge & Marx, 2016, p. 653). A firm may *access* human capital in strategic factor markets (SFMs) for human capital, also called labor markets (Chadwick, 2017), but can *acquire* human capital only temporarily by contracting with individual employees – that is, by hiring them. Within SFMs for human capital, firms face two unique objectives: first, firms may wish to protect their own (outward-flowing) human capital from being accessed by other firms; second, firms may wish to gain access to (inward-flowing) human capital. Most research on SFMs for human capital has focused on the first objective, broadly referred to as "frictions" or "imperfections" within SFMs (Campbell et al., 2012), including the growing literature on employee non-compete agreements (*e.g.*, Marx, Strumsky, & Fleming, 2009; Starr, 2019). In contrast, we focus on the second by considering a public policy change regarding the enforcement of non-compete agreements that increased firm access to (inward-flowing) human capital.

Non-compete agreements limit the ability of employees to work for (or start) a competitive entity after leaving a firm (Marx et al., 2009). Nearly half of all U.S. firms require some or all their employees to sign such agreements (Colvin & Shierholz, 2019). In the U.S., legal enforceability of these agreements is governed by state policymakers and varies widely across states. Management scholars generally view non-compete agreements as a limitation on employee mobility (Marx et al., 2009) that protects firm knowledge contained within the minds of departing employees from being acquired by rival firms (Franco & Mitchell, 2008). A resource-based perspective suggests non-compete agreements create employee mobility barriers, which prevent competitors from accessing valuable firm resources. Thus, non-compete agreements can be a source of sustainable competitive advantage for firms that currently employ such captive human capital. From a human capital perspective, non-compete agreements are considered a "supply-side" labor market imperfection that constrains employee participation in the SFM for human capital (Campbell et al., 2012). Both perspectives suggest a public policy of enforcing employee non-compete agreements creates imperfections in SFMs for human capital by erecting a barrier to the ability of firms to hire employees from competitors.

In this study, we consider a public policy change involving the enforcement of non-compete agreements that provided a subset of firms access to a previously protected labor market. Our empirical setting is the August 2008 California Supreme Court decision in *Edwards v. Arthur Andersen LLP* which changed California's public policy by eliminating the enforcement of out-of-state non-compete agreements. This policy change allowed California firms to hire employees who were previously subject to non-compete agreements from out-of-state competitors. Thus, this policy change removed a factor market imperfection for all firms located in California – but only those firms, as the ability of out-of-state firms to access this SFM remained restricted. Our empirical setting can be considered a quasi-natural experiment that allows us to explore whether access to this previously protected SFM for human capital (that is, all employees outside of California with non-compete agreements) affects the performance of California-based firms. Because this court decision was not anticipated and provided new information, an event study estimating abnormal changes in firms' market value around a publicized event is an ideal method of analysis. While this method gauges investor expectations of future firm performance rather than actual performance, it allows us to isolate the performance impact of a specific event, such as this court decision (Cording, Christmann, & Weigelt, 2010).

Drawing on resource-based theory, we hypothesize that a public policy decision to stop enforcing out-of-jurisdiction employee non-compete agreements increases the market value of firms located within the jurisdiction by providing them access to a previously protected SFM for human capital (*i.e.,* employees outside the jurisdiction subject to non-compete agreements). We then theorize that both factor market rivalry and firm need for skilled human capital will result in firm-level variations of this increase in market value. First, we hypothesize that a firm's market value benefit from gaining access to a previously protected SFM for human capital is smaller if this new SFM access has to be shared with many rivals (Markman, Gianiodis, & Buchholtz, 2009). Second, we hypothesize that firms reliant on skilled human capital, particularly employees whose jobs involve "the creation, distribution, or application of knowledge" (Davenport, 2005, p.10), experience greater increases in market value from access to the previously protected SFM for human capital.

Our results confirm all three hypotheses, and in supplemental analysis we find no evidence that the gains experienced by within-jurisdiction firms were at the expense of out-of-jurisdiction firms.

By responding to Campbell et al.'s (2012) suggestion to investigate how public policies, such as non-compete enforcement, affect firm ability to exploit factor market imperfections, this study makes contributions at the intersection of research on strategic factor markets, strategic human capital, and public policy. Our results suggest that public policy changes affect jurisdiction-specific factor market imperfections, which can have sizable impacts on the market value of within-jurisdiction firms, and that labor market and firm characteristics moderate the effect of changes in jurisdiction-specific factor market imperfections on firm market value. Focusing on a public policy that reduces factor market imperfections for firms within a jurisdiction allows us to explore how and for which firms *access* to previously protected factor markets can be a source of competitive advantage. This contrasts with most studies exploring factor market imperfections using resource-based theory, which focus primarily on how resource *protection* impacts competitive advantage for single firms. We demonstrate that public policies that reduce jurisdiction-specific factor market imperfections directly improve the performance of within-jurisdiction firms.

2. THEORY AND HYPOTHESES

2.1 Factor market imperfections and public policy

Firm resources are acquired, traded, or disposed of in SFMs (Barney, 1986). Imperfections in SFMs provide select firms access to the valuable resources needed to successfully implement their strategies while excluding other firms (Barney, 1986, 1991; Chadwick, 2017). Most prior research on SFM imperfections explores how individual firms protect their resources to gain competitive advantage by creating factor market imperfections, such as by developing firm-specific resources not easily tradable in factor markets (Dierickx & Cool, 1989) like firm-specific human capital (Becker, 1964; Campbell et al., 2012), or by creating idiosyncratic complementarities between firm resources so that the value of a particular resource varies across firms (Adegbesan, 2009). By focusing on factor market imperfections as mechanisms that create competitive advantage by *protecting* firm resources, the question of whether and under what conditions firms obtain competitive advantage from gaining *access* to factor markets has received limited attention. Research that considers how firms *access* protected resources is generally limited to studying acquisitions of other firms, particularly in the context of skilled human capital (*e.g.*, Chatterji & Patro, 2014; Uhlenbruck, Hitt & Semadeni, 2006).

Public policies can be changed or enacted by law-making bodies, including courts, legislative bodies, or elected officials. Changes in public policies can facilitate factor market access by removing SFM imperfections for a large number of firms, specifically the firms located within a public policy's jurisdiction. Such access will only benefit within-jurisdiction firms if some barriers that prevent unfettered access by all firms remain, *i.e.,* some factor market imperfections must persist or else the factor market would become perfectly competitive, and no firm would gain competitive advantage (Chadwick & Flinchbaugh, 2021). Thus, any opening of a factor market to previously excluded firms can result in competitive advantage only if the opening is selective such that other firms remain excluded. Public policies are uniquely able to affect distinct subsets of firms because they regulate the actions of the people, organizations, etc., within their jurisdictions, normally to the benefit of within-jurisdiction members and to the exclusion of those outside their jurisdiction.

Public policy-induced changes in factor market imperfections are quite common, and include intellectual property regulation (such as patents), non-compete agreements, immigration or trade policy, etc. The far-reaching, and often fast-acting, changes in factor market access caused by public policies can be contrasted with firm-induced changes in factor mobility barriers that generally affect only few firms and are often slower, and more incremental, such as a handful of firms patenting around another firm's proprietary technology, or a gradual diffusion of knowledge within an industry. What is common among all these changes in factor market imperfections is that they can affect firm performance by providing or preventing access to strategically important resources, but public policy-induced changes are capable of having a fast impact on the competitiveness of a large number of firms. In this study, we examine how a public policy-induced removal of a factor market imperfection for within-jurisdiction firms affects the performance of these firms. We focus on imperfections in the SFM for human capital, a highly mobile resource critical to firm success.

2.2 Human capital

The mobility of human capital sets it apart from other resources (Coff, 1997). Firms cannot "own" human capital, but only acquire it on a semi-permanent basis by hiring employees. Most strategic human capital (SHC) research explores how to best exploit human capital resources (Coff & Kryscynski, 2011) by identifying conditions under which human capital leads to firm value creation or conditions that allow firms to capture more of the value created by human capital (Chadwick, 2017; Coff, 1999). There is not much research within the SHC literature addressing how firms gain access to human capital in the first place, or whether this access can operate as a source of competitive advantage (Bidwell et al., 2015; see also Campbell et al., 2012; Chadwick & Dabu, 2009). We contribute to this literature by exploring what happens when an imperfection in the SFM for human capital is removed for a subset of firms so that these firms obtain selective access to previously protected human capital.

When SHC research addresses the issue of "access to human capital" it largely focuses on "human capital acquisition" (*i.e.*, hiring) in the context of specific types of employees, such as "star" employees (*e.g.*, Agrawal, McHale, & Oettl, 2017), inventors (*e.g.*, Marx et al., 2009), executives or top management team members (*e.g.*, Mackey, Malloy, & Morris, 2014; Williams, Chen, & Agarwal, 2017). This literature identifies firm-specific mechanisms for human capital acquisition such as repeated interorganizational hiring via so-called "pipelines" (*e.g.*, Brymer, Chadwick, Hill, & Molloy, 2019), via mergers and acquisitions (*e.g.*, Coff, 2002; Younge, Tong, & Fleming, 2015) and via "acqui-hiring" (*e.g.*, Chatterji & Patro, 2014) where a company is acquired primarily for the quality of its employees; or through being a “high-status” employer (Bidwell, Won, Barbulescu, & Mollick, 2015). In contrast, our study considers an entire labor market and how *access* to a SFM for human capital provides firms the possibility to hire, and how this access affects firm performance.

2.3 Employee Non-Compete Agreements

Employee non-compete agreements are used widely in the U.S., reportedly covering 18% of all Americans (Prescott, Bishara, & Starr, 2016), 50% of technical professionals (Marx, 2011), 70% of entrepreneurs receiving venture capital funding (Kaplan & Stromberg, 2003), and 70.2% of executives at publicly traded firms (Garmaise, 2011). Use of non-compete agreements appears to be increasing: a recent study found 49.4% of firms require non-competes for “some” of their employees, while 31.8% of firms use non-competes for all employees; extrapolating these numbers indicates that nationwide 27.8% to 46.5% of (or 36 million to 60 million) private-sector workers have employee non-compete agreements (Colvin & Shierholz, 2019; see also White House, 2016).

Empirical research on non-compete agreements addresses questions such as the impact of such agreements on employee mobility (*e.g.*, Fallick, Fleischman, & Rebitzer, 2006; Marx et al., 2009; Marx, 2011), human capital investment (*e.g.*, Cooper, 2001; Garmaise, 2011), entrepreneurship (*e.g.*, Marx & Fleming, 2012; Stuart & Sorenson, 2003; Starr, Frake, & Agarwal, 2019), and whether non-compete enforcement affects firm research and development (R&D) strategy (Conti, 2014). The effect of non-compete agreements on firm performance also received recent attention (Lavetti, Simon, & White, 2020; Younge & Marx, 2016) and some evidence suggests non-compete agreements may benefit firms. For example, Younge and Marx (2016) find that the value of Michigan-based firms increased by 9.75% after non-competes were allowed in Michigan due to a legislative change in 1985.

Most empirical studies on non-compete agreements take advantage of the fact that legal enforceability of employee non-compete agreements in the U.S. is governed by state law and varies widely across states. In some states, such as North Dakota and Oklahoma, non-compete agreements are not enforceable, while in others, such as Florida, enforcement is quite strong (Starr, 2019). Most prior studies use ratings to measure and compare the stringency of enforcement of non-compete agreements across different states, which raises two issues. First, cross-state comparisons of enforcement ratings may be misleading, as states may have identical numerical rating scores but enforcement regimes with very different practical effects. Second, there are at least five different rating systems classifying state-level enforcement of non-compete agreements[1] which are not entirely consistent. For example, in one framework (Marx et al., 2009) Washington is "non-enforcing" and Virginia is "enforcing," while in two other frameworks, Washington rates as more enforceable than Virginia (Garmaise, 2011; Starr, 2019). Both of these concerns can be avoided if researchers focus on *changes* in enforcement occurring in a single state without making cross-state comparisons, as we do in this study.

2.4 Investor responses to public policy changes

Investors can be expected to be aware of state-level changes in non-complete policy because information about such changes is publicly available and investors "monitor legislation that could affect the profitability of industries" and companies (Jacobson, 1994, p. 441). Investors therefore use this information when making investment decisions. As a result, event studies have been proposed as one of the best ways to evaluate the effects of new or changed business laws on firm performance (Bhagat & Romano, 2002), and event studies have explored the performance impact of legal changes in the U.S. (*e.g.*, Cornett & Tehranian, 1990; Li, Pincus, & Rego, 2008) and other countries (*e.g.*, Korkeamäki, Koskinen, & Takalo, 2007). Event studies assess investor expectations of the impact of an event on future firm performance by measuring the abnormal stock market returns in response to news about an unanticipated event. We therefore suggest that abnormal changes in market returns

[1] The five ratings are: a dummy variable indicator for 10 states (Stuart & Sorenson, 2003); a binary scale (Marx et al., 2009); a 12-factor additive scale (Garmaise, 2011); a weighted version of that scale used (Bishara, 2011); and a reweighted using factor analysis version (Starr, 2019).

after the announcement of a public policy change reflect the extent to which the policy change altered investor expectations about future firm performance for within-jurisdiction firms.

2.5 Research context

We identified 44 state-level changes in public policies regarding non-compete agreements and their enforcement across 25 states which occurred from 1980 to 2018 at the legislative or state Supreme Court level. Online Appendix Section A, which provides details on these changes, shows that their content and scope differ widely. Therefore, the choice of a policy change as the event for our study affects our hypotheses development. We briefly explain the process to choose the policy change for our study here and provide full details in Online Appendix Section A. First, we eliminated policy changes focused on contexts other than employee non-competes, such as those addressing non-competes in the context of the sale of a business. Second, we required the policy change to affect labor market access for an identifiable subset of firms, and therefore eliminated policy changes that failed to do so, including those requiring case-by-case analysis in applying the policy change. Third, at the methodological level, event study methodology requires a selected event be *unanticipated* to generate abnormal returns (Fama, 1970). Non-compete policies can be changed via either court decisions or legislative changes. Court decisions provide new, unanticipated, information because the outcomes and timing of such decisions are unknown before any actual decisions are rendered; they are also normally effective immediately. In contrast, legislative changes may be proposed, then discussed and voted on by a legislative body, and then signed into law with a possible future effective date. Thus, court decisions are usually unanticipated, but legislative changes are less likely to be so, so we eliminated legislative changes. Ultimately, two policy changes met these three criteria: a 2008 California Supreme Court decision and a 2001 Louisiana Court decision. Finally, because we examine market value, we needed a change that affected a sufficiently large number of publicly traded firms to create a sample for our empirical analysis. As is clear from Online Appendix Table

A-1, the 2008 California Supreme Court decision was the obvious choice as California contained approximately 2,000 publicly traded firms in 2008, while Louisiana contained only approximately 56 publicly traded firms in 2001. This decision, *Edwards v. Arthur Andersen LLP*, eliminated the enforcement of out-of-state non-compete agreements in California.

While California never allowed California-based firms to enforce employee non-compete agreements, firms based outside of California had been able to enforce their out-of-state agreements in California should their employees relocate there due to a federal court "loophole" allowing "narrowly tailored" employee non-compete agreements. Importantly, this loophole only applied when there was "diversity jurisdiction" in federal court, which, in this context, almost exclusively would have been out-of-state firms fearful their employees would relocate to California This loophole was completely eliminated on August 7, 2008 (our event date), when the California Supreme Court ruled in *Edwards v. Arthur Andersen LLP* that the loophole was a federal court creation not aligned with California state policy, and that therefore, *all* employee non-compete agreements (and particularly those from out-of-state firms that had sought to exploit the loophole) were unenforceable in California. After this decision, and unlike any other state, California did not allow enforcement of any employee non-compete agreements (Tedesco, 2011). This decision was effective immediately, affected all existing and future agreements, and was also unanticipated,[2] making it ideal for event study analysis. Moreover, the scope of the decision was surprising even to legal scholars who had closely followed the case due to the breadth of the ruling disallowing all exceptions to California's prohibition on employee non-competes (Tedesco, 2011). Furthermore, it

[2] A Factiva search for news articles relating to the *Edwards* case indicated that there was only one news mention, published nearly two months before the decision: a one-line mention in a law firm blog posting titled "Update on Trade Secret Law" stating "expect a decision soon" (Pooley, 2008).

had been ten years since the last time the California courts had addressed the enforceability of employee non-competes, making it difficult to predict what the ruling would be.

2.6 Hypothesis Development

Public policies can change factor market access for within-jurisdiction firms while excluding other firms. For instance, when a jurisdiction ends the enforcement of out-of-jurisdiction employee non-compete agreements, only firms located within the jurisdiction gain access to a previously protected SFM for human capital, *i.e.,* they gain access to employees of out-of-jurisdiction firms subject to non-compete agreements. Notably, firms in other jurisdictions will not enjoy the same SFM access. In fact, such a public policy change creates a resource discontinuity for out-of-jurisdiction firms, defined as a change in the merit of their existing resources (Markman et al., 2009) by reducing the effectiveness of non-compete protection for out-of-jurisdiction firms. This allows within-jurisdiction firms to "leapfrog" (Markman et al., 2009) the resource barrier established by non-compete agreements and access previously protected human capital. Such a policy change therefore allows within-jurisdiction firms to obtain "novel resource positions" (Markman et al., 2009, p. 430), by enabling them to poach employees with non-competes from out-of-jurisdiction rivals.

We propose the value of access to a previously protected SFM for human capital due to a change in non-compete laws increases investor expectations of the performance of within-jurisdiction firms and will be reflected in increases in market returns surrounding the public policy change:

Hypothesis 1 (H1). *A public policy change that provides access to a previously protected SFM for human capital for within-jurisdiction firms increases the market value of these firms*.

We further suggest that not all firms within the jurisdiction benefit equally from the selective SFM access provided by such a public policy change. Resource-based theory suggests factor market rivalry affects the value firms derive from having access to a given factor market, and research on SFMs points to the importance of considering competitive interactions among SFM participants

(Capron & Chatain, 2008; Lerner, Tirole, & Strojwas, 2003). Rivalry in a SFM can raise the prices of resources traded in the market and can preclude some firms from acquiring certain scarce resources. Thus, access to a SFM is more valuable if factor market rivalry is low.

Human capital is particularly prone to factor market rivalry for two reasons. First, because human capital is mobile and cannot be "owned," it can be quickly re-deployed should employees move between firms (Markman et al., 2009). Second, human capital is heterogeneous, as each (potential) new hire brings a unique set of skills and abilities, so that firms may be inclined to compete more fiercely for specific workers with valuable skills or abilities, further driving up their prices in the labor market.

The larger the number of firms competing for similar resources, the smaller the per-firm value of gaining access to previously protected resources. We propose that when out-of-jurisdiction non-compete agreements become non-enforceable, similar within-jurisdiction firms will compete over similar groups of potential new hires (*i.e.*, workers with comparable qualifications, skills, and experiences). Investors can be expected to be aware of factor market rivalry as the number of competing firms is easily available public information and is frequently communicated directly to firm investors via earnings calls and securities filings. We therefore expect the level of factor market rivalry for human capital resources within a jurisdiction will reduce the size of the predicted positive effect of selective access to a previously protected SFM for human capital on market value:

Hypothesis 2 (H2). *The greater a firm's within-jurisdiction rivalry in the SFM for human capital, the smaller the increase in the firm's market value as a result of the public policy change that provided selective access to a previously protected SFM for human capital.*

Resource-based theory suggests that the value of possessing or having access to a specific resource differs across firms. The value a firm can potentially create with a new resource "depends on the resources already possessed" (Wernerfelt, 2011, p. 1369) or on the match between the firm's resource needs and the characteristics of the newly available resource. Thus, heterogeneity amongst firms leads to differential benefits from access to a previously protected SFM for human capital. We argue that firms that rely on skilled human capital, especially so-called “knowledge workers,” defined as employees “with high degrees of expertise, education or experience” whose primary job purpose “involves the creation, distribution, or application of knowledge” (Davenport, 2005, p.10) benefit disproportionately from this new access for several reasons.

Knowledge workers will be disproportionally represented in the newly accessible SFM because such highly skilled workers are more likely to be subject to non-compete agreements. As Drucker notes, “the knowledge between their ears is a totally portable and enormous capital asset” (1999, p. 87). Such workers can be vehicles of inter-firm knowledge transfer, able to take firm knowledge to a competitor (Bhide, 2000). To counter these risks, employers utilize employee non-compete agreements (Bishara, 2006) which are especially common for knowledge workers (Younge et al., 2015), and the mobility of highly skilled workers, such as inventors (Ganco, Ziedonis, & Agarwal, 2015), is sensitive to non-compete enforcement. Therefore, many workers within the previously protected SFM for human capital are likely to be highly skilled knowledge workers.

We suggest that firms reliant on skilled human capital likely need additional knowledge worker hires and will therefore especially benefit from access to the previously protected SFM for human capital. Such firms benefit from a new supply of knowledge workers in order to reinforce or complement their existing workforce. Furthermore, these firms may be better able to make use of incoming knowledge carried by such workers because they can better exploit the knowledge, skills, and capabilities of new knowledge worker hires due to potentially higher absorptive capacity (Cohen & Levinthal, 1990). The value of accessing the previously protected SFM is greatest for firms able to best make use of incoming knowledge carried by knowledge workers. We suggest firms reliant upon skilled human capital are best positioned to ensure inter-firm knowledge transfer (Tsai, 2001) from access to a previously protected SFM comprised of many knowledge workers.

In addition, firms reliant on skilled human capital who already employ knowledge workers may also be more attractive employers to potential new knowledge workers. Under the classic workplace adage "like hires like" – or, more formally, "homophily preferences" which "lead people to prefer to work and interact with others who share certain characteristics with themselves" (Roth, 2004, p. 193) – firms already employing knowledge workers may benefit from preferential or reputational effects that make them attractive employers for potential new knowledge worker hire.

A firm's need for skilled human capital is readily ascertainable by investors. We therefore propose that firms that rely on skilled human capital experience larger increases in market value from selective access to the previously protected SFM for human capital than other firms:

Hypothesis 3 (H3). *The greater a firm's need forskilled human capital, the greater the increase in its market value as a result of the public policy change that provided selective access to a previously protected SFM for human capital.*

3. DATA AND METHOD

3.1 Sample

To construct our sample, we first identified 1,312 firms headquartered in California for which COMPUSTAT reported at least one year of annual report data within 2 calendar years prior to the event date (to allow for variations in firm fiscal year dates). We then used the Chicago Booth School of Business's Center for Research in Security Prices (CRSP) data to determine which firms provided adequate stock market information for the event study methodology and removed 538 firms due to insufficient trading data and two (2) additional firms due to stock ticker changes, yielding an initial sample of 772 firms. We next removed 164 "firms" that were actually mutual funds,187 firms for which we identified confounding events during our event window (McWilliams & Siegel, 1997), and two (2) firms with duplicate records; we also removed four (4) possible outliers. A step-by-step description of our process is available in Online Appendix Section C; in total, we removed 357 firms from the sample. We also checked for confounding events at the state (California) level, including legislation and judicial decisions, and the national (U.S.) level, and did not find any. Our final sample includes 415 California-headquartered firms from 45 different industries (based on two-digit SIC codes) although over 75% of the firms come from seven industries.[3] Just over half of the firms operate in high-technology industries,[4] and just over half of the firms are located in Silicon Valley; 37.59% of firms are both located in Silicon Valley and operate in high-technology industries.

3.2 Variables and measures

Dependent variable: For Hypothesis 1, we operationalize the increase in market value of in-jurisdiction, *i.e.* California, firms as the mean cumulative abnormal return (CAR) for all 415 sample firms over an event window of (+1, +3) where day 0 is the day of the court decision. Firm-level abnormal returns represent the difference between the actual stock market return of a firm and the expected return based on the market (using the market model and the CRSP value-weighted index return with dividends); positive abnormal returns indicate investors believe that an event will increase the future performance of a firm. A firm's CAR measures how much the firm's stock price differs from its anticipated value based on its prior performance and the market return during the event window. The start date of (+1) was chosen because a Factiva search indicated that the earliest news about the court decision was published the day following the decision (+1). While stock prices can fully adjust as quickly as within a few minutes or hours after news about an event becomes public (McWilliams and Siegel, 1997), it takes longer (hours or even days) for stock prices to fully

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[3] These seven industries are: SIC Code 36 (Electronic and other Electrical Equipment and Components, except Computer Equipment, n = 75); SIC Code 73 (Business Services, n = 59); SIC Code 28 (Chemicals and Allied Products, n = 49), SIC Code 38 (Measuring, Analyzing, and Controlling Instruments; Photographic, Medical and Optical Goods; Watches and Clocks, n = 45); SIC Code 60 (Depository Institutions, n = 33); SIC Code 35 (Industrial and Commercial Machinery and Computer Equipment, n = 31), and SIC Code 67 (Holding and Other Investment Offices, n = 21).

[4] We categorized 11 industries as "high tech" by using the U.S. Census Bureau's concordance tables to map Hecker's (2005) most technologically intensive industries (based on the 2002 NAICS codebook) to the 2007 NAICS codes used in our study. Online Appendix Table D-2 lists the NAICS codes we classified as *high-tech.*

adjust to news about major court decisions (Katz, Bommarito, Soellinger, & Chen, 2017). Hence, we use a 3-day event window (+1, +3) and present results for other windows as robustness checks in Section 4.2. Further details on the construction of this measure are in Online Appendix Section B.

To test Hypotheses 2 and 3, which investigate the variation of CARs across within-jurisdiction (California) firms, we use firm-level CARs over the (+1, +3) event window as our dependent variable (*CAR*).

Independent variable – factor market rivalry: The greater the number of similar firms with which a focal firm competes for resources in factor markets, the greater is the rivalry in the factor markets. Thus, we operationalize factor market rivalry (*rivalry*) as the industry concentration within California relative to the United States, using the private (*i.e.*, non-government) establishments (*i.e.*, locations/worksites) location quotient for the state of California for each firm's four-digit NAICS industry from the Q2 2008 Quarterly Census of Employment and Wages (QCEW) from the U.S. Bureau of Labor Statistics (BLS) (available for 410 firms). A location quotient of 1 means that a given industry has the same share of establishments (worksites) in California as it does nationwide, while greater (less) than 1 means that an industry is more (less) concentrated than the nation. .

Independent variable – firm need for skilled human capital: We use the natural log of the number of knowledge workers employed by the firm prior to the public policy change as a proxy for the firm's need for skilled human capital (*firmHC*). We construct this variable by multiplying a measure of knowledge worker intensity in the focal firm's primary industry by the number of employees of the focal firm (from annual reports, available for 408 firms) and applying a natural log transformation to better approximate a normal distribution.

Our measure of knowledge worker intensity in the focal firm's primary industry is the percentage of knowledge workers employed in the firm's primary four-digit NAICS code (industry). We calculated this percentage using data from the Occupational Employment Statistics (OES) Survey of the U.S. Bureau of Labor Statistics (BLS), which provides the nationwide number of employees working in each standard occupational classification (SOC) code in each four-digit NAICS code. Young, Tong and Fleming (2015) propose a classification of knowledge workers based on SOC codes, which classifies occupations with SOC codes lower than 50-000 as "knowledge workers." However, examination of this cutoff suggests potential flaws, which we detail in Section D of the Online Appendix. For example, using 50-000 as a cutoff, "construction and extraction occupations" are classified as "knowledge workers" and, as a result, 98% of workers in the residential building construction industry (NAICS 2361) are classified as "knowledge workers," which is a higher knowledge worker percentage than in the pharmaceutical industry (code 3254) or in semiconductor manufacturing (NAICS 3344). To address these inconsistencies, we reviewed the occupational categories in the SOC classification system for the year 2000 (Bureau of Labor Statistics, 2010), which was in place at the time of the 2008 court decision. We identify a clear divide in worker training and/or education at code 33-000: occupations listed below this code include managers, scientists, engineers, etc., while codes from 33-000 through 50-000 appear to be primarily support and services related, and those above 50-000 clearly do not relate to knowledge work. Therefore, we propose SOC codes below 33-000 as indicative of "knowledge workers" and use this cutoff to calculate the industry knowledge worker ratios in our study. A full comparison of our revised measure with the measure from Younge, et al. (2015) is in Section D of the Online Appendix. To calculate the percentage of knowledge workers in each firm's industry, we divide the nationwide number of knowledge workers in each firm's primary four-digit NAICS code by the total number of employees in the industry using May 2008 BLS data.

Control variables: In our tests of Hypotheses 2 and 3, we include several control variables. First, we control for the incidence of employee non-compete agreements by two-digit NAICS sector (*NCAinc*), using data from Starr, Prescott, and Bishara (2021), as firms in industries where employee non-compete agreements are common would likely benefit differently from the policy change than firms in industries in which such agreements are uncommon. We control for R&D intensity (*RDint*) because R&D is shown to be a complementary asset to human capital (Riley, Michael, & Mahoney, 2017). Firm-specific complementary assets (Hitt et al., 2001) such as R&D can increase the value of firm human capital and thus the value that firms derive from gaining access to a previously protected pool of human capital. Our measure of R&D intensity is constructed as R&D expenditures divided by sales for the fiscal year prior to the court decision. If R&D expenditures were not reported, we conservatively estimate them as zero, and to address extremely high levels of R&D intensity (due to early-stage firms with high R&D expenditures but negligible sales), we cap the maximum value of R&D intensity at 3. We control for firm size (*size*), measured as total sales from COMPUSTAT in the year prior to the court decision because large firms may have more resources to hire from out-of-state or be more attractive employers due to their size. As location may affect a firm's ability to attract human capital (Almazan, De Motta, & Titman, 2007), we control for firm location in the "hot spot" (Pouder & St. John, 1996) of Silicon Valley[5] via a binary variable (*SiliconValley*) equal to 1 if a firm was headquartered in Silicon Valley and 0 otherwise. Finally, we control for firm industry (two-digit SIC code) via industry fixed effects to mitigate concerns that unobserved heterogeneity at the industry level will drive our results.

3.3 Analytical methodology

The methodology for this project follows the steps for an event study outlined by McWilliams and Siegel (1997). To test Hypothesis 1, we evaluate whether California's court decision resulted in a positive mean CAR for our sample of publicly traded firms headquartered in California during the stock market trading days immediately following the announcement of the decision. Detailed methodology for the event study is reported in Online Appendix Section B.

[5] Silicon Valley was defined as including Alameda, Contra Costa, San Francisco, San Mateo, Santa Clara, and Santa Cruz counties (Bureau of Labor Statistics, 2009).

To test Hypotheses 2 and 3, we use regression analysis with firm-level CARs for California-headquartered firms as the dependent variable and estimate whether CARs for California firms in the days after the court decision were affected by factor market rivalry and firm need for skilled human capital. We used the following econometric specification where *i* indexes firms, *w* denotes the event window, *t* denotes the firm's most recent fiscal year ending prior to the court decision, α_i represents industry fixed effects at the two-digit SIC code level, and $u_{i,w}$ represents the error terms:

$$CAR_{i,w} = \beta_0 + \beta_1 rivalry_{i,t} + \beta_2 \ln(firmHC_{i,t}) + \beta_3 NCAinc_{i,t} + \beta_4 RDint_{i,t} \\ + \beta_5 size_{i,t} + \beta_6 SiliconValley_{i,t} + \alpha_i + u_{i,w}$$

Clustered errors. Errors were clustered[6] at the industry level using two-digit SIC codes (Cameron & Miller, 2015; Stock & Watson, 2008) to address concerns with heteroscedasticity in some models and to correct any remaining within-industry correlation (Nichols & Schaffer, 2007).

Regression diagnostics. We conducted a series of sensitivity tests to identify potential overly influential values and identified five (5) such observations. The reported results exclude these observations (n=398). Results including these observations are consistent with the reported results.

3.4 Descriptive Statistics and Preliminary Data Analysis

Summary statistics and correlations are presented in Table 1. The highest correlation is between the knowledge worker variable and the control variable for firm size, which makes intuitive sense.

---INSERT TABLE 1 ABOUT HERE---

4. RESULTS

4.1 Hypothesis Testing

[6] Using Eicker-Huber-White-robust treatment of errors to make as few assumptions as possible (Nichols & Schaffer, 2007). This selection of error treatment is also correct given that the conclusions being drawn from this project are meant to be applied only to the firms in the sample (Abadie, Athey, Imbens, & Wooldridge, 2017).

Hypothesis 1 suggests a public policy change that provides within-jurisdiction firms access to a previously protected SFM for human capital increases the market value for such firms. Our event study results support this hypothesis. Table 2 shows that the mean CAR for the event window (+1, +3) (top row and bolded in Table 2), for all 415 California firms is positive (0.0253) with $p < .001$ for all three test statistics and a 95% confidence interval that does not include zero (0.0202 to 0.0304). Thus, the market value for the average firm in our sample increased by 2.53% in the days after the public policy change.

---INSERT TABLE 2 ABOUT HERE---

Results of regression models to test Hypotheses 2 and 3 are presented in Table 3. Model 1 includes control variables only while Model 2 includes the full model. Hypothesis 2 suggests that within-jurisdiction factor market rivalry reduces the positive effect of selective access to a previously protected SFM for human capital on firm market value. This hypothesis is supported. The coefficient for factor market rivalry in Model 2 is negative (-0..037513) with p = ..007 with a confidence interval of -.0641827 to -0.0108433that does not include zero. This indicates that for every should the industry concentration, as measured by the establishments location quotient, increase by a value of 1, firm-level CAR would decrease by 3.75%

---INSERT TABLE 3 ABOUT HERE---

Hypothesis 3 suggests that firm need for skilled human capital increases the positive effect of selective access to a previously protected SFM for human capital on firm market value. This hypothesis is also supported. The coefficient for the need for skilled human capital variable in Model 2 is positive (0.003933) with p = .043and a 95% confidence interval of 0.0001352to 0.0077308, which does not include zero. The size of the coefficient for our need for skilled human capital variable (operationalized as the natural log of the number of firm knowledge workers) indicates that a firm employing 10% more skilled human capital than another firm experienced an additional 0.039% increase in CAR as a result of the public policy change.

4.2 Robustness checks

To check for biases due to the availability of stock market data, we tested whether all 774 firms (which include mutual funds and firms that experienced confounding events) with sufficient stock market data were significantly different from the 538 firms that lacked such data. Mean values of most variables included in our model did not differ significantly for the two groups of firms, although firms with sufficient data had slightly higher R&D expenditures and factor market rivalry. Thus, excluding firms with insufficient trading data is unlikely to bias our results.

We also obtained consistent results for tests of Hypothesis 1 for all firms reporting sufficient stock market data (*i.e.,* prior to removal of firms from the sample due to firm-level confounding events, status as mutual funds, etc., as described in Section C of the Online Appendix). Results show a positive mean CAR of 1.53% for these 774 firms with $p < .00$ (see Table B-1 in the Online Appendix), which is consistent with the results for our study sample (n=415).

Our event study results (n = 415) are robust to different specifications of the event window, as shown in Table 2. The results are also robust to different model and market index specifications.[7] Nonparametric tests for the (+1, +3) window are also robust and significant,[8] which assuages any concerns around event-induced volatility and the use of parametric tests statistics (Savickas, 2003).

The regression results (n = 398) are similarly robust. Stepwise regressions testing each independent variable alone were entirely consistent. We also performed a regression analysis without

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[7] For example, for the (+1, +3) event window: Market Adjusted Model (mean CAR = 0.02291; Patell Z = 9.14; t-statistic = 7.83; standardized statistic = 9.08); Fama French Three Factor Model (mean CAR = 0.0046; Patell Z = 1.99; t-statistic = 1.67; standardized statistic = 2.00).

[8] Market Model with the CRSP value-weighted index: Generalized Z = 9.651, rank test Z = 3.36, jackknife Z = 3.49. Robustness checks with additional model configurations (such as Market Model with equally-weighted index) are also significant and are available from the authors upon request.

industry-clustered errors since the number of clusters is 44, below the ideal number of 50 (Kézdi, 2004), particularly in light of the unbalanced cluster sizes (from 1 to 72) (Nichols & Schaffer, 2007). This analysis leaves point estimates (coefficients) unchanged but allows for different (here, slightly larger) standard errors; however, we had only mild changes that did not change our conclusions.

We also confirmed the robustness of our measure for factor market rivalry by alternatively measuring rivalry in terms of the number of the number of in-state competitors in the same industry, calculated as the number of other publicly-traded firms headquartered in California in the same four-digit NAICS code as the focal firm. We use the COMPUSTAT data for all California-based firms (n = 1,312) to calculate this variable. Results were marginally significant for rivalry (p = .081) and in the hypothesized direction, and all other variables remained nearly unchanged.

To verify the robustness of our refined knowledge worker variable for this project in particular, we gathered data from the BLS QCEW on industry average weekly wage within California in Q2 2008 and checked its correlation with our knowledge worker industry percentages: our knowledge worker variable has a .8331 correlation, while the original Younge et al. (2015) measure has a correlation of only .2658.

We also confirmed the validity of the incidence of non-compete enforcement control variable by using incidence by similar data from Colvin & Shierholz (2019), which is based on a firm-level survey from March 2017 to July 2017, and reports two different measures for industry frequency: firms reporting that they use employee non-competes for *all* workers and firms reporting that they using non-competes for *any* workers. Coefficients and significance values for our primary variables of interest remained largely unchanged and at least marginally significant.

5. SUPPLEMENTAL ANALYSIS

This project finds highly significant support for the role of employee non-compete policy in shaping the market value of firms. Specifically, the average stock of California-based firms performed an astounding 2.53% higher than anticipated based on overall market performance while controlling for firm risk after the California Supreme Court decision to end enforcement of out-of-state non-compete agreements. These California-based firms are what Markman and colleagues refer to as "attackers," who can "annex the disputed resources" and "fortify their ranks and toughen their position in factor markets and product markets" (2009, p. 432). Our focus in this project has been on the benefits to the California "attackers." However, it is important to consider that the change in California's non-compete agreement enforcement policy may have had a negative effect on firms outside of California because it reduced these firms' ability to protect their human capital. Thus, a possible alternative interpretation of our results is that they do not represent a positive increase in market value for California firms, but, a decrease in market value for non-California firms (i.e., lowering the overall market return and therefore causing California-headquartered firms to exceed the market rate of return). While this interpretation still suggests the policy change caused some degree of state-level human capital-based competitive advantage by generating positive abnormal returns for California-based firms, it suggests these benefits came at the expense of out-of-state firms.

However, we ran multiple event studies utilizing out-of-state firms, and were unable to locate a meaningful diverse group of firms that experienced statistically significant negative cumulative abnormal returns over the same event window. Thus, it does not seem that California firms benefit at the expense of firms in other states or similar industries. The human capital based advantage obtained by California-based firms was likely not a result of out-of-state firms being harmed by the California Supreme Court decision.

6. DISCUSSION & CONCLUSION

This paper examines whether and under what conditions a public policy-induced change in SFM imperfections affects competitive advantage for a subset of firms. We investigate the effects of a public policy change providing in-jurisdiction firms selective access to a previously protected SFM for human capital on the market value of these firms. We empirically test our hypotheses by examining the effect of a 2008 decision by the California Supreme Court to stop enforcing out-of-state employee non-compete agreements – a public policy change that removed a factor market imperfection for California firms by providing them access to a previously protected out-of-state labor market – on the market performance of these firms. Our results show that the market value of California firms increased relative to excluded (out-of-state) firms after the policy change. Further, firms with low factor market rivalry and firms more reliant on skilled human capital reaped larger performance benefits from access to the previously protected labor market.

Although our study focuses on SFMs for human capital, our findings have broader implications as they highlight the important role that public policies regulating factor market access can play in affecting firms' competitive advantage. Public policies that selectively remove jurisdiction-specific factor market imperfections by providing within-jurisdiction firms selective factor market access can be a source of competitive advantage. Such policies allow within-jurisdiction firms to overcome factor market barriers that remain in place for other firms. We build on prior research linking market competition and government intervention (Oberholzer-Gee & Yao, 2018) to theoretically and empirically demonstrate how public policies, specifically those regarding enforcement of non-compete agreements, regulate factor market imperfections with sizable effects on the market value of affected firms. This project therefore provides an explicit example of how public policy can directly improve firm performance. We find that a change in California's non-compete law that provided California firms with selective access to a previously protected SFM for human capital increased the market value of California firms by an average of 2.53%, with the average California firm gaining $84 million in market value,[9] for total value creation of $34.9 billion across

[9] The total market value for each firm at the event date was calculated by multiplying its closing stock price per share by the number of shares outstanding.

the firms in our sample. While the size of this increase is remarkable among management studies, it is in line with prior research on public policies' effect on market value. For example, legislation requiring German firms to include personal deductibles in their insurance policies resulted in these firms experiencing a mean CAR of 2.5% (Lin, Officer, Schmid, & Zou, 2019). Thus, our study reinforces an important point: investors pay substantial attention to public policy changes. Moreover, our study suggests investors are quite nuanced when assessing the impact of a public policy change on individual firms. In particular, we find that they consider variables that affect the value of increased factor market access for each firm in their assessment of changes in firm access to markets for human capital, specifically factor market rivalry and firm need for skilled human capital.

Our finding that firms with lower factor market rivalry experience greater increases in market value suggests that the value of selective factor market access depends on factor market rivalry, *i.e.* how many similar firms with whom the selective access is shared. This finding supports resource-based theories suggesting competitive interactions in factor markets need to be considered as a determinant of competitive advantage (Chatain, 2014; Markman et al., 2009). Investors consider factor market rivalry when assessing the effects of public policy changes on individual firms.

Our finding that firms reliant on skilled human capital experience greater increases in market value from access to the previously protected labor market stems from the fact that skilled human capital, so-called "knowledge workers," are more likely to be covered by non-compete agreements than other workers. Thus, knowledge workers are disproportionally represented in the labor supply of the previously protected SFM for human capital that California firms gained access to. Because firms reliant on skilled human capital are more likely to demand skilled knowledge workers, there is a match between the supply side labor market characteristics and the labor needs and demand of these firms. Further, the existing human capital bases of firms reliant on skilled human capital likely possess higher levels of absorptive capacity (Cohen & Levinthal, 1990) which allows these firms to benefit from the knowledge, skills, and expertise that (potential) incoming knowledge workers bring, and these firms are expected to be attractive, sought-after employers for highly skilled workers seeking new positions. Our results show that market investors are aware of and consider firms' need for skilled human capital in their assessment of the effects of the change in California's non-compete policy on individual firms.

The existing strategy literature on factor market imperfections (Barney, 1986; Chadwick, 2017) has mostly focused on identifying ways in which individual firms can create competitive advantage by protecting their resources, *e.g.*, by creating firm specific resources that other firms cannot readily access or replicate (Hatch & Dyer, 2004; Koch & McGrath, 1996) or by legal protections such as non-compete agreements (Bishara, 2006) or patents (Kim & Marschke, 2005). There is limited work that analyzes how firms gain access to resources by overcoming SFM imperfections. This literature has mainly focused on mergers and acquisitions as a mechanism by which an individual firm can acquire protected resources from other firms (*e.g.*, for human capital resources: Chatterji & Patro, 2014; Uhlenbruck, Hitt & Semadeni, 2006). In contrast, we focus on public policy changes that selectively remove factor market imperfections and provide access to a previously protected factor market to a large number of firms. We thus highlight the asymmetric impact public policy changes can have on within- and outside-jurisdiction firms and propose this as an area worthy of future exploration, particularly regarding how firms' strategic decisions, such as location decisions, may be affected by these legal jurisdiction-specific factor market imperfections. Our study suggests firm decision makers should consider current and expected changes in public policies as strategic factor market imperfections can be location or jurisdiction-specific.

Our research responds to appeals in SHC literature to focus on a broader set of labor market imperfections that affect the mobility of human capital (Campbell et al., 2012). The extant literature focuses mainly on firm specificity of human capital as a source of labor market imperfections that limit *outward* employee mobility. In contrast, our study focuses on the role of public policies that regulate labor market access – the enforcement of non-compete agreements – as a source of supply-side labor market imperfections that affect *inward*-flowing employee mobility. While the human capital literature mostly assumes that general human capital is mobile between firms, non-compete agreements limit the mobility of employees with both specific and general human capital. Our study shows that investors pay keen attention to firms' human capital in their market decisions. The 2.53% increase in market value resulting from California's decision to not enforce out-of-state employee non-compete agreements is consistent with, although of larger magnitude than, results of other empirical studies on the impact of human capital on firm market performance: specifically, Riley and colleagues (2017) found an increase of 1.67% in CAR for firms that won human capital training awards, while Hillman, Zardkoohi, and Bierman (1999) demonstrated gains of 1.6% when an executive left a firm for a Cabinet-level appointment or Congressional position. In contrast to these papers, we explore the general effect of gaining labor market access as opposed to idiosyncratic and firm-specific events. Finally, while research on employee mobility addresses how such mobility impacts the firms employees leave, so-called "source firms" (Campbell, Ganco, Franco, & Agarwal, 2012), we show how employee mobility affects firms employees may join, *i.e.*, "receiving firms."

We also make a methodological contribution to the human capital literature by introducing a refined measure for calculating industry-level ratios of knowledge worker employment, modifying the measure from Younge et al. (2015), by focusing on occupation categories more consistent with "knowledge work," As detailed in Online Appendix Section D, our refined measure is consistent with prior research in the knowledge management space. Our measure more accurately reflects "knowledge workers" by focusing on those with significant training or expertise and whose primary role requires the creation or use of knowledge (Davenport, 2005).

Our study also contributes to research on employee non-compete agreements. First, prior research has predominantly considered non-compete agreements as tools to reduce firms' risk of *outward*-flowing human capital and thereby protect firm knowledge by constraining employee mobility (*e.g.*, Marx et al., 2009). Research on the effects of non-compete enforcement on firms' ability to access new, *inward*-flowing human capital by restricting the availability of potential new hires is limited (Marx & Fleming, 2012; Starr et al., 2019). In this paper, we explore this inward aspect, that is, how non-compete agreements affect firms' market performance via potential inflows of new human capital. Second, to the best of our knowledge, our study is the first to analyze the effects of non-compete agreements on market performance. We demonstrate that market investors do respond – emphatically – to changes in public policy, particularly the enforcement of employee non-compete agreements. This project therefore contributes to the extant literature on employee non-competes by identifying another group of stakeholders, other than firms or employees themselves, that is invested in the impact on firms of employee non-compete policy. Third, we make a methodological contribution to the non-compete literature by conducting an empirical analysis of non-compete enforcement without making any cross-state comparisons or rankings of non-compete enforcement by focusing on a change in non-compete enforcement in a single state and conducting the first event study in this space.

Beyond our focus on employee non-compete agreements, this research demonstrates the potential of public policies to contribute to firm value creation through regulation of factor market access. In addition to non-compete policies there are many other policies that affect labor market access such as immigration policy (particularly specialized visa programs), border closures, and regulations on no-hire or anti-poaching agreements. Policies affecting SFM access are also not limited to the labor market. One recent parallel is the Chinese government's policy of denying foreign patent holders the right to enforce their patents in China. This policy regulates the factor market for knowledge and provides Chinese firms selective access to foreign knowledge, to the likely detriment of foreign firms who developed the knowledge. Therefore, the policy has led to allegations of theft of U.S. intellectual property by Chinese firms (Reuters, 2018).

While intuitively a policy to not enforce out-of-state non-compete agreements could benefit California firms at the expense of out-of-state firms, we find in supplemental analysis that firms outside of California that were most likely to lose employees to California firms were not immediately harmed – in the assessment of investors – by their human capital no longer being protected by non-compete agreements in California. However, it is possible that the negative effect on each non-Californian firm was too small to be significant. Each California firm gained access to a nearly national labor market, and each non-California firm is only a small part of that market, making the negative effect of the potential loss of human capital for each non-California firm likely small.

6.1 Implications for managers and policymakers

Our research highlights that firm executives should pay attention to changes in public policies that affect labor market access and SFM access in general because such policy changes affect firms' market performance. Moreover, executives need to consider competition in the labor market and the match between the firm's desired employee characteristics and the characteristics of the employees in the affected labor market when analyzing how public policy changes that alter access to labor markets for a subset of firms may affect the market performance of these firms. Our study highlights the importance of managing investor expectations as we show that investors are clearly aware of factor market rivalry as well as firm need for skilled human capital when assessing the impact of non-compete enforcement changes on firm performance. Managers should consider such factors in their communications with investors. Finally, despite prior research suggesting non-compete enforcement is unlikely to be a primary consideration in firm location decisions (Garmaise, 2011), this project joins recent scholarship (Starr, Ganco, & Campbell, 2018) in suggesting perhaps it should be a factor in the decision process, as we show that such policies affect firm market value. Thus, firms that desire access to an especially mobile labor pool may find it beneficial to locate in California, and this effect may be especially pronounced for firms that rely heavily on skilled human capital. Such firms may find it advantageous to locate in California due to the selective access they would have to out-of-state labor markets that they would otherwise be unable to access.

Our research also has interesting implications for policymakers. Given the large increases in market value for California firms as a result of the policy change, it would seem rational for other states to follow suit and also end their enforcement of out-of-state non-compete agreements, resulting in a "race-to-the-bottom" of non-compete enforcement policies across states. If this were to happen, California firms would lose the advantage they gained from the selective factor market access over time because firms from other states would also be able to freely access out-of-state employees subject to non-compete agreements. The expected end of such a race-to-the-bottom process would be that all states would stop enforcing out-of-state non-compete agreements, making non-compete agreements a much less effective tool for firms to prevent valuable firm knowledge contained within the minds of their employees from being acquired by competitors.

Nevertheless, we have not observed such a race-to-the-bottom. While there has been much recent attention to non-compete policy (*e.g.*, White House, 2016) and recent legislative proposals on non-compete reform in several states (including Idaho, Maryland, Massachusetts, Michigan, Missouri, New Jersey, and Washington), the crux of this attention has been on how non-compete agreements affect workers (particularly low-wage ones), not how firms can be harmed or benefit from non-compete enforcement policies. Moreover, due to the state-by-state nature of non-compete enforcement, most legislative proposals or changes predominantly address only within-state enforcement of non-competes, not the enforcement of out-of-state agreements. Some states might find it politically disadvantageous to stop enforcing out-of-state non-competes due to the risk of retaliatory actions from other (particularly bordering) states. Turning to actual enforcement policy changes (see Online Appendix Section A), no state has gone as far as California in banning all enforcement of out-of-state non-compete agreements. In this sense, the California policy change can be regarded as a long-term – 12.5 years at the time of writing – regulatory disequilibrium phenomenon because other states have not (yet) followed suit.

6.2 Limitations and suggestions for future research

Our study empirically examines a change in non-compete enforcement in one state, California, and it is not clear if such a policy change would have similar effects in other states. California is home to some of the world's largest technology companies, and these companies operate in industries in which knowledge workers are prevalent (see Online Appendix Table D-2), and industries in which research suggests that employees are often covered by non-compete agreements (Marx, 2011). Further, over 10% of Fortune 500 companies were based in California in 2008 (third only to New York and Texas) (CNN Money, 2008) and it is possible that such headquarter locations employ large numbers of knowledge workers. Given the need for knowledge workers in California and the fact that firms needing knowledge workers experience larger increases in value, the overall increase in market value resulting from ending the enforcement of out-of-state non-compete agreements in the average state may be smaller (which may be another reason why other states have not adopted a similar policy). It would be interesting to see if the conclusions of this study hold when other states change their non-compete agreement enforcement policy but face a different industry mix.

The event in this study occurred in 2008, and perhaps some 13 years later, the results may not hold due to the changing nature of work, particularly for knowledge workers. This is forefront in our minds due to the COVID-19 pandemic and the flexible work arrangements necessitated by public health authorities. Recent research indicates that "work-from-anywhere" policies increase employee output by 4.4% (Choudhury, Foroughi, & Larson, 2021). The implications of such policies on non-competes (or other employment laws at the state level) is an open question in both management and law, raising important questions over which law(s) may apply. With work-from-anywhere, do non-competes become more or less of a mobility barrier? We leave such questions to future research.

This study relied on event study methodology. While this method gauges investor expectations of the effects of the change in California's public policy on in-state firms' performance by looking at a short event window and removing observations with confounding events, we do not know the actual effects of the public policy change on accounting performance, which take longer to realize. Accounting performance is affected by factors other than access to a previously protected market of potential employees. For this event (which occurred on August 7, 2008), looking at performance effects over a longer time period is complicated by the stock market crash on September 29, 2008. Thus, interpreting longer-term measures of firm performance, such as *Tobin's q*, as done in Younge and Marx (2016), or buy-and-hold abnormal returns to investigate longer-term risk-adjusted returns over a holding period (Lyon, Barber, & Tsai, 1999) would be difficult for this particular event.

This study does not analyze whether California firms proceeded to hire more out-of-state employees previously protected by non-compete agreements following the public policy change. Future research could analyze the hiring trends of California firms to explore whether they attempted to or were successful in hiring employees from out-of-state who had signed non-compete agreements after the policy change. Such research could be enriched by interviews with California-based HR executives or employment lawyers. However, such analysis could also be complicated by the September 2008 stock market crash and its potential implications on labor market mobility.

Despite these limitations, this study makes important contributions at the intersection of research on strategic factor markets, strategic human capital, and public policy by considering how public policies that change labor market imperfections impact the competitiveness of affected firms. Our results demonstrate that public policies regulating factor market access, which are not commonly considered in the resource-based theory and human capital literatures, can have sizable effects on firms' market value. We hope that our study sparks additional research that considers how public policies that create or destroy factor market imperfections, especially those involving markets for human capital, influence firm performance.

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TABLES

TABLE 1 Summary statistics and pairwise correlations, n = 398

Variable	Mean	Std. Dev.	Min.	Max.	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(1) CAR (+1,+3)	0.0257	0.0514	-0.1138	0.1948	1						
(2) Factor Market Rivalry	1.0543	0.4941	0.13	3.6	-.01	1					
(3) Firm need for skilled human capital	5.3964	1.8900	0.1297	11.4800	-.01	.01	1				
(4) Incidence of non-competes	0.2334	0.0510	0.09	0.32	.04	-.07	.25	1			
(5) R&D Intensity	0.2942	0.6833	0	3	.11	.052	-.20	.02	1		
(6) Firm Size	1981.496	12120.01	0	203970	-.07	-.04	.31	-.02	-.06	1	
(7) Silicon Valley	0.5226	0.5001	0	1	.08	.21	.14	.07	.15	.07	1

TABLE 2 Final event study results

Sample Size	Event window	Mean CAR	Patell Z[1]	t-statistic[2]	Standardized cross-sectional statistic[3]
415	**(+1, +3)**	**0.0253**	**9.88**	**9.73**	**10.38**
415	(0, +3)	0.0288	10.18	9.48	10.73
415	(+1, +2)	0.0202	9.82	8.28	8.98
415	(+1, + 4)	0.0276	9.22	9.32	10.12
415	(-1, +1)	0.0105	4.85	3.31	4.85

p-values for all test statistics are less than p = .001

[1] Patell Z refers to a statistic calculated as in the study by Patell (1976) using standardized residuals.
[2] The cross-sectional t-statistic at the end of the event window as calculated by Brown and Warner (1985).
[3] The standardized cross-sectional statistic as calculated by Boehmer, Musumeci, and Poulsen (1991).

TABLE 3 Generalized least-squares regression results with cluster-robust errors

Dependent Variable: **Firm level CAR (+1,+3)**	**Model 1 (Control variables only)**	**Model 2 (Full model)**
Independent Variables		
Factor Market Rivalry (H2)		-0.0375 *p = .007*
Firm Need for Skilled Human Capital (H3)		.0039 *p = .043*
Control Variables		
Incidence of non-competes	-.0513657 *p = .021*	-.0514791 *p = . 334*
R&D Intensity	0.0035 *p = .698*	0.0054 *p = .525*
Firm Size	-1.01 x 10^6 *p = .000*	-1.47 x 10^6 *p = .000*
Silicon Valley	0.0068 *p = .285*	0.0071 *p = .302*
Constant	0.0351 *p = .000*	0.0536 *p = .010*
Industry Fixed Effects*	Included	Included
No. of Obs.	398	398
Intraclass correlation (*rho*)	.428	.581
R-Sq. (within industry groups)**	. 020	.0543
R-Sq. (overall model fit)**	. 171	.200

**Fixed effects are based on two-digit SIC code.*

*** The R-squared (within industry groups) is calculated using STATA's* xtreg *command, while the R-Squared (overall model fit) comes from using STATA's* areg *command, due to the* xtreg *version not quantifying the overall fit of the model while the* areg *approach estimates how group effects affect overall model fit (Gould, n.d.).*

ONLINE APPENDIX

SECTION A: Selection of Event

We identified 25 U.S. states that experienced 44 changes in their enforcement of employee non-compete agreements due to either state Supreme Court (or equivalent) legal decisions or state-level legislative changes between 1980 and 2018. These changes are listed by state alphabetical order in Table A-1; states that experienced multiple changes are listed chronologically (oldest on top). Due to the complexity and subjectivity of identifying such changes, we may not have been able to identify all changes/states . We also did not include pending or proposed legislation in this list.

We proceeded through the following process of elimination to identify the event for our analysis, and show the outcome in Table A-1:

1. *Employment related: Was the policy change related to non-competes in the employment context?* Our theory is about labor markets, human capital, and employment, so we wanted to exclude changes to non-compete policy primarily related to business-to-business arrangements such as franchise contracts and sale-of-business. Hence we eliminated changes in non-complete law that were not employment related.
2. *Benefit identifiable subset of firms: Does the policy change affect the labor market access for an identifiable subset of firms to the exclusion of others?* As noted in the main body of the paper, non-compete policy can affect both access to labor markets or can be regarded as a tool to protect firm human capital resources. We looked for changes changed labor market access for a specific and identifiable subset of firms to the exclusion of other firms. We therefore excluded changes not meeting this criteria, including those dealing primarily with contract modification or drafting, clarifications or codifications on existing policy, and those requiring case-by-case interpretation.
3. *Unanticipated: Was the policy change unanticipated*? Legislative policy changes are rarely truly unanticipated because of the process for which a bill must go through before it becomes law: a bill must be introduced, edited, voted on, signed (generally by a governor), and then will have an effective date generally in the future. Moreover, unless explicitly written as such, a legislative change is normally only applicable to non-competes signed on or after the effective date (instead of all existing agreements). In contrast, court decisions are much less likely to be anticipated, and even if a decision is anticipated at some time in the future, the actual content of the decision is not available until it is released by the court. Thus, we eliminated legislative changes in non-compete law and only considered non-complete law changes that resulted from court decisions.
4. *Approximate # of Publicly Traded Firms*: Since we desired to use event study methodology, our data sample would be comprised of publicly traded firms. Thus, a higher number here was desirable. This number was obtained from COMPUSTAT for all firms that reported annual data during the same calendar year of the policy change and is therefore only an approximation.

As is clear from a review of the non-compete policy changes in Table A1-1, the *Edwards* decision in California was the obvious choice for this project.

TABLE A-1 List of Non-Compete Policy Changes in the U.S., 1985-2018

State	Year	Type of Change (*court case name, if applicable*)	*THEORETICAL REQUIREMENTS*		*METHODOLOGICAL CONSIDERATIONS*	
			Employment related?	**Identifiable subset?**	**Unanticipated?**	**# of Publicly Traded Firms**
Alabama	2016	Legislative	Yes	No		
Arkansas	2015	Legislative	Yes	No		
California	2008	Court (*Edwards*)	Yes	Yes	Yes	2,050
California	2017	Legislative	Yes	Yes	No	
Florida	1996	Legislative	Yes	No		
Florida	2017	Court (*White*)	Yes	No		
Georgia	2011	Legislative	Yes	No		
Hawaii	2015	Legislative	Yes	Yes	No	
Idaho	2008	Legislative	Yes	Yes	No	
Idaho	2016	Legislative	Yes	No		
Idaho	2018	Legislative	Yes	Yes	No	
Illinois	2011	Court (*Reliable Fire*)	Yes	No		
Illinois	2016	Legislative	Yes	Yes	No	
Kentucky	2014	Court (*Creech*)	Yes	No		
Louisiana	2001	Court (*SWAT 24*)	Yes	Yes	Yes	56
Louisiana	2003	Legislative	Yes	No		
Massachusetts	2004	Court (*Boulanger*)	No			
Massachusetts	2018	Legislative	Yes	Yes	No	
Michigan	1985	Legislative	Yes	No		
Michigan	1987	Legislative	Yes	No		
Montana	2011	Court (*Wrigg*)	Yes	No		
Nebraska	2015	Court (*Unlimited Opportunity*)	No			
Nevada	2016	Court (*Golden Road*)	Yes	No		
Nevada	2017	Legislative	Yes	No		
New Hampshire	2012	Legislative	Yes	No		
New Mexico	2015	Legislative	Yes	Yes	No	
Ohio	2004	Court (*Lake Land*)	Yes	No		
Oregon	2008	Legislative	Yes	Yes	No	
Oregon	2018	Legislative	Yes	Yes	No	
Pennsylvania	2010	Court (*Missett*)	Yes	No		
Pennsylvania	2016	Court (*Socko*)	Yes	No		

State	Year	Type of Change (*court case name, if applicable*)	***THEORETICAL REQUIREMENTS***		***METHODOLOGICAL CONSIDERATIONS***	
			Employment related?	**Identifiable subset?**	**Unanticipated?**	**# of Publicly Traded Firms**
South Carolina	2010	Court (*Poynter*)	Yes	No		
Texas	1987	Court (*Hill*)	No			
Texas	1989	Legislative	Yes	No		
Texas	1990	Court (*DeSantis*)	Yes	No		
Texas	1994	Court (*Light*)	Yes	No		
Texas	2006	Court (*Sheshunoff*)	Yes	No		
Texas	2009	Court (*Mann Frankfort*)	Yes	No		
Texas	2011	Court (*Marsh*)	Yes	No		
Utah	2016	Legislative	Yes	No		
Utah	2018	Legislative	Yes	Yes	No	
Vermont	2005	Court (*Barnes*)	Yes	No		
Wisconsin	2009	Court (*Star Direct*)	Yes	No		
Wisconsin	2015	Court (*Runzheimer International*)	Yes	No		

SECTION B: Event study details

For the event studies in this project, we used the market model, which assumes a linear relationship between the return of firm i and the return of a market index (MacKinlay, 1997). The market model, as well as parametric tests such as the t-statistic, are generally believed sufficiently powerful for most event study research (Brown & Warner, 1985), and the market model is the norm in most management research (*e.g.*, McWilliams & Siegel, 1997; Riley et al., 2017). Under the market model, the actual rate of return on the share price of firm *i* on day *t*, $R_{i,t}$, is calculated as:

$$R_{i,t} = \alpha_i + \beta_i R_{m,t} + \varepsilon_{i,t} \qquad \text{(Eq. 1)}$$

where $R_{m,t}$ is the rate of return on a market portfolio of stocks (here, the CRSP value-weighted index return with dividends, although see Section 4.2 in the paper regarding the robustness of our results to different market indices) on day *t*, α_i represents the intercept term for firm *i*, β_i represents the systematic risk of firm *i*'s stock, and $\varepsilon_{i,t}$ is the error term for firm *i*, with $E(\varepsilon_{i,t}) = 0$. (Note that any reference in an event study to "days" refers to trading days, which therefore do not include weekends or holidays.)

We estimated this market model via ordinary least squares (OLS) regression for each firm over an estimation window to obtain a measure of expected returns. For all of the event studies referenced in this project, we used an estimation window of the prior year (255 trading days), stopping 5 days before the event date; that is, the estimation window is (-255, -5); and the event window selected for the study was (+1, +3) with day 0 being the day of the California Supreme Court decision (although as noted in Section 4.1, our results are robust to the specification of the event window). The +1 start date of the event window was chosen because there was no indication of any information about the court decision being publicized prior to the day after the court decision (day +1) (see footnote 2 in the main paper). To be included in the data sample for this analysis, we required firms to have at least 3 observations (trades) during the estimation window. Only 774 California-headquartered firms provided sufficient stock market data.

We thus estimated the expected return for each firm *i* on day *t*, $\hat{R}_{i,t}$ as:

$$\hat{R}_{i,t} = a_i + b_i R_{m,t} \qquad \text{(Eq. 2)}$$

where a_i and b_i are ordinary least-squares (OLS) estimates obtained from regressing $R_{i,t}$ on $R_{m,t}$ over the estimation period (-255, 5) prior to the event in question.

We calculate abnormal returns ($AR_{i,t}$) for firm *i* on day *t* by subtracting the expected return (Eq. 2) for each frim from the actual return (Eq. 1):

$$AR_{i,t} = R_{i,t} - \hat{R}_{i,t} \qquad \text{(Eq. 3)}$$

Firm-level abnormal returns for firm *i* on day *t*, $AR_{i,t}$ represent the difference between the actual stock market return of the firm and the expected return based on the market rate. To calculate the cumulative abnormal return (CAR_i) for firm *i* over the event window (t_1, t_2), the daily abnormal returns of firm *i* are summed as follows:

$$CAR_i = \sum_{t=t_1}^{t_2} AR_{i,t} \qquad \text{(Eq. 3)}$$

Since our event window is (+1, +3), $t_1 = 1$ and $t_2 = 3$.

TABLE B-1 Full sample (no confounding events removed) event study results.

Sample Size	Event window	Mean CAR	Patell Z[1]	t-statistic[2]	Standardized cross-sectional t-statistic[3]
774	(+1, +3)	0.0153	7.10 (.000)	5.69 (.000)	5.24 (.000)

Numbers in parentheses are p-values.

[1] Patell Z refers to a statistic calculated as in the study by Patell (1976) using standardized residuals.
[2] The cross-sectional t-statistic at the end of the event window as calculated by Brown and Warner (1985).
[3] The standardized cross-sectional t-statistic as calculated by Boehmer, Musumeci, and Poulsen (1991).

SECTION C Construction of sample

We removed firms from the initial sample of 774 firms due to confounding events or status as mutual funds and corrected minor errors in the data for 3 firms as described in detail here.

First, we examined the list of all 774 firms and noted 138 of the stocks in the sample were iShares listed stocks (see https://www.ishares.com/), which are mutual funds, not firms. These observations were removed from the sample leaving n = 634.

Further examination of the remaining 634 firms revealed that 26 "firms" listed with SIC codes of 6722 or 6726. Like the iShares funds noted above, these represent mutual funds and not firms and were therefore removed from the sample, leaving 608 firms.

We then investigated all firms in the remaining sample of n = 608 for firm-level confounding events that occurred during or around the (+1, +3) event window. Specifically, we completed the following analyses, removing the noted number of firms at each step:

- First, we eliminated all firms who reported quarterly earnings data during the event window, as well as a day before and after due to potential information leakage about earnings reports that occurs frequently the day prior to such formal announcements. Since the event window was in early August (immediately after the close of the second quarter for firms following a standard calendar-year fiscal calendar), this resulted a large number of firms (110) being removed from the sample (leaving n = 498).
- We next searched for analyst recommendations to prevent the results from being affected by an analyst's recommendation to buy/sell/hold/etc. the stock during the event window. We removed 45 firms from the sample for which an analyst made a recommendation during a (0, +3) window, leaving n = 453.
- We then looked for additional stock events occurring during the (0, +3) event window, including dividend announcements (none), stock splits (one), dividend payments, and record dates (eight). Removing these nine events from the sample left 444 firms.
- Next, we checked for all material event filings of 8-Ks with the Securities and Exchange Commission, which includes any press releases occurring during a window of (0, +4). We found 47 of the firms in the sample had filed an 8-K during the applicable time period and manually reviewed each one of these 8-Ks. This allowed us to identify 23 firms that experienced material events during the event window and were then excluded from the sample (leaving 421 firms).

We next noticed that two of the stock tickers were generating duplicates in the Event Study by WRDS platform, so removed them from the sample (leaving 419).

We finally checked for outliers in CAR values, using as a definition of outlier values that were ±3 standard deviations from the mean and identified four such potential values. We first investigated all SEC filings by these firms made during or just prior to the event window, and despite not finding any new information, decided to remove 4 observations from the sample out of an abundance of caution, leaving 415 firms.

We also corrected several errors (n = 3) with NAICS and SIC codes being listed improperly in COMPUSTAT.

To address concerns about confounding events beyond the firm level, we checked for confounding events at the state (California) level, such as other state Supreme Court decisions published around the event window or state-level legislation with an effective date during the event window (we found none). We also checked multiple media sources (Google News, "On This Day in History," etc.) for headline news or other events likely to impact the stock market generally and California firms specifically and found none.

TABLE C-1 Description of firms removed from sample for confounding events or other issues

Reason for Removal	# of firms removed from sample	# of firms remaining in sample	Explanation
Initial Sample		1,312	Unique firms with at least one annual report data in COMPUSTAT in the 2 calendar years prior to the event date (8/7/2008)
Insufficient trading data in CRSP	538	774	Sufficient trading data defined as at least 3 observations (trades) during the estimation window (-255, -5) (see Online Appendix Section B for details)
Stock ticker changes	2	772	These firms were unable to be matched with COMPUSTAT data due to historic stock ticker changes
Mutual fund: iShares	138	634	Mutual funds are not firms
Mutual fund: SIC 6722 or 6726	26	608	Mutual funds are not firms
Confounding event: Earnings announcement	110	498	We removed firms reporting quarterly earnings data during the window (0,+3), and a day before/after due to potential information leakage; because our event date is in early August, it immediately followed the close of the second quarter for firms with calendar-year fiscal years, which resulted in removal of many firms.
Confounding event: Analyst recommendation	45	453	We removed any stock for whom an analyst made a buy/sell/hold recommendation during the (0, +3) window
Confounding event: Stock event	9	444	We removed firms with dividend announcements (none), announced stock splits (1), dividend payments (none), or for whom a record date[1] (8) occurred during the (0, +3) window
Confounding event: Material K-8 filing	23	421	47 firms in the sample filed an 8-K during a (0, +4) window; we manually reviewed each and identified 23 firms that experienced material events (merger announcements, etc.)
Stock ticker duplicate	2	419	Two firms had the same stock ticker; we removed both from the sample.
Possible outliers	4	415	We checked for outliers in CAR values, using as a definition of outlier values that were ±3 standard deviations from the mean; finding four such potential values. We investigated all SEC filings for these firms occurring during or just prior to the event window, and despite not finding any new information, decided to remove these observations out of an abundance of caution.[2]

[1] "Record date" is the date on which the stockholder must be registered as holder of record on the stock transfer records of the company in order to receive a particular distribution directly from the company.

[2] Before removal of these values, mean car for n = 419 is 0.0253532 with a 95% confidence interval of 0.0198046 to 0.0309017; after removal of the 4 possible outliers, mean car for n = 415 is 0.0252612 with a 95% confidence interval of 0.020157 to 0.0303654. Note that neither confidence interval includes 0.

SECTION D Knowledge worker variable calculation

Younge, Tong, and Fleming (2015) maintain that any SOC code lower than 50-000 represents “knowledge workers” (KW). Initially we used this cutoff to construct the percentage of knowledge worker variable for each four-digit NACIS code (industry) by dividing the sum of all KWs (*i.e.,* those employees with SOC codes lower than 50-000) in a particular four-digit NAICS code by the total number of workers employed in that NAICS code nationwide using the BLS May 2008 data. However, further analysis suggested potential issues with this measure, which we describe in detail here.

First, the industry-level percent of knowledge workers variable had a negative correlation of -0.1396 with a high-tech dummy variable from Hecker (2005) (see footnote 4 in the main manuscript and Table D-2 below), but intuitively high technology firms should employ a larger percentage of knowledge workers than firms in other industries – we would therefore expect a positive correlation. Second, industries that are not high technology had higher values of KW percentage than some industries identified as high-tech, another intuitive mis-match. For example, as shown in Table D-2 below, NAICS industry code 2361 (“Residential Building Construction”) had a knowledge worker percentage of 98 percent, a higher value than either the pharmaceutical industry or semiconductor manufacturing. The high knowledge worker percentage for residential building construction resulted from the large number of workers engaged in “construction and extraction occupations” (SOC code 47-0000), which according to the cutoff suggested by Younge and colleagues, classifies such employees as knowledge workers.

Due to these concerns, we examined the SOC classification system from the year 2000 (Bureau of Labor Statistics, 2010), which was the system in place for our 2008 data. Reviewing these categories, there is a clear divide in training or education occurring after code 31-000 (there is no code 32-000); occupations listed at or below this code include managers, scientists, engineers, etc., while codes from 33-000 through 50-000 (which Younge and colleagues include in their knowledge worker definition) are more support and services related. For instance, 37-000 occupations include building and grounds cleaning. Since categories at or over 33-000 are not what would generally be considered to be knowledge intensive (that is, they do not primarily involve “the creation, distribution, or application of knowledge” (Davenport, 2005, p.10)), we propose 31-000 be the last SOC code classified as “knowledge workers” We do admit that this categorization of KW may miss some potentially knowledge intensive occupations, such as sales, which may require extensive on the job training and experience. However, this would imply that any percentage found using our revised classification would be an underestimate of the role of knowledge workers, and it is our opinion that a conservative estimate of the percentage of KWs in an industry is preferable to an inflated one. Moreover, this definition of knowledge worker is consistent with the prior literature on knowledge workers, as discussed below. Table D-1 below shows the occupations included in our classification of knowledge workers compared to the occupations included by Young et al. (2015)’s classification. It is evident that most of these workers included in our classification are required to obtain some type of higher education (such as a Master’s degree or other certification) or have extensive experience (such as managers), which is not the case for all occupations included in Young et al. (2015)’s classification.

A literature search revealed one other paper, Cader (2008), that uses the Occupational Employment Statistics (OES) to calculate a “knowledge ratio” at the industry level. Cader, building on Beck (1992), identifies ten SOC codes as representing knowledge-based workers: (1) Management Occupations; (2) Business and Financial Operations Occupations; (3) Computer and Mathematical Occupations; (4) Architecture and Engineering Occupations; (5) Legal Occupations; (6) Arts, Design, Entertainment, Sports, and Media Occupations; (7) Healthcare Practitioners and Technical Occupations; (8) Life, Physical, and Social Science Occupations; (9) Education, Training, and Library Occupations; and (10) Healthcare Support Occupations. Cader's (2008) measure matches our proposed classification except we also include "community and social service occupations," which includes occupations such as counselors, social workers, and religious officials which are, we acknowledge, rather rare at the for profit firm-level.

In Table D-2 below we compare the percent of knowledge workers in the total industry workforce for all industries included in our sample using Young et al, (2015)'s classification and our classification, and compared with the Hecker (2005) dummy variable for high technology industries. This table shows that for many industries the percentage of knowledge workers calculated using our cutoff (occupations with OES codes below 33-000) is much more intuitive than the percentage calculated using Young et al.'s cutoff (occupations with OES codes below 50-000). Thus, we use the revised classification in our study.

TABLE D-1 Different "Knowledge Worker" classifications using major SOC codes from the 2000 SOC Codelist

<table>
<tr><th>SOC Major Code & Description</th><th></th><th></th></tr>
<tr><td>11-0000 Management Occupations</td><td rowspan="20">Younge, Tong, & Fleming (2015) "knowledge worker" occupations</td><td rowspan="11">Revised classification of "knowledge worker" occupations used in this study</td></tr>
<tr><td>13-0000 Business and Financial Operations</td></tr>
<tr><td>15-0000 Computer and Mathematical Occupations</td></tr>
<tr><td>17-0000 Architecture and Engineering Occupations</td></tr>
<tr><td>19-0000 Life, Physical, and Social Science Occupations</td></tr>
<tr><td>21-0000 Community and Social Services Occupations</td></tr>
<tr><td>23-0000 Legal Occupations</td></tr>
<tr><td>25-0000 Education, Training, and Library Occupations</td></tr>
<tr><td>27-0000 Arts, Design, Entertainment, Sports, and Media Occupations</td></tr>
<tr><td>29-0000 Healthcare Practitioners and Technical Occupations</td></tr>
<tr><td>31-0000 Healthcare Support Occupations</td></tr>
<tr><td>33-0000 Protective Service Occupations</td><td></td></tr>
<tr><td>35-0000 Food Preparation and Serving Related</td><td></td></tr>
<tr><td>37-0000 Building and Grounds Cleaning/Maintenance Occupations</td><td></td></tr>
<tr><td>39-0000 Personal Care and Service Occupations</td><td></td></tr>
<tr><td>41-0000 Sales and Related Occupations</td><td></td></tr>
<tr><td>43-0000 Office and Administrative Support Occupations</td><td></td></tr>
<tr><td>45-0000 Farming, Fishing, and Forestry Occupations</td><td></td></tr>
<tr><td>47-0000 Construction and Extraction Occupations</td><td></td></tr>
<tr><td>49-0000 Installation, Maintenance, and Repair Occupations</td><td></td></tr>
<tr><td>51-0000 Production Occupations</td><td></td><td></td></tr>
<tr><td>53-0000 Transportation and Material Moving Occupations</td><td></td><td></td></tr>
<tr><td>55-0000 Military Specific</td><td></td><td></td></tr>
</table>

TABLE D-2 Knowledge Workers calculations for industries included in our study using different Knowledge Workers classifications and compared with high technology industries

Using May 2008 OES Data from the BLS; sorted by hightech and then NAICS code.

Four Digit NAICS Code	NAICS Description	High Technology Industry Dummy (Hecker, 2005)	Knowledge worker percentage using Younge et al.'s (2015) 50-000 SOC cutoff	Knowledge worker percentage using the 31-000 SOC cutoff developed in this study
1113	Fruit and Tree Nut Farming*	0	0.840	0.029
2111	Oil and Gas Extraction	0	0.819	0.451
2211	Electric Power Generation, Transmission and Distribution	0	0.851	0.307
2213	Water, Sewage and Other Systems	0	0.643	0.114
2361	Residential Building Construction	0	0.985	0.138
2362	Nonresidential Building Construction	0	0.952	0.190
2373	Highway, Street, and Bridge Construction	0	0.868	0.090
3114	Fruit and Vegetable Preserving and Specialty Food Manufacturing	0	0.293	0.069
3116	Animal Slaughtering and Processing	0	0.174	0.034
3119	Other Food Manufacturing	0	0.357	0.095
3121	Beverage Manufacturing	0	0.474	0.098
3152	Cut and Sew Apparel Manufacturing	0	0.243	0.080
3162	Footwear Manufacturing	0	0.165	0.070
3222	Converted Paper Product Manufacturing	0	0.284	0.090
3231	Printing and Related Support Activities	0	0.387	0.117
3241	Petroleum and Coal Products Manufacturing	0	0.500	0.252
3256	Soap, Cleaning Compound, and Toilet Preparation Manufacturing	0	0.460	0.184
3259	Other Chemical Product and Preparation Manufacturing	0	0.420	0.188

Four Digit NAICS Code	NAICS Description	High Technology Industry Dummy (Hecker, 2005)	Knowledge worker percentage using Younge et al.'s (2015) 50-000 SOC cutoff	Knowledge worker percentage using the 31-000 SOC cutoff developed in this study
3273	Cement and Concrete Product Manufacturing	0	0.340	0.062
3313	Alumina and Aluminum Production and Processing	0	0.309	0.089
3325	Hardware Manufacturing	0	0.325	0.128
3332	Industrial Machinery Manufacturing	0	0.536	0.307
3333	Commercial and Service Industry Machinery Manufacturing	0	0.555	0.312
3339	Other General Purpose Machinery Manufacturing	0	0.435	0.209
3353	Electrical Equipment Manufacturing	0	0.391	0.216
3359	Other Electrical Equipment and Component Manufacturing	0	0.368	0.189
3363	Motor Vehicle Parts Manufacturing	0	0.301	0.148
3371	Household and Institutional Furniture and Kitchen Cabinet Manufacturing	0	0.254	0.068
3391	Medical Equipment and Supplies Manufacturing	0	0.399	0.207
3399	Other Miscellaneous Manufacturing	0	0.414	0.155
4231	Motor Vehicle and Motor Vehicle Parts and Supplies Merchant Wholesalers	0	0.697	0.092
4234	Professional and Commercial Equipment and Supplies Merchant Wholesalers	0	0.900	0.321
4235	Metal and Mineral (except Petroleum) Merchant Wholesalers	0	0.523	0.110
4236	Electrical and Electronic Goods Merchant Wholesalers	0	0.860	0.234
4242	Drugs and Druggists' Sundries Merchant Wholesalers	0	0.884	0.222

Four Digit NAICS Code	NAICS Description	High Technology Industry Dummy (Hecker, 2005)	Knowledge worker percentage using Younge et al.'s (2015) 50-000 SOC cutoff	Knowledge worker percentage using the 31-000 SOC cutoff developed in this study
4244	Grocery and Related Product Merchant Wholesalers	0	0.516	0.090
4412	Other Motor Vehicle Dealers	0	0.931	0.063
4422	Home Furnishings Stores	0	0.914	0.056
4451	Grocery Stores	0	0.832	0.045
4481	Clothing Stores	0	0.971	0.027
4511	Sporting Goods, Hobby, and Musical Instrument Stores	0	0.969	0.048
4529	Other General Merchandise Stores	0	0.865	0.044
4541	Electronic Shopping and Mail-Order Houses	0	0.881	0.216
5111	Newspaper, Periodical, Book, and Directory Publishers	0	0.819	0.415
5121	Motion Picture and Video Industries	0	0.635	0.147
5151	Radio and Television Broadcasting	0	0.996	0.706
5152	Cable and Other Subscription Programming	0	0.996	0.396
5171	Wired Telecommunications Carriers	0	0.995	0.296
5179	Other Telecommunications	0	0.998	0.339
5191	Other Information Services	0	0.987	0.611
5221	Depository Credit Intermediation	0	0.947	0.290
5222	Nondepository Credit Intermediation	0	0.998	0.408
5231	Securities and Commodity Contracts Intermediation and Brokerage	0	0.790	0.185
5239	Other Financial Investment Activities	0	0.997	0.574
5241	Insurance Carriers	0	0.998	0.512
5242	Agencies, Brokerages, and Other Insurance Related Activities	0	0.999	0.248
5259	Other Investment Pools and Funds	0	0.985	0.538

Four Digit NAICS Code	NAICS Description	High Technology Industry Dummy (Hecker, 2005)	Knowledge worker percentage using Younge et al.'s (2015) 50-000 SOC cutoff	Knowledge worker percentage using the 31-000 SOC cutoff developed in this study
5311	Lessors of Real Estate	0	0.980	0.165
5312	Offices of Real Estate Agents and Brokers	0	0.995	0.147
5322	Consumer Goods Rental	0	0.871	0.062
5324	Commercial and Industrial Machinery and Equipment Rental and Leasing	0	0.793	0.147
5331	Lessors of Nonfinancial Intangible Assets (except Copyrighted Works)	0	0.988	0.482
5412	Accounting, Tax Preparation, Bookkeeping, and Payroll Services	0	0.991	0.547
5416	Management, Scientific, and Technical Consulting Services	0	0.968	0.621
5419	Other Professional, Scientific, and Technical Services	0	0.972	0.561
5613	Employment Services	0	0.614	0.200
5614	Business Support Services	0	0.965	0.140
5615	Travel Arrangement and Reservation Services	0	0.975	0.123
6215	Medical and Diagnostic Laboratories	0	0.994	0.664
6219	Other Ambulatory Health Care Services	0	0.930	0.732
6233	Community Care Facilities for the Elderly	0	0.983	0.588
7211	Traveler Accommodation	0	0.966	0.060
7221	Full-Service Restaurants	0	0.992	0.023
7225	Restaurants and Other Eating Places**	0	0.992	0.023
9999	Nonclassifiable Establishments***	0	0.940	0.403
3254	Pharmaceutical and Medicine Manufacturing	1	0.684	0.495
3341	Computer and Peripheral Equipment Manufacturing	1	0.860	0.704

Four Digit NAICS Code	NAICS Description	High Technology Industry Dummy (Hecker, 2005)	Knowledge worker percentage using Younge et al.'s (2015) 50-000 SOC cutoff	Knowledge worker percentage using the 31-000 SOC cutoff developed in this study
3342	Communications Equipment Manufacturing	1	0.736	0.534
3344	Semiconductor and Other Electronic Component Manufacturing	1	0.549	0.415
3345	Navigational, Measuring, Electromedical, and Control Instruments Manufacturing	1	0.713	0.531
3364	Aerospace Product and Parts Manufacturing	1	0.649	0.456
5112	Software Publishers	1	0.993	0.792
5182	Data Processing, Hosting, and Related Services	1	0.981	0.576
5413	Architectural, Engineering, and Related Services	1	0.969	0.776
5415	Computer Systems Design and Related Services	1	0.995	0.791
5417	Scientific Research and Development Services	1	0.976	0.806

* OES data not available at the 4-digit industry or 3-digit subsector codes, so was proxied by 2-digit sector (11).
** OES data not available at the 4-digit industry code, so was proxied by four-digit code 7221 due to similarity.
*** OES data not available at the 4-digit industry, so was proxied by 3-digit subsector (999).

ADDITIONAL REFERENCES IN THE ONLINE APPENDIX

In addition to the references listed in the main body of paper, we reference the following sources in this online appendix only.

Beck, N. (1992). Shifting Gears: Thriving in the New Economy. Toronto: Harper Ed.

Cader, H. A. (2008). The evolution of the knowledge economy. J*ournal of Regional Analysis and Policy, 38*(2): 117-129.

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