How Network Hiring by Entrepreneurs Shapes Firm Formation and Performance

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November 11, 2025

Abstract

Many entrepreneurs rely on their personal networks to hire their first employees. How important is this practice for the formation and performance of new firms? I study this question using Norwegian administrative data that allow me to link entrepreneurs to their firms, employees, and former coworkers. To identify causal effects, I develop an instrumental variables framework that jointly models entry and network hiring, allowing for endogenous selection on both margins. The results reveal three main findings. First, each ex-coworker hired in the firm's first year raises annual revenues in the following four years by over $250K and crowds in other hires, without reducing average productivity. Second, without the ability to hire ex-coworkers, a quarter of network-hiring entrepreneurs would not have started their firms at all. Third, counterfactual simulations show that, compared to entry subsidies, networks enable entry of entrepreneurs who create substantially more jobs, survive longer, and achieve higher value added per worker. Interpreted through the lens of a simple model, the data suggest that private information about coworker quality is a key driver of network hiring. Taken together, the results show that access to human capital through networks is an important determinant of entrepreneurial entry and success.

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*Department of Economics, Stanford University. I am grateful to my advisors Alessandra Voena, Luigi Pistaferri, and Isaac Sorkin for feedback and encouragement. I am also grateful to Nick Bloom, Sydnee Caldwell, Mihai Codreanu, Caroline Hoxby, Lars Lefgren, Neale Mahoney, Magne Mogstad, Paul Oyer, Elena Pastorino, Alex Torgovitsky, Merrill Warnick, and seminar participants at Stanford and Brigham Young University for thoughtful discussions and comments. I thank Andreas Fagareng for expert assistance with the administrative data. This material is based upon work supported by the National Science Foundation Graduate Research Fellowship under grant DGE-1656518 and the Dixon and Carol Doll Graduate Fellowship through a grant to the Stanford Institute for Economic Policy Research. I also acknowledge support from the European Research Council under the European Union's Horizon 2020 research and innovation program under grant agreement No. 851891, hosted by Statistics Norway (ref. 21/5634).

1 Introduction

Entrepreneurs rely heavily on their personal networks to recruit their first employees. Recent evidence from linked employer-employee data shows that over a third of entrepreneurs with employees hire their former coworkers, who together account for one in ten hires at new firms (Muendler et al., 2024; Rocha and Brymer, 2025). This pattern reflects a broader tendency of entrepreneurs to build their early teams through social ties—friends, relatives, and other acquaintances—rather than hiring from the open labor market (e.g., Aldrich et al., 2002; Kelley et al., 2019; Kerr and Kerr, 2021).

Despite this prevalence, we know little about how essential network hiring is for the formation and performance of new firms. If networks merely provide a convenient way to recruit employees, then they may change how firms hire but not which firms start or grow. But if network hiring arises because new firms face severe hiring frictions—that is, if many entrepreneurs would otherwise be unable to find or recruit suitable workers—then networks are a key determinant of firm entry and success. Such frictions may be especially acute for new ventures, which lack both the capacity to efficiently screen candidates[1] and the reputation to attract talented workers.[2] As a result, new firms may struggle to identify and evaluate job candidates precisely when a single employee can have an outsized impact on firm performance (Choi et al., 2023). These frictions may matter beyond the firm level: because young firms are key drivers of job creation and business dynamism (e.g., Haltiwanger et al., 2013, 2016; Akcigit and Ates, 2023), understanding how networks shape entrepreneurial activity helps illuminate the micro-level forces underlying economic growth.

This paper is the first to quantify how critical network hiring is for the performance and very existence of new firms, focusing on a common type of network hire: entrepreneurs' former coworkers. To estimate these effects, I develop a choice framework that separates the effects of network hiring on firm performance from its influence on who becomes an entrepreneur. Applying this framework to rich administrative data from Norway that link entrepreneurs to their firms, employees, and ex-coworkers, I show that these hires are decisive: without them, entrepreneurs would often hire no early-stage employees, and in many cases would not start a firm at all. Network hiring

[1] In their comprehensive study of organizational growth, Aldrich and Ruef (2006, p.97) summarize recruiting at new firms as follows: "Given their lack of HRM [human resource management] personnel, many startups fall back on simple assessments of *cultural fit*, often relying on socio-demographic homophily."

[2] Studying science-based startups, Bryan et al. (2025) find that workers face incomplete information about new firm quality. Because failure rates at new firms are high (Haltiwanger et al., 2013; Pugsley and Şahin, 2018) and often lead to long-run wage penalties for their initial employees (Sorenson et al., 2021), skilled workers may rationally prefer established firms of known reputation over new firms of unknown quality.

facilitates entry by productive firms, substantially increases firm revenues, and crowds in other hires. By demonstrating the key role network hiring plays at new firms, these results highlight the importance of human capital for entrepreneurial entry and success.

I begin with a stylized model of entrepreneurial entry and hiring choice that illustrates how networks can influence firm formation. To fix ideas, the model emphasizes the potential role of asymmetric information: entrepreneurs have less information than incumbent employers, leading them to face an adversely selected labor market. The ability to recruit a known coworker sidesteps this adverse selection, enabling entrepreneurs to enter and hire even when coworker productivity is modest. The model highlights how network hiring operates on two margins: it can affect firm outcomes directly, and it can endogenously change the composition of entrants by enabling some individuals to start firms who otherwise would not.

To assess how network hiring affects entrepreneurs in practice, I use administrative data from Norway covering all corporations founded between 2001 and 2018. By linking entrepreneurs to their employees and their work histories, I can track whether entrepreneurs recruit coworkers from their previous jobs. I define an *early-stage network hire* as an ex-coworker who joins the entrepreneur's firm within a year of incorporation. Empirically, such network hiring is widespread. Over a third of new firms with employees recruit entrepreneurs' ex-coworkers, and these hires make up nearly fifteen percent of aggregate employment at new firms.[3]

Descriptive correlations show that network-hiring entrepreneurs recruit more experienced workers and build significantly higher-performing firms. Ex-coworker hires have, on average, about five more years of experience than other hires and earned nearly twice as much in their prior jobs. Entrepreneurs who hire ex-coworkers have firms that go on to achieve revenues and employment roughly twice as large as those of firms that hire from other sources. They generate substantially more value added per worker and are more likely to survive, with these advantages persisting well beyond the startup phase.

These correlations may be misleading if entrepreneurs who hire from their networks also differ in other ways that affect firm performance. For example, entrepreneurs who are skilled at recognizing arbitrage opportunities in the product market—as in Schumpeter's (1934; 1942) classical theories of entrepreneurship—may also be skilled at recognizing and recruiting underutilized members of their networks. As a result, simple correlations would overstate the true effects of network hiring. But the opposite is also possible: less productive entrepreneurs may depend more heavily on network ties

[3]These patterns are strikingly similar to those that have been documented in Denmark (Rocha and Brymer, 2025) and Brazil (Muendler et al., 2024).

because they struggle to attract outside talent, in which case simple correlations would understate the true effect. Moreover, if the ability to hire network members affects who becomes an entrepreneur in the first place, then these correlations confound not only differences in firm performance, but also differences in the composition of entrants.

To allow for endogenous selection into both entrepreneurship and network hiring, I develop a partially ordered choice framework that separates the causal effects of hiring ex-coworkers on firm performance from its effects on the composition of entrants. The framework models the joint decision by embedding a standard ordered choice model (for hiring ex-coworkers) within a binary choice model (for starting a firm). It introduces two latent variables, one governing the entry decision and another governing the network hiring decision, allowing for heterogeneous preferences and outcomes along both margins. In particular, average outcomes and treatment effects can vary not only with entrepreneurs' propensities to hire from their networks, but also between marginal entrants (who start firms only because of network hiring) and inframarginal entrants (who would enter regardless). I demonstrate how the underlying parameters can be identified using separate instrumental variables for each choice. This structure makes it possible to disentangle the effects of network hiring on firm outcomes from its effect on the composition of entrants.

This framework offers a general approach for identifying the effects of an ordered treatment that is tied to an extensive-margin participation decision, which I call a partially ordered choice. The key insight is that participation (in, e.g., a market or a program) and treatment intensity are jointly determined but conceptually distinct decisions: the treatment itself is only observed for those who choose to participate, yet the potential benefit of treatment can itself influence the participation decision. As a result, the marginal participants induced by the treatment may differ systematically from inframarginal participants in both their baseline outcomes and their treatment effects. Such structure arises naturally in many settings. For example, in education, individuals decide whether to attend college and, if so, what quality of institution to attend; in labor supply, they decide whether to work and, if so, whether to work part- or full-time. In each case, participation and treatment intensity are distinct but linked choices. Failing to account for that link can bias treatment effect estimates and obscure the behavior of the marginal participants, who may be the most policy-relevant group.

Identification in my setting relies on two distinct instruments: one for entry and one for network hiring. For entry, I exploit a 2012 policy reform that reduced the minimum amount of capital needed to start a corporation, leading to a sharp increase in firm entry. I show that this reform did not appear to change investment decisions or outcomes for existing young firms, supporting the necessary assumption that it affected entry without affecting post-entry optimization. For network hiring, I leverage idiosyncratic shocks to the employers of entrepreneurs' former coworkers, which generates variation in their willingness to join new firms. I provide evidence that these shocks appear unrelated to competitive interactions between entrants and incumbents in the product market, suggesting instead that they capture exogenous shifts in the labor supply curve facing the entrepreneur.

These two sources of variation affect entrepreneurs' choices in distinct but complementary ways. The network shock alters the effective costs of hiring ex-coworkers, leading more entrepreneurs to hire from their networks and inducing entry among those for whom network hiring makes entrepreneurship attractive. The resulting reduced form relationship between the network shock and firm outcomes therefore conflates changes in who becomes an entrepreneur with the causal effects of network hiring on firm performance. To separate these margins, the framework leverages the entry instrument: after the 2012 reform, new firms tended to have worse outcomes, revealing that marginal entrants are smaller and less productive than inframarginal ones. Together, these patterns provide the foundation for the identification strategy: network shocks reveal how easier access to ex-coworkers jointly shapes entry and performance, while the entry instrument makes it possible to separate the resulting changes in the composition of entrepreneurs from the causal effects of network hiring on firm performance.

Combining these instruments with the choice framework, I find that the ability to hire ex-coworkers is often decisive for firm entry and early-stage hiring. About one-quarter of entrepreneurs who hired ex-coworkers would not have started a firm at all without that option. Another quarter would still enter, but delay or forgo hiring employees. Overall, fewer than half of network-hiring entrepreneurs would both enter and recruit employees to the new firm if it were not possible to hire ex-coworkers.

Beyond facilitating entry, network hiring enables entrepreneurs to build larger firms without sacrificing productivity, suggesting that networks help new firms expand efficiently. Each early-stage ex-coworker hire raises annual revenues by an average of roughly US$270K over the subsequent four years. Ex-coworker hires also increase other hires at a rate close to one-for-one, suggesting that ex-coworkers crowd in rather than crowd out other hiring channels. By contrast, I find no evidence that ex-coworker hires affect average productivity, as measured by value added per worker or firm survival.

Selection is central to interpreting these effects: more productive entrepreneurs are more likely to enter, and entrepreneurs who benefit most from networks are more likely to rely on them. Given this positive, Roy (1951)-style selection, the marginal entrants induced by the ability to hire ex-coworkers are naturally weaker than the inframarginal entrants who would prefer to hire ex-coworkers, but would have entered regardless. Yet, despite having lower baseline productivity, these marginal entrants experience especially large gains from hiring ex-coworkers. Network hiring thus plays two roles: it expands the scale of inframarginal entrepreneurs, while also enabling entry and improving performance of marginal ones who would otherwise not start firms.

Compared to the marginal entrants that would be induced by broad entry subsidies, however, network hiring induces a much more productive group of entrepreneurs to start firms. To make this comparison, I first compute the targeted subsidy that would induce the network-hiring marginal entrants to start their firms without their networks. I then use the implied fiscal cost of this (infeasible) targeted subsidy to simulate an untargeted, flat subsidy for all potential entrants. The results show that the subsidy-induced entrants would generate 50% lower value added per worker and survive at rates 10 percentage points lower than the network-induced entrants would *without their network hires*. This highlights that the marginal entrants facilitated by networks are a particularly high-potential group of entrepreneurs.

The evidence is most consistent with entrepreneurs hiring their ex-coworkers for reasons related to worker quality, rather than liquidity constraints or favoritism. If financial constraints were the key motive, we would expect network hires to accept wage discounts to help cash-constrained entrepreneurs. Yet, comparing the hired ex-coworkers of lower-wealth entrepreneurs to their other former coworkers reveals no wage declines around the job transition, inconsistent with a liquidity motive. Extending the stylized model, I show that favoritism would predict the opposite pattern of what I observe: entrepreneurs who are most likely to hire from their networks would experience the largest losses (Becker, 1971; Goldberg, 1982). Instead, I observe that the entrepreneurs most likely to hire from their networks experience the greatest gains. Together, these results point toward quality-based explanations for network hiring.[4]

Among reasons related to worker quality, I find empirical support for the view that private information about network members drives entrepreneurs to hire from their networks. The stylized model illustrates that if entrepreneurs have private information about their coworkers, they should rely more on their networks precisely when worker productivity is most uncertain in the broader labor market. To test this, I use occupational data to construct an index that reflects how strongly ability translates into productivity across jobs, since steeper ability-to-productivity gradients imply greater uncertainty about worker performance. Consistent with the theory, when per-

[4]This contrasts with literature on hiring relatives, which are a different type of personal connection. Focusing on mature firms, this work typically finds that such practices lower firm performance, consistent with favoritism rather than quality-based motives (e.g., Smith and Amoako-Adu, 1999; Bennedsen et al., 2007; Bertrand et al., 2008).

formance depends more steeply on individual ability, entrepreneurs hire fewer workers overall and rely more heavily on ex-coworkers. This suggests that network hiring helps entrepreneurs overcome information frictions in an adversely selected labor market.

Collectively, these results show that network hiring plays a key role in overcoming frictions that constrain firm entry and growth. They are consistent with an environment in which network hiring facilitates entry, enhances firm performance, and spurs additional job creation. The findings suggest that entrepreneurs face severe hiring frictions that their networks help alleviate, a parallel to the role of liquidity constraints often emphasized in the entrepreneurship literature (e.g., Evans and Jovanovic, 1989; Hurst and Lusardi, 2004). Barriers to hiring from networks—such as non-compete agreements—may therefore deter entry and lower entrepreneurial performance, while policies that encourage worker mobility and improve labor market transparency may encourage entry of talented entrepreneurs. More broadly, because young firms are central to job creation and productivity growth (e.g., Haltiwanger et al., 2013, 2016; Akcigit and Ates, 2023), network hiring represents a micro-level mechanism contributing to the process of "creative destruction" that underpins aggregate economic growth.

This paper contributes to three literatures.

Entrepreneurial entry and success. This paper builds on work on the determinants of entrepreneurial entry and success by emphasizing the importance of hired human capital acquired through personal networks. A large body of work studies how liquidity constraints affect entrepreneurial entry and firm performance (e.g., Evans and Jovanovic, 1989; Holtz-Eakin et al., 1994; Dunn and Holtz-Eakin, 2000; Hurst and Lusardi, 2004; Hvide and Møen, 2010; Guo and Wallskog, 2024). While this literature examines the importance of access to financial capital, much less attention has been given to the role of human capital (i.e., the ability to recruit capable employees). Recent work using employer-employee data connects new firm performance to network hiring, showing that entrepreneurs who recruit former coworkers or classmates tend to run more successful firms (Rocha and Brymer, 2025). I build on this descriptive evidence by directly addressing endogeneity in both the entry and hiring decisions, providing causal estimates of how network hiring shapes firm formation, composition, and performance.

These results may also help explain why entrepreneurs are more likely to locate and succeed in their home regions (Michelacci and Silva, 2007; Dahl and Sorenson, 2012), where personal networks are denser. They also connect to evidence that exposure to former entrepreneurs in the workplace increases an individual's own likelihood of starting a firm (Nanda and Sørensen, 2010; Wallskog, 2025) by showing that coworkers may not only inspire entrepreneurship, but also provide a recruiting source that makes firm formation possible.

Referral-based hiring. This paper complements research on referral-based hiring at established firms by highlighting how network hiring impacts new firms. Prior work has shown that workers hired through referrals exhibit higher wages and lower turnover (Simon and Warner, 1992; Burks et al., 2015; Dustmann et al., 2016; Pallais and Sands, 2016), which is consistent with these matches being of higher quality.[5] This work examines mature organizations with formal recruiting processes and staff, and it generally does not observe the impact of referral-hiring on firm performance itself.[6] By contrast, this paper establishes a causal link between the "self-referrals" of firm founders in the firm's early stages—when one might expect talented hires to be most impactful—and the performance of their firms.[7] Related work shows that even mature firms sometimes acquire other firms primarily to access their human capital, rather than their products or assets (e.g., Chatterji and Patro, 2014; Younge et al., 2015; Ouimet and Zarutskie, 2020). This highlights the importance—and the difficulty—of identifying and securing capable workers, even at mature firms.

The findings of this paper also provide empirical context for theoretical work on referral- and network-based hiring, which examines the efficiency and equity implications of such practices. Chandrasekhar et al. (2020) show that entrepreneurs may hire inefficiently few workers if non-network hires expect similar pay as network hires. Other models highlight how network hiring can lead to less efficient worker-firm matches and perpetuate inequality across workers (Calvó-Armengol and Jackson, 2004; Bolte et al., 2020). Such effects may be particularly detrimental for minority workers if entrepreneurs exhibit homophily in their hiring behavior, as has been documented in Brazil by Miller and Schmutte (2021). By contrast, my findings show how network hiring at new firms often enables entry and spurs further job creation, rather than displacing other potential hires.

Marginal treatment effects. Finally, this paper makes a methodological contribution by extending the marginal treatment effects (MTE) framework to a setting in which a binary participation decision is jointly determined with an ordered treatment. The underlying partially ordered choice framework accommodates unob-

[5]That wages and turnover are sufficient statistics for match quality follows from the theoretical models of Jovanovic (1979, 1984), where each worker is paid what the firm infers their marginal product to be.

[6]One exception is Burks et al. (2015), who find that referred workers generate greater profits at seven call center firms and one trucking firm. Another is Black and Hasan (2020), who infer network hiring as occurring when a firm repeatedly hires employees away from another firm and find that this practice is positively correlated with firm revenues but negatively correlated with firm-level employment growth.

[7]Using administrative data on new U.S. firms, Choi et al. (2023) find that the loss of an early-stage employee (via premature death) has large and persistent negative effects on firm performance, underscoring the impact these employees can have.

served heterogeneity on two margins of indifference: the participation margin and the treatment-intensity margin. This builds on the original MTE framework developed for a binary treatment without an explicit participation decision (Björklund and Moffitt, 1987; Heckman and Vytlacil, 1999, 2005). Because participation and treatment-intensity are conceptually distinct choices, my choice framework relates to extensions of selection models and the MTE approach to both unordered treatments (e.g., Kline and Walters, 2016; Heckman and Pinto, 2018; Walters, 2018; Mountjoy, 2022; Humphries et al., 2025) and ordered treatments (e.g., Rose and Shem-Tov, 2021; Kamat et al., 2024), blending elements of both. It further relates to work that demonstrates how to bound average treatment effects under sample selection using minimal assumptions (Lee, 2009; Chen and Flores, 2015; Bartalotti et al., 2023). My framework places additional structure on the joint decision process to point identify outcomes and treatment effects of the marginal participants, who are often of primary policy relevance.

The remainder of the paper proceeds as follows. Section 2 introduces a stylized model of entrepreneurship and network hiring. Section 3 describes the linked data, and Section 4 summarizes the descriptive relationship between network hiring and new firm performance. Section 5 develops the econometric framework. Section 6 presents the empirical results, and Section 7 uses these results to conduct counterfactual simulations. Section 8 extends the stylized model to explore the underlying drivers of network hiring. Section 9 concludes.

2 Model of Entrepreneurial Entry and Hiring Choice

To illustrate how networks can influence firm formation, I develop a simple model of entrepreneurial entry and hiring choice. The model highlights one potential mechanism: entrepreneurs may know less about worker quality than incumbent employers, generating adverse selection in the labor market that discourages hiring and entry. Recruiting a known coworker mitigates this adverse selection, leading many entrepreneurs hire from their networks and enabling some to start firms who otherwise would not.

Focusing on private information is useful for building intuition, but it is only one reason why entrepreneurs may hire through their networks. In Section 8, I extend the model to incorporate other motivations, generating testable predictions. And while the precise channel matters for interpreting my empirical results, all share a common implication: networks can facilitate entry and hiring that might not occur otherwise.

2.1 Setup

The model is static. At the start of the period, an individual i must decide whether to remain a wage worker or become an entrepreneur. An entrepreneur may hire at most one employee, for whom she must compete with incumbent firms. Incumbents, which are all homogeneous, observe the incumbent marginal products θ_i for all individuals.[8] These values are normally distributed with mean $\bar{\theta} > b \geq 0$ and variance normalized to 1, where b is the outside option (unemployment). Incumbents engage in Bertrand competition for workers, offering each worker their marginal product in equilibrium.

The individual chooses entrepreneurship if her expected net income is highest there. Otherwise, she chooses wage work and earns θ_i.[9] If the entrepreneur ran her firm alone, it would generate earnings α_i. If she hired worker j, the firm would generate additional earnings β_{ij}, which reflects the worker's marginal product (or "match value") at i's firm. Match values are distributed with mean $\bar{\beta}_i = \mathbb{E}[\beta_{ij}]$ and variance σ_i^2. The entrepreneur receives as net income the earnings her firm generates, minus any wage costs $w \geq 0$ and entry costs $c \geq 0$.

The marginal products at new and incumbent firms are correlated. Specifically, β_{ij} and θ_j are jointly normal with correlation $\rho_i > 0$.[10] This correlation is larger when entrepreneurs benefit from the same skills as incumbents. At the time of hiring, the entrepreneur knows only the distributions of β_{ij} and θ_j, much as in employer-learning models where the true ability of hires is unknown ex ante (Jovanovic, 1979). However, she has at most one network member k (a "coworker") for whom she knows with certainty the value of θ_k.[11]

2.2 Hiring under asymmetric information

The entrepreneur, who can hire at most one employee, chooses between three options: hire her coworker; post a wage w in the broader labor market to recruit a non-network worker; or hire no one. I discuss the main results and their intuition here and provide detailed derivations in Appendix C.

An entrepreneur who posts a wage w will attract applicants who would earn no

[8] This is a natural assumption if incumbents include workers' prior employers. Moreover, allowing incumbents to instead observe an unbiased but noisy signal of θ_i would leave the results intact. The key distinction is that incumbents have the resources necessary to learn about worker ability, while entrepreneurs do not.

[9] For simplicity, I assume $\theta_i > b$ so that the potential entrepreneur never chooses unemployment herself.

[10] Ruling out $\rho_i \leq 0$ aligns with Becker's (1964) concept of general human capital and is consistent with empirical evidence finding that labor market skills transfer across firms (Poletaev and Robinson, 2008; Gathmann and Schönberg, 2010) as do worker "fixed effects" (Abowd et al., 1999; Card et al., 2013).

[11] The empirical literature on peer effects in the workplace shows that coworkers act as if they are aware of one another's output (Mas and Moretti, 2009; Bandiera et al., 2010).

more than w from incumbent firms. Job applicants are thus adversely selected on incumbent productivity. The severity of this adverse selection is amplified when the entrepreneur requires similar skills as the incumbents (high ρ_i) or when match values are highly variable, increasing the likelihood of a very poor match (high σ_i). If the entrepreneur hires a random applicant at the surplus-maximizing wage, her expected market-hiring surplus is $\bar{\beta}_i - b - f(\rho_i \sigma_i)$, where f and f' are both positive. Hiring from the market thus requires high expected match values $\bar{\beta}_i$, a lower productivity correlation ρ_i, and/or a lower dispersion in match values σ_i. Otherwise, entrepreneurs would rather hire no one than hire a worker of unknown quality.[12]

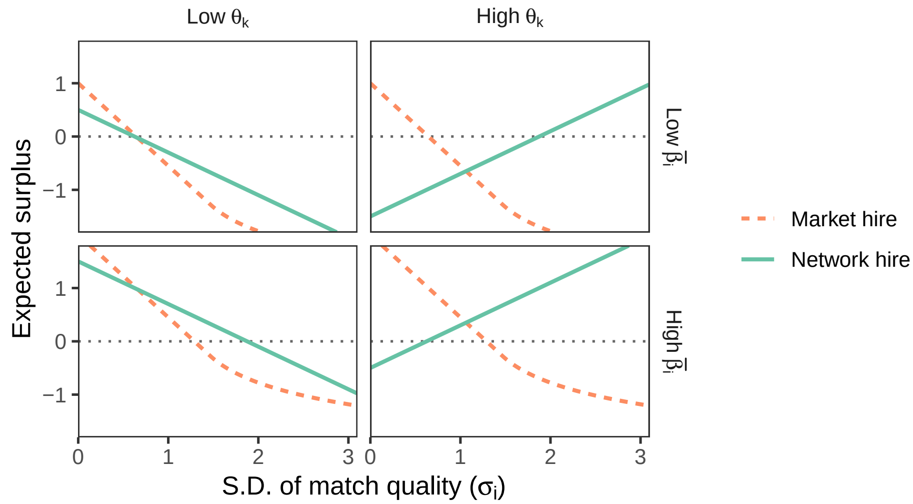
The entrepreneur's coworker demands a wage of at least θ_k, so the entrepreneur can maximize her network-hiring surplus by matching this wage. The expected match value $\mathbb{E}[\beta_{ik} \mid \theta_k]$ is increasing in the wage θ_k: skilled coworkers are more productive, but also more expensive. If $\rho_i \sigma_i > 1$, this surplus is increasing in θ_k: entrepreneurs prefer to hire high-ability coworkers despite the greater wages they demand. When $\rho_i \sigma_i < 1$, the surplus is decreasing in θ_k: wage costs dominate, so lower-ability coworkers are more profitable. Regardless of the distribution of match quality, the entrepreneur will be willing to hire a slightly (or severely) below-average coworker over an adversely selected candidate of unknown productivity. More formally, there is always a value $\theta_k < \bar{\theta}$ for which network hiring is preferred over market hiring.

Figure 1 illustrates how the network- and market-hiring surpluses vary with average match quality $\bar{\beta}_i$, coworker productivity θ_k, and the standard deviation of match quality σ_i. These plots underscore two points. First, when the entrepreneur hires a high productivity coworker, the second-best option—what the entrepreneur would have done had they been unable to hire that coworker—is generally not hiring at all, as opposed to hiring from the market. Second, network-hiring and market-hiring are generally only substitutes when σ_i is small, $\bar{\beta}_i$ is high, or both.[13]

[12] This reluctance to hire mirrors Greenwald's (1986) theoretical result that asymmetric information between firms about workers leads to adverse selection in the labor market and sharply restricts firm-to-firm (and in this case, incumbent-to-entrant) mobility.

[13] Empirically, the distribution of productivity across young firms is characterized by a low mean and a high variance (Haltiwanger et al., 2016). If the productivity distribution of potential workers *within* a new firm mirrors this productivity distribution *across* firms, most new firms will have low $\bar{\beta}_i$ and high σ_i, leading to a low expected surplus from market hiring.

Figure 1: Hiring surplus attainable by the entrepreneur



Notes: This figure illustrates the expected surplus the entrepreneur can obtain by hiring a coworker with incumbent marginal product θ_k or hiring a worker of unknown quality from the labor market. The y-axis reflects the expected net surplus of hiring each type, relative to not hiring. "High" ("low") θ_k places $\theta_k = \bar{\theta} \pm 1$, while "high" ("low") $\bar{\beta}_i = \bar{\theta} \pm 0.5$. The incumbent marginal products θ are normally distributed with mean $\bar{\theta} = 2$ and variance 1. The plot fixes the productivity correlation $\rho_i = 0.8$ and the worker outside option $b = 0.5$.

2.3 Selection into entrepreneurship

The would-be entrepreneur knows her own parameters $(\alpha_i, \bar{\beta}_i, \rho_i, \sigma_i)$, along with her coworker's θ_k, before she commits herself on whether to start a business.[14] Knowing these parameters, she will choose to start a business if and only if her expected net income from doing so exceeds that from wage work:

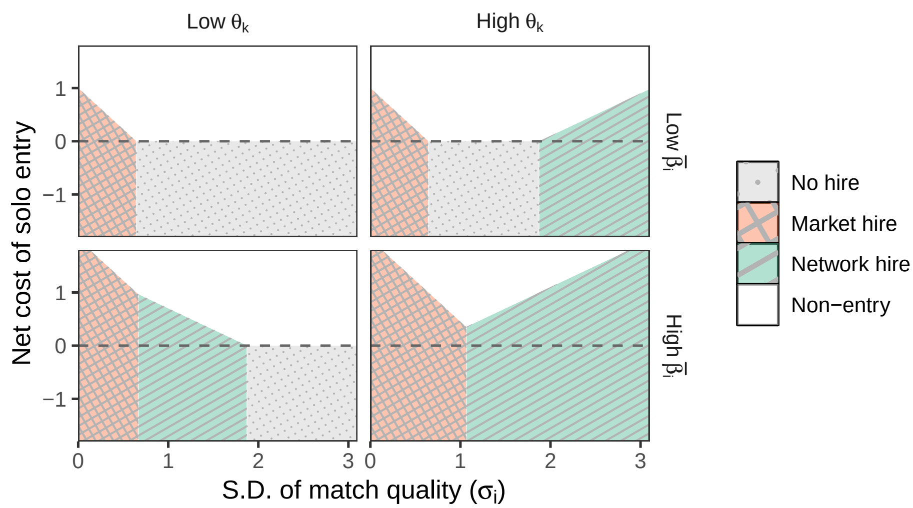
$$\alpha_i + S^*(\theta_k; \bar{\beta}_i, \rho_i\sigma_i) - c \geq \theta_i \iff S^*(\theta_k; \bar{\beta}_i, \rho_i\sigma_i) \geq \overbrace{\theta_i - \alpha_i + c}^{\text{net cost of solo entry}} \tag{1}$$

where S^* is the maximized net expected benefit of hiring (which is zero if the entrepreneur hires no one). Individuals who select into entrepreneurship have a higher net self-employment benefit (i.e., $\alpha_i - \theta_i$), a higher expected benefit of hiring, or both.

The reformulation of (1) makes explicit how hiring and entry decisions can be tightly linked: without a viable hire (sufficiently high S^*), some potential entrepreneurs will not enter at all. Illustrating this point, Figure 2 plots the entrepreneur's joint entry and hiring decisions as a function of her parameters and her coworker's productivity. When the net cost of solo entry is negative, reflected by the region below the dashed

[14] This is similar to the standard models of Lucas (1978) and Evans and Jovanovic (1989), in which potential entrepreneurs are assumed to know their own abilities before they make entry decisions.

Figure 2: Entry and hiring decisions



Notes: This figure plots the entry and hiring decisions an entrepreneur would make if they had the option to hire a coworker with incumbent marginal product θ_k. The y-axis reflects the net cost of solo entry from (1), given by $\theta_i - \alpha_i + c$. "High" ("low") θ_k places $\theta_k = \bar{\theta} \pm 1$, while "high" ("low") $\bar{\beta}_i = \bar{\theta} \pm 0.5$. The incumbent marginal products θ are normally distributed with mean $\bar{\theta} = 2$ and variance 1. The plot fixes the productivity correlation $\rho_i = 0.8$ and the worker outside option $b = 0.5$.

line, the firm is always started. By contrast, a set of marginal entrants will only start firms if it is possible to hire suitable workers. These individuals face a higher net cost of solo entry, either because their firms require additional labor to operate effectively (low α_i) or because their opportunity cost is substantial (high θ_i).

The stylized model abstracts away from other forces that could affect entrepreneurs' hiring choices, such as non-pecuniary benefits, relationship capital, and liquidity motives. The empirical framework introduced in Section 5 imposes no restrictions on the precise mechanism, and Section 8 extends the model to investigate these alternatives. Regardless of the underlying motivation, the key implications are unchanged: network hiring can shape which entrepreneurs start firms and create jobs.

3 Linked Data on Entrepreneurs, Networks, and Hires

To study how network hiring affects entrepreneurs' choices and outcomes empirically, this paper uses rich administrative data from Norway. Norway's exceptionally detailed records allow me to link entrepreneurs to their firms and to a key subset of their networks: former establishment-level coworkers. This integration is essential for identifying when entrepreneurs hire from their networks and examining how those hiring decisions shape firm formation and performance.

3.1 Institutional context

Norway's institutional context provides a useful backdrop for studying network hiring, given the combination of relatively low barriers to entrepreneurship and the meaningful commitments involved in hiring employees.

Entrepreneurship in Norway is similar to that in other industrialized countries. To start a limited liability company (*aksjeselskap*, or LLC), an individual must register their company with the Register of Business Enterprises (*Foretaksregisteret*). At the time of registration, the only substantive requirement is that the firm must have at least 30,000 NOK (approximately US$5,000 using the 2012 exchange rate) of paid-in share capital.[15] Norway consistently ranks among the easiest industrialized countries in which to start a firm, with rankings comparable to countries such as the United States and the United Kingdom (Djankov et al., 2002; World Bank, 2019).

Hiring employees subjects entrepreneurs to additional regulations. After registering as an employer with the State Register of Employers and Employees (*Aa-registeret*), the firm is required to set up an occupational pension scheme for its employees and to purchase compulsory workers-injury insurance. Employers are also responsible for social-security contributions, which are generally 14.1% of gross wages. Employers are mandated to provide sick pay (16 days a year) and holiday pay (4 weeks a year) to all employees.

In addition, it is difficult to dismiss employees in Norway. New employees face a probationary period of up to six months, during which the employer can deem them unsuitable for the job and dismiss them. After the probationary period, an employee may in general only be dismissed for explicit breach of contract or because of a necessary reduction in business size. In the latter case, employees have a right to be re-hired if the business expands again.[16]

These institutional features make the Norwegian context well-suited for studying network hiring, as similar regulatory frictions exist in many countries where hiring employees involves both financial and legal commitments. In such settings, entrepreneurs may rely more heavily on trusted, known hires from their existing networks.

3.2 Defining firms and entrepreneurs

The focus of this paper is on the establishment and performance of new firms. To ensure that the sample reflects entrepreneurs engaged in genuine economic activity

[15] Prior to 2012, this amount was 100,000 NOK (US$17,000 in 2012). The 2012 reform generated a sharp increase an entrepreneurial entry (Bacher et al., 2025), which is discussed in greater detail in Section 5.3.1.

[16] These are all clauses within Norway's Working Environment Act, *Arbeidsmiljøloven*, which was first introduced in 1977.

and whose firms have the formal structure needed to expand, I define entrepreneurs and firms using the following criteria.

I define an entrepreneur as a controlling owner of a newly established limited liability corporation (LLC), i.e., a firm founder. Following Bonney et al. (2025), I define a controlling owner as an individual who owns a plurality of outstanding shares, with an ownership stake of no less than 1/3.[17] By limiting to incorporated firms, I exclude sole proprietors, who are commonly viewed as being "self-employed" rather than entrepreneurs (Levine and Rubinstein, 2017). To avoid mistakenly counting sole-proprietorship to LLC conversions as new firms, I exclude firms for which the owners were sole proprietors in both of the two years prior to incorporation.[18]

I do not require firms to have employees. Instead, I treat early-stage employment as an endogenous decision made by the entrepreneur, and I analyze later-stage employment as an outcome. This allows for the possibility, emphasized theoretically in Section 2, that some entrepreneurs would not hire at all if they could not hire from their networks.

Some individuals may start LLCs whose sole purpose is to hold assets or facilitate tax planning, without any operational intent or employees (i.e., "shell" companies). To avoid referring to such instances as entrepreneurship, I restrict my attention to firms outside of the financial and real estate sectors. I similarly remove new firms that have ownership stakes in other firms but have no non-owner employees.

3.3 Linking entrepreneurs to their networks and firms

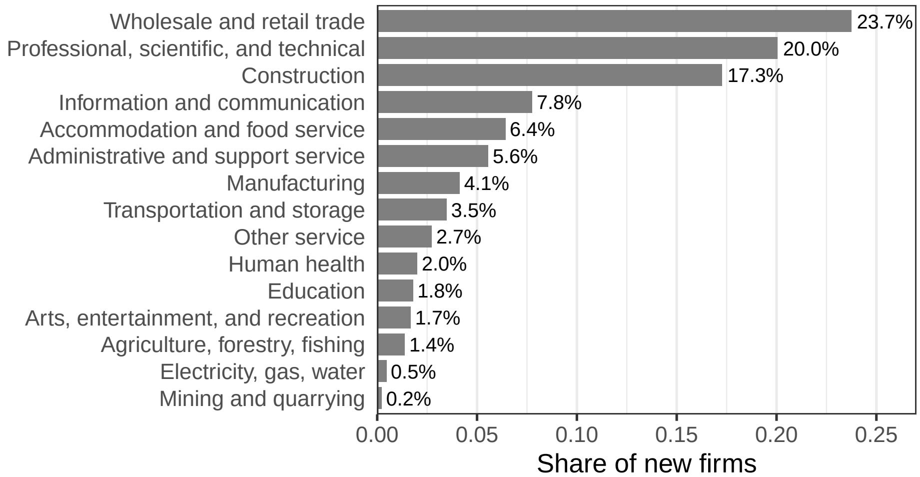
The analysis relies on a comprehensive dataset that links entrepreneurs, their firms, former coworkers, and employees. This section describes how I assemble this dataset using several Norwegian administrative registers.

I begin by identifying entrepreneurs and their firms using security-level information on ownership of shares in all LLCs present in the Norwegian Central Securities Depository (VPS) from 2001 to 2018. VPS contains information on both public and private (unlisted) firms, allowing me to identify ownership in the year a new firm was incorporated. I allow for ownership to be direct or via pass-through entities. Most entrepreneurs are only observed starting one firm; for those who start multiple firms, I limit my focus to the first firm they are observed starting. In total, I observe 36,621 unique firms started by 42,851 unique entrepreneurs during my sample period.

[17] Imposing a threshold of 1/3 follows Chodorow-Reich et al. (2024) and Hvide and Oyer (2025), who study other aspects of entrepreneurship in Norway.

[18] Because many businesses are started as sole proprietorships and then incorporated within a matter of months, I do allow for owners to be new sole proprietors up to one year prior to incorporation.

Figure 3: Industry composition of new firms



Notes: Sample includes N = 36,621 limited liability companies started by Norwegian entrepreneurs between 2001 and 2018. Industries are classified according to the Norwegian Standard Industrial Classification (SIC).

The sample includes both blue-collar and white-collar industries, as seen in Figure 3. Roughly 24% of firms in the sample are in wholesale and retail trade, which includes sellers of clothing, groceries, vehicles, and other goods. Over 17% are construction businesses. The two predominant white-collar industries of professional, scientific, and technical (which includes engineers, consultants, etc.) and information and communication (which includes, e.g., tech companies engaged in software or hardware production) collectively make up 28% of the sample. This industrial composition mirrors that of new firms in other high-income countries, including the United States (see, e.g., Fairlie et al., 2023, Chapter 2).

To obtain measures of firm performance, I link each firm to their annual financial statements as reported to the Norwegian tax authority. These financial statements, which I observe from 2001-2019, include information on total sales, operating costs, salaries paid, profits, and employment. I use these data to construct a measure of value added at each firm in each year, which is calculated as total sales minus capital and input (i.e., non-labor) costs. I also calculate value added per worker, which is value added divided by total employees plus owners. Value added per worker is commonly used to assess differences across firms when labor productivity is the focal point (e.g., Bloom et al., 2019).

I then use the employer-employee registers to link each entrepreneur/firm to their employees. These registers link workers to firms in each calendar year, with information about their earnings and the plant/establishment of employment. I define early-stage employees as those who were hired by the end of the firm's first year of operation. Only 33% of new firms hire any early-stage employees, while the others remain entirely owner-operated during this initial phase.[19]

To identify specific instances of network hiring at these firms, I link entrepreneurs to their former coworkers using the same employer-employee registers. I identify the sets of individuals with whom each entrepreneur worked (as an employee, not an owner) on the establishment level at some point during the five years prior to the incorporation of the entrepreneur's firm. I refer to these individuals as the entrepreneur's coworkers. By overlaying the set of an entrepreneur's employees with the entrepreneur's former coworkers, I can identify specific instances of network hiring. The final panel dataset provides the foundation for analyzing the relationship between network hiring, firm entry, and firm outcomes.

4 Descriptive Evidence

Using the linked data, I show that network hiring, in the form of hiring former coworkers, is widespread among new entrepreneurs in Norway. This prevalence is consistent across industries and over time, and it far exceeds what would occur by chance. Moreover, entrepreneurs who hire their former coworkers tend to have firms that grow larger and survive longer than entrepreneurs who hire from other sources. While these patterns are descriptive, they suggest that network hiring may be an important channel through which entrepreneurs build and grow their firms.

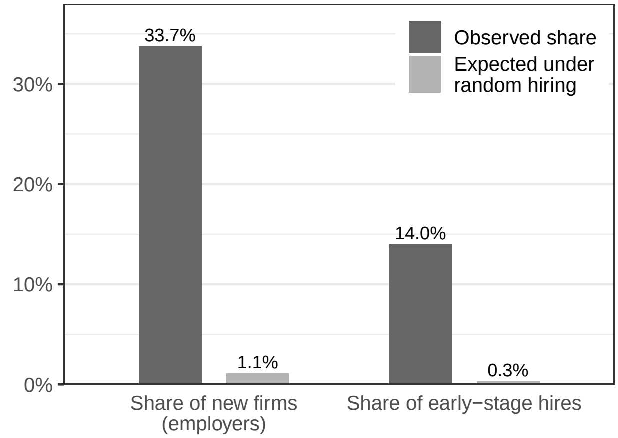
4.1 How often do entrepreneurs hire their former coworkers?

Entrepreneurs frequently hire their former coworkers. Figure 4 shows that out of all firms with any early-stage employees (hired before the end of the firm's first full year of operation), 34% hire at least one coworker of the founding entrepreneur. These network hires make up 14% of all early-stage hires made by new firms.

These patterns are consistent across industries and stable throughout the sample period. Within sectors, the share of new firms (with employees) that hire at least one former coworker typically ranges from 20-40% (Figure 5a). Since 2001, the overall share has generally hovered between 30-40%, while former coworkers account for roughly 10-20% of all early-stage employees (Figure 5b). There is no indication that network hiring is declining over time.

[19]By comparison, Fairlie et al. (2023, Chapter 6) use data from the U.S. Census Bureau and find that approximately 37% of U.S. startups hire employees. This figure uses a restrictive sample that removes many sole proprietors, similar to my own sample construction.

Figure 4: Frequency of entrepreneurs hiring ex-coworkers



Notes: This figure illustrates statistics on how often entrepreneurs hire their former coworkers in practice, as well as how often we would see this behavior if entrepreneurs hired job-switchers in the local labor market randomly (conditional on worker characteristics). The underlying sample includes firms that hired at least one employee by the end of the firm's first full year of operation. The dark gray bars show (i) what share of these firms hire from the founding entrepreneurs' ex-coworkers and (ii) what share of all early-stage employees, summed across firms, are ex-coworkers of the founding entrepreneurs. The light gray bars calculate what shares would be observed if entrepreneurs hired from the labor market at random, following the procedure described in the text. This procedure holds fixed the 2-digit industry of the firm and the education level, age (in 5 year bins), municipality of residence, and quarter of hire for all new employees.

While these patterns suggest that entrepreneurs hire from their networks as an intentional strategy, it is useful to assess how much of this could be driven by chance. For example, in a setting of pure monopsony where a single incumbent employs all labor, including the future entrepreneur, *any* early-stage hire the entrepreneur makes would be a former coworker.[20] This would be true even if entrepreneurs were not intentionally implementing a network-based hiring strategy.

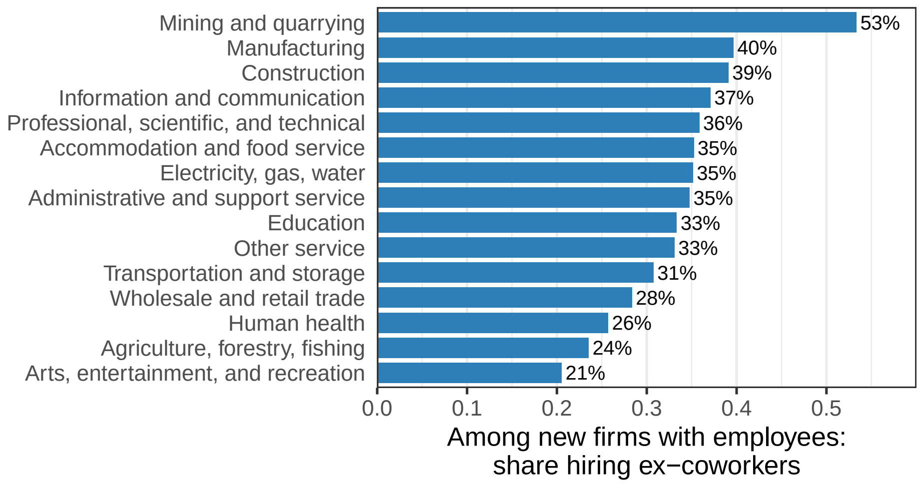
To examine how often such hiring would occur by chance, I hold fixed the age (5-year bin), education-level, and residential municipality[21] of each early-stage hire an entrepreneur makes. I then calculate the total number of individuals with these equivalent observable characteristics who started a new job in the same quarter and 2-digit industry, but at any firm (not only the entrepreneur's firm). I treat these observably comparable workers as the pool of potential candidates for each job at the new firm.

[20]For consistency with my definition of coworker, which is on the establishment level, the incumbent in this example must employ all its workers at a single, large establishment.

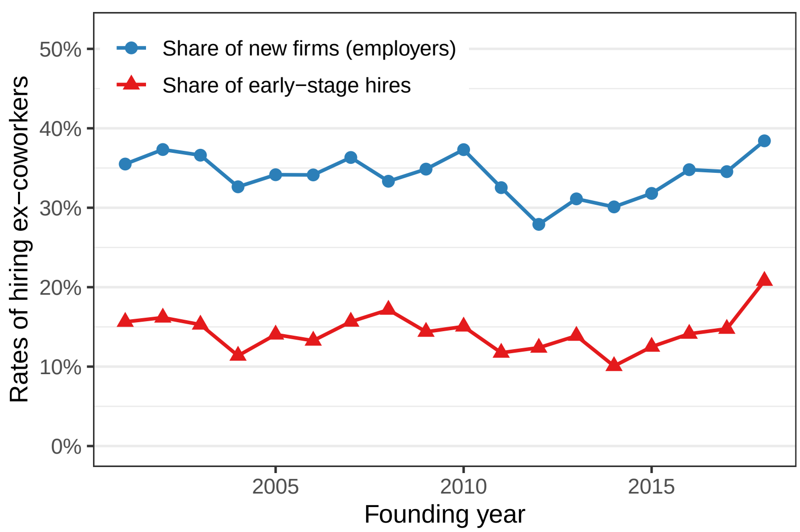
[21]In each year of my sample period, there were over 400 municipalities in Norway. The average adult population of each municipality was approximately 9,100. Cross-municipality commuting will lead my calculations to understate the number of "potential hires," thus overstating the probability of hiring ex-coworkers by chance.

Figure 5: Early-stage network hiring, by industry and year

(a) Hiring ex-coworkers by industry



(b) Hiring ex-coworkers by year



Notes: These figures plot the share of new firms (with employees) that hired at least one employee from the founding entrepreneurs' ex-coworkers. The underlying sample includes firms that hired at least one employee by the end of the firm's first full year of operation. In Panel (a), these shares are plotted separately by broad industry. In Panel (b), these shares are plotted separately by the year in which the new firm was founded as blue circles. Panel (b) also plots as red triangles the share of all early-stage hires across all new firms which are the founding entrepreneur's ex-coworkers.

I then use these pools of potential candidates to compute the probability that a given job would be randomly filled by an entrepreneur's former coworker. These simulated probabilities are provided for comparison in Figure 4. If job-switchers were hired at random conditional on observables, only 1.1% of new firms would hire the entrepreneur's former coworkers, accounting for roughly 0.3% of all early-stage hires. By contrast, the observed hiring rates are at least 30 times larger. This gap is consistent with the interpretation that entrepreneurs indeed actively recruit from their networks.

4.2 Characteristics of entrepreneurs, employees, and firms

Table 1, Panel A, compares three different types of entrepreneurs: those who did not hire any early-stage employees, those who hired but did not hire any ex-coworkers, and those who hired at least one ex-coworker. Characteristics are measured in the year that the firm was founded. Entrepreneurs who hired ex-coworkers are strikingly similar to those who hired non-coworkers, with similar ages, years of education, and years of experience. Compared to those who did not hire any employees, the early-stage hiring entrepreneurs have fewer years of education, less experience, and lower prior wages. Entrepreneurs who hired their ex-coworkers tended to work in smaller establishments than other entrepreneurs and have smaller networks. This might suggest that having more coworkers doesn't always translate into developing stronger bonds, or that network size may not be positively correlated with network quality.

Panel B shows that ex-coworker hires are much older, slightly more educated, and substantially more experienced than other hires. Part of this gap reflects selection: by definition, ex-coworkers were recently employed, making them about 30 percentage points more likely than other hires to have been employed in the year prior to firm founding. Conditional on being employed, ex-coworkers earned much higher wages (an average of \$55K versus \$28K for other hires). This suggests that former coworkers differ significantly from other early employees in terms of human capital and labor force attachment.

These differences in who entrepreneurs hire are mirrored in how their firms perform. Figure 6 shows that entrepreneurs who hire their ex-coworkers tend to run more robust firms. The top right panel shows the share of firms that generated any revenue, by age. Approximately 68% of firms that hired ex-coworkers are still active by age 5, compared to 59% of firms that hired others and 55% of firms that made no early-stage hires. These shares decline to 49%, 41%, and 41%, respectively, by the age of 10.

In addition to being positively correlated with survival, network hiring is also associated with differences in firm size and productivity. Firms that hired entrepreneurs'

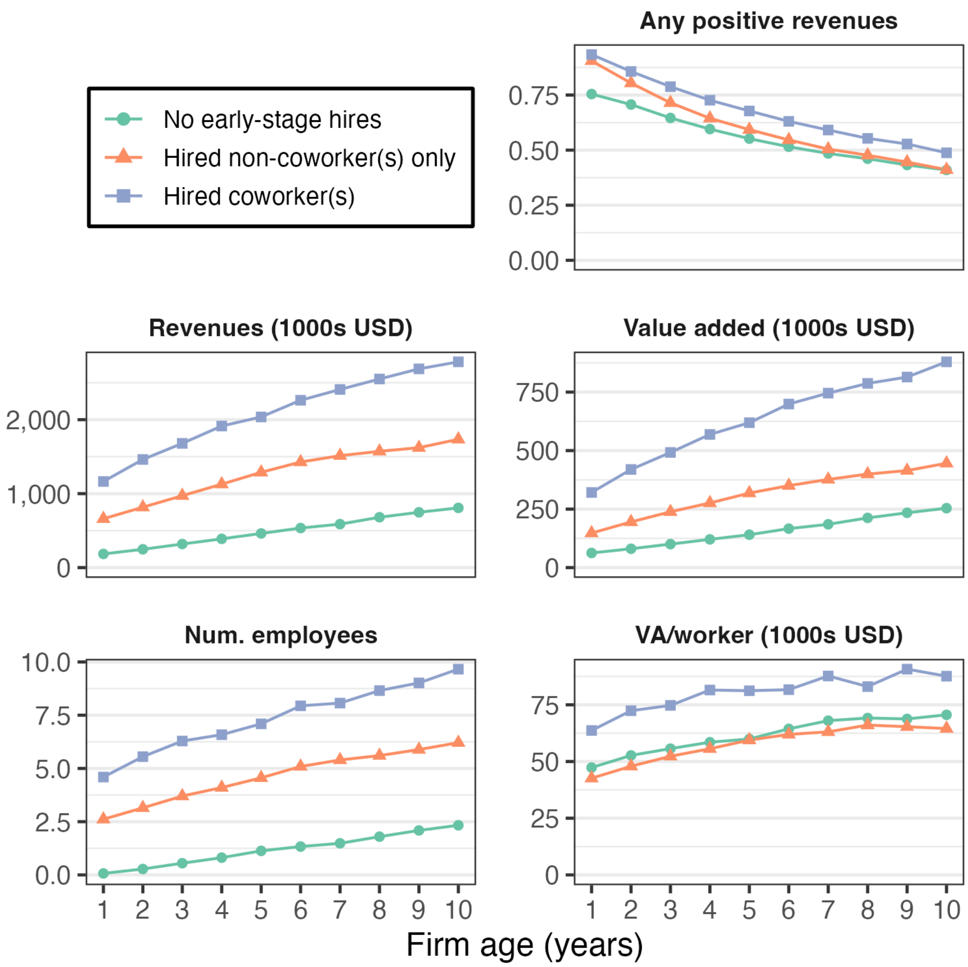
Table 1: Characteristics of entrepreneurs and hires by hiring type

		Hiring type		
	All	Coworkers	Others	None
	(1)	(2)	(3)	(4)
Panel A: Entrepreneur characteristics				
Age	38.3	37.5	37.3	38.7
Male	0.78	0.79	0.74	0.80
Native-born	0.78	0.75	0.74	0.80
Years of education	13.8	13.4	13.3	14.0
Years of experience	17.2	16.3	16.3	17.7
Previous wage (USD)	94,246	86,347	82,314	99,693
N coworkers, prior year (median)	41	29	36	47
N coworkers, prior five years (median)	156	99	137	176
Num. entrepreneurs	42,851	4,263	10,167	28,421
Panel B: Hire characteristics				
Age	29.7	34.8	28.8	
Male	0.53	0.68	0.51	
Native-born	0.62	0.70	0.61	
Years of education	12.5	12.8	12.4	
Years of experience	8.9	13.9	8.0	
Employed in prior year	0.69	0.94	0.65	
Previous wage† (USD)	32,839	55,392	28,163	
Num. hires	78,093	11,199	66,894	

†Conditional on variable being positive.

Notes: Sample includes incorporated firms founded by Norwegian entrepreneurs between 2001 and 2018. All statistics are measured in the year the new firm was founded. Panel A provides statistics on entrepreneurs, separately by early-stage hiring at their firms. Early-stage hiring is hiring that occurs before the end of the firm's first full year of operation. In Panel A, column (2) includes entrepreneurs who hired at least one coworker, while column (3) includes entrepreneurs who hired exclusively non-coworkers. Panel B provides statistics on the early-stage hires at these firms.

Figure 6: Firm outcomes by hiring practice



Notes: This figure plots average firm outcomes over time, separately by the types of employees hired by the end of the firm's first full year of operation. "Hired coworker(s)" includes firms that hired at least one ex-coworker of the entrepreneurs, while "hired non-coworker(s) only" includes firms that hired exclusively from other sources. "No early-stage hires" includes firms that did not hire any employees before the end of the firm's first year, but may have hired later on. Employment, revenues, value added, and value added per worker are shown conditional on the firm still being active. Employment and revenues are winsorized at the 99th percentile. Value added outcomes are winsorized at the 1st and 99th percentiles. The entrepreneurs themselves are not included as employees, but they are included as workers when computing value added per worker.

ex-coworkers are generally over twice as large, both in terms of employment and revenues, and they exhibit higher growth rates. They also have higher value added per worker, which reflects output for size. It is calculated by subtracting all non-labor input costs from revenues, and then dividing by the number of workers at the firm (including the entrepreneurs themselves). Firms that hire entrepreneurs' ex-coworkers early on have value added per worker that is, on average, over twice as high as other firms with early-stage employees. All of these gaps remain statistically significant after adjusting for firm industry and early-stage size (see Appendix Table A.1).

These descriptive patterns reveal meaningful differences across entrepreneurs, their firms, and their early-stage employees, but they do not establish whether hiring an ex-coworker causally affects firm performance. Unobserved differences between entrepreneurs may drive both hiring decisions and outcomes. For example, suppose the most talented entrepreneurs are particularly skilled at identifying arbitrage opportunities (Schumpeter, 1934, 1942), both in product markets *and* in labor markets—including within their networks. In that case, observational comparisons may overstate the true effect of network hiring.

Moreover, if the ability to hire from one's network enables some individuals to start firms who otherwise would not, then simple correlations also confound differences in firm performance with differences in the composition of entrants. The stylized model in Section 2 highlights this possibility: without network hires, the marginal entrants (who start firms because they can hire known coworkers) may have lower productivity than the inframarginal entrepreneurs, who start firms regardless. In that case, descriptive comparisons understate the true effect of network hiring on firm performance.

In the next section, I introduce a framework that directly addresses these endogenous, jointly determined entry and hiring decisions.

5 Estimating the Effects of Network Hiring

To estimate the causal effects of hiring ex-coworkers on firm formation and performance, I develop a partially ordered choice framework. Standard ordered choice models would treat the entry decision as independent of the network hiring decision. If some entrepreneurs only start firms because they can hire ex-coworkers, however, this assumption is too restrictive.

The partially ordered choice framework addresses this limitation by extending the standard ordered choice model to allow an ordered choice (hiring ex-coworkers) to affect a binary participation decision (starting a firm). In doing so, it accommodates distinct unobserved preferences for entrepreneurship and network hiring. The underlying parameters are identified using instrumental variables, coupled with an assumption on the joint distribution of preferences. These parameters can then be used to estimate the causal effects of network hiring on firm outcomes while allowing for rich heterogeneity across entrepreneurs.

5.1 Partially ordered choice framework

An individual i is a potential entrepreneur who must make two decisions. First, she must decide whether or not to start her firm, with $E_i \in \{0, 1\}$ indicating entry. Conditional on entry, she must decide how many ex-coworkers to recruit as early-stage employees, denoted by $D_i \in \{0, 1, \ldots, \bar{D}\}$. I assume away any relevant differences within the set of ex-coworkers the entrepreneur would possibly hire so that the scalar D_i fully reflects the network hiring decision.[22] Conditional on entry, the entrepreneur's firm realizes the outcome Y_i.

Network hiring as an ordered choice

Let the latent variable D_i^* represent the optimal number of ex-coworkers an individual would hire conditional on entry, with $D_i = E_i D_i^*$. I assume that this decision can be represented by the standard ordered choice model

$$\mathbb{1}[D_i^* \geq d] = \mathbb{1}[V_i^h - \psi(d, X_i, Z_i^h) \geq 0] \quad \text{for} \quad d \geq 1, \tag{2}$$

where the variables Z_i^h are exogenous factors affecting the costs of hiring ex-coworkers. The latent random variable V_i^h captures unobserved "preferences" for hiring ex-coworkers, which could reflect outcome-relevant attributes (e.g., coworker quality) or pure tastes (e.g., how much i enjoys working with friends). The function $\psi(d, x, z)$ can be interpreted as the perceived net costs of hiring the dth ex-coworker for individuals with observed characteristics (x, z). These costs are assumed to be increasing in d, so individuals with the strongest preferences for network hiring—the largest V_i^h—would hire the most ex-coworkers.

Because entry and network hiring decisions are determined jointly, the ordered choice representation in (2) is incomplete. Individuals also make entry decisions, choos-

[22] This assumption allows me to treat network hiring as an ordered choice problem. Similar simplifications are standard in many settings—for example, years-of-schooling models typically ignore the choice of coursework or school type (see, e.g., Willis and Rosen, 1979; Card, 1999; Cunha et al., 2007). Without this restriction, the decision would involve choosing a subset of hires from an idiosyncratic choice set of ex-coworkers. This generates a high-dimensional subset selection problem (Benson et al., 2018) that poses severe challenges to identification and computation.

ing a bundle

$$(E_i, D_i) \in \{\underbrace{(0,0)}_{\text{non-entry}}, (1,0), (1,1), \ldots, (1, \bar{D})\}.$$

The non-entry option $(0,0)$ is fundamentally different from the sequence $\{(1,d)\}$. It is not another threshold on the network hiring ladder, but instead reflects a separate decision of whether to enter at all. I therefore refer to this as a *partially ordered* choice: an ordered choice extended to include a non-participation option. Accounting for this outside option is critical when the ability to hire from one's network can affect the entry decision itself, a possibility emphasized by the theory from Section 2.

From ordered to partially ordered choice

To model partially ordered choices, I introduce a framework in which entry decisions may depend on network hiring decisions. I assume that choices are given by

$$E_i = \mathbb{1}[U_i^e + \omega(X_i)\, U_i^h(D_i^*) \geq 0] \tag{3}$$

$$D_i^* = \arg\max_{d \geq 0} U_i^h(d) \tag{4}$$

where U_i^e is the expected utility an individual receives from entry before accounting for ex-coworkers, and $U_i^h(d)$ is the expected additional utility an individual receives from hiring ex-coworkers. Entry decisions are based on a weighted average of these two utilities, where the relative weight placed on the network hiring decision is $\omega(X_i) \geq 0$. If $\omega(X_i) = 0$, then individuals do not take network hiring into account when making entry decisions. But if $\omega(X_i) > 0$, then network hiring influences entry, affecting who becomes an entrepreneur.

To build intuition, it helps to see how this general setup nests the stylized model from Section 2. In that case, the baseline entry utility would be

$$U_i^e = \alpha_i - \theta_i - c + \max\{S_i^m, 0\},$$

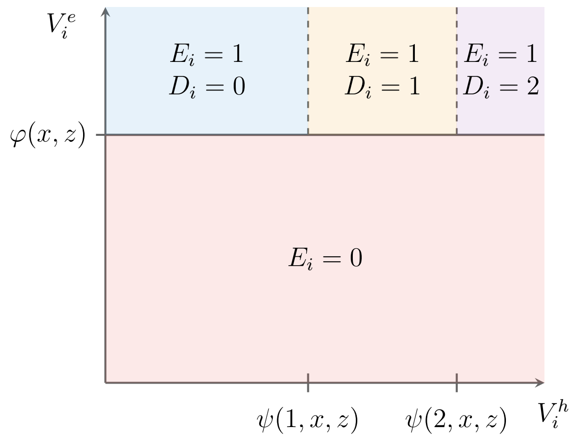
where $\alpha_i - \theta_i - c$ is the net benefit of solo entry and S_i^m is the surplus from market hiring. With $\omega(X_i) = 1$, the incremental value of hiring one's ex-coworker would be

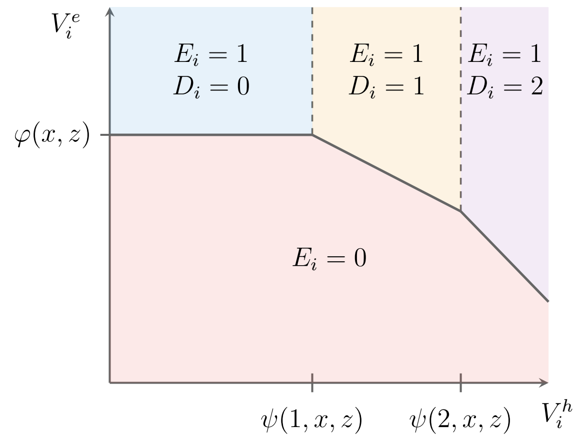
$$U_i^h(d) = d(S_i^n - \max\{S_i^m, 0\}),$$

where S_i^n is the surplus from network hiring. The stylized model is therefore a special case of the general setup from (3)-(4), which preserves the core intuition that network hiring can shift the entry margin itself. In what follows, I work with more general representations of U_i^e and U_i^h that can accommodate many possible mechanisms.

Figure 7: Illustration of partially ordered choice framework

(a) Entry separable from hiring ($\omega(x) = 0$) (b) Entry nonseparable from hiring ($\omega(x) > 0$)





Notes: These figures illustrate the intuition of the partially ordered choice framework in a stylized case where an entrepreneur can hire up to 2 ex-coworkers. The x-axis is the latent variable reflecting unobserved preferences for network hiring, V_i^h. The y-axis is the latent variable reflecting unobserved preferences for entrepreneurship, V_i^e. Panel (a) illustrates a case where $\omega(x) = 0$, so that network hiring does not affect entry decisions. Panel (b) illustrates a case where $\omega(x) > 0$, so that network hiring affects entry decisions.

I write the baseline utility of entry U_i^e under the usual additively separable representation

$$U_i^e = V_i^e - \varphi(X_i, Z_i^e), \tag{5}$$

where the variables X_i are observable characteristics and Z_i^e are exogenous factors affecting the entry decision. The latent random variable V_i^e represents unobserved "preferences" for entrepreneurship. This could capture entrepreneurial productivity, a distaste for having a boss, and so on. The function $\varphi(x, z)$ can similarly be interpreted as the perceived net costs of entry for individuals with observed characteristics $(X_i, Z_i^e) = (x, z)$.

To model the utility from network hiring, I use a representation that embeds the standard ordered choice model:

$$U_i^h(d) = \sum_{j=1}^{d} [V_i^h - \psi(j, X_i, Z_i^h)]. \tag{6}$$

This does not change the optimal network hiring choice D_i^*—it is easily verified that maximizer of (6) is also the unique value satisfying (2).

The key advantage of the partially ordered choice framework is that it allows the expected benefits of network hiring to feed back into the entry decision itself. Figure 7 illustrates this for a stylized case with $D_i \leq 2$. Panel (a) shows the case where $\omega(x) = 0$, so entry depends only on the entrepreneurship preference V_i^e. Network hiring matters only after the entry decision is made, and all entrants are inframarginal in the sense that they would have started firms regardless of network hiring opportunities. Panel (b) shows the case where $\omega(x) > 0$, so entry depends both on V_i^e and V_i^h. In this case, a set of marginal entrants with $V_i^e < \varphi(X_i, Z_i)$ enter only because they can hire from their networks. This distinction is the defining feature of this framework, which accommodates entrepreneurs whose very decision to start a firm hinges on the availability of network hires.

Ignoring such endogenous entry introduces bias to treatment effect estimates, even when D_i^* itself is randomly assigned. It follows that standard linear IV estimators may not be sufficient to recover causal effects in these settings. I return to this point in greater detail in Section 5.4.

Potential outcomes

To link choices to firm outcomes, I define potential outcomes for firm performance under different numbers of ex-coworker hires. $Y_i(d)$ denotes the outcome that would be realized if entrepreneur i enters and hires d ex-coworkers. Potential outcomes are only observed conditional on $D_i = d$ and $E_i = 1$, with

$$Y_i = E_i \times \sum_{d=0}^{\bar{D}} \mathbb{1}[D_i = d] Y_i(d). \tag{7}$$

These potential outcomes place no restrictions on the number of other early-stage hires the entrepreneur would choose to hire. This implies, for example, that the treatment effect $Y_i(1) - Y_i(0)$ reflects the total causal effect of the first ex-coworker hire—including any non-network workers this ex-coworker crowds out or crowds in.[23]

To separate the role of observable characteristics X_i from unobservable characteristics (V_i^e, V_i^h), I decompose average potential outcomes as

$$\mathbb{E}[Y_i(d) \mid X_i = x, V_i^e = v^e, V_i^h = v^h] = \mu_d(x) + \theta_d(v^e, v^h, x). \tag{8}$$

The function $\mu_d(x)$ provides the average outcome that would be experienced by indi-

[23]For example, consider the stylized model from Section 2 where the entrepreneur can only hire one worker. Let firm income be the outcome. In this case, the treated potential outcome from hiring the sole coworker k is $Y_i(1) = \alpha_i + \beta_{ik}$. The untreated potential outcome depends on the counterfactual: $Y_i(0) = \alpha_i$ if the entrepreneur would instead hire no one, while $Y_i(0) = \alpha_i + \beta_{ij}$ if the she would instead hire worker j from the market. In general, since the type of hire (ex-coworker, other, or none) is an unordered choice, we identify the effect of hiring an ex-coworker relative to the "next best" option (Heckman et al., 2008).

viduals with observed characteristics $X_i = x$ if they were to start a firm and hire d ex-coworkers. The function $\theta_d(v^e, v^h, x)$ captures deviations from this average in terms of the unobserved characteristics $(V_i^e, V_i^h) = (v^e, v^h)$. It is informative about the nature of selection into entrepreneurship and network hiring. For example, if individuals with stronger preferences for network hiring (V_i^h) are also those whose firms gain more from such hiring (larger $Y_i(d) - Y_i(d-1)$), then $\theta_d - \theta_{d-1}$ would be increasing in v^h for fixed x and v^e.

Broader relevance of partially ordered choice

The partially ordered choice model is well-suited for modeling early-stage network hiring by entrepreneurs, but it also applies more broadly. It is useful whenever extensive and intensive margin decisions are jointly determined in the presence of heterogeneous participation costs.

One example of such a partially ordered choice is college attendance: students decide whether to enroll in college at all, and conditional on enrolling, what quality of institution to attend. Because the returns to attending higher-quality colleges are substantial (Dillon and Smith, 2020), the opportunity to attend such a school may itself induce enrollment.[24] As a result, the ability to attend a higher-quality college may induce a set of marginal enrollees who differ systematically from the inframarginal ones. A similar logic applies to labor supply: individuals decide whether to work, and conditional on working, whether to work part-time or full-time. Because labor force participation can entail substantial commuting or childcare costs (Mas and Pallais, 2019), some people will only accept jobs if they can work enough hours to justify those fixed costs. Again, the marginal workers—those who work only if they can secure full-time employment—may differ substantially from the inframarginal workers who would work regardless.

Partially ordered choices arise in many other economic settings. In migration, individuals often face large fixed costs of relocation (Borjas, 1987), which may lead some to migrate only if they expect to stay long enough to recoup those costs. And in public health, patients place high value on continuity of care (Valentine et al., 2003; Chen et al., 2020), and so some may forgo treatment altogether if they do not expect to see a physician regularly. In both cases, the marginal migrants/patients may have systematically different outcomes and treatment effects from the inframarginal ones.

These examples illustrate the broader relevance of the partially ordered choice framework, which applies whenever fixed costs tie participation to treatment inten-

[24]Evidence suggests that individuals face fixed costs of enrolling in college, borrowing costs, and opportunity costs in the form of foregone earnings (Dynarski, 1999; Cameron and Taber, 2004).

sity. Ignoring this linkage risks mischaracterizing treatment effects and overlooking the marginal participants, who are often a particularly policy-relevant group. This framework therefore facilitates both sharper empirical inference and more informative policy analysis.

5.2 Identification

Assume that the instruments $Z_i = (Z_i^e, Z_i^h)$ are independent of potential outcomes and of individual preferences, conditional on observed characteristics:

Assumption 1 (Exogeneity)**.** $V_i^e, V_i^h, \{Y_i(d)\}_{d=0}^{\bar{D}} \perp\!\!\!\perp Z_i \mid X_i$.

As is common in models with multiple latent variables (e.g., Tebaldi et al., 2023), nonparametric identification under Assumption 1 alone requires instruments that generate substantial, continuous variation in the costs of entry and hiring.[25] This motivates the parametric assumptions below, which enable identification under much weaker requirements on the instruments.

Assumption 2 (Joint normality of latent variables)**.** V_i^e and V_i^h follow a bivariate standard normal distribution with correlation $\rho(X_i)$.

Assumption 3 (Linearity in latent variables)**.** For all $d \geq 0$ and all x, average potential outcomes are linear in V_i^e and V_i^h, with

$$\mathbb{E}[Y_i(d) \mid X_i = x, V_i^e = v^e, V_i^h = v^h] = \mu_d(x) + \theta_d^e(x)v^e + \theta_d^h(x)v^h$$

for some unknown functions μ_d, θ_d^e, and θ_d^h.

Assumptions 2 and 3 generalize the assumptions invoked in the original Heckman (1979) selection framework.[26] These assumptions are rich in their identifying content. Under Assumption 2, instruments that induce modest changes in choice probabilities can be used to identify the full joint distribution of (V_i^e, V_i^h) and the entry/hiring cost functions $\varphi(X_i, Z_i^e)$ and $\psi(d, X_i, Z_i^h)$. Under Assumption 3, such instruments can also be used to identify the outcome equation. The following theorem formalizes the conditions under which the choice parameters and outcome equations are identified.

[25] For example, one requirement is an instrument Z_i^e that can make the entry cost $\varphi(X_i, Z_i^e)$ low enough to induce *any* individual to become an entrepreneur. Such instruments are not available in practice.

[26] Kline and Walters (2016) make similar assumptions when studying discrete preschool choice. They maintain a version of Assumption 2 that also imposes homoskedasticity of the unobservables, whereas the weight $\omega(X_i)$ permits heteroskedasticity in the partially ordered choice framework. They also adopt a version of Assumption 3 in which the coefficients on the latent variables are constant across covariates, an assumption which I also leverage in my empirical estimation.

Theorem 1. Assume that Assumptions 1 and 2 hold. Partition the instruments (Z_i^e, Z_i^h) into $(\tilde{Z}_i^e, \tilde{Z}_i^h, \tilde{Z}_i)$, where $\tilde{Z}_i^e$ contains variables in Z_i^e but not Z_i^h, $\tilde{Z}_i^h$ contains variables in Z_i^h but not Z_i^e, and $\tilde{Z}_i$ contains variables in both Z_i^h and Z_i^e. Assume that $\bar{D} \geq 2$ and at least one of the following holds for each (x, z) in the support of $(X_i, \tilde{Z}_i)$:

(i) $\varphi(X_i, Z_i^e)$ has at least two support points conditional on $X_i = x$ and $\tilde{Z}_i = z$, or

(ii) $\psi(d, X_i, Z_i^h)$ has at least three support points for all $d \geq 1$ conditional on $X_i = x$ and $\tilde{Z}_i = z$.

Then $\varphi(X_i, Z_i^e)$ is identified on the support of (X_i, Z_i^e), $\psi(D_i, X_i, Z_i^h)$ is identified on the support of (D_i, X_i, Z_i^h) for $D_i \geq 1$, and $\rho(X_i)$ and $\omega(X_i)$ are identified on the support of X_i. Moreover, suppose Assumption 3 holds, and that each of the following relevance conditions hold:

(iii) $(\varphi(X_i, Z_i^e), \psi(1, X_i, Z_i^h))$ has at least three support points conditional on $X_i = x$

(iv) $(\varphi(X_i, Z_i^e), \psi(d, X_i, Z_i^h), \psi(d+1, X_i, Z_i^h))$ has at least three support points for all $d \in \{1, \ldots, \bar{D}-1\}$ conditional on $X_i = x$

(v) $(\varphi(X_i, Z_i^e), \psi(\bar{D}, X_i, Z_i^h))$ has at least three support points conditional on $X_i = x$.

Then $\mu_{D_i}(X_i)$, $\theta^e_{D_i}(X_i)$, and $\theta^h_{D_i}(X_i)$ are identified on the support of (D_i, X_i).

Proof: See Appendix B.

Theorem 1 shows that by imposing joint normality on the latent variables, identification of the choice parameters can be achieved with a single binary instrument. This instrument must affect the fixed costs of entry, $\varphi(X_i, Z_i^e)$, but not the costs of hiring, per condition (i). Identification can alternatively be attained with an instrument that affects network hiring but not the fixed costs of entry, per condition (ii); this instrument must have at least three values.

The second part of Theorem 1 shows that under linearity in the latent variables, the parameters of the outcome equations can be identified when the support of Z_i contains at least three points (e.g., two binary instruments would suffice). This is intuitive, given the representation in Assumption 3: there are three unknown parameters per (d, x), and these are only uniquely pinned down by instruments taking at least three values.[27] More specifically, under Assumption 3, we have

$$\mathbb{E}[Y_i \mid E_i = 1, D_i = d, X_i, Z_i] = \mu_d(X_i) + \theta^e_d(X_i)\lambda^e(d, X_i, Z_i) + \theta^h_d(X_i)\lambda^h(d, X_i, Z_i) \tag{9}$$

$$\text{where} \quad \lambda^e(d, x, z) = \mathbb{E}[V^e_i \mid E_i = 1, D_i = d, X_i = x, Z_i = z] \tag{10}$$

$$\text{and} \quad \lambda^h(d, x, z) = \mathbb{E}[V^h_i \mid E_i = 1, D_i = d, X_i = x, Z_i = z]. \tag{11}$$

The control functions $\lambda^\ell(d, x, z)$ reflect the average values of the unobserved preferences among those who select a given bundle (E_i, D_i). Put differently, they summarize how selection into entry and network hiring changes the distribution of latent variables. Under Assumption 2, these control functions are identified from variation in the instruments satisfying conditions (i) or (ii) of Theorem 1. For a given $X_i = x$ and $D_i = d$, the three parameters left to identify are $\mu_d(x)$, $\theta^e_d(x)$, and $\theta^h_d(x)$. Hence, identification requires three control function estimates (produced by three values of Z_i).

To summarize, the minimum conditions for identification require either (i) a single multi-valued instrument that shifts entry or network hiring costs alone, or (ii) a pair of binary instruments, one of which affects entry alone.

5.3 Instruments

In my empirical approach, I use a binary instrument for entrepreneurial entry and a multivalued instrument for network hiring.

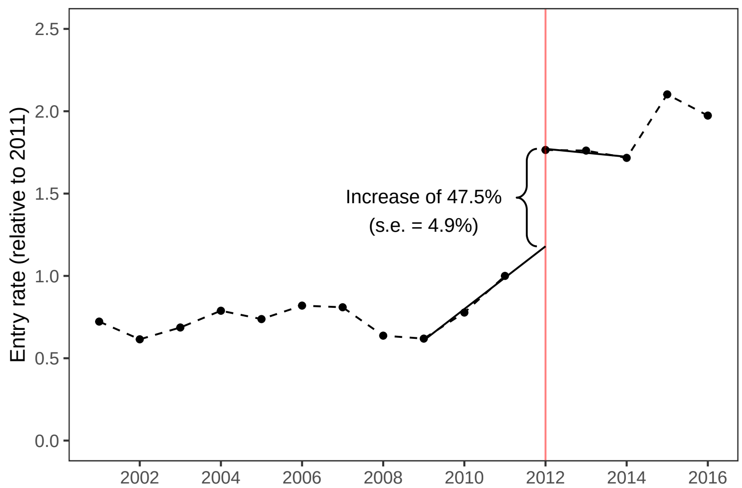
5.3.1 Instrument for entrepreneurial entry

My instrument for entrepreneurial entry Z^e_{it} is based on a policy change that explicitly lowered the costs of incorporating a firm in Norway.

To incorporate a firm, Norwegian entrepreneurs must demonstrate that their firm has a minimum amount of paid-in capital—that is, a minimum amount that the entrepreneurs(s) or other investors have paid into the firm. This capital requirement is frequently satisfied by opening a business bank account in the firm's name and depositing the requisite amount. This paid-in capital cannot be loaned out or distributed to shareholders unless the firm is liquidated, and business owners must submit to formal audits to demonstrate to the government that their firm meets the minimum capital requirement.

[27] This mirrors the identification result of Brinch et al. (2017), who show how a binary instrument can identify average potential outcome functions that are linear in a single latent variable.

Figure 8: Change in entry rate after lowering of capital requirement



Notes: This figure plots the entry rate of first-time entrepreneurs starting limited liability companies, relative to the entry rate in 2011 (which was 0.15%). The panel includes a sample of N = 2,615,957 adults who worked as wage earners in the prior year and had not yet started an incorporated firm. The entry rate excludes conversions of sole proprietorships into LLCs. This figure plots the number of entrepreneurs incorporating a new firm for the first time each year. Excludes conversions of established sole proprietorships into LLCs. The red solid line indicates the capital requirement reform, which came into effect in January 2012. The reported estimate and robust standard error (clustered on the individual level) are from a regression of an indicator for entry on a post-reform indicator, indicators for each year outside the 2009-2014 window, and separate linear functions in year for the 2009-2011 and 2012-2014 periods.

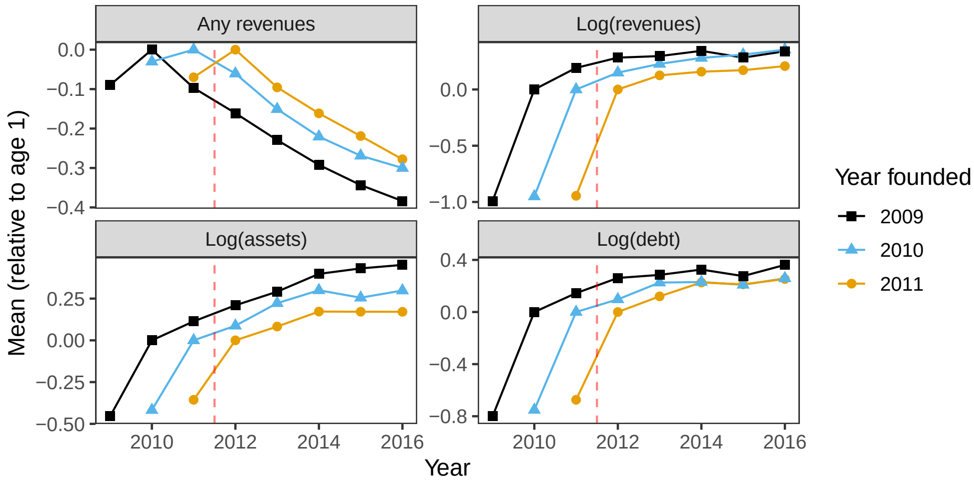
The government's initial rationale for this policy was to protect creditors. From 1997 until 2011, the capital requirement was NOK 100,000 (US$17,000 in 2012). In September 2011, the Ministry of Justice of Norway argued that the policy was ineffective at its initial aims—protecting creditors—and instead only served as a barrier to entrepreneurial entry (GOV, 2011). This argument was met with little opposition, and in January 2012, the capital requirement was lowered to NOK 30,000 (US$5,000). This policy change was isolated in time, with no other major structural shifts occurring in the surrounding period that would affect entrepreneurial entry (Bacher et al., 2025).[28]

As shown in Figure 8, the reform sharply increased the entry rate of new, incorporated firms by roughly 48% relative to the pre-reform trend. This suggests that the policy substantially lowered the perceived costs of entrepreneurship. My sample construction omits cases where previously self-employed individuals (i.e., sole proprietors) incorporated, so these are truly new firms rather than incorporations of established sole proprietors.[29] To use the reform empirically, I follow Bacher et al. (2025) and model

[28]One exception is an elimination of a prior auditing requirement for small entrants, which occurred in May 2011. Bacher et al. (2025) examine entry on the quarterly level and find no changes in response to this change in the auditing requirement.

[29]Because individuals who plan to incorporate may often operate briefly as sole proprietors, I allow for a

Figure 9: No changes for young firms started prior to the reform



Notes: This figure plots average outcomes over time for the $N = 3{,}601$ firms started in 2009, 2010, and 2011, the three years prior to the lowering of the capital requirement. To facilitate comparison of trends, the age 1 outcomes are normalized to 0. The vertical dashed line separates the pre-reform and post-reform years. Averages for any revenues are unconditional on survival, while averages for the log outcomes are conditional on reporting any positive revenues, assets, or debt, respectively.

rate of entry linearly for three years pre- and post-2012, using as Z_{it}^{e} a post-reform indicator. I include indicator variables for all other years as covariates.

The key identifying assumption is that the reform affected only the entry decision and not potential outcomes $Y_i(d)$ or the network hiring decision D_i^*. Conceptually, the reform represents a sunk cost of entry. Standard models of firm optimization imply that once the firm is operating, this initial level of "bound equity" should not distort subsequent firm investment decisions, since it is not a variable cost of production and cannot be withdrawn (e.g., Hopenhayn, 1992; Ericson and Pakes, 1995). Reassuringly, Figure 9 shows that firms started in the years prior to the reform do not make any systematic changes to their investment decisions after the reform. Appendix Table A.2 implements the regression analog to this figure, finding no evidence of any response. Collectively, this theoretical and empirical evidence supports the identifying assumption that the reform only affected entry decisions and not post-entry optimization.

one-year grace period of operation as a sole proprietorship prior to incorporation. One theoretical possibility is that some individuals who would have started sole proprietorships decided to start incorporated firms instead, in which case the observed increase would overstate true entrepreneurial entry. However, Appendix Figure A.2 shows that the reform did not appear to change the rate of entry for sole proprietors, suggesting that this is not happening on an empirically relevant scale. Bacher et al. (2025) access more granular data on sole proprietors and reach the same conclusion.

5.3.2 Instrument for hiring former coworkers

The instrument I construct for hiring former coworkers is motivated by a simple intuition: a worker will only move to a new firm if he thinks it will make him better off than staying with his current employer. If his current employer experiences an unanticipated downturn, his expected benefit of staying with that employer declines. He may then be more likely to join a coworker's start-up.[30]

I operationalize this intuition by grouping all Norwegian employers into decile ranks based on their revenues, where deciles are computed within year and 2-digit industry cells. I then calculate the year-over-year change in (or shock to) this revenue rank, ΔR_{jt}. Because rankings are done separately by calendar year and firm industry, these incumbent employer shocks reflect firm-specific changes in performance that are orthogonal to broader sectoral shifts in consumer demand.

For each individual in the data and each year t, my instrument for network hiring Z^h_{it} is the average shock experienced by that individual's $t-5$ to $t-1$ coworkers,

$$Z^h_{it} = |\mathcal{K}_{it}|^{-1} \sum_{k \in \mathcal{K}_{it}} \Delta R_{j(k,t-1)t} \tag{12}$$

where $\mathcal{K}_{it}$ is the set of individual i's recent coworkers as of year t and $j(k,t)$ is k's employer at year t. This is a formula instrument that leverages differences in exposure to network shocks. Because the networks themselves—and thus the exposure to shocks—are not exogenous, I recenter ΔR_{jt} on its conditional expectation given the lagged rank R_{jt-1} before taking these averages (Borusyak and Hull, 2023). This ensures that the instrument is mean zero conditional on coworkers' prior employers, mitigating concerns about mechanical omitted variables bias arising from the formula.

The identifying variation comes from comparing individuals with similar networks but whose coworkers experienced different firm-specific shocks. I control for the network size $|\mathcal{K}_{it}|$ and the average prior rank of network members' employers, obtained by replacing ΔR_{jt} with R_{jt-1} in (12). I also use the shocks to one's own prior employer, $\Delta R_{j(i,t-1)t}$, as a second instrument for entry, so Z^h_{it} doesn't reflect changes in one's own outside option. As a result, Z^h_{it} captures variation across otherwise similar individuals, with similar networks, but whose recent coworkers are more primed to join a new firm.

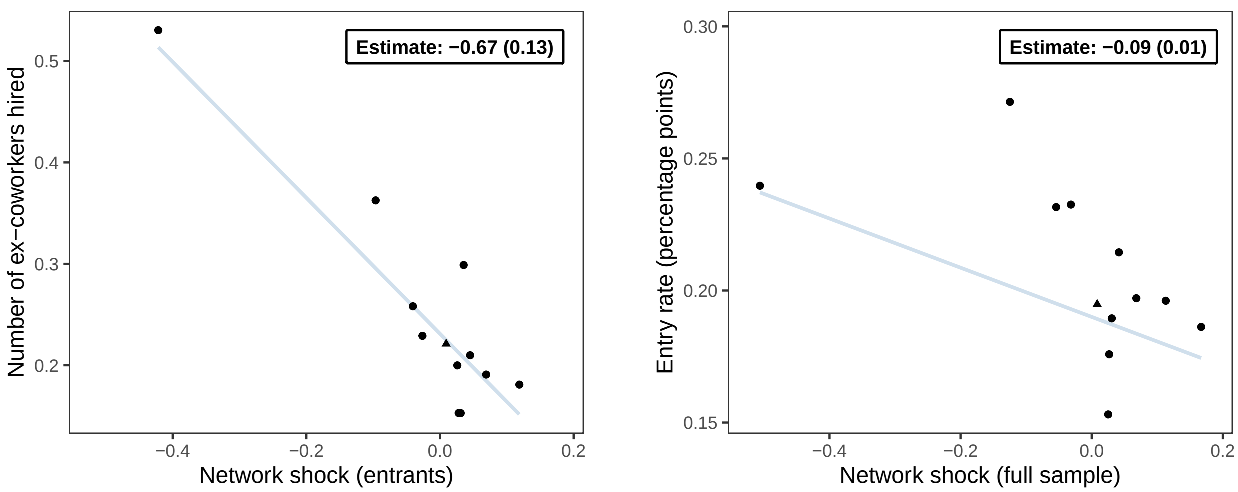
Figure 10a visualizes the first stage relationship between the network shock Z^h_{it} and the number of ex-coworkers an entrepreneur hires in their firm's first year. Network hiring responds strongly to the network shock, with a shock of -0.1 (a one-decile decline

[30]This intuition aligns with standard search-and-matching models, where workers move jobs only when the outside option exceeds the value of staying their current employer (see, e.g., McLaughlin, 1991).

Figure 10: Relationship between network shock, hiring ex-coworkers, and entry

(a) Network hiring

(b) Firm entry



Notes: These figures plot the relationship between the network shock instrument, hiring, and entry. In both panels, along the x-axis is the network shock instrument in the year the firm entered. The instrument has been recentered based on lagged employer rank and residualized on calendar year dummies, individual age, number of establishment-level coworkers in the past 5 years (log), average lagged wage of recent coworkers (log), entrepreneur lagged liquid wealth (log), and the lagged average employer revenue rank among recent coworkers. In Panel (a), the sample includes entrants, and the outcome is the number of ex-coworkers hired by the end of the firm's first full year. In Panel (b), the sample includes all adults who worked in the prior year. In both panels, the outcomes are residualized on the same set of covariates as the instrument, with the overall mean added to the residuals. Plotted triangles include the 71%/79% of entrants/individuals with instrument values between -0.025 and 0.025. Circles reflect equally-sized bins. Estimates are from regressions of the outcomes on the recentered network hiring instrument and the covariates, with cluster robust standard errors in parentheses.

at network members' employers) increasing the expected number of ex-coworkers hired by 0.067, or 29% of its baseline mean of 0.231.

At the same time, Figure 10b shows that the network shock also has a significant reduced form relationship with the entry decision: a shock of -0.1 increases the entry rate by roughly 0.01 percentage points, or 5% of its baseline mean of 0.2 percentage points. This reduced form is consistent with the partially ordered choice mechanism: network shocks affect entry indirectly through their impact on the feasibility of network hiring. The choice framework plays a key role in helping us interpret these patterns.

The key identifying assumption is that shocks to incumbent employers' relative revenues affect new firm outcomes, Y_i, only through their impact on how many ex-coworkers an individual would hire, D_i^*. Conceptually, these shocks shift the outside options of potential hires but should not directly alter the productivity or growth prospects of the new firm, except in the unlikely case where the entrant competes head-to-head with the incumbent.

Two threats could violate this assumption. First, if the entrant immediately steals substantial market share from incumbents upon entry, then entry and network hiring could affect incumbent performance directly. In practice, such reverse causality seems unlikely. In its founding year, the median entrant generates less than 1% as much revenue as the median incumbent[31] and generally starts off with no more than 1-2 employees compared to the incumbent median of 29.[32] To eliminate the most likely cases of such head-to-head competition, I restrict my sample of incumbents throughout to those with at least 10 employees in the year prior to the new firm's entry, and I explore robustness to increasing this cutoff. A second, related concern is that declines in incumbent performance provide a vacuum that can be filled by the entrant, leading the instrument to overstate the impact of network hiring. Again, the risk appears small in practice: over 80% of entrepreneurs start firms in a different industry than their former employer (see Figure A.3), and my results are robust to restricting to this subset.

5.4 From instruments to effects

The central identification challenge is that the ability to hire ex-coworkers can also change who becomes an entrepreneur in the first place, which motivates using the partially ordered choice framework.

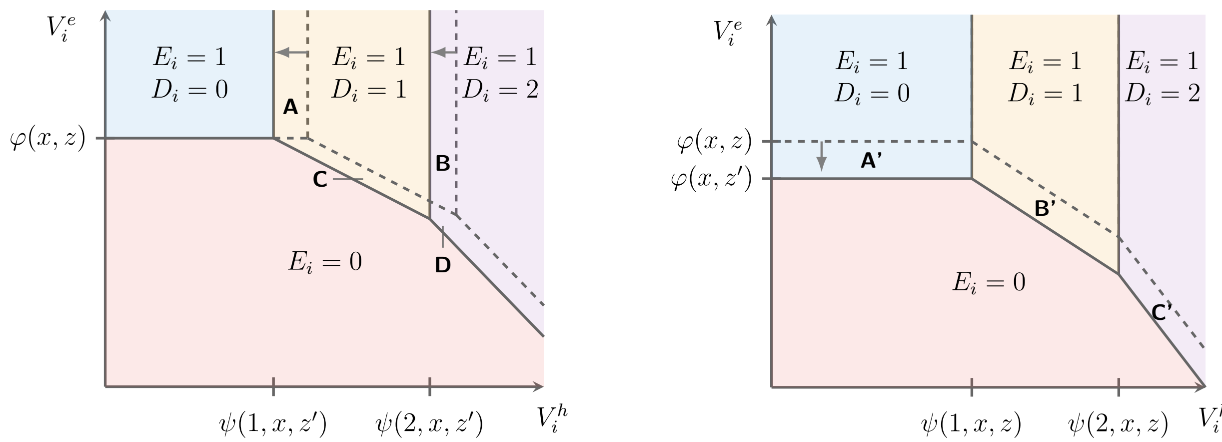
[31] Specifically, the median entrant generated roughly \$12,500 of revenue in its founding year. The median incumbent employer generated over \$5.5 million.

[32] This is the median across entrepreneurs' former employers. Because more individuals work for larger firms, a more appropriate statistic may be the median across entrepreneurs, which is 85.

Figure 11: Illustration of how instruments shift choice behavior

(a) Network hiring instrument

(b) Entry instrument



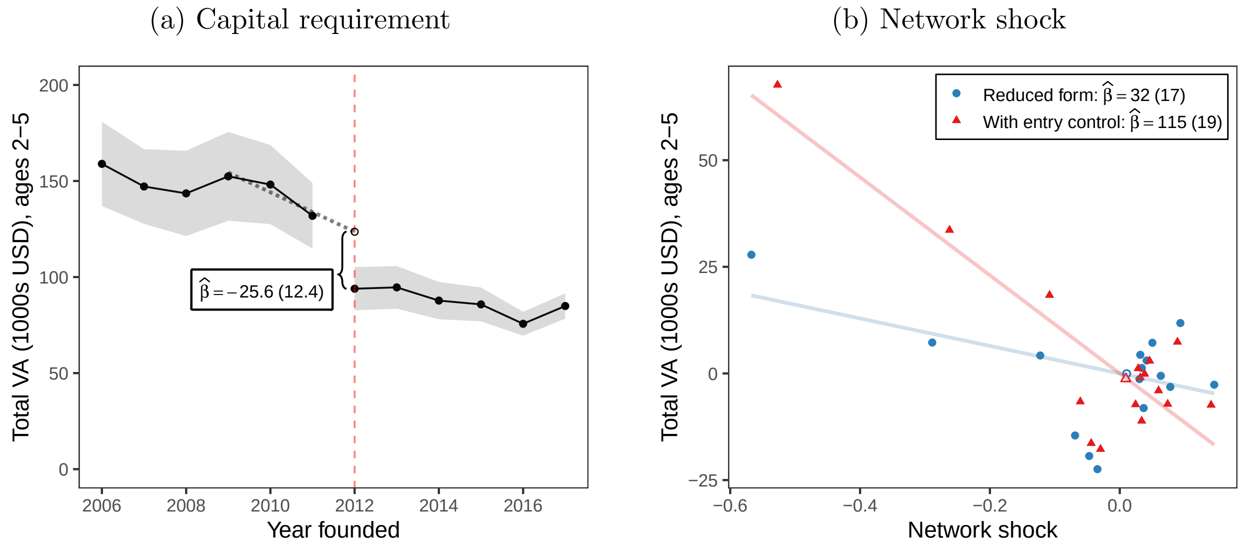
Notes: These figures illustrate how instruments affect choice behavior in a stylized case where an entrepreneur can hire up to 2 ex-coworkers. The x-axis is the latent variable reflecting unobserved preferences for network hiring, V_i^h. The y-axis is the latent variable reflecting unobserved preferences for entrepreneurship, V_i^e. Panel (a) illustrates a shift in the network hiring instrument from z (dashed lines) to z' (solid lines), which affects both network hiring and entry choices. Panel (b) illustrates a shift in the entry instrument from z (dashed lines) to z' (solid lines).

To illustrate how the framework identifies causal effects, consider the stylized example in Figure 11. In Figure 11a, the instrument Z_i^h changes the "costs" of hiring ex-coworkers. For some entrepreneurs who would have started firms regardless (V_i^e above the dashed line), this simply changes their hiring choices. For example, individuals in region **A** are inframarginal entrepreneurs who go from hiring no ex-coworkers to hiring one, while those in region **B** move from one ex-coworker to two. But for others—those with weaker entry preferences, but sufficiently high network-hiring preferences—the same shift in Z_i^h induces entry that would not have otherwise occurred. Regions **C** and **D** contain these marginal entrepreneurs who start firms only because the instrument shift made it optimal to hire ex-coworkers. If these marginal entrants differ from the inframarginal entrants, the reduced-form relationship between outcomes Y_i and the instrument Z_i^h mixes causal and compositional effects. Standard linear IV or 2SLS estimation would conflate these two effects, leading to inconsistent treatment effect estimates.[33]

To separate the causal effect of hiring from selection into entrepreneurship, we need variation that shifts entry decisions without altering latent hiring choices. The entry instrument Z_i^e provides this leverage. As shown in Figure 11b, reducing Z_i^e

[33] In different terminology, entry itself acts as a collider variable (Greenland et al., 1999): because it is affected by the treatment of interest, conditioning on it leads to so-called collider bias. A direct implication is that Z_i^h is not exogenous if we condition on entry.

Figure 12: Reduced form effects of instruments on total value added



Notes: These figures plot the reduced form relationships between the instruments and the total value added (VA) experienced by entrants over the ages 2-5. VA is measured in 1000s of USD and is winsorized at the 1st and 99th percentiles. Panel (a) plots the estimates and 95% confidence intervals from a regression of VA on dummies for the year of founding and controls. Panel (b) plots the relationship between the network shock instrument and VA, both of which have been residualized on controls. The "with entry control" points also control for $\lambda^e(D_i, X_i, Z_i)$, which is the control function component corresponding to the entry preference. Hollow points include the 79% of entrants with instrument values between -0.025 and 0.025. Solid points reflect equally-sized bins. In all cases, standard errors are clustered on the firm level.

from z to z' induces additional entrepreneurs to start firms without affecting the latent hiring decision D_i^*. By consequence, the marginal entrants in region $\mathbf{B}'$ (panel b) have similar values of V_i^e as the marginal entrants in region $\mathbf{C}$ (panel a), while those in region $\mathbf{C}'$ (panel b) have similar values as the marginal entrants in region $\mathbf{D}$ (panel a). In this fashion, the variation induced by the entry instrument reveals how outcomes differ between marginal and inframarginal entrants while holding fixed network hiring preferences V_i^h. The model uses this information to disentangle endogenous selection into entry from the exogenous shocks to network hiring.

Figure 12 shows how this logic appears in the data for one outcome: total value added. To capture firm performance beyond the initial startup phase, annual value added is measured over years 2-5 after entry. Figure 12a shows that post-reform entrants went on to generate average value added that was roughly \$25K lower than that of pre-reform entrants. Because these effects on the average reflect a change in the composition of entrants, this is consistent with the idea that individuals induced to enter by the reform—those with weaker entrepreneurship preferences, i.e., lower V_i^e—tend to generate lower value added.[34] In other words, entrepreneurs are positively selected

[34] Bacher et al. (2025) also find that the marginal entrants induced by the capital requirement reform

on productivity: those who enter most readily also tend to run higher-output firms.

This positive selection means that simple reduced-form regressions understate the true effect of networks. In Figure 12b, the OLS relationship between value added and the network shock (blue circles) captures both differences in network hiring *and* the greater presence of marginal entrants for larger (negative) shocks. If these marginal entrants have lower baseline outcomes, as emphasized by Figure 12a, then this reduced form is flatter than an "idealized" reduced form that holds the composition of entrants constant. Indeed, adding the control function λ^e—the component corresponding to preferences for entrepreneurship—as a covariate increases the slope over three-fold, as reflected by the red triangles. This implies that the network shock induced a substantial share of marginal entrants with lower baseline outcomes, making it critical to account for this entry to capture the instrument-induced impact of network hiring on firm outcomes. Although this is not itself a structural regression, it highlights how the model accounts for changes in the composition of entrepreneurs in order to isolate the exogenous variation that identifies the causal effect of network hiring.

5.5 Estimation

Estimation proceeds in two steps. First, I estimate the utility cost functions, the relative weight on network hiring for entry decisions, and the joint distribution of latent preferences. These estimates are used to construct control functions. Second, I regress firm outcomes on covariates, the number of ex-coworker hires, and the control functions to recover the outcome parameters.

For tractability in the first step, I model the costs of network hiring to be quadratic in d. This implies marginal utility costs that are linear in d, which I implement as

$$\psi(d, X_i, Z_i^h) = \psi_1(X_i, Z_i^h) + \psi_2(X_i, Z_i^h)d. \tag{13}$$

The function $\psi_1(x, z)$ captures the linear costs of hiring ex-coworkers, while $\psi_2(x, z)$ reflects the quadratic costs of hiring ex-coworkers. To maintain consistency with the ordered choice model, I impose $\psi_2(X_i, Z_i) \geq 0$, ensuring increasing marginal costs. I approximate $\varphi(X_i, Z_i)$, $\psi_1(X_i, Z_i)$, and $\ln \psi_2(X_i, Z_i)$ as linear functions in X_i and Z_i. I also approximate $\ln \omega(X_i)$ and $\tanh^{-1} \rho(X_i)$ as linear in X_i, which guarantees that $\omega(X_i)$ is non-negative and $\rho(X_i) \in (0, 1)$. With these first-step estimates, I construct the control functions $\lambda^e(D_i, X_i, Z_i)$ and $\lambda^h(D_i, X_i, Z_i)$ as described in Appendix B.1.3.

I then use these control functions in the second step, where the goal is to recover

operate smaller firms, which they describe as being optimally small. My interpretation emphasizes how these firms are smaller in absolute terms.

outcome parameters. For tractability in this step, I assume that potential outcomes are quadratic in d with random coefficients that are linear in the latent variables:

$$\mathbb{E}[Y_i(d) \mid X_i, V_i^e, V_i^h] = \sum_{k=0}^{2} d^k \left(X_i' \mu_k + \sum_{\ell \in \{e,h\}} \theta_k^\ell V_i^\ell \right), \tag{14}$$

with X_i being a column vector of covariates that includes a constant. This specification strengthens Assumption 3 by imposing additive separability between observed and unobserved heterogeneity.[35] In my results, I show that my average treatment effect estimates are unchanged if I allow for nonseparability.

The relationship between observable characteristics, unobservable preferences, and potential outcomes is thus parameterized by $\{\mu_k, \theta_k^e, \theta_k^h\}_{k=0}^2$. It follows that

$$\mathbb{E}[Y_i \mid E_i = 1, D_i, X_i, Z_i] = \sum_{k=0}^{2} D_i^k \left(X_i' \mu_k + \sum_{\ell \in \{e,h\}} \theta_k^\ell \lambda^\ell (D_i, X_i, Z_i) \right). \tag{15}$$

This amounts to regressing Y_i on (D_i, D_i^2), covariates X_i and their interactions with (D_i, D_i^2), the control functions λ, and interactions between (D_i, D_i^2) and the control functions. The coefficients μ and θ capture heterogeneity on observable and unobservable characteristics, respectively. I then compute standard errors using a sandwich estimator that accounts for sampling variation in both stages (see, e.g., Hardin, 2002).

6 Effects of Network Hiring on Firm Formation and Performance

The ability to hire ex-coworkers affects both entry decisions and firm outcomes, shaping who becomes an entrepreneur and which firms succeed. Without this ability, a quarter of network-hiring entrepreneurs would not have entered at all. Early-stage ex-coworker hires crowd in additional workers and expand revenues without decreasing per-worker productivity. Individuals strongly self-select into entrepreneurship and network-hiring, making it essential to account for this selection when estimating causal effects.

6.1 Choice parameter estimates

The choice parameters are estimated on a panel of Norwegian adults from 2001-2017. The sample is restricted to individuals who had not been observed owning a business previously during the sample period and who worked as a wage earner in the prior

[35] Additive separability is commonly invoked when studying the relationship between unobserved heterogeneity, selection, and treatment effects (e.g., Dubin and McFadden, 1984; Carneiro et al., 2011; Kline and Walters, 2016; Brinch et al., 2017; Walters, 2018).

year. In total, it includes roughly 2.7 million unique individuals. Covariates include individual age, the log of personal financial wealth in the prior year, and the lagged decile rank of the individual's most recent employer. They also include network characteristics, including the log number of establishment-level coworkers an individual had in the prior five years and the log of average wage earnings within this group. Controls for calendar year are included as described in Section 5.3.1 to accommodate the post-reform instrument for entry. Because covariates are demeaned, the estimated constants can be interpreted as estimates evaluated at covariate means.

The estimates reveal that the ability to hire ex-coworkers shapes entrepreneurial entry. Column (1) of Table 2 shows that the weight on network hiring, $\omega(\bar{X}) = 0.11$, is highly significant. We can thus reject the null hypothesis that entry and network hiring are fully separate decisions. While an entrepreneur's own skills and market opportunities remain the dominant drivers of entry—roughly 10 times more important than networks—the fact that ω is nonzero means that some entrepreneurs start firms only because they can recruit their former coworkers.

This dependence on networks is especially important because the expected utility costs of entry are substantial. Column (2) shows the estimated fixed costs are 2.94 on average, reflecting the low observed entry rate in the population. Wealthier and younger individuals face somewhat lower entry costs, as do those with fewer and higher-earning coworkers. A similar pattern holds for the costs of network hiring, shown in columns (3) and (4). Importantly, however, individuals only hire from their networks when the net benefit of doing so is positive, so even modest gains from network hiring can induce entry.

Moreover, the individuals who have the strongest preferences for network hiring do not always have the strongest preferences for entrepreneurship. This is reflected by the average correlation of 0.38, shown in column (3). This correlation is somewhat higher for older entrepreneurs, as well as those with larger and lower-income networks. But it remains far from perfect, reinforcing the idea that networks can make starting a firm more attractive to individuals who would otherwise have little inclination toward entrepreneurship.

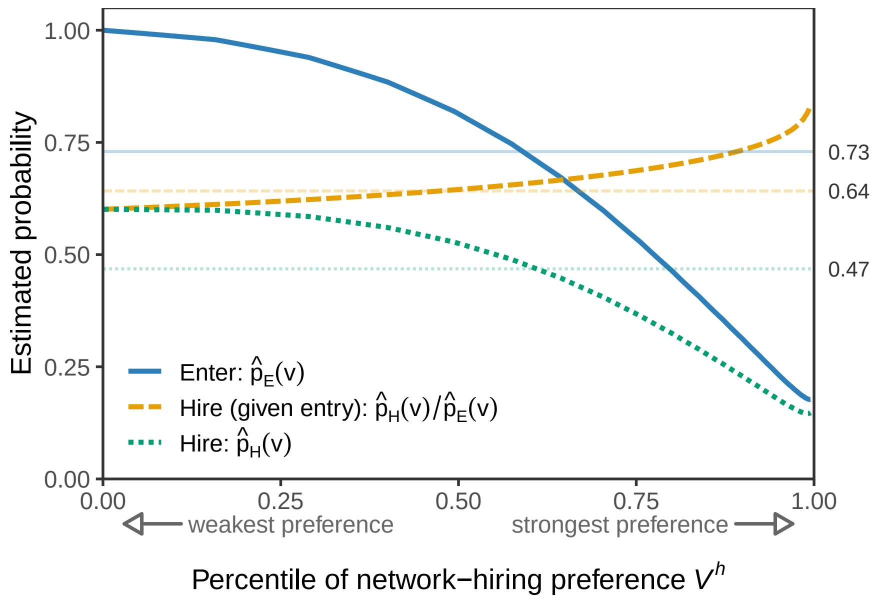
The instruments shift entry and network hiring costs in ways consistent with the reduced form evidence from Section 5.3. Panel B shows that lowering the capital requirement significantly decreased the fixed utility costs of entry, consistent with the reform's intent and the empirical increase in entry rates. Negative shocks to one's own employer also lower entry costs, while negative shocks to coworkers' employers reduce the costs of network hiring. These patterns underscore that the framework is channeling the same variation from Section 5.3 in a manner that allows us to understand

Table 2: Entrepreneurship choice parameter estimates

	Entry			Network hiring	
	Network hiring weight, $\omega(X)$	Fixed cost $\varphi(X, Z^e)$	Latent vars. corr., $\rho(X)$	Linear cost $\psi_0(X, Z^h)$	Quadratic cost $\psi_1(X, Z^h)$
	(1)	(2)	(3)	(4)	(5)
Panel A: Covariates					
Constant ($X = \bar{X}$)	0.108	2.937	0.381	3.277	0.043
	(0.005)	(0.011)	(0.029)	(0.027)	(0.004)
Age	0.001	0.005	0.030	0.019	0.000
	(0.001)	(0.000)	(0.002)	(0.001)	(0.000)
Liquid wealth (log)	-0.002	-0.026	-0.015	-0.023	-0.001
	(0.002)	(0.001)	(0.007)	(0.003)	(0.001)
Num. coworkers (log)	0.005	0.035	0.044	0.059	-0.005
	(0.003)	(0.001)	(0.009)	(0.004)	(0.001)
Avg. coworker wage (log)	0.064	-0.215	-0.091	0.009	-0.009
	(0.007)	(0.004)	(0.034)	(0.027)	(0.002)
Avg. employer decile	-0.034	-0.063	0.171	0.067	-0.034
	(0.034)	(0.015)	(0.117)	(0.067)	(0.012)
Panel B: Instruments					
Lower capital req.		-0.091			
(post-reform)		(0.012)			
Employer shock, self		0.113			
		(0.009)			
Employer shock, network				0.256	0.017
				(0.047)	(0.010)

Notes: This table reports maximum likelihood estimates of the choice parameters. Covariates (with the exception of calendar year) are demeaned so that the constant can be interpreted as the estimate when covariates are set equal to their means. The sample is a panel of Norwegian adults who (i) worked as wage earners in the previous year, (ii) whose previous employer had at least 10 employees, and (iii) who had not started a firm in any prior year during the sample period. The panel covers the years 2001-2017 and includes 2,695,723 individuals (19,366,298 observations). Columns (1), (3), and (5) report marginal effects where all variables are evaluated at their means. In addition to the control variables in Panel A and to accommodate the capital requirement reform, I include separate linear calendar year controls for 2009-2011 and 2012-2014. I include calendar year dummies for all other years. The specification underlying column (2) also includes interactions between the post-reform dummy and the covariates in Panel A. The specification underlying columns (4)-(5) includes similar interactions, but between the network shock and covariates. Standard errors are clustered at the individual level.

Figure 13: Entry and hiring when ex-coworkers aren't an option



Notes: This figure plots counterfactual entry and hiring rates among entrepreneurs who hired at least one ex-coworker. The entry probabilities are computed from the choice parameter estimates in Table 2, holding covariates and instruments at their means. The x-axis is the percentile of network-hiring preference V_i^h among the network-hiring entrepreneurs. The horizontal lines reflect the aggregated counterfactual probabilities.

the underlying choice behavior.

6.2 Effects on entry and early-stage hiring

The previous subsection showed that network hiring affects firm formation. I now ask: *how much* does it matter? To answer this, I compare entrepreneurs' observed choices to what the estimates imply they would have chosen in a world without network hiring.

To measure how much networks affect entry, I compute the counterfactual entry probabilities $p_E(v)$ for entrepreneurs who actually hired ex-coworkers. For a given network-hiring preference $V_i^h = v$, $p_E(v)$ gives the probability that such an entrepreneur would still enter if network hiring were unavailable. To calculate this, I use the estimates from Table 2, but set $\omega = 0$, holding instruments and covariates at their means. Intuitively, $p_E(v)$ answers: among entrepreneurs who did enter with network hires, what fraction would have still started firms without them?

This exercise shows how some entrepreneurs are willing to start firms regardless of whether they can bring former coworkers, while others depend critically on that ability. Figure 13 plots $p_E(v)$ against percentiles of V_i^h, so that the x-axis effectively ranks network-hiring entrepreneurs by the strength of their network-hiring preference.

Those with the weakest preferences derive little value from network hiring and almost always start their firms regardless. But as preferences strengthen, the probability of entry declines sharply: those who value their network hires the most are unlikely to enter without them. Overall, 73% of network hiring entrants are inframarginal and would have entered regardless, while 27% are marginal entrants who only start because they can recruit their ex-coworkers.

To capture further implications for early-stage team formation, I compute the counterfactual employer probabilities, $p_H(v)$. This is the probability that a network-hiring entrepreneur with $V_i^h = v$ would have hired early-stage employees if ex-coworkers were unavailable. Because entry is a precondition for hiring, we necessarily have $p_H(v) \leq p_E(v)$. To estimate $p_H(v)$, I combine the choice estimates from Table 2 with a control function regression, where the outcome is an indicator for hiring any non-coworker employees. Appendix Table A.3 provides the regression estimates in tabular form.

The results reveal that network hiring shapes which new firms hire employees. Alongside the entry probabilities, Figure 13 plots the employer probabilities $p_H(v)$ and the conditional probabilities of hiring for the inframarginal entrants, $p_H(v)/p_E(v)$. On average, only 64% of those who would enter without their ex-coworkers would still hire any workers, and this probability is increasing with the network-hiring preference. This pattern is consistent with networks being most valuable when the entrepreneur's production process requires external labor to operate effectively. Overall, fewer than half (47%) of network-hiring entrepreneurs would both start a firm and employ other workers if they couldn't hire ex-coworkers.

These results show that the ability to hire ex-coworkers is a precondition for many entrepreneurs to enter and hire. The next subsection points to one possible motivation: network hires have large, positive impacts on firm performance.

6.3 Effects on firm performance

I now turn to firm performance: how hiring ex-coworkers in the first year shapes firm scale, output efficiency, and survival over the following four years. To ensure comparability across sectors, I adjust outcomes at each firm age for 2-digit industry.[36] I summarize the results by reporting estimates of the average "treatment" effects on

[36] Outcomes are adjusted at each firm age by subtracting differences between age-specific industry-level outcome means and overall means. This is analogous to including industry-by-age fixed effects, which cannot be included because firm industry is not observed for non-entrants.

the treated entrepreneurs (ATTs), which I define as

$$\text{ATT} = \mathbb{E}\left[\frac{Y_i(D_i) - Y_i(0)}{D_i} \mid E_i = 1, D_i > 0.\right]. \tag{16}$$

These parameters capture the average per-coworker effect of network hiring for entrepreneurs who do so.

Network hires substantially expand firm scale beyond the first year, increasing revenues and total value added while crowding in additional employees. This is evident from columns (1)-(3) in Table 3, Panel A, which provide the estimated ATTs on annual outcomes over firm ages 2-5. Network hiring has an estimated effect of \$269K on revenues, with an effect of \$95K on total value added. The estimated effect on hires (excluding entrepreneurs' ex-coworkers) is 0.727, implying that each ex-coworker hire crowds in close to one additional employee on average. To assess the magnitude of these effects, we can compare them to the average untreated potential outcome, which shows what each entrepreneur would have experienced had they entered but been unable to hire ex-coworkers. This reveals that network hires increase firm scale by more than 40% relative to what the firm would have achieved without those hires. Panel B shows that these effects are declining with the number of network hires, indicating that the initial network hires have the largest impact.

In contrast to the strong effects on firm scale, ex-coworker hires do not alter productivity or long-run viability—at least, not on average. The estimated effects on value added per worker and five-year survival, provided in columns (4)-(5) in Panel A, are close to zero and statistically insignificant. This is noteworthy given the positive raw correlations from Figure 6. This suggests that network hires help entrepreneurs expand without sacrificing output efficiency: they enable scale, but do not fundamentally change the production technology or the quality of the business idea.

These averages, however, conceal important heterogeneity between entrepreneurs, who differ both in their baseline productivity and in how much they gain from hiring ex-coworkers. Panels C and D provide evidence to this point. Panel C shows selection on levels: individuals with stronger preferences for entrepreneurship (higher V_i^e) have higher average outcome levels for all outcomes, consistent with positive (or Roy-style) selection into entrepreneurship. Those with stronger preferences for network hiring (higher V_i^h) are also positively selected, leading descriptive comparisons to overstate treatment effects. In the case of value added per worker and survival, it is entirely this positive selection that drives descriptive differences between entrepreneurs who hire ex-coworkers and those who do not.

Panel D shows how treatment effects vary systematically with latent preferences.

Table 3: Selection-corrected IV estimates of network hiring on firm outcomes

	Outcome, firm ages 2-5				
	Revenues (1000s USD)	Value added (1000s USD)	Employees (excl. network)	VA/worker (1000s USD)	5-year survival
	(1)	(2)	(3)	(4)	(5)
Panel A: Averages, network-hiring entrepreneurs					
ATT of network hiring	268.6***	95.3***	0.727***	0.14	-0.010
	(48.8)	(11.2)	(0.167)	(1.76)	(0.047)
Untreated potential outcome	632.8***	167.2***	2.236***	53.54***	0.695***
	(76.4)	(17.4)	(0.268)	(2.91)	(0.098)
Panel B: Main effects, network-hiring entrepreneurs					
Network hires	331.3***	118.4***	0.986***	0.20	-0.001
	(66.8)	(15.7)	(0.239)	(2.43)	(0.066)
Network hires^2	-29.8**	-11.0***	-0.123**	-0.03	-0.004
	(14.0)	(3.4)	(0.050)	(0.46)	(0.010)
Panel C: Selection on levels					
Preference for network hiring: V^h	75.3**	22.3***	0.351***	8.66***	0.061
	(31.4)	(7.2)	(0.112)	(1.21)	(0.046)
Preference for entrepreneurship: V^e	1,239.8***	177.9***	1.644*	44.11***	0.279
	(241.7)	(57.1)	(0.882)	(12.81)	(0.590)
Panel D: Selection on gains/losses					
Preference for network hiring: V^h	976.8**	161.0*	1.414	8.79	0.164
	(430.5)	(92.7)	(1.217)	(12.15)	(0.183)
Preference for entrepreneurship: V^e	-384.0	-200.0	-6.80***	-43.20**	-0.643**
	(530.0)	(128.8)	(1.86)	(17.10)	(0.283)

Notes: This table reports selection-corrected estimates of the effects of hiring ex-coworkers in a firm's first year on its performance in the following years. In all columns, covariates are the same as in Table 2. Control functions are computed using the parameter estimates from Table 2. Revenues and employment are winsorized at the 99th percentile, and VA/worker is winsorized at the 1st and 99th percentiles. Outcomes are also adjusted for 2-digit industry and firm age. Panel A reports the average treatment effect of the realized first, second, and third ex-coworker hires, which are functions of the estimates in Panels B-D. Panel A also reports the average untreated potential outcome for entrepreneurs with any network hires. Panel B reports the main (uninteracted) coefficients from the specification (15), where covariates and control functions are demeaned within the sample of entrepreneurs who hire at least one ex-coworker so that estimates reflect averages for the treated individuals. Panels C and D report the relationship between unobserved preferences and outcome levels (Panel C) and treatment effects (Panel D), where each entrepreneur receives equal weight. Standard errors are clustered at the firm level and are adjusted for estimation of the control functions. *, **, and *** indicate p-values below 0.1, 0.05, and 0.01, respectively.

Across all outcomes, the point estimates tell a consistent story: entrepreneurs with stronger preferences for network hiring (higher V_i^h) experience the greatest benefits from doing so, particularly for firm output measures (revenues and total value added). The estimates also reveal that the marginal entrepreneurs (lower V_i^e) actually experience greater gains, particularly in employment, value-added per worker, and survival—implying that the near-zero estimates in Panel A mask meaningful heterogeneity, with inframarginal entrants experiencing slight productivity declines. One possible interpretation is that highly productive inframarginal entrepreneurs need outside labor to achieve scale, but relying on others dilutes per-worker output. By contrast, marginal entrepreneurs may benefit from important synergies when hiring ex-coworkers.

Figure 14 illustrates these selection patterns visually, focusing on value-added metrics. The blue lines correspond to outcomes and treatment effects for the inframarginal entrants: those with sufficiently high entrepreneurship preferences V_i^e that they would have started firms even without ex-coworkers. The orange lines correspond to the marginal entrants: those with lower V_i^e who only enter because they can recruit ex-coworkers. The x-axis ranks entrepreneurs within each group (inframarginal or marginal) by the strength of their network-hiring preference V_i^h, so that $x = 0.5$ corresponds to the group median. In this way, the figure highlights unobserved heterogeneity along both dimensions: preferences for entrepreneurship (V_i^e) and preferences for network hiring (V_i^h).

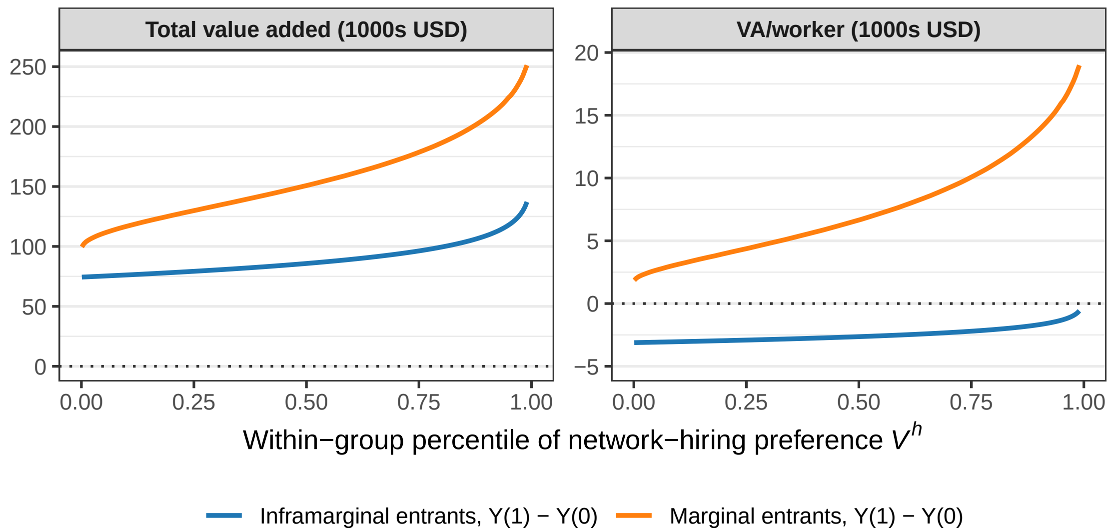
Figure 14a shows how the marginal entrants benefit far more from networks than the inframarginal entrants do. For inframarginal entrants, the effects on value added per worker are slightly negative and relatively flat across the network-hiring preference. By contrast, the marginal entrants experience clear increases in value added per worker. The marginal entrants with the highest network-hiring preferences—and thus the lowest entrepreneurship preferences—experience the greatest gains.

Figure 14b contextualizes these treatment effects by showing the underlying potential outcome levels. Without their networks, the inframarginal entrants would outperform the marginal entrants, consistent with the positive selection into entrepreneurship observed in Panel C of Table 3. The marginal entrants would have relatively low outcomes on their own, but the benefits of their networks draw them closer to the inframarginal entrants.

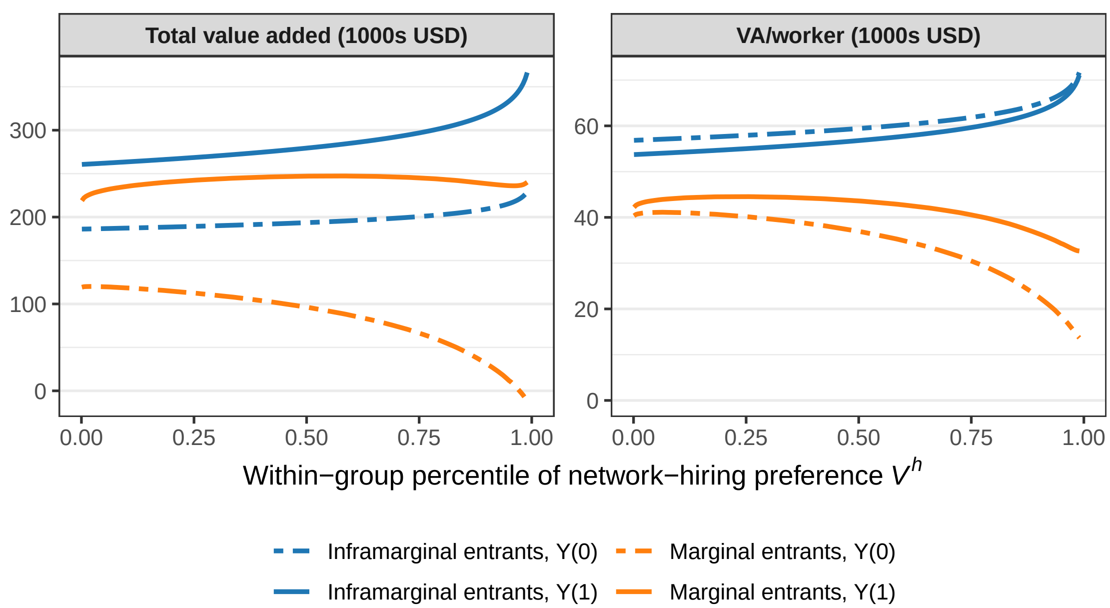
Taken together, the evidence suggests that ex-coworker hires play two related roles. First, they causally expand the scale of inframarginal entrepreneurs, who would have started firms even without those hires. Second, they enable entry and growth for marginal entrepreneurs who would have otherwise remained out of the market.

Figure 14: Heterogeneity by selection into entrepreneurship and network hiring

(a) Heterogeneity in average effects of 1st network hire



(b) Heterogeneity in average potential outcome levels



Notes: These figures plot average treatment effects and potential outcomes conditional on the latent network-hiring preference V_i^h, separately for marginal and inframarginal entrants. These averages are conditional on having $D_i > 0$ and $E_i = 1$, and they are computed fixing covariates and instruments at their mean values. Inframarginal entrants are those who would start firms without ex-coworkers, with $V_i^e \geq \varphi(\bar{X}, \bar{Z})$. Marginal entrants are those who would only start firms given the ability to recruit ex-coworkers, with $\sum_{d=1}^{D_i} \psi(d, \bar{X}, \bar{Z}) - D_i V_i^h \leq V_i^e - \varphi(\bar{X}, \bar{Z}) < 0$. In both panels, the x-axis is the percentile of V_i^e within each group (marginal or inframarginal entrant). Panel (a) plots the estimated average treatment effects $Y_i(1) - Y_i(0)$ of the first network hire. Panel (b) plots the estimated average potential outcomes $(Y_i(0), Y_i(1))$.

6.4 Robustness to alternative samples and specifications

Table 4 examines how sensitive the main estimates are to alternative specifications and sample restrictions. Column (1) reproduces the baseline results for reference, with Panel A reporting the key choice parameters from Table 2 and Panel B reporting the ATT estimates from Table 3. Column (2) estimates a nonseparable version of the model that allows for richer heterogeneity by interacting covariates with instruments in the choice model and with control functions (and their interactions with D_i and D_i^2) in the second step. The results are nearly identical to the baseline, suggesting that the functional form restrictions in the separable specification are not driving the findings.

Columns (3) and (4) retain the baseline specification but apply sample restrictions to test whether the network shock instrument might be contaminated by competitive interactions between the entrant and the incumbent. The estimated effects remain stable across these samples, consistent with the assumption that the network shock captures variation in the entrepreneur's labor market rather than in the product market.

6.5 Comparison to OLS and 2SLS

Table 5 contrasts the baseline estimates with those from conventional OLS and linear IV (2SLS) approaches. The OLS estimates are uniformly larger, consistent with positive selection into network hiring: entrepreneurs who hire ex-coworkers tend to operate larger and more productive firms, even without those hires. Ignoring this selection inflates observational comparisons, particularly for value added per worker and survival, where the OLS coefficients are large and significant despite the average causal effects being close to zero. In short, OLS attributes the success of inherently stronger entrepreneurs to network hiring itself.

By contrast, 2SLS estimates that use the network shock as an instrument, ignoring endogenous entry, produce treatment effects that are almost uniformly smaller than the baseline. This pattern reflects the mechanism discussed in Section 5.4: 2SLS fails to account for marginal entrants who only start firms because network shocks make it easier to recruit ex-coworkers. These marginal entrepreneurs have lower baseline outcomes, which IV comparisons do not account for, biasing treatment effects downward.[37]

It is instructive, though not theoretically justified within the partially ordered choice framework, to include the control function component λ^e as an additional covariate in 2SLS estimation. Because λ^e captures latent preferences for entrepreneurship, adding

[37] The linear IV estimator has other interpretation challenges due to the presence of covariates (Słoczyński, 2024; Blandhol et al., 2025). I abstract away from these challenges in this discussion.

Table 4: Robustness checks

	Baseline (1)	Nonseparable specification (2)	Larger incumbents (3)	Different industry (4)
Panel A: Choice parameters				
Network-hiring weight for entry, $\omega(X)$	0.108***	0.108***	0.125***	0.113***
	(0.005)	(0.005)	(0.006)	(0.006)
Latent vars. corr. $\rho(X)$	0.381***	0.388***	0.354***	0.363***
	(0.029)	(0.028)	(0.034)	(0.034)
Lower capital req. (post-reform)	0.091***	0.116***	0.124***	0.086***
	(0.012)	(0.012)	(0.013)	(0.013)
Employer shock, network	-0.026***	-0.040***	-0.026***	-0.015***
	(0.005)	(0.007)	(0.006)	(0.005)
Panel A: ATT estimates, network hiring				
Revenues (1000s USD)	268.6***	273.1***	227.3***	313.1***
	(48.8)	(59.8)	(43.6)	(54.6)
Total VA (1000s USD)	95.3***	103.4***	89.3***	111.4***
	(11.2)	(13.8)	(12.4)	(13.3)
Employees (excl. network)	0.727***	0.752***	0.809***	0.966***
	(0.167)	(0.195)	(0.182)	(0.194)
VA/worker (1000s USD)	0.14	4.11**	1.65	1.40
	(1.76)	(1.99)	(1.99)	(1.99)
5-year survival	-0.010	0.000	0.009	0.014
	(0.047)	(0.059)	(0.035)	(0.051)
Num. observations	37,580	37,580	30,008	31,019

Notes: This table compares the baseline selection-corrected estimates to an alternative specification and additional sample restrictions. The first column reproduces the ATT estimates from Table 3, Panel A. The second column re-estimates the model using a nonseparable specification that interacts covariates (excl. calendar year) with instruments in the first step, and interacts those same covariates with treatment-control function interactions in the second step. The third column uses the baseline specification, excluding from the network shock instrument any network members whose current employer employs fewer than 25 workers. The fourth column uses the baseline specification, excluding any entrepreneurs who entered the same industry as their most recent employer. Estimates are evaluated setting covariates and instruments equal to their means. Standard errors are clustered at the firm level and are adjusted for estimation of the control functions. *, **, and *** indicate p-values below 0.1, 0.05, and 0.01, respectively.

Table 5: Comparison to alternative estimation methods

	Outcome, firm ages 2-5				
	Revenues (1000s USD)	Value added (1000s USD)	Employees (excl. network)	VA/worker (1000s USD)	5-year survival
	(1)	(2)	(3)	(4)	(5)
Baseline	268.6***	95.3***	0.727***	0.14	-0.010
	(48.8)	(11.2)	(0.167)	(1.76)	(0.047)
OLS	395.5***	124.7***	1.127***	11.15***	0.060***
	(16.5)	(3.9)	(0.057)	(0.54)	(0.006)
2SLS	25.5	61.7**	0.045	9.72	-0.058
	(128.2)	(29.7)	(0.395)	(7.49)	(0.073)
2SLS (entry control)	397.4***	173.8***	0.720**	21.58***	0.041
	(110.7)	(34.2)	(0.293)	(6.25)	(0.052)
Model-implied ACR	378.3	112.9	1.008	0.97	0.037

Notes: This table compares the selection-corrected estimates to ordinary least squares and linear IV (2SLS) estimates. The first row reproduces the ATT estimates from Table 3, Panel A. The second row provides an OLS estimate of the same parameter, using the same specification but omitting the control functions. The third row estimates 2SLS using the same covariates, where the network shock is used as an instrument for ex-coworker hiring. The fourth row estimates the same 2SLS specification, but including the control function component λ^e as an additional covariate. The final row simulates the ACR implied by the baseline estimates, defined in the same manner as the ATT (as in (16)) but restricted to individuals whose hiring decision would be affected if the network shock moved from +1 standard deviation to -1 standard deviation. Standard errors are clustered at the firm level. *, **, and *** indicate p-values below 0.1, 0.05, and 0.01, respectively.

it as a control demonstrates how accounting for lower baseline outcomes of marginal entrants changes the estimated treatment effects. The resulting estimates are substantially higher than 2SLS that ignores endogenous entry, indicating that those 2SLS estimates are driven largely by differences between marginal and inframarginal entrepreneurs rather than by the causal effects of network hiring.

An additional difference between the baseline estimates and 2SLS is one of interpretation. In an idealized setting, 2SLS identifies a local average causal response (ACR) for "compliers" whose hiring decisions are influenced by the instrument (Angrist and Imbens, 1995). By contrast, the baseline estimates capture average treatment effects across all network-hiring entrepreneurs (ATTs), including those "always-takers" who would hire ex-coworkers regardless of the network shock. To provide a more direct comparison for 2SLS, I simulate the ACR implied by my model estimates, defined analogously to the ATT but restricted to the compliers whose hiring decision would be affected by a shift in the network shock from one standard deviation above to one below its mean. The resulting local effects are quite similar to the entry-controlled 2SLS estimates, both of which typically exceed the baseline ATT estimates. This pattern suggests that many compliers are also marginal entrepreneurs, who, as shown in Table 3, benefit the most from network hiring.

Empirical studies of entrepreneurship often use instrumental variables to estimate the effects of endogenous treatments on new firm outcomes. Examples include work examining how employee experience (Jara-Figueroa et al., 2018), patented innovations (Kato et al., 2022), or the availability of external financing such as private equity (Colombo and Grilli, 2005) and venture capital (Bertoni et al., 2011; Nanda and Rhodes-Kropf, 2013; Eldar and Grennan, 2023) affect firm performance and survival. Yet in all of these settings, the treatments of interest may also influence the decision to start a firm in the first place. My results show that when this occurs, standard IV estimates conflate the treatment's causal effect on firm outcomes with compositional changes in who becomes an entrepreneur. Studies of entrepreneurship that ignore the linkage between treatment and entry risk misinterpreting the magnitude of causal effects—likely understating them if the marginal entrants operate smaller, more fragile, or less productive firms.

7 Network vs. Subsidy-Induced Entrepreneurship

A key focus of entrepreneurship policy is not only to increase entry rates, but to promote entry of productive entrepreneurs. This raises a natural question: how effective are networks at inducing entry of productive firms compared to policies that lower entry barriers more generally? To answer this question, I compare the marginal entrants from a targeted subsidy that replicates network-induced entry with an untargeted, fiscally equivalent subsidy that increases entry more broadly. The comparison reveals that network-induced entrepreneurs are more productive, create more jobs, and survive longer than those induced by entry subsidies.

7.1 Counterfactual subsidy regimes

To benchmark the performance of network-induced entrepreneurs against those who might enter under alternative policies, I simulate two counterfactual subsidy regimes in an environment where entrepreneurs cannot hire their former coworkers.

The first regime is an infeasible, targeted subsidy designed to induce the same marginal entrants who currently rely on network hiring to enter—*without* those network hires. The outcomes for these entrepreneurs correspond directly to their untreated potential outcomes:

$$\mathbb{E}\left[Y_i(0) \mid V_i^e \in \left[\varphi(X_i, Z_i^e) + \omega U_i^h(D_i),\, \varphi(X_i, Z_i^e)\right)\right], \tag{17}$$

where $U_i^h(d)$ is the additional utility from network hiring as given by (6). These averages can be simulated directly from the estimated choice parameters from Table 2 and the second-stage outcome regressions underlying Table 3, without placing a monetary scale on utility.

While the relevant untreated outcomes can be identified without monetizing utility, translating the targeted subsidy into monetary terms allows for a clean, fiscally-equivalent comparison with a flat subsidy regime. To do so, I assume that the fixed utility costs of entry are logarithmic in the potential funds the entrepreneur could devote to their business. Letting $\tilde{Z}_i^h$ denote the capital requirement in levels and $s \geq 0$ denote the size of a subsidy, take

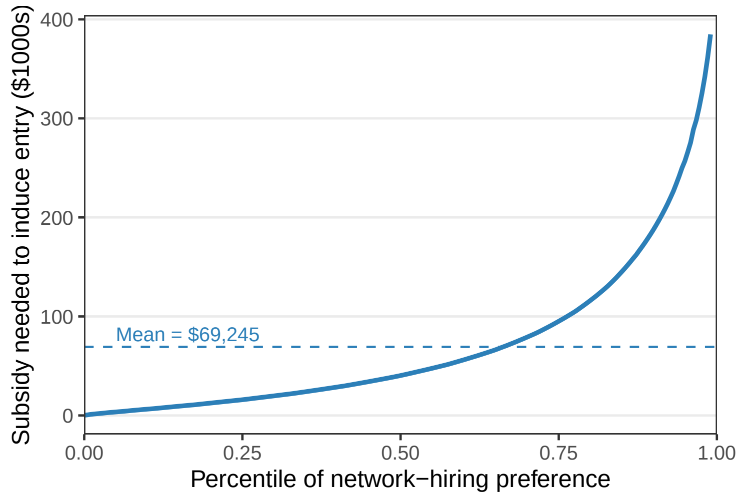
$$\varphi(X_i, Z_i^e; s) = \varphi_0(X_i) + \zeta g(X_i, \tilde{Z}_i^h, s) \tag{18}$$

$$\text{where} \quad g(x, z, s) = \ln\left(h(x) - \tilde{Z}_i^h + s\right). \tag{19}$$

I set $h(x)$ equal to the sum of the entrepreneur's liquid wealth and \$50K, an amount intended to represent the public subsidies currently available to Norwegian entrepreneurs.[38]

[38] Kisseleva (2024) uses data on all Norwegian corporations from 2001-2018 and finds that the median grant applicant received NOK 920,000 (roughly \$165,000 in 2012 USD) from Innovation Norway, which provides startup grants. However, grants are not universal: many firms do not apply, and about 30% of applicants are rejected. The choice of \$50,000 as an illustrative "expected" subsidy does not affect the qualitative takeaways from the comparison, which are driven by the positive selection into entrepreneurship and network hiring

Figure 15: Targeted subsidy for network-hiring marginal entrants



Notes: This figure plots the average subsidy value S_i^* (in thousands of USD) needed to induce entry by network-hiring marginal entrants, if network hiring were not an option. Subsidy values are computed using the choice parameter estimates and the utility-to-money parameterization given by (19), fixing covariates and instruments at their mean values. The x-axis is the percentile of network-hiring preference V_i^h among the network-hiring entrepreneurs. The horizontal line reflects the overall average.

Given this parameterization, I solve for the targeted subsidy S_i^* that makes the marginal network-hiring entrants indifferent between entry and non-entry:

$$V_i^e = \varphi(X_i, Z_i^e; S_i^*). \tag{20}$$

Figure 15 plots the distribution of S_i^* across these marginal entrepreneurs. Those with weaker preferences for network hiring require only small subsides to enter, while those in the top quartile of preferences require six-figure transfers. On average, the targeted subsidy is approximately $69,000.

Although these targeted subsidies are not feasible in practice, their aggregate fiscal cost provides a benchmark for an alternative, flat (and therefore feasible) subsidy $\bar{s}$ of equal cost. This flat subsidy is paid to all entrants: the inframarginal entrepreneurs, who would enter regardless, and the marginal entrants induced by the subsidy. The outcomes for those marginal entrants are

$$\mathbb{E}\left[Y_i(0) \mid V_i^e \in \left[\varphi(X_i, Z_i^e; \bar{s}),\, \varphi(X_i, Z_i^e; 0)\right)\right], \tag{21}$$

where $\bar{s}$ is calibrated so that the total cost of the program—including transfers to

shown in Table 3.

Table 6: Simulated outcomes for different marginal entrants

		No networks	
	Networks (1)	Targeted subsidy (2)	Flat subsidy (3)
Panel A: Policy costs and entry effects			
Avg. subsidy	0	69,245	1,974
Avg. cost per induced entrant	0	69,245	46,214
Increase in marginal entry	—	—	49.8%
Panel B: Outcomes, marginal entrants			
Total value added (1000s USD)	239.9	55.7	38.6
Total employees	5.44	1.13	0.68
VA/worker (1000s USD)	38.89	27.91	14.10
5-year survival rate	0.700	0.537	0.431

Notes: This table provides details on simulated costs, entry effects, and outcomes for marginal entrepreneurs under different policy regimes: (1) the ability to hire ex-coworkers (status quo), (2) a subsidy targeted to those who would hire ex-coworkers if permitted, and (3) a flat subsidy of the same fiscal cost as (2) that is provided to all entrants. Simulations are evaluated setting covariates and instruments to their mean values.

both inframarginal and newly induced entrants—equals the total cost of the targeted subsidies for the network-hiring entrepreneurs. Table 6, Panel A provides details on the resulting flat subsidy. This subsidy itself is \$1,970, which induces 45% more marginal entrants than the targeted subsidy. Because it is paid to inframarginal entrants as well, the total cost per induced entrant is roughly \$46,200.

7.2 Comparing marginal entrants

Table 6, Panel B summarizes how the performance of marginal entrants differs across the three policy regimes: networks, the targeted subsidy, and the fiscally equivalent flat subsidy. The first comparison, between columns (1) and (2), captures the direct causal effect of network hiring for the same set of marginal entrants—the original set of individuals who are induced to start firms because of their networks. When these entrepreneurs are prevented from hiring their ex-coworkers, their firms produce substantially less output, employ fewer workers, and are less likely to survive. This reiterates how network access is a key input for these entrepreneurs: without it, their businesses perform substantially worse.

Comparing those network-induced entrepreneurs to the entrants drawn in by a flat entry subsidy reveals a sharp difference in selection. Although the flat subsidy induces more entrepreneurship, those firms tend to be smaller, less productive, and less likely to survive. By contrast, the entrepreneurs who would have entered because of their ex-coworkers are stronger even without those network hires: their firms are over 40 percent larger, with value added per worker nearly twice as high and survival rates roughly 10 percentage points greater. This pattern suggests that networks encourage entry among capable, higher-quality entrepreneurs, while the average firms induced by broad subsidies are smaller and more fragile.

7.3 Discussion

Policymakers and researchers have long recognized entrepreneurship as a key source of innovation, job creation, and productivity growth (e.g., Haltiwanger et al., 2013, 2016; Akcigit and Ates, 2021), motivating substantial interest in ways to lower barriers to entrepreneurship.[39] Both research and policy initiatives have primarily emphasized liquidity constraints—the extent to which potential entrepreneurs lack the capital to start firms (see, e.g., Kerr and Nanda, 2011). The results here point to a different friction: the difficulty of finding suitable employees. Moreover, the evidence shows that relaxing financial versus hiring frictions attracts fundamentally different types of entrepreneurs. Broad entry subsidies increase the number of entrepreneurs, but they may draw in firms with relatively low productivity. By contrast, network hiring enables entry by entrepreneurs who are more productive but constrained in their ability to recruit suitable workers. In doing so, network hiring mitigates hiring frictions that limit productive entrepreneurship.

Networks themselves are not a policy instrument, but the importance of network hiring points to the types of frictions that policy can affect. Restrictions that limit entrepreneurs' ability to recruit former coworkers, such as non-compete agreements and other barriers to employee mobility, may inadvertently suppress the entry of high-potential entrepreneurs (Starr et al., 2019; Marx, 2022). Ultimately, the scope for proactive entrepreneurship policy depends on the mechanisms that drive network hiring, which I explore in the next section.

[39] Public policy in many industrialized economies explicitly aims to encourage firm entry by offering grants, loans, and tax incentives for new firms (OECD, 2023). Examples include Norway's Innovation Norway startup grants, the United States Small Business Administration programs, and the European Innovation Council's entrepreneurship initiatives.

8 Why Do Entrepreneurs Hire from Their Networks?

Having shown that network hiring shapes firm entry and performance, I now ask: *why* do entrepreneurs hire from their networks? Understanding the underlying mechanisms is essential for interpreting these results and for guiding entrepreneurship policy. To explore this question, I extend the stylized model from Section 2 to incorporate several potential motives—taste-based hiring, liquidity motives, and quality-related considerations in the form of private information and relationship capital—and derive testable predictions. The evidence rules out non-pecuniary and liquidity motives as primary drivers, instead pointing to entrepreneurs hiring from their networks to exploit private information about their coworkers' abilities.

8.1 Competing motives: quality, utility, liquidity

The model from Section 2 emphasized one potential mechanism, which was the private information an entrepreneur has about her former coworker. I now extend the stylized model to incorporate two additional motives unrelated to coworker quality: a taste-based motive, where the entrepreneur derives utility from working with a friend, and a liquidity motive, where the coworker accepts a lower wage than would be offered by incumbent employers.

For expositional simplicity, consider the case in which entrepreneur i would have hired worker j from the market if she did not hire her network member k. (Allowing for the no-hire option does not change the theoretical implications.) If we abstract away from observable characteristics, the entrepreneur's expected net surplus from network hiring is equivalent to the network hiring preference V_i^h from the empirical framework:

$$V_i^h = \Delta\hat{\beta}_i + u_i - \Delta w_i \tag{22}$$

$$\text{where} \quad \Delta\hat{\beta}_i \equiv \mathbb{E}[\beta_{ik} \mid \theta_k] - \mathbb{E}[\beta_{ij} \mid \theta_j \leq w_i^*] \tag{23}$$

$$\text{and} \quad \Delta w_i \equiv (1 - \tau_{ik})\theta_k - w_i^*, \tag{24}$$

with w_i^* denoting the posted wage that maximizes the market-hiring surplus, $\mathbb{E}[\beta_{ij} \mid \theta_j \leq w_i^*] - w_i^*$. The term $\Delta\hat{\beta}_i$ measures the expected productivity advantage of hiring the coworker relative to a market hire. The difference Δw_i captures the wage cost of hiring the coworker relative to a market hire, where τ_{ik} reflects any wage discount offered by the coworker. The random variable u_i captures the entrepreneur's non-pecuniary utility from working with their coworker. Together, these components highlight three possible drivers of network hiring: quality ($\Delta\hat{\beta}_i$), utility (u_i), and liquidity (Δw_i).

Maintaining the standard normal assumption on V_i^h and leaving dependence on V_i^e

implicit, the relationship between entrepreneurs' network-hiring preferences and the effects on firm output can be written as:

$$\mathbb{E}\left[\frac{\partial \mathbb{E}[\Delta\beta_i \mid V_i^h = v]}{\partial v}\right] = \underbrace{\mathrm{Cov}(\Delta\beta_i, \Delta\hat{\beta}_i)}_{\text{quality motive}} + \underbrace{\mathrm{Cov}(\Delta\beta_i, u_i)}_{\text{utility motive}} - \underbrace{\mathrm{Cov}(\Delta\beta_i, \Delta w_i)}_{\text{liquidity motive}}, \tag{25}$$

where $\Delta\beta_i \equiv \beta_{ik} - \beta_{ij}$ is the true productivity gain from hiring the coworker.

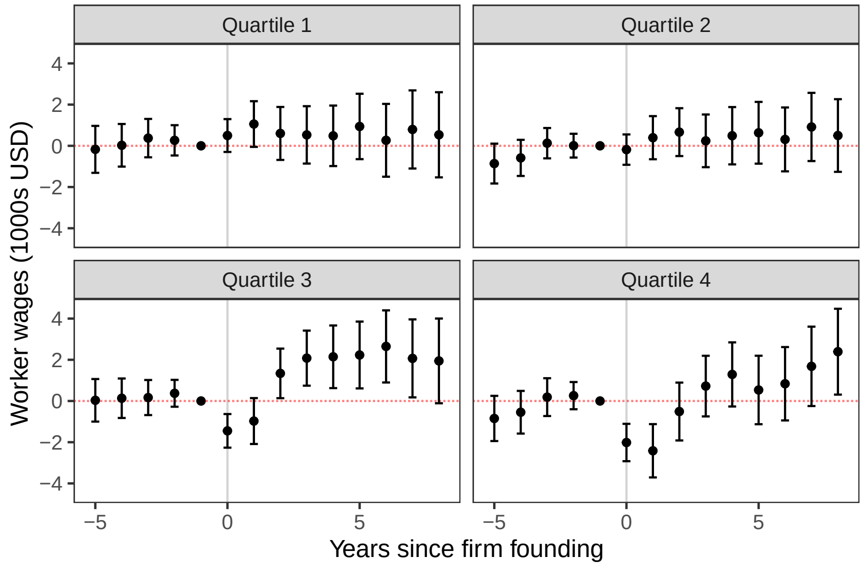
This decomposition clarifies how different motives affect the slope of marginal treatment effects with respect to entrepreneurs' preferences for network hiring. Under unbiased beliefs, the first covariance is positive, so stronger quality motives should increase the slope. The second covariance is expected to be negative: if utility substitutes for quality, taste-based hiring should flatten or reverse the slope.[40] The third covariance is ambiguous: more talented coworkers tend to demand higher wages, which would lower the slope, but this could be offset or reversed if coworkers are willing to accept large wage discounts (large τ_{ik}).

Although the sign of the slope alone cannot fully distinguish between these motives, it provides guidance about which forces dominate. A positive relationship between effects on firm performance and network hiring preferences (i.e., selection on gains) would imply that the utility motive cannot be the dominant driver of network hiring. By contrast, a negative relationship (i.e., selection on losses) would rule out quality motives as dominant. In either case, the direction of selection is not informative about the liquidity motive, which I examine directly in the next subsection.

The derivative (25) corresponds empirically to selection on gains/losses with respect to the network hiring preference, V_i^h. These estimates were reported earlier in Table 3, Panel D. The outcome in the stylized model is firm income, which corresponds most closely to the empirical outcomes of revenues and total value added. For both of these, the estimated slope is positive: entrepreneurs with stronger preferences for network hiring are precisely those whose firms benefit the most from doing so. This pattern rules out taste-based motives as a first-order driver of network hiring. Instead, it supports a quality-based interpretation in which entrepreneurs rely on their networks to identify and recruit high-performing coworkers.

[40] In principle, $\mathrm{Cov}(\Delta\beta_i, u_i)$ could be positive if tastes and ability are complements—that is, entrepreneurs happen to enjoy working with their most talented peers. This, however, contrasts with standard models of taste-based hiring, in which productivity is sacrificed to satisfy social preferences (Becker, 1971; Goldberg, 1982).

Figure 16: Wage responses for network hires



Notes: This figure plots event study estimates of the effect on ex-coworkers of moving to the entrepreneur's firm, separately by the $t = -1$ financial wealth quartile of the entrepreneur. Event time $t = 0$ is the year of firm founding, and the treated group includes network members who were hired by the end of $t = 1$. Treated workers are not required to stay at the entrepreneur's firm. The control group includes network members who worked with the entrepreneur in the same year but were not hired. Covariates include dummies for worker age, years of schooling, and calendar year. The sample includes N = 4,079 network hiring firms. 95% confidence intervals are based on standard errors that are clustered on the entrepreneur level.

8.2 Liquidity motives

The prior subsection showed that the relationship between firm performance and network hiring preferences is inconsistent with a taste-based motive, while the role of liquidity motives remains theoretically ambiguous. Unlike coworker quality and the entrepreneur's utility, however, the wages of network hires are observable in the data. This allows for a direct empirical test of whether these hires accept wage discounts when joining a friend's firm.

To test whether $\tau_{ik} > 0$—that is, whether network hires accept wage discounts relative to their outside options—I examine whether ex-coworkers experience wage declines upon moving to the entrepreneur's new firm. For each hired ex-coworker, I construct a control group consisting of the same entrepreneur's other former coworkers who were not hired. I estimate wage dynamics separately by quartiles of the entrepreneur's pre-entry liquid wealth, under the intuition that if liquidity constraints drive network hiring, wage declines should be larger (i.e., τ_{ik} higher) for the network hires of entrepreneurs with less wealth (Evans and Jovanovic, 1989; Hurst and Lusardi, 2004).

Figure 16 shows that the ex-coworker hires of lower-wealth entrepreneurs exhibit no wage declines, where we cannot reject the null hypothesis that $\tau_{ik} = 0$. By contrast, the network hires of wealthier entrepreneurs experience modest wage declines, on the order of \$1,500-\$2,000 in the initial years. It seems unlikely that only higher-wealth entrepreneurs would rely on their networks due to a liquidity motive, particularly given the small magnitudes of the wage declines. One more plausible explanation is that coworkers of wealthier entrepreneurs are more willing to trade off wages for non-wage amenities—such as job autonomy or equity potential—in place of monetary compensation.[41]

In summary, the wage evidence is inconsistent with liquidity motives as a first-order driver of network hiring. Having also ruled out taste-based motives, the evidence points to quality-based mechanisms, which could arise through private information about coworkers' abilities or through relationship capital. I examine these channels next.

8.3 Testing for private information

I divide quality-based motives into two distinct channels. The first is private information, where the entrepreneur knows more about her coworker's abilities than she does about the abilities of other job candidates. The second is relationship capital, where a shared history of working together improves match quality—for example, by improving communication, coordination, or trust.

Both channels imply benefits from network hiring, but they differ in their implications. If private information matters, then policies that improve labor market signaling, lower screening costs, or expand professional networks may promote entrepreneurship. If quality instead operates entirely through relationship capital, then the benefits of network hiring reflect accumulated experiences that policy cannot as easily replicate.

To assess whether private information drives network hiring, I draw again on insights from the stylized model. In Appendix C.2, I extend the model to incorporate relationship capital directly. The extended model shows that, even when relationship capital is present, private information implies three testable predictions:

P1. Entrepreneurs hire less from the broader labor market when potential match values at the new firm are more dispersed.

[41] Standard theories of compensating differentials emphasize that workers are willing to accept lower wages in exchange for higher non-wage amenities (Rosen, 1986), particularly if those individuals are themselves wealthier (Weiss, 1976). Empirical evidence shows that workers often move to lower-paying firms to obtain such compensating differentials (Sorkin, 2018). Moreover, many workers value alternative work arrangements (Mas and Pallais, 2017), which is a specific compensating differential that some entrepreneurs may be well-suited to provide.

P2. Entrepreneurs hire more higher-quality ex-coworkers as dispersion increases.

P3. The strength of this response grows with coworker ability.

The intuition is straightforward: when productivity at the new firm is highly variable, the downside risk of hiring a poor worker rises, as does the benefit of hiring a good one. This makes hiring unfamiliar or lower quality workers less attractive, while increasing the value of private information about high quality coworkers.

To bring these predictions to the data, I construct an empirical proxy for this variance in worker productivity that corresponds roughly to how much "ability matters" on the job. To construct this proxy, which I refer to as the Performance Sensitivity Index (PSI), I use occupational requirements from O*NET. Specifically, I focus on the proficiency levels required in cognitive and problem-solving domains, such as critical thinking, judgment and decision making, deductive/inductive reasoning, and active learning. These level ratings capture *how much* of each ability a worker must possess to perform effectively, rather than simply whether the ability is important for the job. I standardize these levels across occupations and average them within each occupation to obtain a composite index (see Appendix D for details). Similar indices have been constructed in other studies to examine the task content and skill demands of occupations (e.g. Autor et al., 2003; Deming, 2017).

The resulting PSI captures the steepness of the mapping from worker ability to productivity. When this mapping is steeper, small differences in underlying ability translate into large productivity differences—suggesting greater dispersion in, and uncertainty about, match values. Occupations with high PSI values (e.g., engineer, accountant, salesperson) therefore correspond to settings where productivity is likely to vary more across potential workers, while in low PSI occupations (e.g., retail clerks, delivery drivers, or construction laborers), productivity is expected to be more uniform. Although PSI is not a perfect measure of match value dispersion, the key is that it captures cases where information about worker ability is particularly valuable, in ways plausibly unrelated to one's past history of co-working (i.e., relationship capital).

Because many new firms do not immediately hire workers, I aggregate PSI to the 2-digit industry-year level, assigning each entrepreneur the modal occupation among workers hired by new firms in that cell. By so doing, I capture the typical hiring environment facing new entrants in a given sector and year, regardless of their ultimate decision to hire or not. I then regress early-stage hiring on this measure. Prediction P1 implies that if the returns to hiring the right worker are steeper (i.e., high PSI) such that productivity is more dispersed, entrepreneurs face greater uncertainty about potential hires. Hiring from the broader labor market becomes riskier, so we should observe fewer initial hires on average.

The first row in Table 7 confirms this prediction: the relationship between PSI and the total number of early-stage hires at new firms is negative and highly significant. This result is robust to including a range of controls that capture factors potentially correlated with both PSI and hiring behavior. I control for a routine-task index following Autor et al. (2003), since more routine occupations are less dependent on cognitive ability and are more easily automated. I also include a measure of labor market thickness—the number of workers in the modal occupation each year—to account for the possibility that it is simply harder to find workers for high PSI occupations. I control for average employment among "mature" (5+ year-old) firms to capture typical differences in firm scale across industries. Finally, I control for the entrepreneur's own prior salary, which may reflect their own ability or financial resources. Across all specifications, the estimated coefficient on PSI remains negative and highly significant.[42]

The second row of Table 7 repeats the exercise using the number of early-stage ex-coworker hires as the outcome, conditional on total hiring. Here, the relationship reverses: entrepreneurs who would hire workers into high PSI occupations rely more heavily on their former coworkers. If network hiring were driven solely by relationship capital, such that private information was irrelevant, this slope would be flat.

The model predicts that the slope should vary systematically with the quality of network hires. When productivity is more dispersed, entrepreneurs should rely more on high-ability ex-coworkers (prediction P2) and relatively less on low-ability ones (prediction P3). To examine whether this is the case, I first residualize wages within the full employer-employee panel to remove age, experience, industry, and calendar year effects. I then classify ex-coworker hires into two groups: those with lagged residualized wages above the sample mean (58% of these hires) and those below the sample mean (42% of these hires). I then estimate separate regressions, using the number of above- and below-average ex-coworker hires as outcomes.

The results, reported in the third and fourth rows of Table 7, again align closely with the model's predictions. The estimated coefficient on PSI is positive and significant only for the above-average ex-coworker hires. For the below-average hires, the slope declines dramatically and is statistically insignificant. This asymmetry would arise naturally if private information matters: as the dispersion of potential match values increases, the expected payoff from hiring a high-ability coworker rises, while the benefit

[42]Appendix Figure A.5 provides a visual representation of the regression results from Table 7, column (5). The figure illustrates that the estimated relationships hold throughout the distribution of PSI and are not driven by outliers.

Table 7: Early-stage hiring and performance sensitivity index

Outcome:	(1)	(2)	(3)	(4)	(5)
Total early-stage hires	-0.742***	-0.987***	-0.885***	-0.903***	-0.913***
	(0.146)	(0.207)	(0.203)	(0.197)	(0.200)
Num. ex-coworker hires	0.150***	0.229***	0.195***	0.192***	0.184***
	(0.028)	(0.042)	(0.041)	(0.041)	(0.039)
Num. ex-coworker hires	0.132***	0.204***	0.182***	0.181***	0.175***
(above avg. "quality")	(0.022)	(0.032)	(0.031)	(0.031)	(0.029)
Num. ex-coworker hires	0.018	0.024	0.013	0.012	0.009
(below avg. "quality")	(0.012)	(0.018)	(0.018)	(0.018)	(0.017)
Additional covariates:					
Routine task index		✓	✓	✓	✓
Num. workers with modal occupation			✓	✓	✓
Avg. employment level at older firms				✓	✓
Entrepreneur salary (lagged)					✓

Notes: This table provides OLS regression coefficients of early-stage hiring outcomes on the performance sensitivity index (PSI). In order to include firms that do not hire, PSI is assigned to firms based on the modal occupation at new firms in each 2-digit industry in each calendar year. Workers are divided into "quality" groups based on log wages, which are residualized on dummies for calendar year, age, years of schooling, years of experience, and 2-digit industry of employment. Using residuals in the year before the new firm was started, 42% of ex-coworker hires have below average residualized wages, while 58% have above average residualized wages. All columns control for calendar year dummies and the number of ex-coworkers (log, including separate controls for above average and below average ex-coworkers). For network hiring outcomes, the regressions also control for the total number of early stage hires (log). With the exception of the routine task index, additional covariates are specified in logs. Standard errors are clustered on the 2-digit industry-by-year level. *, **, and *** indicate p-values below 0.1, 0.05, and 0.01, respectively.

of hiring a low-ability coworker does not. Entrepreneurs respond by becoming more selective, relying more on the ex-coworkers they believe to be the most capable.

Collectively, these patterns are most consistent with private information playing a central role: when the returns to ability are high but information about worker quality is limited, entrepreneurs selectively recruit their most capable former coworkers.

8.4 Discussion

Taken together, the evidence points to quality-based motives as the primary reason entrepreneurs hire from their networks, with private information about coworkers' abilities playing a central role. Entrepreneurs appear to rely on former coworkers not because of financial constraints or social preferences, but because networks help them overcome information frictions in the labor market. In conjunction with the evidence from Section 7, this suggests that policies aimed at subsidizing entry or hiring are unlikely to address these information frictions: when entrepreneurs cannot identify capable workers, lowering hiring costs will not make those workers easier to find.

By contrast, policies that improve labor market transparency (e.g., certification systems or formal skill assessments) may facilitate entrepreneurship by directly addressing information asymmetries.[43] Moreover, greater flexibility in employment relationships can lower the risks of hiring under uncertainty, consistent with evidence that firing costs and employment protection reduce efficient reallocation and firm growth (e.g., Hopenhayn and Rogerson, 1993; Autor et al., 2006). More broadly, the findings highlight how access to talented workers constrains entrepreneurship. Programs that expand entrepreneurs' professional networks—such as accelerators, mentoring programs, or even shared workspaces—may therefore foster entrepreneurship by replicating, in part, the information advantages provided by networks (Gonzalez-Uribe and Leatherbee, 2017).

9 Conclusion

This paper empirically quantifies the effects of a common type of network hire—entrepreneurs' former coworkers—on firm entry and performance. To do so, it develops an instrumental variables framework that separates the effects of network hiring on firm performance from its effects on the composition of entrants. This framework is applied to data from Norway that link entrepreneurs to their employees, firms, and ex-coworkers.

[43] Experimental evidence suggests that providing credible signals about worker ability can improve outcomes for entry-level workers (Pallais, 2014). The results in this paper suggest that such signals may also improve outcomes for entry-stage firms.

The estimates reveal that these network hires play a critical role in the entrepreneurial ecosystem. Many entrepreneurs would not enter if they could not hire their ex-coworkers. Many others would enter, but would postpone or forgo hiring employees. The effects of network hiring on entrepreneurship are two-fold: first, the ability to hire ex-coworkers increases output and crowds in other hires without lowering firm productivity. Second, it induces entry by a set of high-performing entrepreneurs who would otherwise forgo entrepreneurship entirely. Counterfactual simulations suggest that networks are much more effective than entry subsidies at encouraging high-productivity entrepreneurship.

The results are consistent with entrepreneurs facing substantial information frictions in the labor market, which are alleviated by their networks. When entrepreneurs cannot easily identify capable workers, lowering entry or hiring costs is insufficient to spur productive entrepreneurship. Policies that improve transparency about worker skills, reduce barriers to mobility, or help entrepreneurs build broader professional networks may therefore be especially effective at fostering high-quality entrepreneurship. Conversely, restrictions such as non-compete agreements that limit entrepreneurs' ability to recruit from their prior workplaces may inadvertently suppress the creation and growth of productive new firms.

More broadly, the results underscore that entrepreneurship may depend just as much on access to human capital as access to financial capital. Because ex-coworkers are only one type of network tie, a natural next step is to understand how other personal connections—such as classmates or relatives—shape firm entry and performance. Future work examining how labor market institutions, hiring frictions, and networks jointly shape entrepreneurs' access to talent may go far toward explaining the origins of productive new firms and the jobs they create.

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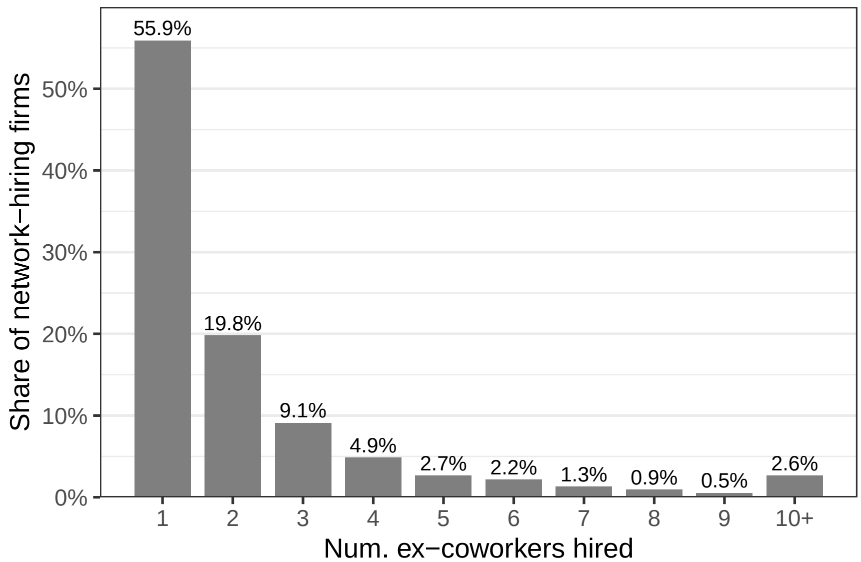
A Additional Exhibits

Figure A.1: Number of ex-coworkers hired



Notes: This figure shows, among firms that hire at least one ex-coworker, how many ex-coworkers are hired by the end of the firm's first full year of operation. The sample includes $N = 4{,}079$ network hiring firms.

Table A.1: OLS estimates, early-stage hiring and later-stage (ages 2-10) firm outcomes

	(1) Revenues (1000s USD)	(2) Value added (1000s USD)	(3) Current employees	(4) VA/worker (1000s USD)	(5) Firm active
Num. early-stage ex-coworker hires	209.8	66.57	0.803	4.42	0.019
	(15.8)	(3.81)	(0.051)	(0.36)	(0.002)
Num. early-stage other hires	75.3	15.05	0.356	0.14	0.003
	(3.9)	(0.80)	(0.014)	(0.07)	(0.001)
Constant	303.0	93.25	0.758	35.04	0.586
	(6.8)	(1.63)	(0.022)	(0.36)	(0.003)
p-val., ex-coworkers = others	0.000	0.000	0.000	0.000	0.000
N firms	33,002	33,002	33,002	33,002	33,002

Notes: This table provides estimates from ordinary least squares regressions of firm outcomes on the numbers hired (by the end of the firm's first full year of operation) of (i) entrepreneurs' ex-coworkers and (ii) other employees. Regressions control for the number of firm owners, a full set of interactions between 2-digit industry and firm age dummies, and a full set of interactions between calendar year and firm age dummies. The sample includes firms aged 2 to 10, which excludes firm started in 2018 (for which we only observe outcomes up to age 1). If firms are not active in a given year, outcomes are imputed as zero. Revenues, employment, and early-stage hiring variables are winsorized at their 99th percentiles (based on non-zero values). Value added outcomes are similarly winsorized at their 1st/99th percentiles. Standard errors are clustered on the firm level.

Figure A.2: Number of new sole proprietors, by year

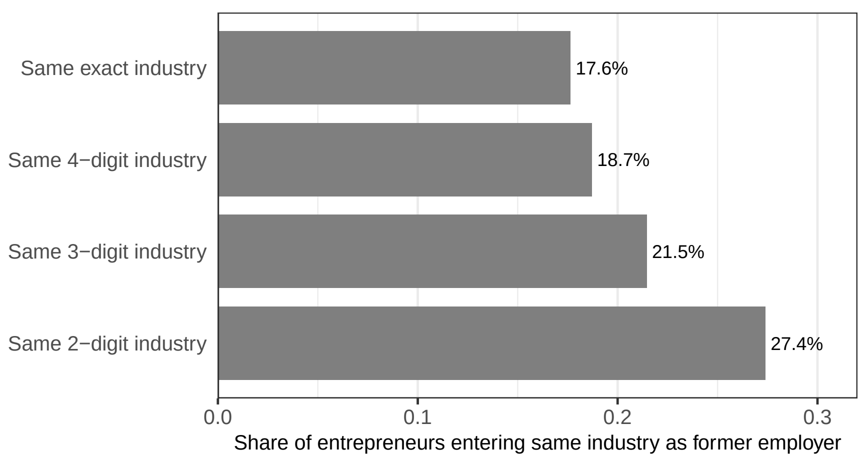
Notes: This figure plots the total number of first-time earners of income from unincorporated firms, which primarily reflect sole proprietors. The vertical red line indicates the capital requirement reform, which came into effect in 2012.

Table A.2: Non-response of prior entrants after capital requirement reduction

	(1) Any revenues	(2) log(revenues)	(3) log(assets)	(4) log(debts)	(5) log(salaries)
Post-reform indicator	0.003	0.009	-0.004	-0.003	0.004
	(0.003)	(0.014)	(0.015)	(0.016)	(0.013)
Firm fixed effects	✓	✓	✓	✓	✓
Age × industry indicators	✓	✓	✓	✓	✓
N firms	11,851	11,368	11,826	11,716	10,191
N obs.	107,595	97,928	106,980	105,249	83,777

Notes: This table provides estimates from ordinary least squares regressions of firm outcomes on an indicator variable for the years 2012 onward, controlling for firm fixed effects and full sets of interactions between firm age dummies and 2-digit industry dummies. The sample is restricted to firms started in the years 2001-2011. Standard errors are clustered on the firm level.

Figure A.3: Entry into same industry as former employer



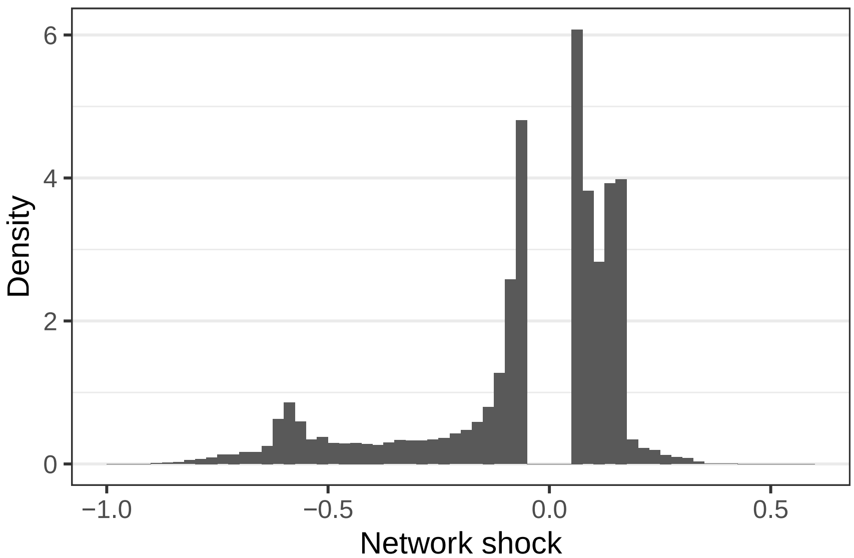
Notes: This figure plots the share of entrants who started firms in the same industry as their prior employer, by the granularity of the classification. Industries are classified according to the Norwegian Standard Industrial Classification (SIC).

Table A.3: Selection-corrected estimates of network hiring on hiring other early-stage workers

	Any non-coworker hires	
Panel A: Averages		
ATT, ex-coworker hires	0.082	(0.017)***
Untreated potential outcome	0.615	(0.031)***
Panel B: Main effects		
Ex-coworker hires D	0.099	(0.022)***
D^2	-0.008	(0.003)**
Panel C: Selection (levels)		
Preference for network hiring V^h	0.109	(0.015)***
Preference for entrepreneurship V^e	0.351	(0.169)**
Panel D: Selection (gains)		
$D \times V^h$	-0.071	(0.034)**
$D^2 \times V^h$	0.007	(0.002)***
$D \times V^e$	-0.130	(0.039)***
$D^2 \times V^e$	0.009	(0.003)***

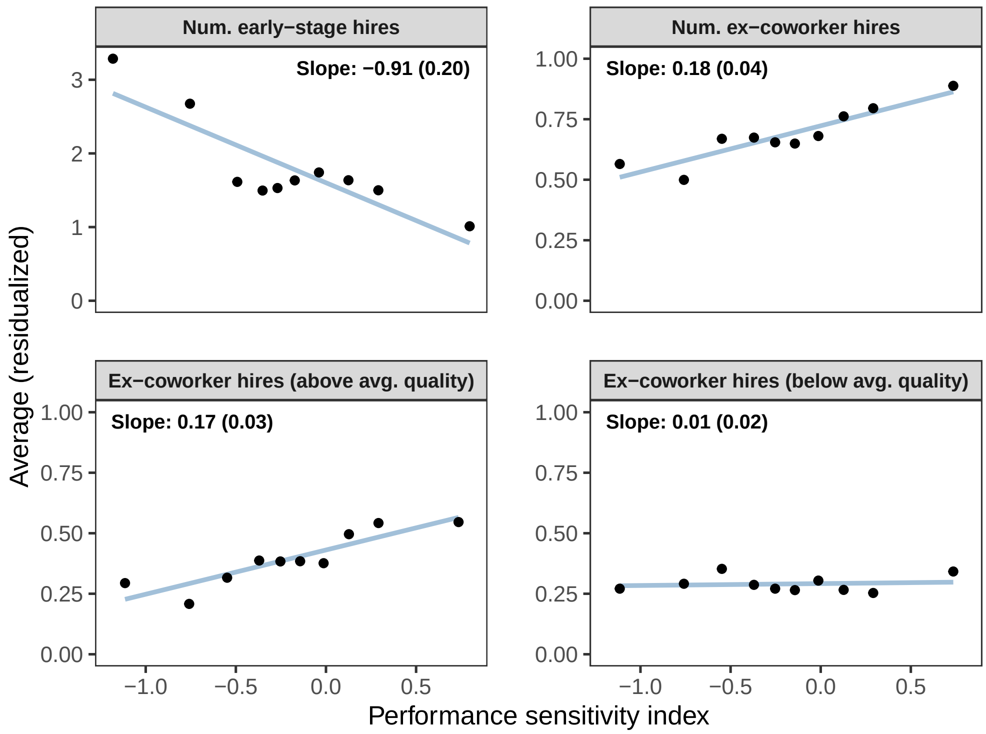
Notes: This table reports selection-corrected estimates of the effects of hiring ex-coworkers on having any non-coworker employees by the end of the firm's first full year of operation. The outcome is adjusted for 2-digit industry and firm age. In all columns, covariates are the same as in Table 2. Panel A reports the average (entrepreneur-level) treatment effect, weighting each network-hiring entrepreneur equally. These estimates are functions of the estimates underlying Panels B-D. Panel A also reports the estimated average untreated potential outcome for entrepreneurs with any network hires. In Panels B-D, estimates are from the specification 15, where the control functions are computed using the parameter estimates from Table 2. Covariates and control functions are demeaned within the sample of entrepreneurs who hire at least one ex-coworker so that estimates reflect average treatment effects for the treated individuals. Standard errors are clustered at the firm level and are adjusted for estimation of the control functions. *, **, and *** indicate p-values below 0.1, 0.05, and 0.01, respectively.

Figure A.4: Density of network shock instrument



Notes: This figure plots the density of the network shock instrument among the population of potential entrepreneurs. The instrument has been recentered based on lagged employer rank and residualized on calendar year dummies, individual age, number of establishment-level coworkers in the past 5 years (log), average lagged wage of recent coworkers (log), entrepreneur lagged liquid wealth (log), and the lagged average employer revenue rank among recent coworkers. The density omits the 85% of individuals with residualized instrument values between -0.05 and 0.05.

Figure A.5: Relationship between hiring and PSI



Notes: This figure plots hiring outcomes for new firms against the performance sensitivity index (PSI) for their 2-digit industry and year. Outcomes in the top row include the total number of early-stage hires (top left panel) and the total number of early-stage ex-coworker hires (top right panel). In the second row, the total number of ex-coworker hires is split based on their residualized wages from the year before the new firm was started, with wages residualized on dummies for calendar year, age, years of schooling, years of experience, and 2-digit industry of employment. "Above average" includes individuals with residualized wages above average, with "below average" defined analogously. PSI is assigned based on the modal occupation at new firms in a given industry in each calendar year. In all panels, PSI and outcomes are residualized on the same covariates underlying Table 7, column (5). Circles reflect equally-sized bins. Standard errors are clustered on the 2-digit industry-by-year level.

B Identification Proofs

To condense on notation, I suppress conditioning on $X_i = x$. Such conditioning should be taken as implicit throughout the proofs.

B.1 Parametric identification (Theorem 1)

B.1.1 Setup

The choice parameters to be identified are the entry costs $\varphi(z^e)$, the network hiring costs $\{\psi(d, z^h\}_{d=1}^{\bar{D}}$, the correlation ρ between V_i^h and V_i^e, and the weight ω placed on network hiring during the entry decision. The outcome parameters to be identified are the intercepts and slopes $\{\mu_d, \theta_d^e, \theta_d^h\}_{d=0}^{\bar{D}}$.

I first express the observed conditional choice probabilities in terms of the choice parameters. Define $\psi(0, Z_i^h) = -\infty$ and $\psi(\bar{D}+1, Z_i^h) = \infty$. For instrument values (z^e, z^h), the probability of starting a firm and hiring $d \in \{0, 1, \ldots, \bar{D}\}$ ex-coworkers is

$$\begin{aligned}
p_d(z^e, z^h) &\equiv \mathbb{P}[E_i = 1, D_i = d \mid Z_i^e = z^e, Z_i^h = z^h] \\
&= \mathbb{P}[E_i = 1, D_i^* = d \mid Z_i^e = z^e, Z_i^h = z^h] \\
&= \mathbb{P}\left[V_i^e - \varphi(z^e) + \omega \sum_{j=1}^{d} (V_i^h - \psi(j, z^h)) \geq 0, V_i^h \geq \psi(d, z^h), V_i^h < \psi(d+1, z^h)\right] \\
&= \mathbb{P}\left[V_i^*(d) \geq \frac{\varphi(z^e) + \omega \sum_{j=1}^{d} \psi(j, z^h)}{\sqrt{1 + \omega^2 d^2 + 2\omega d \rho}}, V_i^h \in \left[\psi(d, z^h), \psi(d+1, z^h)\right)\right]
\end{aligned} \tag{26}$$

where we've defined

$$V_i^*(d) \equiv \frac{V_i^e + \omega d V_i^h}{\sqrt{1 + \omega^2 d^2 + 2\omega d \rho}}. \tag{27}$$

The latent variable $V_i^*(d)$ is the composite variable determining entry given $D_i^* = d$ network hires, normalized to have unit variance. Note that the third equality of (26) invokes Assumption 1 to write the probabilities without conditioning on the instrument realizations.

Under Assumption 2, the latent variables V_i^h and $V_i^*(d)$ are jointly standard normal with correlation

$$g(d;\omega,\rho)=\frac{\rho+\omega d}{\sqrt{1+\omega^2d^2+2\omega d\rho}}. \tag{28}$$

We can therefore write (26) in terms of the bivariate standard normal CDF:

$$\begin{aligned}p_d(z^e,z^h)=\Phi_2\big(-t_d(z^e,z^h;\varphi,\psi,\omega,\rho),\psi(d+1,z^h),-g(d;\omega,\rho)\big)\\-\Phi_2\big(-t_d(z^e,z^h;\varphi,\psi,\omega,\rho),\psi(d,z^h),-g(d;\omega,\rho)\big)\end{aligned} \tag{29}$$

where

$$t_d(z^e,z^h;\varphi,\psi,\omega,\rho)=\frac{\varphi(z^e)+\omega\sum_{j=1}^{d}\psi(j,z^h)}{\sqrt{1+\omega^2d^2+2\omega d\rho}}. \tag{30}$$

This expresses the observed conditional choice probabilities as functions of the parameters to be identified.

B.1.2 Identification of choice parameters

When showing identification of the choice parameters, I assume without loss of generality that $\tilde{Z}_i$ contains no variables; i.e., there are no instruments that impact both E_i and D_i^*. Such variables could always be conditioned on nonparametrically without affecting the results.

A simple counting exercise shows that the data provide $(\bar{D}+1)\mathcal{Z}^e\mathcal{Z}^h$ independent probabilities, where $\mathcal{Z}^k$ denotes the cardinality of the support of Z_i^k. We have $\mathcal{Z}^e+\bar{D}\mathcal{Z}^h+2$ choice parameters. Hence, given $\bar{D}\geq 2$, the number of probabilities weakly exceeds the number of parameters only if $\mathcal{Z}^e\geq 2$ or $\mathcal{Z}^h\geq 3$. Below, I provide a formal justification for identification in each of these two cases.

Lemma 1 (Identification with binary Z_i^e)**.** Maintain the assumptions of Theorem 1. Suppose the support of Z_i^e is given by $\operatorname{supp}(Z_i^e)=\{z_0^e,z_1^e\}$, where $\varphi(z_0^e)\neq\varphi(z_1^e)$. Suppose that $\operatorname{supp}(Z_i^h)=\{z_0^h\}$, such that $\psi(d,z)=\psi(d)$ (i.e., no instrument for D_i^*). Then $\varphi(z_0^e)$, $\varphi(z_1^e)$, ρ, and ω are identified, as is $\psi(d)$ for all $d\in\operatorname{supp}(D_i)$.

Proof. It is sufficient to show that the mapping from parameters to probabilities is one-to-one, such that if two parameter vectors generate the same probabilities $\{p_j(z_0^e),p_j(z_1^e)\}_{j=0}^{\bar{D}}$, then those parameter vectors coincide. I do this in four steps:

1. Use the probabilities $p_0(z_k^e)$ for $k \in \{0,1\}$ to show that $\varphi(z_k^e)$ is uniquely determined for $k \in \{0,1\}$ given $(\psi(1), \rho)$.
2. Use the probabilities $p_1(z_k^e)$ for $k \in \{0,1\}$ to show that ω and $\psi(2)$ are uniquely determined given $(\psi(1), \rho)$.
3. Use the probabilities $p_d(z_0^e)$ to show that $\psi(d+1)$ is uniquely determined given $(\psi(1), \rho)$ for $d \geq 2$.
4. Use the probabilities $p_2(z_1^e)$ and $p_{\bar{D}}(z_1^e)$ to show that $(\psi(1), \rho)$ are uniquely determined, guaranteeing uniqueness of all parameters given steps 1-3.

First, using the $D_i = 0$ conditional probabilities, we have

$$p_0(z_k^e) = \Phi_2(-\varphi(z_k^e), \psi(1), -\rho). \tag{31}$$

By definition, the bivariate normal CDF is strictly monotone in its first argument. Hence, for any fixed $(\psi(1), \rho)$, the parameters $\varphi(z_0^e)$ and $\varphi(z_1^e)$ are uniquely determined by the choice probabilities.

Second, using the $D_i = 1$ conditional probabilities, we have

$$\begin{aligned} p_1(z_k^e) = \Phi_2\big(&-t_1(z_k^e; \varphi, \psi, \omega, \rho), \psi(2), -g(1; \omega, \rho)\big) \\ &- \Phi_2\big(-t_1(z_k^e; \varphi, \psi, \omega, \rho), \psi(1), -g(1; \omega, \rho)\big). \end{aligned} \tag{32}$$

By definition of the CDF, this probability is strictly increasing in $\psi(2)$. Hence, we can define $\tilde{\psi}(2; \omega, k)$ as the unique value of $\psi(2)$ implied by the observed probability $p_1(z_k^e)$ given ω. Define the discrepancy

$$\Delta(\tilde{\omega}) \equiv \tilde{\psi}(2; \tilde{\omega}, 1) - \tilde{\psi}(2; \tilde{\omega}, 0). \tag{33}$$

Using the implicit function theorem, we can show that $\Delta(\tilde{\omega})$ is strictly monotone (either increasing or decreasing) in $\tilde{\omega}$. It follows that, if the model is correctly specified, there is exactly one ω^* solving $\Delta(\omega^*) = 0$. We therefore have $\omega = \omega^*$, and $\psi(2)$ is the common value $\psi(2) = \tilde{\psi}(2; \omega, 1) = \tilde{\psi}(2; \omega, 0)$. Hence, for any fixed $(\psi(1), \rho)$, the parameters $(\omega, \psi(2))$ are uniquely determined by the choice probabilities.

Third (and only if $\bar{D} > 2$), for $d \geq 2$, write the $D_i = d$ conditional probabilities as

$$p_d(z_k^e) = \Phi_2\big(s_{dk}, \psi(d+1), r_d\big) - \Phi_2\big(s_{dk}, \psi(d), r_d\big) \tag{34}$$

$$\text{where} \quad s_{dk} \equiv -t_d(z_k^e; \varphi, \psi, \omega, \rho) \tag{35}$$

$$\text{and} \quad r_d \equiv -g(d; \omega, \rho). \tag{36}$$

Consider first the case with $d = 2$. For fixed $(\psi(1), \rho)$, the terms s_{2k} and r_2 are uniquely determined, as shown in the previous steps. This leaves one parameter, $\psi(3)$, in which (34) is strictly increasing. Hence, $\psi(3)$ is uniquely determined by $p_2(z_0^e)$. Proceeding recursively, it follows that given $p_d(z_0^e)$, $\psi(d+1)$ is uniquely determined for all $d \in \{2, \ldots, \bar{D}-1\}$.

Fourth, if $\bar{D} > 2$, form the residuals

$$R_2(\psi(1), \rho) \equiv \Phi_2\big(s_{21}, \psi(3), r_2\big) - \Phi_2\big(s_{21}, \psi(2), r_2\big) - p_2(z_1^e) \tag{37}$$

$$R_{\bar{D}}(\psi(1), \rho) \equiv \Phi(s_{\bar{D}1}) - \Phi_2\big(s_{\bar{D}1}, \psi(\bar{D}), r_{\bar{D}}\big) - p_{\bar{D}}(z_1^e), \tag{38}$$

where we use the fact that all other parameters can be written as functions of $\psi(1)$, ρ, all conditional choice probabilities for $d < 2$, and the $Z_i^e = z_0^e$ conditional choice probabilities for $d \geq 2$. We thus have two equations in two unknowns, $\psi(1)$ and ρ. Moreover, each residual R_d depends on (bivariate) normal probabilities, but with different indices s_{d1} and correlations r_d. Hence, ∇R_2 and $\nabla R_{\bar{D}}$ are generically not collinear, and the corresponding Jacobian is full rank. By the implicit function theorem, the system $R_2 = R_{\bar{D}} = 0$ has a unique solution $(\psi(1), \rho)$.

If $\bar{D} = 2$, replace $R_{\bar{D}}$ with

$$R_2' = \Phi_2\big(s_{20}, \psi(3), r_2\big) - \Phi_2\big(s_{20}, \psi(2), r_2\big) - p_2(z_1^e).$$

The same argument applies, where now differences in s_{20} and s_{21} are what guarantee that the Jacobian is full rank.

Having shown that $(\psi(1), \rho)$ are uniquely determined by the choice probabilities, it follows immediately from the earlier steps that $(\varphi(z_0^e), \varphi(z_1^e), \omega)$ and $\{\psi(d)\}_{d=1}^{\bar{D}}$ are uniquely determined.

Q.E.D.

Lemma 2 (Identification with multivalued Z_i^h)**.** Maintain the assumptions of Theorem

1. Suppose the support of Z_i^h is given by $\text{supp}(Z_i^h) = \{z_0^h, z_1^h, z_2^h\}$, with $\psi(d, Z_i^h)$ nondegenerate in Z_i^h. Suppose that $\text{supp}(Z_i^e) = \{z_0^e\}$, such that $\varphi(z^e) = \varphi$ (i.e., no instrument for E_i). Then φ, ρ, and ω are identified, as is $\psi(d, z^h)$ for all $d \in \{1, \ldots, \bar{D}\}$ and all $z^h \in \text{supp}(Z_i^h)$.

Proof. Fix the candidate triple (φ, ρ, ω). For $d = 0$, the probability $p_0(z_0^e, z_m^h)$ is strictly increasing in $\psi(1, z_m^h)$, hence the parameter $\psi(1, z_m^h)$ is uniquely determined. In addition, $p_d(z_0^e, z_m^h)$ is strictly increasing in $\psi(d+1, z_m^h)$, so $\psi(d+1, z_m^h)$ is also uniquely determined for all $d < \bar{D}$. It follows that for fixed (φ, ρ, ω), the parameters $\psi(d, z_m^h)$ are uniquely identified for all $d \in \{1, \ldots, \bar{D}\}$.

I now show that (φ, ρ, ω) are uniquely determined. Take the $d < \bar{D}$ conditional probabilities as given, such that $\psi(d, z_m^h)$ are implicit functions of (φ, ρ, ω). Using the $D_i = \bar{D}$ conditional probabilities, we have the residuals

$$R_m(\varphi, \rho, \omega) = \Phi\big(s_{\bar{D}}(z_m^h)\big) - \Phi_2\big(s_{\bar{D}}(z_m^h), \psi(\bar{D}, z_m^h), r_{\bar{D}}\big) - p_{\bar{D}}(z_0^e, z_m^h) \tag{39}$$

$$\text{with} \quad s_{\bar{D}}(z_m^h) = -\frac{\varphi + \omega \Psi_m}{\sqrt{1 + \omega^2 \bar{D}^2 + 2\omega \bar{D} \rho}} \tag{40}$$

$$\text{and} \quad r_{\bar{D}} = -\frac{\rho + \omega \bar{D}}{\sqrt{1 + \omega^2 \bar{D}^2 + 2\omega \bar{D} \rho}} \tag{41}$$

where $\Psi_m = \sum_{j=1}^{\bar{D}} \psi(j, z_m^h)$. Stack $F(\varphi, \rho, \omega) = (R_0, R_1, R_2)$. Because the truncated normal CDF is a smooth, strictly monotone function in each of its arguments, and under our assumption that Ψ_m is nondegenerate in z_m^h, the three gradients ∇R_m are linearly independent at the true parameter values. The corresponding Jacobian ∇F is therefore nonsingular. Applying the implicit function theorem, the solution set of $F(\varphi, \rho, \omega) = 0$ is a singleton in a neighborhood around the true values. Moreover, monotonicity of the normal distribution guarantees that there is at most one solution, such that $F(\varphi, \rho, \omega) = 0$ is a singleton globally.

Hence, (φ, ρ, ω) are uniquely determined. From the previous step, it follows that $\{\psi(d, z_m^h\}_{d=1}^{\bar{D}}$ are uniquely determined.

Q.E.D.

B.1.3 Identification of outcome parameters

Under Assumption 3, we have

$$\mathbb{E}[Y_i \mid E_i = 1, D_i = d, Z_i] = \mu_d + \theta_d^e \lambda^e(d, Z_i) + \theta_d^h \lambda^h(d, Z_i) \tag{42}$$

$$\text{where} \quad \lambda^e(d, z) = \mathbb{E}[V_i^e \mid E_i = 1, D_i = d, Z_i = z] \tag{43}$$

$$\text{and} \quad \lambda^h(d, z) = \mathbb{E}[V_i^h \mid E_i = 1, D_i = d, Z_i = z]. \tag{44}$$

We have

$$\lambda^k(d, z) = \mathbb{E}\left[V_i^k \,\middle|\, V_i^*(d) \geq \frac{\varphi(z^e) + \omega \sum_{j=1}^{d} \psi(j, z^h)}{\sqrt{1 + \omega^2 d^2 + 2\omega d \rho}}, V_i^h \in \left[\psi(d, z^h), \psi(d+1, z^h)\right) \right], \tag{45}$$

where $V_i^*(d)$ is as defined in (27). Hence, λ^h is the average of a truncated bivariate normal variable, with truncation above and below. Given identification of the choice parameters, the thresholds are known. It follows that (45) itself is identified (Tallis, 1961). A similar argument shows identification of $\mathbb{E}[V_i^*(d) \mid E_i = 1, D_i = d, Z_i^e = z^e, Z_i^h = z^h]$. Replacing $V_i^*(d)$ with (27) and using the linearity of the expectation, we identify λ^e.

Under the support conditions (iii)-(v) from the Theorem, we have at least three instrument realizations, with $\text{supp}(Z_i) = \{z_0, z_1, z_2\}$, in which the parameters $\varphi(z_m^e)$ and $\{\psi(z_m^h)\}_{d=1}^{\bar{D}}$ are nondegenerate. It follows immediately that the control functions λ are also nondegenerate in the instrument values, and that $(\lambda^e(d, x, z_m), \lambda^h(d, x, z_m))$ and $(\lambda^e(d, x, z_\ell), \lambda^h(d, x, z_\ell))$ are not collinear for $m \neq \ell$. Hence, population linear regression of Y_i on $\lambda^h(d, Z_i)$ and $\lambda^e(d, Z_i)$ in the $E_i = 1, D_i = d$ subpopulation identifies $(\mu, \theta_d^e, \theta_d^h)$.

C Details on stylized model

C.1 Hiring choice

The entrepreneur, who is constrained to hire at most one employee, chooses between three options: hire her coworker; post a wage w in the broader labor market to recruit a non-network worker; or hire no one.

If the entrepreneur chooses to hire from the broader labor market by posting a wage w, she can expect to receive applicants who would earn no more than w from incumbent firms. The expected surplus from hiring a non-network worker at wage w is given by

$$S_m(w; \bar{\beta}_i, \rho_i\sigma_i) = \mathbb{E}[\beta_{ij} \mid \theta_j \leq w] - w = \bar{\beta}_i - \rho_i\sigma_i\lambda(w - \bar{\theta}) - w \tag{46}$$

where $\lambda(x) = \phi(x)/\Phi(x)$ is the inverse Mills ratio.

Proposition 1 (Market-hiring surplus)**.** For an entrepreneur with match value expectation $\bar{\beta}$ and productivity covariance $\kappa = \rho\sigma$, the maximum expected surplus from hiring a worker of unknown quality is given by

$$S_m^*(\bar{\beta}, \kappa) = \bar{\beta} - b - f(\kappa), \tag{47}$$

where $f(0) = 0$, $f'(\kappa) > 0$, and $f(1) > \bar{\theta} - b$.

Proof. Posting a wage of at least b always leads to a hire, and the optimal such wage maximizes the market-hiring surplus:

$$S_m^*(\bar{\beta}_i, \kappa_i) = \max_{w \geq b} \bar{\beta}_i - \kappa_i\lambda(w - \bar{\theta}) - w. \tag{48}$$

At an interior maximum, the first-order condition is

$$-\lambda'(w - \bar{\theta}) = \kappa_i^{-1}. \tag{49}$$

Defining $g(x) = -\lambda'(x) = \lambda(x)[x + \lambda(x)]$, this leads to the solution

$$w_i^* = \begin{cases} g^{-1}(\kappa_i^{-1}) + \bar{\theta} & \text{if } \kappa_i > \underline{\kappa} \\ b & \text{otherwise} \end{cases} \tag{50}$$

where $\underline{\kappa} = 1/g(b - \bar{\theta})$. Substituting w_i^* into the surplus expression, we obtain the market-hiring surplus

$$S_m^*(\bar{\beta}_i, \kappa_i) = \bar{\beta}_i - b - f(\kappa_i) \tag{51}$$

$$\text{where} \quad f(\kappa) = \begin{cases} \bar{\theta} - b + h(\kappa) & \text{if } \kappa_i > \underline{\kappa} \\ \kappa_i \lambda(b - \bar{\theta}) & \text{otherwise} \end{cases} \tag{52}$$

$$\text{with} \quad h(x) = x\lambda(g^{-1}(1/x)) + g^{-1}(1/x). \tag{53}$$

This provides the claimed expression. We have $\underline{\kappa} \geq 0$, since $g(x) = -\lambda'(x) > 0$ for all x, so that $f(0) = 0$ as claimed. We also have

$$f'(\kappa) = \begin{cases} \lambda(g^{-1}(1/x)) & \text{if } \kappa > \underline{\kappa} \\ \lambda(b - \bar{\theta}) & \text{otherwise,} \end{cases} \tag{54}$$

so it follows that $f'(\kappa) > 0$. Finally, that $f(1) > \bar{\theta} - b$ follows from the properties of the inverse Mills ratio: $h(x) > 0$ (for the $\kappa > \underline{\kappa}$ case), and $\lambda(-x) > x$ for $x \geq 0$ (for the $\kappa \leq \bar{\kappa}$ case). *Q.E.D.*

The entrepreneur's coworker demands a wage of at least θ_k, so the entrepreneur can maximize her surplus by matching this wage. The resulting surplus accruing to the entrepreneur is

$$S_n(\theta_k; \bar{\beta}_i, \rho_i\sigma_i) = \mathbb{E}[\beta_{ik} \mid \theta_k] - \theta_k = \bar{\beta}_i + \rho_i\sigma_i(\theta_k - \bar{\theta}) - \theta_k. \tag{55}$$

In her hiring decision, the entrepreneur first compares the surplus she would attain from hiring her coworker with the surplus she could attain from hiring a worker of unknown quality from the general labor market. The following proposition characterizes when network hiring is preferred over non-network hiring.

Proposition 2 (Network vs. market hiring)**.** An entrepreneur with productivity covariance $\kappa = \rho\sigma$ prefers to hire a coworker with incumbent marginal product θ_k over an unknown market worker when $\kappa \geq 1$ and $\theta_k > \theta^*(\kappa)$, or when $\kappa < 1$ and $\theta_k < \theta^*(\kappa)$. Moreover, the threshold $\theta^*(\kappa) < \bar{\theta}$ for all $\kappa \geq 1$.

Proof. Comparing the network surplus (55) to the market hiring surplus (47), the entrepreneur will prefer network hiring over non-network hiring if

$$\kappa(\theta_k - \bar{\theta}) - \theta_k \geq -b - f(\kappa) \tag{56}$$

where we have eliminated the common $\bar{\beta}$. Rearranging, we have

$$\theta_k(\kappa - 1) \geq \bar{\theta}\kappa - b - f(\kappa). \tag{57}$$

Hence, the entrepreneur prefers network hiring over non-network hiring if

$$\theta_k > \theta^*(\kappa) \text{ and } \kappa \geq 1 \quad \text{ or } \quad \theta_k < \theta^*(\kappa) \text{ and } \kappa_i < 1 \tag{58}$$

where

$$\theta^*(\kappa) = \bar{\theta} - \begin{cases} h(\kappa)/(\kappa - 1) & \text{if } \kappa_i > \underline{\kappa} \\ [\kappa\lambda(b - \bar{\theta}) - (\bar{\theta} - b)]/(\kappa - 1) & \text{if } \kappa_i \leq \underline{\kappa} \end{cases} \tag{59}$$

with h as defined in the proof of Proposition 1. This provides the claimed form. That $\theta^*(\kappa) < \bar{\theta}$ when $\kappa \geq 1$ follows from $h(x) > 0$. *Q.E.D.*

Proposition 2 shows that there is always a value $\theta_j < \bar{\theta}$ for which network hiring is preferred over market hiring, regardless of the productivity covariance $\rho_i\sigma_i$ or average match quality $\bar{\beta}_i$. That is, regardless of the distribution of match values, there is always a below-average productivity level for which the entrepreneur would prefer to hire a coworker over a market worker of unknown productivity.

C.2 Private information vs. relationship capital

To extend the model to allow for relationship capital, we can allow the average match quality $\bar{\beta}_i$ to differ between network and non-network members by a factor of δ_i, with

$$\mathbb{E}[\beta_{ik} \mid \theta_k] = \delta_i\bar{\beta}_i + \rho_i\sigma_i(\theta_k - \bar{\theta}). \tag{60}$$

This extension does not affect the market surplus, which is still given by (47). The network hiring surplus becomes

$$S_n(\theta_k; \bar{\beta}_i, \delta_i, \rho_i\sigma_i) = \delta_i\bar{\beta}_i + \rho_i\sigma_i(\theta_k - \bar{\theta}) - \theta_k. \tag{61}$$

The first term $\delta_i\bar{\beta}_i$ captures average quality, enhanced by relationship capital. The second term $\rho_i\sigma_i(\theta_k - \bar{\theta})$ reflects the updated expectation given private information on θ_k.

Differentiating each surplus with respect to σ, we obtain the following relationships:

$$\frac{\partial S_m^*}{\partial \sigma} = -\rho f'(\rho\sigma) < 0 \tag{62}$$

$$\frac{\partial S_n}{\partial \sigma} = \rho(\theta_k - \bar{\theta}). \tag{63}$$

The first derivative is unambiguously negative (per Proposition 1): as σ increases, the expected surplus from hiring an unknown worker from the market declines. This makes entrepreneurs less likely to hire from the market. The sign of the second derivative depends on coworker productivity, θ_k. If $\theta_k > \bar{\theta}$, this derivative is positive: as match quality becomes more dispersed (higher σ), the gains from hiring an above average increase, so network hiring generates more surplus. If $\theta_k < \bar{\theta}$, the opposite is true, and network hiring generates less surplus.

Importantly, these predictions turn out to be unaffected by the addition of relationship capital to the model. This is because relationship capital affects average coworker quality, which does not change the benefits of private information.

D Constructing the Performance Sensitivity Index

All occupational data is from the O*NET 28.0 database. I first link Norwegian occupation codes (STYRK-98) to U.S. O*NET occupations using semantic similarity between occupation titles and descriptions. I do this by embedding both machine-translated Norwegian occupation titles and the O*NET occupation titles and descriptions using a sentence-transformer model (all-MiniLM-L6-v2). Then, for each Norwegian occupation, I select the O*NET occupation with the highest cosine similarity.

D.1 Measurement

I construct a Performance Sensitivity Index (PSI), which proxies for the extent to which worker ability matters in a given occupation or "how steeply" productivity rises with worker skill. The intuition is that when ability is more important for effective performance, differences in worker quality generate larger productivity variation at new firms (i.e., higher σ in the model).

I use the Level (LV) ratings from O*NET. These quantify how much of a skill or ability is required to perform the occupation effectively. An alternative construction would be to use the Importance (IM) ratings, as were used by Autor et al. (2003) and Deming (2017) when studying different job skills; however, IM ratings indicate whether a skill matters as opposed to the proficiency depth required. The proficiency depth is important: for example, both computer programmers and cashiers require mathematical ability, but the proficiency required is much higher for programmers.

I focus on cognitive and problem-solving skills, using the following Level-rated skills and abilities:

- **Skills:** Critical Thinking, Complex Problem Solving, Judgment and Decision Making, Reading Comprehension, Active Learning, Mathematics, Programming
- **Abilities:** Deductive Reasoning, Inductive Reasoning, Problem Sensitivity, Oral Comprehension, Written Comprehension, Information Ordering

D.2 Computation steps

Let $x_{o,k}$ denote the Level score for occupation o and skill/ability k. The PSI is constructed as follows:

1. For each skill or ability k, compute the z-score across all SOC occupations:

$$z_{o,k} = \frac{x_{o,k} - \mu_k}{\sigma_k},$$

   where μ_k and σ_k are the mean and standard deviation of item k across all occupations.

2. Average across the selected items within each occupation:

$$\text{PSI}_o^{\text{raw}} = \frac{1}{K_o} \sum_{k \in K_o} z_{o,k},$$

   where K_o is the set of available items for occupation o.

3. After merging to the final sample of occupations appearing in the data, standardize PSI to have mean zero and unit variance:

$$\text{PSI}_o = \frac{\text{PSI}_o^{\text{raw}} - \bar{\text{PSI}}^{\text{raw}}}{\text{sd}(\text{PSI}^{\text{raw}})}.$$

The metric PSI_o is then assigned to new firms based on the modal occupation within 2-digit industry $\times$ year.