

# **Penalized or Protected? Field-Experiment Evidence on Spousal Cofounding and the Hiring Penalty in New Ventures**

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## **ABSTRACT**

Founding teams often include personal relationships that can enhance internal coordination, yet we know far less about how such ties are evaluated by external audiences. We focus on spousal cofounding and propose that it can disadvantage startups in recruitment by triggering concerns among prospective job candidates. Using a representative sample of U.S. entrepreneurial teams from the Panel Study of Entrepreneurial Dynamics II (PSED II), we show that spousal cofounding is common and is associated with a hiring penalty that is concentrated among male-led startups. We then conduct a preregistered field experiment on LinkedIn to provide causal evidence that candidates are indeed less likely to apply to spousal-founded startups, with particularly pronounced effects for male-led ventures. Finally, a preregistered online experiment replicates this pattern and shows that perceived boundary-crossing and candidate career concerns explain the evaluative discount. Together, these results indicate that spousal cofounding reduces applicant attraction, especially for male-led ventures, revealing an underexplored external liability of spousal relationships in entrepreneurship.

**Keywords:** Spousal Cofounding, Startup Hiring, Founding Team Composition, Gender, Field Experiment

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## INTRODUCTION

Teams are a central organizational unit through which firms coordinate activity, execute strategy, and generate innovation (Aven & Hillmann, 2018; Beckman et al., 2007; Burton & Beckman, 2007). This is especially salient in entrepreneurial settings, where early-stage ventures must assemble small teams capable of operating under acute uncertainty with limited track records. Ample research examines how founding team composition affects venture outcomes through internal mechanisms such as coordination, information sharing, and decision quality (Beckman et al., 2007; Burton & Beckman, 2007; Eisenhardt & Schoonhoven, 1990; Ruef et al., 2003). Yet teams are also objects of external evaluation. Because startups are difficult to assess directly, external audiences, including investors, employees, and customers, often rely on observable signals about founders and founding teams to infer a venture's future value and potential (Ruef et al., 2003; Vissa & Chacar, 2009; Xu & Ruef, 2007).

A distinct feature of early-stage teams is that cofounders frequently share existing personal relationships among cofounders, such as friendships and family ties (Aldrich & Ruef, 2003; Ruef et al., 2003; Uzzi, 1997; Vissa, 2011). Prior research on founding teams has treated such ties as inputs to internal processes, including trust, resilience, and coordination among team members (Bird & Zellweger, 2018; Eisenhardt & Schoonhoven, 1990; Tsai & Ghoshal, 1998), while paying comparatively little attention to how they affect the perceptions and decisions of outside parties considering whether to transact with the venture. Whether personal ties are interpreted by outsiders as a signal of cohesion and commitment or as a source of nepotism and governance risk remains an open question, and one with consequences for how early-stage ventures attract resources and partners.

We examine this question through a prevalent form of personal relationship in entrepreneurship: spousal cofounding teams. Spousal cofounders, or married couples or long-term partners who lead a venture together, constitute a substantial share of early-stage ventures, especially among small businesses and family-owned firms (Aldrich & Cliff, 2003; Kim, 2005; Yang & Aldrich, 2014). We shift attention from internal team dynamics to external evaluation by asking how spousal cofounding teams are valued by prospective employees, a key and consequential audience for early-stage venture development and scaling (Hietaniemi et al., 2024; Honoré & Ganco, 2023).

Drawing on role theory (Anglin et al., 2022; Kahn et al., 1964) and research on category spanning, we propose that spousal cofounding may be evaluated negatively in the labor market. While spousal ties may facilitate trust and coordination inside the venture (Bird & Zellweger, 2018), we expect them to function as a negative signal to prospective hires, raising concerns about blurred boundaries between personal and professional domains and limited advancement opportunities for outsiders. We therefore hypothesize a labor-market penalty: relative to otherwise similar non-spousal founding teams, spousal cofounding teams will attract less interest from job candidates and face higher compensation demands when recruiting talent.

We further examine whether such negative evaluations in the labor market vary depending on the gender of the lead founder. Violations of normative expectations may be more pronounced in spousal teams where men are the lead founders because male founders are culturally associated with professional, instrumental partnerships rather than relational ones (Eagly & Karau, 2002; Heilman & Eagly, 2008; Snellman & Solal, 2023). Given prevailing beliefs that men have broader access to professional networks (Brands et al., 2015; Renzulli et al., 2000), selecting a spouse as cofounder may be perceived as incongruent with the prototype of male entrepreneurial leadership,

triggering a stronger evaluative penalty. By contrast, female founders are more often expected to draw on interpersonal support networks, including spousal involvement (Blair-Loy, 2003; Ely & Meyerson, 2000), particularly in entrepreneurial contexts (Yang & Aldrich, 2014). Gendered expectations may thus render male-led ventures more vulnerable to negative evaluations from prospective employees, while such penalties may be weaker or less detectable for female-led ventures.

We evaluate these propositions using a multi-method design that addresses both external validity and causal identification. We begin with an observational analysis of PSED II, a nationally representative dataset of U.S. nascent ventures (Reynolds & Curtin, 2009a) (Study 1). The PSED II allows us to document the prevalence of spousal cofounding and examine whether spousal teams are associated with weaker hiring outcomes in the field. These patterns are informative, but they may be endogenous to venture characteristics that influence employer attractiveness. To establish causality, we conducted a preregistered, ethics-approved field experiment in collaboration with an early-stage startup hiring for a marketing role (Study 2). Using LinkedIn Recruiter, we randomly varied founder gender and founding team composition (spousal vs. non-spousal) across otherwise identical outreach messages sent to real job candidates. Because candidates were randomly assigned to receive outreach from ventures that differed only in these attributes, the design isolates the causal effect of spousal cofounding on candidates' willingness to engage with the opportunity. Finally, we complement the field experiment with a preregistered online experiment on Prolific to probe the underlying mechanism (Study 3). This study examines evaluations not only among individuals who would apply, but also among those who would not, allowing us to measure perceived boundary/role violation and related inferences (i.e., concerns about professionalism, impartial governance, and insularity) that are difficult to observe in field recruiting data. By

directly comparing three team compositions (spousal, sibling, and coworker teams), we can also assess whether the hiring penalty is specific to spousal cofounding rather than reflecting a broader aversion to family ties. Together, the three studies —PSED II analysis, the LinkedIn field experiment, and the Prolific experiment —provide convergent evidence on the prevalence, causal effects, and mechanisms underlying job seekers’ reactions to spousal founding teams.

Across the three complementary studies, we find evidence of a spousal team penalty in the labor market, which appears primarily among male-led startups. This penalty is statistically detectable in male-led startups, though not in female-led startups. The findings persist across both archival data (Study 1) and experimental studies (Studies 2-3), demonstrating robustness across methodological approaches and measurement strategies. Finally, the Prolific experiment shows that boundary maintenance concerns mediate the relationship between spousal cofounding teams and willingness to apply to a startup. Hence, our results suggest that the spousal cofounding penalty stems at least in part from normative violations—perceptions that mixing romantic and professional relationships transgresses organizational boundaries. Such normative violations undermine applicants' confidence in the startups and thus lead them to forgo potential job opportunities.

Our study makes three contributions to organizational studies and entrepreneurship research. First, we show that spousal cofounding constitutes a distinctive relational configuration that functions as a signal to external audiences independent of founders’ individual qualifications. In particular, we document that job seekers evaluate not only who founders are, but also whether the relationships among them conform to normative expectations about how professional ventures should be organized and governed. Whereas prior work on private ties in entrepreneurial teams has primarily emphasized their internal benefits (Bird & Zellweger, 2018; Eisenhardt &

Schoonhoven, 1990; Tsai & Ghoshal, 1998), our study establishes a countervailing effect, whereby spousal teams can incur *evaluative penalties* in the labor market. By documenting that the relational architecture of the founding team functions as a signal in its own right, we broaden founding-team research beyond internal team functioning to incorporate how private ties influence external assessments relevant for recruitment and, potentially, other resource-acquisition processes.

Second, we contribute to the growing literature on startup employment by theorizing and providing evidence that candidates in entrepreneurial labor markets attend to, and respond to, founding-team structure as part of their assessment of employment opportunities (Hurst et al., 2024; Kim & Pergler, 2025; Snellman & Younkin, 2021). Bridging role theory (Biddle, 1986; Kahn et al., 1964) with research on evaluation and labor-market sorting, we propose that spousal cofounding can be perceived as boundary-spanning across personal and professional roles, generating uncertainty for prospective joiners. Hence, we extend startup employment research beyond founder-level signals to show that candidates also infer job quality and organizational viability from the *relationships within* the founding team.

Finally, we extend theories of gender disparities in entrepreneurship (Guzman & Kacperczyk, 2019; Kanze et al., 2018; Snellman & Solal, 2023; Yang & Triana, 2019) by identifying how founding-team structure interacts with gender to influence access to human capital. Prior work shows that gender stereotypes affect access to financing (Kanze et al., 2018), network inclusion (Renzulli et al., 2000), and perceptions of entrepreneurial competence (Heilman & Eagly, 2008). We demonstrate that team structure can activate gendered expectations that affect how audiences evaluate ventures. Our findings depart from prior research emphasizing disproportionate penalties for female founders by showing that norm violations can potentially

disadvantage male entrepreneurs when their behaviors transgress gendered expectations about appropriate role boundaries and relational conduct.

## **THEORY**

### **Past Research**

Popular narratives of entrepreneurship emphasize lone visionaries or small teams of technically skilled founders launching ventures through individual talent. Yet systematic empirical evidence reveals a different pattern. Venture founding is predominantly collaborative rather than individual (Aldrich & Cliff, 2003; Audia & Rider, 2005; Kim, 2005; Ruef, 2010; Yang & Aldrich, 2014), and these collaborations frequently emerge from preexisting personal relationships (Kim et al., 2013). Particularly noteworthy is the prevalence of spousal cofounding: Ruef, et al. (2003)'s analysis of a nationally representative sample of U.S. nascent ventures reveals that over half (53%) of multi-founder startups included a spousal or domestic partner, a pattern that raises important questions about how intimate ties affect both internal dynamics and external evaluations of new firms.

Extant research has focused on the internal advantages of spousal cofounding. In particular, past studies have established that spousal cofounding teams represent a distinctive organizational form characterized by relational embeddedness, whereby founders select spouses as partners because marital relationships confer trust, shared identities, and reciprocal obligation, assets that are especially valuable in resource-constrained startups (Yang et al., 2020). Dating back to Penrose's (1959) seminal work, scholars have proposed that a firm's growth depends not only on its resources but also on the relationships among its founding members. Granovetter (1985) and subsequent scholars have similarly emphasized that interpersonal relationships rooted in trust and shared interests can serve as critical resources for achieving economic goals (Granovetter, 1985;

Kim et al., 2013; Krackhardt, 1992; Ruef et al., 2003; Xu & Ruef, 2007). Parsons (1949) described spousal pairings as “solidarity units,” highlighting their cohesion and durability. Building on this logic, Bird and Zellweger (2018) subsequently theorized that spousal cofounding can leverage three key dimensions of embeddedness, such as trust, identification, and reciprocal obligation, to foster coordination, emotional commitment, and resilience under uncertainty (Coleman, 1990; Eisenhardt & Schoonhoven, 1990; Tsai & Ghoshal, 1998). Consistent with this view, empirical research shows that spousal teams can outperform sibling teams on measures of venture performance, an advantage attributed to stronger strategic alignment and deeper emotional solidarity (Becker, 1991; Jaffe, 2009).

Prior research thus foregrounds the *internal* benefits of spousal cofounding, characterized by greater trust, stronger mutual obligations, and more effective coordination and decision-making (Eisenhardt & Schoonhoven, 1990; Jung et al., 2015; Ruef et al., 2003; Yang et al., 2020). Yet far less is known about the *external* consequences of these configurations and how outside audiences interpret them. New ventures rely heavily on endorsement and resources from investors, employees, customers, and alliance partners (Freeman et al., 1983; Stinchcombe, 1965; Stuart & Sorenson, 2005). In the absence of hard evidence about quality, these audiences may routinely rely on founders’ attributes and team configurations as proxies for venture potential (Campero & Kacperczyk, 2020; Snellman & Solal, 2023). Spousal teams may act as a signal affecting external evaluations. However, past research has largely overlooked how such ventures are externally evaluated, leaving the possible costs of spousal cofounding poorly understood. In this study, we shift the focus from internal benefits to external evaluations, examining how spousal cofounding structures are received by one critical audience — prospective employees — whose attraction is vital to venture survival.

## **Spousal Teams as Signals to Jobseekers**

Early hires are critical to a venture's success and scalability (Honoré & Ganco, 2023; Roach & Sauermann, 2024; Yang & Aldrich, 2017). Yet from the perspective of prospective employees, joining a startup is often fraught with uncertainty, as new ventures typically lack observable performance metrics, funding history, or established market traction (Campero & Kacperczyk, 2020; Freeman et al., 1983; Stinchcombe, 1965; Yang & Aldrich, 2017). Under such conditions, job seekers, like other external audiences, such as investors (Bapna, 2019, Kanze et al., 2018; Snellman & Solal, 2023) or customers (Younkin and Kuppuswamy, 2019) often rely on the attributes of the founding team as heuristic cues (Kanze et al., 2018; Snellman & Solal, 2023; Snellman & Younkin, 2021). Founder characteristics and team structures thus function as salient signals, influencing beliefs about a venture's performance and growth potential.

If spousal team structure serves as a signal to prospective hires, the key question is how job seekers interpret it during the application decision. Boundary-work and role theory perspectives imply that spousal-led ventures will be especially vulnerable to discounting at this early evaluation stage because the arrangement can be read as a role-boundary violation, one that blurs the culturally salient separation between "family" and "work." Role theory posits that audiences evaluate actors through shared expectations about appropriate conduct within and across roles (Kahn et al., 1964). When a focal arrangement merges roles governed by different norms and obligations, evaluators may anticipate role conflict, biased judgment, or impaired performance, and they often treat such overlap as a deviation from what "should" occur (Ashforth et al., 2000). Consistent with this logic, research on categorical boundaries shows that organizations and individuals who span or blur established categories can be penalized, either because the configuration is more difficult to interpret or because it is perceived to violate implicit social rules

about keeping domains distinct (Leung & Sharkey, 2013; Ruef & Patterson, 2009; Zuckerman et al., 2003) .

Extending this argument to entrepreneurial teams, these studies imply that blending multiple roles, especially from distinct domains, such as personal and professional, can trigger normative clashes that could undermine external evaluators' assessments (Li & Piezunka, 2020; Stryker, 1987). The “ideal-type” script of founding teams prescribes that cofounders should be assembled to maximize venture performance and operate according to impartial, merit-based logics rather than personal ties or non-economic considerations (Hellmann & Wasserman, 2016; Ruef, 2010; Wasserman, 2012; Yang et al., 2020). With respect to team formation, the underlying assumption is that ventures are more likely to succeed when members are selected on the basis of their capacity to advance organizational goals, rather than through relational favoritism or role obligations (Ruef, 2010). With respect to team operation, the expectation is that decisions will be made according to impartial, meritocratic principles rather than interpersonal obligations (Sine et al., 2006; Wasserman, 2017). When a founding team blurs the boundary between family and firm, it can appear to depart from this professional template, triggering concerns about whether the team was formed and will be governed primarily in the venture's interest.

Spousal cofounding is especially likely to activate these boundary-violation inferences. Although other close ties (i.e., friends or relatives) may also introduce ambiguity, marriage is culturally coded as intimate, affect-laden, and exclusive, and thus as normatively governed by commitments that sit uneasily alongside expectations of workplace impartiality (Ashforth et al., 2000; Ridgeway & Correll, 2004; Townsend, 2002; Zelizer, 2005). In the hiring context, this boundary crossing can raise two distinct concerns for prospective employees. First, it can prompt doubts about team formation, whereby outsiders may infer that the team was assembled for

personal or familial reasons rather than through a purely performance-oriented selection process, even if both spouses are highly qualified. Second, it can trigger concerns about startup governance, whereby job seekers may anticipate that marital intimacy and exclusivity will influence key decisions, such as hiring, promotion, conflict resolution, equity allocation, or voice, potentially undermining procedural fairness and creating an insular “inner circle” at the top. These concerns are likely to be amplified in early-stage ventures, where applicants face high uncertainty and limited verifiable information, making them more prone to rely on categorical cues and default interpretations of closed networks or nepotistic dynamics (McPherson et al., 2006; Ruef, 2010; Zahra & Sharma, 2004). Overall, because spousal founding teams visibly combine the marital role with the entrepreneurial leadership role, they may be particularly prone to boundary-crossing penalties in the labor market. We therefore expect prospective employees to be less likely to pursue jobs at ventures led by spousal teams than at otherwise similar ventures led by non-spousal founding teams.

### **Gendered Expectations and Potential Exemption from Penalty**

While spousal cofounding may trigger concerns about boundary violations, the severity of these concerns likely depends on who leads the venture. Gendered expectations attached to male versus female lead founders may alter how audiences interpret the same team structure.

Entrepreneurial ventures, though typically cofounded, rarely feature egalitarian partnerships. Instead, founding teams exhibit clear hierarchical orderings, with one member emerging as the lead founder who orchestrates team assembly and commands authority (Beckman & Burton, 2008; Yang & Aldrich, 2014). This hierarchy is important because gendered expectations attach asymmetrically to male versus female leadership, influencing how audiences interpret the spousal cofounding structure (Eagly & Karau, 2002; Heilman & Eagly, 2008;

Ridgeway, 2011). Entrepreneurship is culturally constructed as a masculine domain, associated with traits such as agency and instrumentality (Snellman & Solal, 2023; Thebaud, 2016). Within this cultural construction, the prototypical male founder is often presumed to represent an agentic leader who leverages professional networks to recruit qualified (often male) collaborators and operates the venture in a strictly professional manner (Audia & Rider, 2005; Ruef, 2010; Wasserman, 2012). Because men are expected to embody this professional ideal, forming and leading teams to maximize venture performance, deviations from it are more salient and more heavily sanctioned. Spousal cofounding is likely to violate this masculine template. When a male founder partners with his wife, audiences may view with this arrangement as an intrusion of intimacy into professional space, suggesting that personal bonds have displaced rational team construction. These teams risk being perceived as nepotistic, signaling that the founder values relational comfort over competitive advantage. For male-led ventures, spousal cofounding may therefore function as a stigmatized signal, inviting doubts about the founder's professionalism and ultimately deterring prospective talent.

By contrast, female-lead ventures are likely to face different evaluations. Growing research shows that the agentic, instrumental stereotype of a founder is less likely to apply to women. Instead, gender stereotypes often cast women as more relational and communal (Acker, 1990; Gorman, 2005; Heilman & Eagly, 2008; Ridgeway, 2001), potentially rendering spousal cofounding more role-congruent. What appears as norm violation for men may be interpreted as role fulfilment for women. Consistent with this claim, research shows that, in entrepreneurial contexts, women are routinely perceived through familial lenses, as motivated by work-family balance, constrained by caregiving responsibilities, or oriented toward relational rather than purely economic goals (Kacperczyk & Younkin, 2021; Thébaud, 2015; Yang & Aldrich, 2014). A

husband's presence on the team is therefore less likely to generate dissonance, and may even confer a form of masculine legitimation, offsetting the perceived incongruity between femininity and the founder role. Structural constraints that women face may further reinforce these evaluative patterns. Women founders face well-documented barriers to team formation, including restricted access to venture capital, exclusion from elite networks, and systematic disadvantages in recruiting top-tier talent (Guzman & Kacperczyk, 2019; Kanze et al., 2018; Snellman & Solal, 2023). Therefore, spousal cofounding may be reframed from a signal of questionable judgment into one of pragmatic adaptation, a response to systemic exclusion rather than a lapse in professionalism. For female-led ventures, evaluators may infer that the choice is not as a signal of nepotism but a navigation of an inhospitable environment. Overall, we therefore expect that whereas spousal cofounding will likely result in a hiring penalty for male-led ventures, while this penalty may be weaker for female-led ventures.

## **RESEARCH DESIGN**

We propose that spousal founding teams will incur significant penalties when mobilizing labor, in part, because job candidates will be less likely to perceive such teams as attractive employers. We proceed with a two-stage research design. First, we begin with large-scale observational data to determine whether there is support for our motivating claims that (a) spousal founding teams are less able to attract labor than their non-spousal counterparts, and that (b) there are persistent differences in such tendencies across female- and male-founded startups. Second, we conduct a LinkedIn field experiment that leverages random assignment of job candidates to founders of different gender and thus allows for identifying the causal relationships. Finally, to explore the mechanisms, we conduct a preregistered online experiment on Prolific. Overall, our

approach has the benefit of pairing correlational evidence from observational data with causal evidence from a field experiment and online experiment.

### **Study 1: Observational Evidence**

We first assess observational patterns using U.S. Panel Study of Entrepreneurial Dynamics II (PSED II), which provides a nationally representative sample of U.S. entrepreneurial teams, PSED II is particularly suitable for our analyses because the data provide rich and detailed information on founding teams, including their demographics, marital status as well as leadership positions within new ventures. PSED II began in 2005 with the identification of 1,214 nascent entrepreneurs from a nationally screened sample of 31,845 adults. Respondents were then tracked annually until either they exited the startup process, or their ventures were formally established, with data collection concluding in 2011. PSED II employed a two-phase design that first screened a nationally representative adult population and then followed identified entrepreneurs longitudinally. The research design consists of two phases. In the first phase, a representative sample of 31,845 individuals living in the contiguous 48 states and the District of Columbia was screened in 2005 to identify nascent entrepreneurs. When an adult 18 years of age or older was identified and agreed to respond to the survey, a screening interview was conducted to identify nascent entrepreneurs, using a set of three general qualification questions.<sup>1</sup>

Respondents who answered “yes” to any of the initial questions were asked several additional questions, including whether they had undertaken startup activities to create a new venture. Second, respondents were asked if the new business was still at an early stage of venture

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<sup>1</sup> These three questions are: (1) “Are you, alone or with others, currently trying to start a new business, including any self-employment or selling any goods or services to others?” (2) “Are you, alone or with others, currently the owner of a business you help manage, including self-employment or selling any goods or services to others?” (3) “Are you, alone or with others, currently trying to start a new business or a new venture for your employer, an effort that is part of your normal work?”

creation and on the way to becoming a fledgling firm rather than merely having been established (Yang & Aldrich, 2017). In the second phase, individuals who reported starting a new business were followed annually from 2005 to 2011. They were observed until they reported terminating the business, at which point observation ceased. Full interviews were conducted to collect detailed information about each founder, cofounder, and the startup (Reynolds & Curtin, 2009b). On average, startups are followed for 54 months (SD=35 months), with the observation period ending in the year of termination.

*Sample.* We constructed the analytical sample in several steps. First, we applied an action criterion: entrepreneurs must have undertaken at least one activity toward creating a new business, ensuring that the venture was more than just an idea. Between 2005 and 2011, in each interview wave, PSED II collected data on more than fifty entrepreneurial activities, such as registering with a government agency, listing with D&B, or joining a trade or industry association. Based on this information, we excluded six startups that had not undertaken any activity, leaving 1,212 startups. Second, following prior work (Lichtenstein et al., 2007; Reynolds & Curtin, 2009a), we excluded startups whose first recorded activity occurred more than ten years before the observation period, removing 68 already-mature efforts and reducing the sample to 1,144 startups. Third, because our objective was to follow startups prospectively, we began observations at the start of the observation period (the initial interview) and discarded all monthly spells that had been reported retrospectively or prior to the observation period. This reduced our sample to 1,087 startups. To additionally avoid left-truncation in our focal event—hiring the first employee—we excluded 139 startups that had already hired employees before the observation period. Combined, these exclusions resulted in a sample of 948 startups. After listwise deletion of cases with missing values, our analytic sample consisted of 909 startups. Finally, 34 of the 909 startups had male and female entrepreneurs leading

the business together. We dropped them from the sample, resulting in an unbalanced panel of 875 startups and 4,671 spells of observations for them.

### **Dependent Variable**

***First Hire.*** We focus on the first hire because this milestone carries particular significance for early-stage ventures. The initial hire marks the transition from self-employment to an organization that must coordinate non-founder labor (Yang & Aldrich, 2017). According to Aldrich and Ruef (2006), successfully recruiting the first employee signals the emergence of the startup as an entity independent from its founders—a critical juncture in organizational formation. Prior to this point, the venture exists primarily as an extension of the founding team; afterward, it begins to function as an organization with distinct boundaries between founders and employees. The first hire also provides an empirically comparable benchmark across ventures. While startups vary considerably in their subsequent growth trajectories, some scaling rapidly, others remaining small, virtually all must confront the threshold decision of whether and when to bring on their first non-founder employee. This common decision point enables comparisons across heterogeneous ventures that would otherwise be difficult to benchmark against one another. Furthermore, the first hire is a context in which external evaluations are especially consequential. Early-stage ventures lack established track records, formal HR processes, and reputational signals that later-stage organizations can leverage to attract talent. Prospective employees must therefore rely heavily on observable cues about the founding team when assessing whether to join (Honoré & Ganco, 2023). This evaluative dependence on founder characteristics makes the first hire particularly informative for examining whether spousal cofounding triggers boundary-crossing concerns that deter potential applicants.

In the PSED II survey, founders were asked: “*Are any managers or employees, including exclusive subcontractors, now working for pay who would not share ownership?*” A “yes” response indicates that the venture has hired at least one paid worker. Respondents were then asked: “*In what month and year was the first person hired for this (new) business?*” Using this information, we construct a monthly, time-varying indicator coded 1 beginning in the month the first hire occurred and 0 otherwise. This measure indicates both the *occurrence* and *timing* of the first hiring event, enabling us to examine how quickly ventures cross the threshold from founder-only to employer organizations.

### **Independent Variables**

***Spousal vs. Non-Spousal Cofounding Teams.*** During the phone interview, respondents were asked: “*How many people will legally own this new business—only you, you and your spouse, or you and other people or businesses?*” If respondents indicated shared ownership, they were asked to identify up to five additional owners with the highest levels of ownership. For each cofounder, respondents then reported the nature of their social relationship, specifically whether the tie was spousal, friendship, or kinship. Based on this information, we classified ventures into three types: (1) solo entrepreneurs, (2) spousal cofounding teams, and (3) non-spousal cofounding teams.<sup>2</sup>

***Lead Founder Gender.*** This variable is coded using two pieces of information: (1) who is identified as leading the business; and (2) the gender of that lead entrepreneur. A valid measure of the lead entrepreneur encompasses two dimensions: authority and responsibility (Beckman et al.,

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<sup>2</sup> About 8.5 percent of entrepreneurial teams included both spouses and other cofounders. Because this subgroup is relatively small, we do not treat mixed-relationship teams as a separate category. Instead, we tested the robustness of our results by classifying these teams in three ways: as spousal, as non-spousal, or excluded altogether. Across all specifications, the results were unchanged. The final models reported in the paper exclude these mixed-relationship teams.

2007; Yang & Aldrich, 2014). In PSED II, respondents were asked the question “*Which of the owners would be considered in charge of day-to-day operations of the new business?*” Respondents were allowed to report (1) one individual owner is in charge, (2) several owners jointly are in charge, or (3) all owners are equally in charge. The self-reported answer regarding “who is in charge of the daily operation of a new business” indicates both authority and responsibility. Although respondents are allowed to report multiple lead entrepreneurs, only 9% of multi-member teams, and 6% of mixed-gender teams have more than one owner taking the lead, consistent with previous research suggesting that leadership in task groups is mostly assumed by a single individual for efficiency reasons (Gould, 2002). Respondents reported gender, male or female, for themselves and their co-owner(s). We thus code *gender of the lead entrepreneur* as “1” for women, and “0” for men.

***Control variables.*** We include a set of team- and startup-level covariates, measured at the time of founding to mitigate potential endogeneity. In the PSED II survey, the responding entrepreneur provided information on each founder and cofounder. Using the information on each founder, we constructed covariates for team-level attributes, including average years of work experience in the same industry where a new firm has been created and average years of managerial experience. Prior experience in the same industry provides founders with domain-specific knowledge, legitimacy, and access to sectoral networks, all of which can accelerate early hiring (Shane, 2000). Managerial backgrounds equip founders with organizational skills that directly affect the ability to recruit and manage employees (Beckman et al., 2007). We also measured average startup experience, indicated by the number of other new businesses created and the number of other businesses owned. Both variables reflect prior startup experience, which may help startups successfully recruit employees (Honoré, 2022). We include two general human capital

variables, including average education level, years of full-time paid work experience, gender composition of the founding team, measured as gender diversity calculated based on the Blau index  $\sum_{i=1}^N (P_i^2)$ , where  $i$  indicates an owner,  $N$  indicates the total number of owners, and  $P_i$  is the proportion of men or women. Controlling for gender composition is important to ensure that the difference between spousal and non-spousal teams reflect relationships rather than gender compositions. We also include a dummy variable for whether the startup has received *revenue* in the first year of the operation. Controlling for differences in initial revenue allows us to compare spousal teams and non-spousal teams with a comparable revenue status, but our results remain robust even when excluding revenue status from our specifications. Finally, we include year-fixed effects to account for macroeconomic conditions affecting labor market dynamics, and industry fixed effects (first-digit NAICS code) to control for sectoral differences in labor intensity and hiring norms. [Table A1](#) in Appendix A presents the descriptive statistics and the correlation table.

## Results

Our analysis of the PSED II sample reveals both the prevalence of entrepreneurial teams and its gender patterns in how those teams are formed. Importantly, women are slightly less likely than men to start businesses with cofounders—40% of the 331 women-led startups involve teams, compared with 48% of the 544 men-led startups. Yet when women do cofound, they are much more likely to include a spouse: 61% of women-led teams involve a spousal partnership, compared with only 48% of men-led teams. In short, women are less team-oriented overall, but when they do build teams, they disproportionately turn to spousal partners. For men, by contrast, spousal cofounding is less common and more unconventional. This gendered contrast therefore suggests that male-led spousal ventures are especially likely to face a discount from prospective applicants, as their unusual team structure raises questions about professionalism and organizational

boundaries. The descriptives also show that 11% of startups made the first hire during our observation period. Among those that hired employees, the average time taken to make the first hire is 35 months (SD=26 months).

We next examine the hazard rate of the first hire using Cox proportional hazards models. Due to repeated observations, we cluster standard errors by a startup. [Table 1](#) shows that, while spousal cofounding is not significantly associated with the hazard of the first hire in the full sample, this relationship varies substantially by founder gender. For male-led ventures, spousal cofounding is associated with a significantly slower hiring rate: the hazard rate of making a first hire is only 20% that of male-led non-spousal teams [ $\exp(-1.634)$ ,  $p < 0.05$ ]. Thus, spousal cofounding reduces male-led ventures' hazard of first hire by 80%. At the same time, we do not find a significant impact of spousal founding team on female-led ventures. This correlational finding suggests that the spousal cofounding penalty in attracting early employees may be concentrated among male-led startups.

**[Insert Table 1 here]**

## **Study 2: A Field Experiment**

Our initial evidence from PSED II suggests that spousal cofounding is associated with a labor-market penalty, particularly for male-led ventures. However, two concerns complicate causal interpretation. First, this observed relationship may be spurious if unobserved factors, such as venture culture or growth potential, simultaneously influence both spousal team formation and job attractiveness. This concern is particularly acute because entrepreneurs self-select into team (Ruef et al., 2003)s, and their decisions may reflect considerations that also affect labor-market appeal. Second, although our theory focuses on external evaluation of the startup by prospective employees, the findings from PSED II may instead reflect the founding team's own hiring decisions. For instance, spousal teams may be slower at making the first hire because it takes

longer time for them to become operational. To address these endogeneity concerns, we employ a preregistered two-stage reverse-audit field experiment.<sup>3</sup> As Figure 1 summarizes, this design enables us to causally estimate the effect of spousal cofounding teams on a startup’s attractiveness to job seekers.<sup>4</sup>

**[Insert Figure 1 here]**

**Partner Setting.** We partnered with an early-stage U.S.-based marketing startup (founded in 2024) specializing in AI-powered influencer networks for social media marketing and live commerce. The startup has two cofounders (one male, one female) and 22 employees (10 full-time and 12 part-time), with the female cofounder serving as the operating lead. The venture sought to hire entry-level marketing specialists for each of their offices in California, New York City, Chicago, and Austin, with remote or hybrid options available.<sup>5</sup> Compensation was listed as \$5,000–\$12,000 per month, reflecting market benchmarks and allowing us to observe applicants’ salary expectations and location preferences.

**Stage-1 outreach (closed search).** We leveraged a reverse-audit, two-stage design (Teng, Wright, Kacperczyk, 2026). In the first stage, a coauthor acting as recruiter used LinkedIn Recruiter to conduct targeted outreach. LinkedIn Recruiter enables employers to discover qualified candidates and contact them directly through InMails, a widely used practice in entrepreneurial hiring (Castilla & Rho, 2023; Snellman & Younkin, 2021). We filtered candidates based on: (1) location proximity to target offices (California, New York City, Chicago, and Austin); (2) 0–3 years of full-time work experience; (3) skills in digital marketing, influencer marketing, or social

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<sup>3</sup> Preregistered on AsPredicted #228431 on May 15th, 2025: <https://aspredicted.org/jg9g-8x9t.pdf>

<sup>4</sup> This study has received ethics clearance through the London Business School Research Ethics Committee [REC No: 998] and the Institution Review Board at University of Pennsylvania [IRB PROTOCOL#: 857577].

<sup>5</sup> While these locations reflect the recruiting needs of the company, they also enhance the external validity of our experiment, spanning the East Coast, Midwest, and West Coast of the United States and thus reflecting variation in cultural norms and labor market characteristics. These areas have a relatively high concentration of active talent on the LinkedIn Recruiter platform, making it feasible to reach enough qualified candidates.

media marketing; and (4) active job search status. Each candidate received a uniform InMail describing the role, salary range, and office location (tailored to the candidate's state). The company name was anonymized to prevent interference from other organizational characteristics. Candidates were instructed to express interest by direct reply.

***Stage-2 randomization and Survey.*** Only candidates who expressed interest advanced to the second stage. Interested candidates received a link to a Qualtrics survey containing the job description (identical to the outreach message) and, upon clicking “Next,” were randomly assigned (equal probability) to one of four experimental conditions in a 2×2 design: 1) a male-leading spousal cofounding team; 2) a female-leading spousal cofounding team; 3) a male-leading non-spousal cofounding team; and 4) a female-leading non-spousal cofounding team. Meanwhile, the Qualtrics backend would record their survey progress. Immediately after treatment exposure, candidates answered two verification questions to ensure treatment compliance. They then completed the application form, which collected their name, expected salary, working arrangement, and a resume upload. Candidates were informed that only applications with correct verification answers and a submitted resume would be considered valid.

This two-stage design offers several advantages. First, by administering the treatment only to candidates who expressed initial interest, we ensured that subsequent responses reflected genuine interest rather than casual browsing. This pre-screening increased response rates and enabled randomization among a more qualified candidate pool. Second, randomly assigning treatments after initial interest minimized selection bias, ensuring comparable baseline motivation across treatment conditions and enhancing internal validity. Finally, while targeted outreach differs from open-market search, it represents a common and consequential recruitment practice. Firms

frequently use closed search strategies to identify prospective employees (Black et al., 2024), particularly on platforms like LinkedIn where targeted outreach is routine (LinkedIn, 2023)<sup>6</sup>.

***Application Process.*** The Qualtrics survey was designed to collect both candidates' formal job applications and optional responses. Candidates were instructed to complete their applications within five days of receiving the application link. The final round of applications was due on May 25, 2025. It took the candidate 10-15 minutes to finish the job application. The application form was divided into four main sections: (1) manipulation, (2) job application, (3) optional demographic questions, and (4) optional follow-up questions related to perceptions and decision-making. Upon completing all the answers, candidates were automatically redirected to a debriefing page notifying them of their participation in a research study.

***Manipulation.*** After reading the first page, which replicated the company and job description presented in the initial outreach message, all candidates who clicked "Next" were randomly assigned to one of four experimental conditions, each featuring a unique message from the cofounders. We leveraged cofounders' pseudonyms, signoffs, and descriptive framing to signal both the gender of the leading founder and the nature of the relationship between the two cofounders, as [Appendix C](#) shows. The four experimental messages were otherwise identical, differing only in the manipulated elements. Recognizing that names may unintentionally convey other cues, such as ethnic information, and introduce noise, we carefully selected two race-neutral<sup>7</sup>

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<sup>6</sup> <https://www.linkedin.com/business/talent/blog/talent-acquisition/helping-recruiters-save-time-increase-candidate-engagement-with-ai?>

<sup>7</sup> We considered names that are less likely to represent a given race as "race neutral." Here we selected the names that match closely the race distribution of the general population in the United States: 63.9% White, 16.3% Hispanic origin, 12.3% Black, 4.9% Asian or Pacific Islander, 1.8% Two or More Races, and 0.7% American Indian or Alaskan Native.

yet gender distinct names, Hugh<sup>8</sup> and Elise<sup>9</sup>, to represent the founding team in a way that aligns with our research purposes. These pseudonyms were approved by the real cofounders prior to implementation. We also manipulated the perceived relationship between cofounders using surnames at the sign-off. Specifically, we mixed and matched two commonly used, race-neutral surnames, Smith<sup>10</sup> and Anderson<sup>11</sup>, in the sign-off lines. In the spousal conditions, the cofounders shared the same last name (i.e., “Hugh Smith, with my wife Elise Smith”) to indicate a marital relationship. In the non-spousal conditions, the cofounders had different surnames (i.e., “Elise Smith, with my colleague Hugh Anderson”). The explicit mention of the spouse and a shared family name thus served as a salient cue for a spousal relationship, presented consistently at the end of the message.<sup>12</sup> In addition to the naming conventions, we manipulated leadership roles through the descriptive framing of the founders. In all conditions, candidates read a message written from the perspective of the lead founder. For example, in the male-leading spousal condition, the message stated: “I am Hugh Smith, and I oversee the daily operations of our startup... I cofounded the company alongside my wife, Elise Smith.” In contrast, in the female-leading non-spousal condition, the message stated: “I am Elise Smith, and I oversee the daily

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<sup>8</sup> According to MyNameStats.com, the race distribution of the people with the first name “Hugh” is roughly estimated to be 78.7% White, 2.1% Hispanic origin, 15.4% Black, 1.5% Asian or Pacific Islander, 1.5% Two or More Races, and 0.7% American Indian or Alaskan Native. It is also used as a male name 100% of the time, according to the Social Security Administration's (SSA) Baby Names database.

<sup>9</sup> The race distribution of the people with the first name “Elise” is roughly estimated to be 77.8% White, 6.3% Hispanic origin, 11.4% Black, 2.5% Asian or Pacific Islander, 1.5% Two or More Races, and 0.6% American Indian or Alaskan Native. It is also used as a female name 100% of the time.

<sup>10</sup> The race distribution of the people with the last name “Smith” is roughly estimated to be 73.3% White, 1.6% Hispanic origin, 22.2% Black, 0.4% Asian or Pacific Islander, 1.6% Two or More Races, and 0.8% American Indian or Alaskan Native.

<sup>11</sup> The race distribution of the people with the last name “Anderson” is roughly estimated to be 77.6% White, 1.6% Hispanic origin, 18.1% Black, 0.5% Asian or Pacific Islander, 1.6% Two or More Races, and 0.7% American Indian or Alaskan Native.

<sup>12</sup> A pilot A/B test on Prolific varying whether the founder’s signature mentioned a spouse shows no difference in willingness to join. Open-ended evaluations of the founder letter contain no references to the sign-off.

operations of our startup... I cofounded the company alongside my colleague, Hugh Anderson.”

Parallel adjustments were implemented across all conditions to isolate the effects of both cofounder relationship (spouse versus non-spouse) and gendered leadership. To ensure that candidates received and registered the treatment, and to deter bots, we included two verification questions immediately after the treatment. Candidates were asked to identify (1) the name and gender of the lead founder (e.g., “The startup is led by Hugh, a male cofounder”) and (2) the relationship between the cofounders (spouses, colleagues, or siblings). Candidates were told these questions served as bot checks, and only those who answered both correctly were treated as having received the intended manipulation. Candidates were explicitly informed that incorrect responses would disqualify their applications, as such errors created uncertainty about whether the application reflected genuine human effort or bot activity. After the application deadline, applicants who failed the manipulation check were contacted and asked to confirm their application decision after being shown their assigned treatment again. This procedure served two purposes: it identified respondents who accurately received the assigned treatment and reinforced treatment exposure for those who may have proceeded without fully registering the experimental condition.

The second section contained the mandatory job application form for all candidates who proceeded beyond the reinforced treatment. The form collected (1) basic contact information (name, email, phone number), (2) minimum expected monthly salary, (3) preferred work arrangement (no preference, fully on-site, fully remote, or hybrid), and (4) a résumé upload. Before entering this section, candidates were informed that applications would be considered valid only upon successful résumé submission. The third section collected optional demographic information, including employment status, highest educational qualification, age, gender, race/ethnicity, years

of full-time work experience (overall and in startups), and marital status. Finally, candidates rated their willingness to apply to startups with different lead-founder gender and cofounder-relationship configurations (e.g., a male-leading spousal team, a female-leading non-spousal team) and were invited to provide open-ended comments. Upon completion, all candidates were automatically redirected to a debrief form explaining that the posting was part of a research study on the effects of spousal confounding on early-stage startup hiring. The debrief also clarified that responses to the optional sections would remain confidential and would not affect candidates' standing.

**Sampling.** We preregistered our field experiment with the goal of collecting at least 400 valid applications (i.e., completed applications with submitted resumes) or reaching the outreach cap of 3,000 pre-screened candidates, whichever came first. Based on a power analysis from a pilot study (80% power to detect a moderate effect size of  $d = 0.4$  at  $\alpha = 0.05$ ), we aimed for at least 100 valid applications per experimental condition (400 in total) in our  $2 \times 2$  between-subjects design. In practice, we expanded outreach to 4,495 candidates in California, New York City, Texas, and Illinois to offset higher-than-expected attrition.<sup>13</sup> Of these, 933 expressed interest in the position, yielding an interest rate of 20.8%.<sup>14</sup> Of these, 644 (69%) clicked the job application

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<sup>13</sup> We conducted a pilot study in early April. Due to the timing of the recruitment season, the formal experiment launched in mid-May experienced a higher-than-expected attrition rate at the initial contact stage. For example, although our outreach targeted candidates labelled as "active talent" on LinkedIn Recruiter, many of them had already accepted new jobs but had not updated their profiles to reflect their current employment status.

<sup>14</sup> We assessed whether candidates who expressed interest but did not complete applications differed systematically from those who did, addressing concerns about attrition bias and whether targeted outreach attracts lower-commitment candidates (Black et al., 2024; Kim & Pergler, 2025). Using Revelio Labs' workforce data, we constructed demographic and human capital indicators to compare applicants with non-applicants. As shown in [Table B1](#), applicants were more likely to hold graduate degrees (31% vs. 19%;  $p < 0.001$ ) but less likely to hold only bachelor's degrees (58% vs. 65%;  $p < 0.05$ ), resulting in marginally higher overall bachelor's-or-higher attainment (89% vs. 84%;  $p < 0.10$ ). No significant differences emerged in sub-bachelor education (3% vs. 5%) or gender composition (43% vs. 45%). These patterns suggest two conclusions. First, the lack of systematic demographic differences indicates minimal attrition bias in the analytic sample. Second, targeted outreach did not yield low-commitment applicants; those who completed applications were disproportionately graduate degree holders—precisely the candidates typically valued in the labor market—while those with only bachelor's degrees were more likely to disengage.

link, and 630 (68%) were assigned to one of the four conditions.<sup>15</sup> The final round of surveys closed on May 25, 2025.

Data cleaning followed three preregistered steps. First, of the 630 who entered the treatment, we excluded 3 entries that were either test responses or contained fabricated information and 3 anonymous entries that failed to answer the manipulation check questions. Second, we examined IP addresses and removed 26 duplicate entries, retaining only the first entry based on the earliest recorded survey start time.<sup>16</sup> After these steps, our final sample consisted of 598 candidates confirmed to have received the treatment, of whom 471 submitted applications and 127 did not. Of the 471 valid applicants, 362 completed all questions in the optional section.

## Variables

**Dependent Variable.** Our primary dependent variable,  $Apply_i$ , is a binary indicator measuring whether a candidate submitted a complete valid application (passed the verification check and uploaded their resumes) following random assignment. Similar to prior reverse-audit experiments evaluating candidate engagement (Hurst et al., 2024; Kacperczyk & Younkin, 2021), this measure reflects the behavioral outcome of job interest beyond mere click-through or message open rates.

**Independent Variable.** Our main independent variable,  $Spousal_i$ , is a binary treatment variable denoting whether the candidate  $i$  was randomly assigned to view a job opportunity at a startup cofounded by a spousal team ( $Spousal = 1$ ) or a non-spousal team ( $Spousal = 0$ ). The manipulation was embedded in the message from the founder, where cofounders were either

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<sup>15</sup> These entries are entirely blank except for the fields for state date and end data. The Qualtrics system failed to record any identifying information about the candidates, including their assigned experimental condition.

<sup>16</sup> Duplicate IP entries typically reflected the same candidate accessing the survey from different devices or browsers. To maintain the integrity of random assignment, we retained only the initial entry, ensuring that candidates' responses reflected their first and most authentic exposure to the treatment.

described as spouses or colleagues (non-spousal). All other job details were held constant across conditions.

**Control Variables.** We collected additional information on applicants' gender, age and salary—all based on optional self-reported responses following valid applications.  $Male_i$ , is a binary indicator of the candidate's gender;  $Age_i$ , a continuous variable measured in years; and  $\log salary_i$ , a log-transformed continuous variable of the candidate's current or most recent monthly salary in US dollars. These variables allow us to account for baseline variation in job search behavior and application likelihood (i.e., higher-earning individuals may be more selective in startup employment). We also collected information on other demographic factors, including *ethnicity* (White, Hispanic/Latino, Black, Asian, or other races), *marital status* (single vs. partnered), and *highest educational attainment* (less than high school, high school diploma, college degree, bachelor's degree, or postgraduate/master's degree), as these characteristics may relate to candidates' mobility and perceived fit with spousal cofounders. We accounted for candidates' *geographic location* at the time of application (New York City, California, Illinois, or Texas) to adjust for variation in cultural norms and labor market characteristics.

Finally, we collected information on candidates' *employment status* (employed full-time, employed part-time, unemployed but actively seeking, or unemployed and not actively seeking), which may influence their opportunity cost and urgency to apply. We also collected information on prior work experience, including years of full-time employment and full-time startup experience, which may shape candidates' familiarity and comfort with startup environments.

## **Results**

**Descriptive Results.** We begin by examining whether the presence of a spousal relationship between cofounders affects the likelihood of job seekers completing a startup application, our main

outcome. As shown in [Figure 2A](#), the spousal treatment reduced application completion rates by 6 percentage points (0.82 for control group vs 0.76 for the treatment group), indicating that candidates were less likely to apply to startups with spousal cofounding teams than to those with non-spousal teams.

[Table 2](#) reports descriptive statistics and covariate balance across experimental conditions for the full sample, including group means, standard deviations, sample sizes, and standardized mean differences (SMDs). The results indicate that time spent on the application, percentage completion, and the share of male-led teams are well balanced across treatment conditions. Standardized mean differences are consistently small, with few approaching the conventional 0.1 threshold indicating substantive imbalance. By contrast, the likelihood of applying differs substantially across conditions, with a 6-percentage point difference favoring the non-spousal condition.

We next assess whether the observed “spousal penalty” varies by founder gender, as suggested by the observational data. [Figure 2B](#) displays application rates across the four treatment conditions: male-led non-spousal teams (83%), female-led non-spousal teams (80%), female-led spousal teams (78%), and male-led spousal teams (73%). Ventures led by male founders with a spousal cofounder attracted the fewest applications. Relative to male-led non-spousal teams, male-led spousal teams experienced a 10-percentage point reduction in application rates. Male-led spousal teams also received 5 percentage points fewer applications than female-led spousal teams, suggesting that the spousal discount is most pronounced when the lead founder is male. <sup>17</sup>

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<sup>17</sup> [Table B2](#) reports covariate balance for applicants to male-led spousal (treatment) and male-led non-spousal (control) teams. The application rate for the spousal condition was again 10 percentage points lower, while other covariates remained balanced across the two conditions. Finally, [Table B3](#) presents the same analysis for female-led spousal (treatment) and female-led non-spousal (control) teams. In this case, application rates were only 2 percentage points lower for the spousal condition, a difference that is not statistically significant. Again, the other covariates are balanced across the conditions, indicating that randomization worked as intended.

**[Insert Table 2 and Figures 2A, 2B]**

*Parametric Estimates.* To estimate the impact of spousal treatment on candidates' application behavior in all the three samples (full, male-led, and female-led), we specify the following logistic and ordinary least squares (OLS) regression models:

$$\text{Logistic: } \log \left( \frac{P(\text{Apply}_i=1)}{1-P(\text{Apply}_i=1)} \right) = \beta_0 + \beta_1 \cdot \text{Spousal}_i + \varepsilon_i$$

$$\text{OLS: } \text{Apply}_i = \beta_0 + \beta_1 \cdot \text{Spousal}_i + \varepsilon_i$$

where  $\text{Apply}_i$  represents a binary indicator for whether candidate  $i$  submitted a completed application. The key independent variable,  $\text{Spousal}_i$ , is a binary indicator for whether the job posting was attributed to a startup cofounded by a spousal team rather than a non-spousal team.  $\varepsilon_i$  is the error term for unexplained variation in application outcomes stemming from factors not captured by the model.

Table 3 presents estimates from both logistic and ordinary least squares (OLS) regression models examining how spousal cofounding affects job application rates. Across all six model specifications, the estimated treatment effects are negative, indicating that exposure to a spousal cofounding team is associated with reduced job-seeking behavior. In the full sample, the treatment effect is marginally significant:  $-0.371$  in the logit model ( $p < 0.1$ , column 1) and  $-0.062$  in the OLS model ( $p < 0.1$ , column 4). These coefficients correspond to approximately a 7.6 percentage point reduction in application likelihood (based on the OLS model), relative to the control group's baseline application rate of 82%.

**[Insert Table 3 here]**

Effects are substantially more pronounced among founding teams where the male partner serves as lead founder. In this subgroup, the treatment effect is  $-0.622$  in the logit model ( $p < 0.05$ , column 2) and  $-0.105$  in the OLS model ( $p < 0.05$ , column 5), implying a 12.8 percentage point

reduction in application likelihood relative to the baseline rate of 82%. By contrast, treatment effects among female-led startups are small and statistically insignificant:  $-0.124$  (logit, column 3) and  $-0.020$  (OLS, column 6), translating to only a 2.4 percentage point decline relative to the same baseline.<sup>18</sup> These subgroup analyses reveal a substantially larger application penalty for male-led spousal teams compared to female-led spousal teams. However, a formal test comparing coefficients across the two subsamples indicates that this difference does not achieve conventional levels of statistical significance ( $\chi^2 = 1.52$ ;  $p = 0.217$ ).

We thus observe a consistent pattern. The subgroup analyses of [Table 3](#) (column 2, 3, 5, and 6) reveal that spousal confounding reduces application likelihood, with this penalty being substantially larger and statistically significant among male-led startups (12.8 percentage point reduction,  $p < 0.05$ ) than among female-led startups (2.4 percentage point reduction, not statistically significant). Accordingly, we find a significant spousal confounding penalty for male-led startups but not for female-led startups, suggesting that the spousal hiring penalty is associated with perceived norm violations—a mechanism we will further explore with a Prolific experiment.

### **Supplemental Analyses: Applicant Sorting**

We also examined the heterogeneity of the applicant pool across conditions. One possibility is that spousal startups, while attracting fewer applicants overall, may draw relatively higher human capital candidates compared to non-spousal teams. [Table B4](#) reports applicants' observable characteristics, conditional on applying, across the different experimental conditions. As shown, there were no significant differences in education, ethnicity, workplace status between applicants to spousal versus non-spousal teams, suggesting no systematic variation in standard human capital

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<sup>18</sup> Our main analyses use the 471 applicants drawn from 598 candidates confirmed to have received the treatment. To rule out selection concerns from the 32 entries removed in data cleaning, we re-estimated the models with an intent-to-treat specification on the full 630 candidates. The findings are robust.

indicators across conditions. However, several notable differences emerge. Applicants to spousal teams were more likely to be women (46% vs. 37%) than those applying to non-spousal teams, and reported substantially higher current salaries (\$4,251 vs. \$2,253). Thus, spousal startups do not appear to attract applicants based on formal human capital credentials, they may nonetheless appeal disproportionately to higher-earning, and more female applicants.

Tables BA5 and AB6 present balance tables for male and female-led startups, respectively. Interestingly, male-led spousal startups attract applicants who are more likely to prefer hybrid work arrangements (33% vs. 22%), though there were no systematic differences in education, workplace status, or other human capital indicators. Hence, these startups do not disproportionately attract higher- or lower-human capital applicants. For female-led startups, applicants to the spousal condition were slightly older (23 vs. 22 years;  $p < 0.05$ ) and reported significantly higher current salaries (\$3,172 vs. \$1,919;  $p < 0.05$ ), but again showed no notable differences in education, ethnicity, workplace status, application time, or progress. Collectively, we interpret these results as suggesting that the reduced applicant volume for male-led spousal startups is unlikely to be driven by human capital sorting and more by gendered perceptions of legitimacy, with men disproportionately opting out of male-led spousal teams.

### **Study 3: Online Experiment and Mechanisms**

The LinkedIn field experiment provides causal estimates of how spousal cofounding teams affect applicant responses, but two design features limit our ability to identify underlying mechanisms. First, observing only candidates who completed applications creates a truncation problem, whereby we cannot compare perceptions between applicants and non-applicants. This limitation, inherent to actual recruitment processes where data on non-applicants are unavailable, restricts our ability to characterize the full population of potential candidates. Second, field settings constrain

direct measurement of the evaluative inferences that drive candidates' behaviors. We observe whether candidates apply, but not why.

To address these limitations, we complement the field experiment with a preregistered online experiment on Prolific,<sup>19</sup> a participant-recruitment platform widely used in the social sciences that enables rapid data collection from a large pool of vetted respondents (Palan and Schitter, 2018). The online setting offers two methodological advantages for investigating mechanisms. First, we observe evaluations from all participants regardless of application decisions, eliminating field experiments' truncation problem. This allows for comparing perceptions across the full distribution of potential applicants, not just positively selected respondents. Second, we directly measure hypothesized mediators (i.e., perceptions of boundary violation) that would be difficult to measure in field settings without compromising ecological validity, since live recruitment process would rarely ask candidates' about their perceptions of founders. Together, these features enable us to replicate the field experiment's behavioral patterns while identifying the mechanisms that underlie spousal teams' penalty when attracting applicants.

### ***Experimental Design.***

The online experiment employed the same design as the field experiment with two notable exceptions: (a) adding a sibling condition alongside spousal and coworker conditions (yielding a  $3 \times 2$  factorial design with 6 experimental conditions), and (b) expanding to two occupations: sales and marketing. We introduce sibling co-founding teams to assess whether the observed penalty for spousal teams stems specifically from the romantic nature of spousal relationships or from family involvement more broadly. Sibling teams share a key feature with spousal teams—family ties within the founding dyad—but lack the romantic or intimate dimension that our boundary-

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<sup>19</sup> <https://aspredicted.org/7yj8jr.pdf>

violation theory emphasizes. If the penalty is driven by concerns about mixing professional and romantic domains, as we theorize, spousal teams should incur greater penalties than sibling teams. Conversely, if the penalty reflects general aversion to family-based founding teams, we would expect similar penalties for both configurations. Second, we broaden the set of roles beyond marketing by examining sales positions, thereby strengthening the external validity of our findings and mitigating concerns about marketing-specific idiosyncrasies. Marketing roles might be perceived as a female-typed function, potentially depressing outcomes for male-led ventures (or inflating outcomes for female-led ventures) through role-gender stereotype congruence effects. To address this concern, we therefore replicate the analyses using sales roles, which are more commonly characterized as male typed. By replicating our analyses across both functions, we can assess whether the observed patterns reflect general hiring dynamics or are artifacts of gender-role stereotyping in specific functional domains.

***Procedure and Sample.*** Participants were randomly assigned to one of six experimental conditions. Following informed consent, they viewed a brief description of an early-stage startup, including information about the founding team. The descriptions for spousal and coworker teams were identical to those used in the field experiment. After reviewing the startup and job information, participants completed the dependent measures. Finally, participants completed a brief demographic questionnaire and were debriefed. We recruited 1,200 participants from Prolific, a platform widely used for behavioral research that provides access to diverse and attentive respondents. Eligibility criteria required participants to be U.S.-based adults currently employed or actively seeking employment, ensuring relevance to the job-seeking context under investigation. Participants who failed attention checks were excluded from the final sample,

yielding an analytic sample of 1,190 participants. The sample was approximately 58% female, with a mean age of 43 years (SD =12.18).

**Dependent Variable:** *Willingness to join the startup as an employee.* The question asked to a participant is: “Given the information about the founding team, on a scale of 1 to 5, how likely would you be to join this startup as an employee?”

**Mediators:** We measure several mediators using 1-5 Likert scale. We begin by measuring *Professionalism* to capture broader evaluations of whether the venture will operate in a legitimate, workplace-appropriate manner beyond any single practice: “How professional does this startup appear to you?” We next measure *Growth Potential*: “How likely do you think this startup can grow in the next 3 years?”, *Career Advancement*: “To what extent does this startup appear attractive for employee career advancement?”, and *Employment Risks*: “How stable do you think joining this startup would be? (stable = positive, risky = negative).” These four mediators aim to capture the overall instrumental attractiveness of the startup. We next assess three related perceptions capturing the founding team’s professional legitimacy, role boundaries, and decision-making norms, each operationalized using two agreement-based survey items on a 1-5 Likert scale. We first assess *Team Competence*, particularly regarding team formation logic, since our theory predicts that candidates will question the reasons for team formation. We use two agreement items: 1) “The founding team inspires confidence in the venture's long-term prospects,” and 2) “The founding team is formed based on considerations of each cofounder's competence.” Second, we measure *Boundary Violation* perceptions, as our theorizing emphasizes that audience reactions hinge on whether the founding team is seen as blurring personal and business domains. We use two agreement items: 1) “The founding team would keep personal relationships and business decisions separate,” and 2) “The founding team would manage the venture in a professional

*manner.*” We also measure *Fairness* because our argument implicates whether the team adheres to impartial, transparent decision-making and remains open to employee voice. Two items measure this construct: 1) “*The founding team would make business decisions in an impartial and transparent manner,*” and 2) “*The founding team would be open to employees' input in making decisions.*” To assess whether competence attributions differ between founders, we measure perceptions of each role separately. *Leading Founder Competence* uses: 1) “*The lead founder demonstrates competent and effective leadership in managing the venture,*” and 2) “*The lead founder is well-connected to startup-relevant professional and industry networks.*” *Supporting Founder Competence* uses: 1) “*The supporting founder possesses the skills and expertise required for their role,*” and 2) “*The supporting founder complements the lead founder's strengths and capabilities.*”

### **Online Experiment Results**

**Sample Balance.** Table D1 presents descriptive statistics and balance tests across experimental conditions. Our sample comprises 1,190 participants randomly assigned to spousal cofounding teams (N=398), non-spousal cofounding teams (N=397), or sibling cofounding teams (N=395). Demographic and background characteristics exhibit strong balance across conditions, confirming successful randomization. This balance supports the internal validity of our design and allows observed differences in outcomes to be attributed to the experimental manipulation rather than pre-existing participant characteristics. Survey completion times also show no significant differences across conditions (spousal condition 8.25 seconds shorter,  $p > 0.10$ ), suggesting comparable attention and effort regardless of assignment.

**Model Results.** Table 4 reports the treatment effects for the pooled sample (columns 1 and 4) and examines heterogeneity in the spousal cofounding penalty by founder gender leadership

(columns 2–3 and 5–6), comparing spousal teams with non-spousal teams. This sample excludes the sibling cofounding teams, resulting in a sample of 795 participants. Consistent with earlier results from the field experiment, spousal cofounding significantly reduces willingness to join relative to unrelated cofounding teams: the estimated treatment effect is  $-0.430$  in the ordered logit model ( $p < 0.001$ ; column 1) and  $-0.292$  in the OLS model ( $p < 0.001$ ; column 4), which corresponds to an approximately 0.29-point decline on the five-point Likert scale relative to the control-group mean of 3.19. Subgroup analyses further suggest an asymmetry by founder gender leadership. Similar to the estimates in the field experiment, for male-led spousal teams, the penalty is larger ( $-0.640$  in the ordered logit model,  $p < 0.001$ ; column 2; and  $-0.408$  in the OLS model,  $p < 0.001$ ; column 5), implying a sizeable reduction in willingness to join relative to baseline levels. By contrast, the estimates for female-led spousal teams are smaller and not statistically significant ( $-0.240$  in ordered logit; column 3; and  $-0.176$  in OLS; column 6;  $p$ -values above conventional thresholds). However, the difference between the male-led and female-led treatment effects does not reach statistical significance ( $\chi^2 = 2.42$ ,  $p = 0.119$ ). Overall, the results show a pronounced hiring penalty associated with spousal founding for male-led ventures, whereas such a hiring penalty is not detectable for female-led ventures. This pattern is robust across job roles: as shown in Appendix [Table D2](#), the spousal penalty is similar for marketing and sales positions, with no meaningful differences across roles.

**[Insert Table 4 here]**

*Mediation Analyses.* After confirming the spousal hiring penalty, we turn to the mechanisms with mediation analyses. [Table 5](#) presents mediation analyses examining the pathways through which spousal cofounding reduces participants' willingness to join the startup. We formally tested mediation using the potential-outcomes (counterfactual) framework with bias-

corrected bootstrap confidence intervals (500 replications) to estimate the ACME (Average Causal Mediation Effect) (i.e., Hicks and Tingley, 2011). This approach defines natural indirect (NIE) and natural direct (NDE) effects and accommodates nonlinearity and exposure–mediator interactions. The a-path captures how spousal confounding influences each mediator, while the b-path estimates how each mediator predicts willingness to join, conditional on treatment assignment.<sup>20</sup>

The mediation analyses ( $N = 795$ ) show that spousal confounding teams reduce applicant interest by triggering distinct evaluative concerns. As [Table 5](#) shows, the overall negative effect on willingness to join (total effect =  $-0.2919$ ) operates primarily through perceived boundary violations and lack of professionalism and fairness, the core mechanisms our theory emphasizes. Boundary violation perceptions emerge as the dominant pathway. When measured as failure to separate personal and business decisions, boundary violations produce an indirect effect of  $-0.3390$  ( $p < .001$ ), larger than the total effect itself, with a proportion mediated exceeding one (1.1613). This pattern of inconsistent mediation indicates that a small positive direct effect (0.0471) is overwhelmed by the strong negative indirect pathway through perceived boundary blurring. The alternative operationalization (unprofessional management) yields nearly complete mediation, with an indirect effect of  $-0.2909$  ( $p < .001$ ) that captures virtually the entire total effect, leaving almost no direct pathway ( $-0.0010$ ). Perceived fairness concerns similarly play a central role, mediating 90% and 69% of the total effect across the two measures (indirect effects of  $-0.2622$  and  $-0.2005$ , both  $p < .001$ ). Together, these findings confirm that applicants view spousal

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<sup>20</sup> While our main specifications are linear, as a robustness check, we re-estimated the mediation analyses using ordered logit models for both the mediator and the outcome, mirroring the specification in the main analysis. The pattern of indirect effects and the relative ranking of mediators are fully consistent with the OLS results reported in [Table D3](#).

teams as violating fundamental norms governing the separation of personal and professional spheres.

Competence concerns operate as a secondary channel. Perceived deficits in founder competence (of both leading and supporting founders), professionalism, and team competence each mediate 54–59% of the total effect (indirect effects ranging from  $-0.158$  to  $-0.174$ , all  $p < .001$ ). These competence-based pathways align with our argument that spousal teams raise doubts about whether cofounders were selected for capability rather than personal connection. Finally, diminished career advancement opportunities and perceived firm growth potential transmit substantial portions of the effect ( $-0.1467$  and  $-0.1412$ , both  $p < .001$ ), suggesting that participants view jobs in spousal startups as less attractive, relative to jobs in non-spousal startups, in part due to multiple norm violations they perceive. Importantly, perceived employment risks show the weakest mediation ( $-0.0822$ ,  $p = .068$ ), suggesting that applicants' aversion stems less from worries about organizational longevity and more from fundamental concerns about fairness, boundary management, and governance quality.<sup>21</sup> Taken together, these results are consistent with the notion that spousal cofounding reduces venture appeal primarily because of norm violations, signalling boundary violations and questionable team formation logic, rather than simply raising doubts about business viability.

**[Insert Table 5 here]**

***Supplemental Analyses on Family Relationships.*** Although our findings establish a discount for spousal founding teams, an important question is whether this pattern reflects a broader penalty for family involvement in entrepreneurship or stigma that is specific to spousal cofounding. To distinguish between these explanations, we test whether the spousal cofounding

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<sup>21</sup> In additional analyses we aggregated the items for boundary violation, fairness, supporting founder, team competence and leading founder. The results, reported in [Table D3](#), show similar results.

penalty persists when sibling cofounding teams, rather than unrelated teams, serve as the comparison condition. [Table D4](#) reports this analysis, directly comparing spousal teams to sibling teams in the full sample. Across both the ordered logit (column 1) and OLS (column 2) specifications, spousal cofounding significantly reduces participants' willingness to join relative to sibling teams. The ordered logit model yields a treatment coefficient of  $-0.263$  ( $p < 0.05$ ), and the OLS specification produces an effect of  $-0.174$  ( $p < 0.05$ ). The OLS estimate implies that exposure to a spousal cofounding team lowers willingness to join by approximately 0.17 points on the five-point scale, corresponding to a 5.7% reduction relative to the sibling-condition mean of 3.08. Columns 3–4 further show that the sibling condition is not statistically distinct from a colleague condition and that the effect does not vary by founder gender. Taken together, these results indicate that spousal teams elicit distinctly more negative responses than sibling teams even when both involve family members working together in leadership roles. Thus, our findings are unlikely to be driven by generic anti-family-firm sentiment; instead, spousal cofounding appears to carry a distinctive stigma that extends beyond concerns about family involvement per se.

## **DISCUSSION**

Although spousal cofounding teams are prevalent in entrepreneurship, prior research has largely emphasized their internal benefits, such as trust, commitment, and coordination (Aldrich & Cliff, 2003; Bird & Zellweger, 2018; Kim, 2005), while leaving underexplored the external liabilities these founding teams may encounter. In this study, we shift the focus from the internal advantages of spousal cofounding teams to their external evaluations, examining how they are perceived by prospective employees and how these perceptions affect ventures' ability to attract talent. Drawing on role theory (Anglin et al., 2022; Ashforth, 2015; Kahn et al., 1964), we propose that spousal cofounding arrangements invite evaluative bias on the part of job seekers because they

deviate from normative expectations of professionalism in team formation and governance. We further examine whether male-led spousal teams face a stronger penalty than female-led spousal teams, as spousal cofounding is less consistent with cultural prototypes of male entrepreneurial leadership, which emphasize reliance on professional networks, independence, and the instrumental selection of partners (Eagly & Karau, 2002; Heilman & Eagly, 2008; Snellman & Solal, 2023).

Our multi-method research design provides convergent evidence in support of these claims. We first draw on observational data from a nationally representative U.S. dataset to document the striking correlational patterns: spousal cofounding teams are more prevalent among female-led ventures and that male-led spousal ventures are associated with slower hiring. These descriptive findings are complemented by a preregistered field experiment, which reveals a consistent behavioral pattern: startups led by spousal cofounding teams receive fewer job applications than otherwise comparable startups led by non-spousal teams, thereby establishing a causal link between team structure and applicant attraction. The negative effect is more detectable among male-led ventures, where spousal teams attract markedly fewer applications than their non-spousal counterparts. By contrast, female-led spousal teams exhibit little difference than their non-spousal counterparts, indicating that the hiring-market penalty might be contingent on founder gender. Finally, an online experiment probes the micro-level mechanisms underlying these effects, showing that spousal team structures increase concerns about boundary crossing and the blurring of professional and personal domains, which in turn dampens perceived job attractiveness. Taken together, these findings suggest that although spousal cofounding is often framed as an internal strength, it can generate external liabilities in labor markets and that these liabilities are systematically conditioned by gender.

Given these findings, our study makes several contributions. First, we advance research on founding teams by shifting attention from internal dynamics to external evaluations. Prior work has shown that team attributes such as functional diversity, prior ties, and demographic similarity shape coordination, innovation, and performance inside ventures (Beckman et al., 2007; Ruef et al., 2003; Yang et al., 2020). We extend this literature by documenting that founding team structure itself can function as a signal, affecting talent acquisition in the entrepreneurial labor market.

Second, we advance research on startup employment by highlighting founding-team *structure*, not only founder attributes, as a factor influencing job seekers' evaluations of new venture opportunities (Hurst et al., 2024; Kim & Pergler, 2025; Snellman & Younkin, 2021). Drawing on role theory (Biddle, 1986; Kahn et al., 1964) and insights from audience evaluation and labor-market sorting, we propose that spousal cofounding signals boundary blurring between personal and professional roles. By documenting that candidates systematically respond to these relational cues, we broaden startup employment research beyond founder-centric signals and show that job seekers infer job quality and organizational viability from the *relationships among* founding team members.

Finally, our study extends research on gender and entrepreneurship by identifying a new mechanism through which access to human capital is differentially constrained. Prior work shows that gender stereotypes affect access to financing (Kanze et al., 2018), network inclusion (Renzulli et al., 2000), and perceptions of entrepreneurial competence (Heilman & Eagly, 2008). We extend this line of inquiry by demonstrating that team structure interacts with gender to generate asymmetric penalties: male founders are evaluated more negatively for partnering with a spouse than female founders, reflecting the incongruity between spousal collaboration and cultural prototypes of male entrepreneurial leadership. These findings also contribute to sociological and

organizational debates on how cultural schemas influence access to critical resources and inequality in entrepreneurial labor markets (Ridgeway & Correll, 2004; Snellman & Solal, 2023).

Our findings also point to several opportunities for future research. First, our field experiment was conducted in the context of a single hiring campaign for a marketing role in an early-stage venture. While this design strengthens causal inference, it constrains generalizability. Spousal penalties may be more pronounced in some settings, such as technology ventures with strong professionalization norms, and less salient in others, such as lifestyle or family-oriented businesses where spousal involvement is normalized. Future work should test for such boundary conditions across industries, job functions, and venture stages. Second, although our experiment isolates the causal impact of spousal framing on application rates, the design also imposes a limitation. Given the setting of “live” recruiting, we observe only the characteristics of those who expressed interest and applied, not those of candidates who received outreach but chose not to apply. As a result, we cannot fully assess how spousal framing influences *who* is deterred from applying and whether certain demographic groups or professional profiles are more likely to disengage at the earliest stage. Future research should address this issue by combining experimental outreach with richer labor-market datasets or platforms that provide information on both applicants and non-applicants. Finally, while our outcomes focus on initial applicant attraction critical for early-stage ventures, they offer limited insight into whether hired employees perform effectively, remain with the venture, or eventually revise their perceptions of legitimacy. The observational evidence confirms that spousal teams are indeed less likely to hire than their non-spousal counterparts, but future work could examine post-hire outcomes, including employee retention, satisfaction, and performance, to assess whether spousal penalties persist or diminish once candidates experience the work environment.

In sum, our study demonstrates that while spousal cofounding is a common and important organizational form, it can also generate significant external liabilities in the labor market for talent. Evidence from observational data, a field experiment and an online experiment shows that spousal cofounding reduces applicant interest in startups, particularly when the lead founder is male. These findings advance our understanding of founding teams by highlighting how team composition functions as a signal to external audiences; extend role theory by showing how overlapping personal and professional roles undermine legitimacy and perceived professionalism; and contribute to research on gender and entrepreneurship by uncovering asymmetric penalties rooted in cultural expectations. Our results also open new avenues for research on how entrepreneurs might mitigate these liabilities and how evaluative biases affect inequality in entrepreneurial labor markets.

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## Tables – Observational Evidence from PSED II

**Table 1.** Cox proportional models of the first hire by startups: the effects of spousal

Male lead entrepreneur	0.218 (0.249)	1.163 (0.689)	+
Solo entrepreneur	-0.491 (0.294)	0.322 (0.777)	+
Spousal team	-0.565 (0.417)	0.469 (0.666)	
Male lead entrepreneur X Solo entrepreneur		-0.883 (0.768)	
<b>Male lead entrepreneur X Spousal team</b>		<b>-1.634 (0.796)</b>	<b>*</b>
Gender diversity of team	0.761 (0.899)	1.179 (0.983)	
Average age of team	-0.029 (0.017)	-0.031 (0.018)	+
Average education of team	0.271 (0.107)	0.290 (0.106)	**
Avg. paid work experience of team	-0.003 (0.019)	0.000 (0.020)	
Avg. no. of startups created before	-0.015 (0.056)	-0.027 (0.058)	
Avg. managerial experience of team	0.025 (0.018)	0.024 (0.018)	
Avg. industry experience of team	-0.002 (0.013)	-0.001 (0.013)	
Achieved revenue in first year	0.126 (0.219)	0.087 (0.223)	
Industry fixed effects	Yes	Yes	
Year fixed effects	Yes	Yes	
-2 LOG L	991.644	986.301	

N=4671 startup-month observations

Clustered standard errors are within parentheses.

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

[\[Back to text\]](#)

**Table 2: Field Experiment:** Balance table for all survey entries by treatment in the full sample: non-spousal cofounders (control group) versus spousal cofounders (treatment group)

	Control Obs	Control Mean	Treatment Obs	Treatment Mean	Difference
Application	312	0.82	286	0.76	-0.06 +
Male-leading team	312	0.50	286	0.49	-0.01
Survey Progress (in %)	312	92.11	286	91.37	-0.75
Survey Duration (in seconds)	312	4945.21	286	4408.17	-537.04
Number of observations	598				
Sample	Full				

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Robust standard errors in parentheses.

[\[Back to text\]](#)

**Table 3: Field Experiment:** The effect of spousal treatment on job application.

	(1)	(2)	(3)	(4)	(5)	(6)
Sample	Full	M-led	F-led	Full	M-led	F-led
Model	Logit	Logit	Logit	OLS	OLS	OLS
Treatment	-0.371+	-0.622*	-0.124	-0.062+	-0.105*	-0.020
	(0.201)	(0.288)	(0.284)	(0.034)	(0.048)	(0.047)
Constant	1.498***	1.609***	1.394***	0.817***	0.833***	0.801***
	(0.147)	(0.216)	(0.201)	(0.022)	(0.030)	(0.032)
Num.Obs.	598	296	302	598	296	302

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001;

Robust standard errors in parentheses.

[\[Back to text\]](#)

**Table 4. Online (Prolific) Experiment:** The Effect of Spousal Treatment on Willingness to Join (excluding sibling teams)

	(1)	(2)	(3)	(4)	(5)	(6)
Sample	Full	M-led	F-led	Full	M-led	F-led
Model	ologit	ologit	ologit	OLS	OLS	OLS
Treatment	-0.430***	-0.640***	-0.240	-0.292***	-0.408***	-0.176
	(0.128)	(0.183)	(0.180)	(0.086)	(0.116)	(0.126)
Constant				3.194***	3.265***	3.122***
				(0.060)	(0.080)	(0.089)
Num. Obs.	795	396	399	795	396	399

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Robust standard errors in parentheses.

[\[Back to text\]](#)

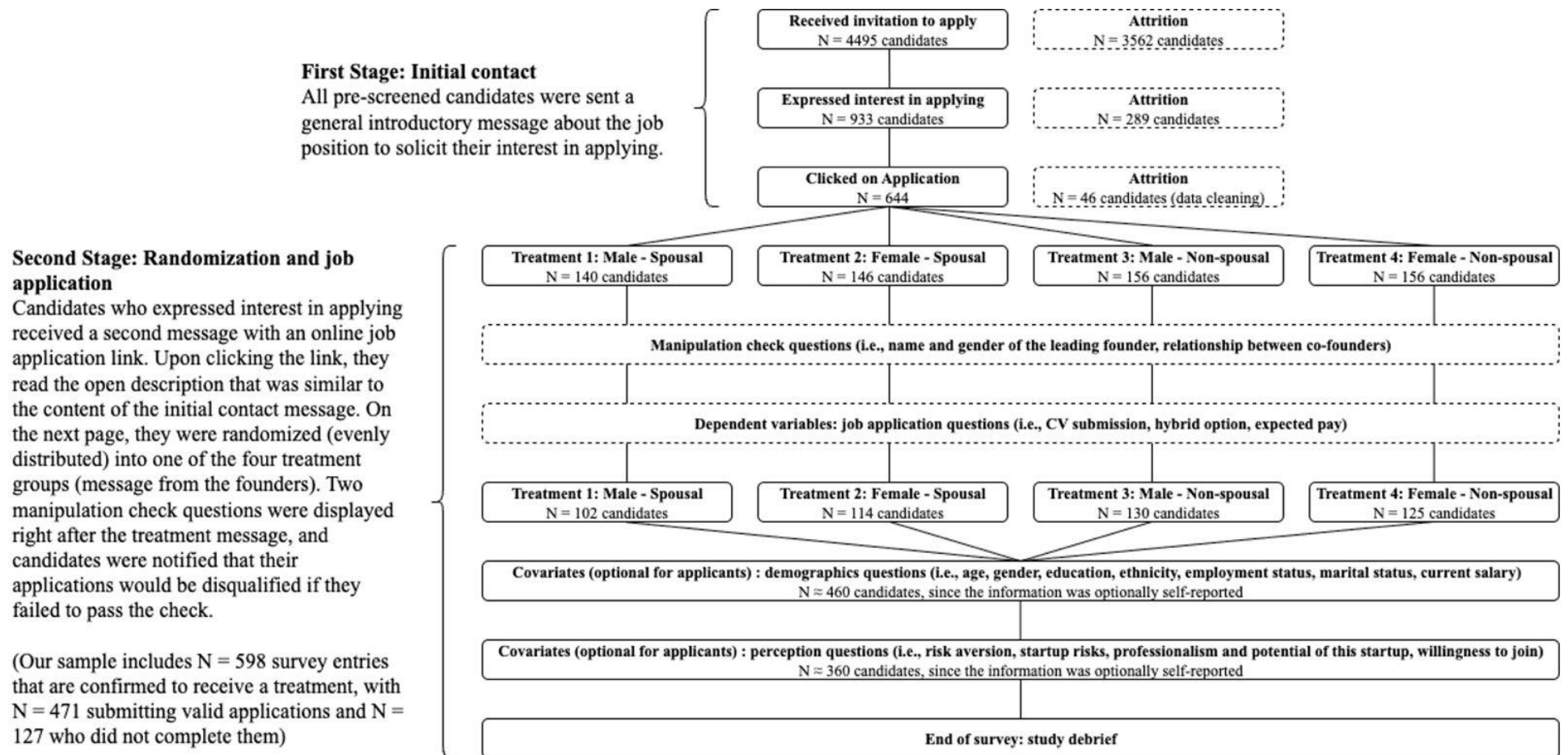
**Table 5: Online (Prolific) Experiment: Mediation Analysis (Full Sample): Spousal vs Non-spousal (excluding sibling teams)**  
a-path: OLS mediator ~ spousal; b-path: OLS join ~ spousal + mediator. Indirect effect = a×b (bootstrap 500 reps, seed = 20260106).  
Proportion mediated = (a×b)/total effect, where total effect is from OLS join ~ spousal.

Mediator	a-path	b-path	Indirect effect	Direct effect	Total effect	Proportion mediated	p-value (indirect)
<i>Boundary Violation (1)</i>	-0.9693	0.3498	-0.3390	0.0471	-0.2919	1.1613	0.000
<i>Boundary Violation (2)</i>	-0.5302	0.5486	-0.2909	-0.0010	-0.2919	0.9965	0.000
<i>Fairness (1)</i>	-0.5221	0.5022	-0.2622	-0.0297	-0.2919	0.8982	0.000
<i>Fairness (2)</i>	-0.3509	0.5713	-0.2005	-0.0915	-0.2919	0.6867	0.000
<i>Supporting Founder (1)</i>	-0.3614	0.4802	-0.1735	-0.1184	-0.2919	0.5944	0.000
<i>Professionalism</i>	-0.2712	0.6333	-0.1717	-0.1202	-0.2919	0.5882	0.000
<i>Supporting Founder (2)</i>	-0.3764	0.4444	-0.1673	-0.1247	-0.2919	0.5729	0.000
<i>Leading Founder (1)</i>	-0.2864	0.5773	-0.1653	-0.1266	-0.2919	0.5662	0.000
<i>Team Competence (1)</i>	-0.2528	0.6246	-0.1579	-0.1340	-0.2919	0.5409	0.000
<i>Career Advancement</i>	-0.2448	0.5993	-0.1467	-0.1452	-0.2919	0.5025	0.000
<i>Startup Growth Potential</i>	-0.2077	0.6799	-0.1412	-0.1507	-0.2919	0.4838	0.000
<i>Leading Founder (2)</i>	-0.2658	0.4716	-0.1254	-0.1666	-0.2919	0.4294	0.000
<i>Team Competence (2)</i>	-0.2457	0.5018	-0.1233	-0.1687	-0.2919	0.4223	0.000
<i>Employment Risks</i>	-0.1435	0.5730	-0.0822	-0.2097	-0.2919	0.2817	0.068
Number of observations	795						

[\[Back to text\]](#)

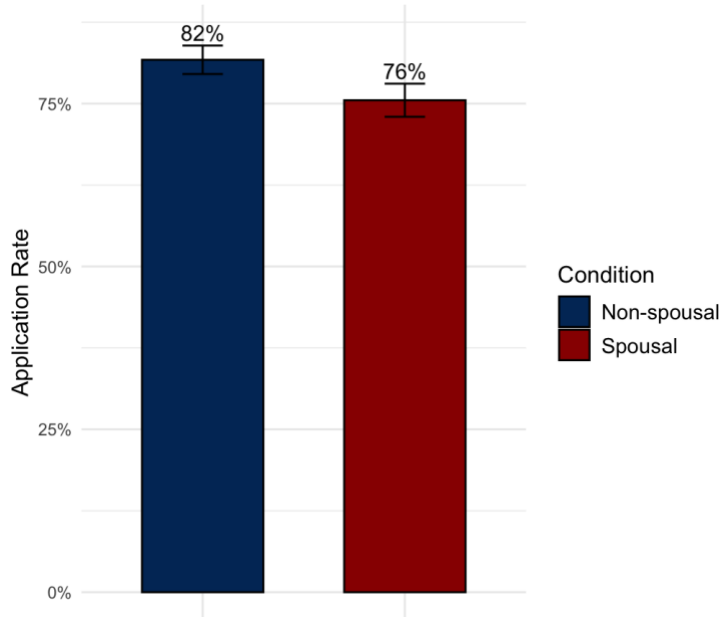
## Figures

**Figure 1:** Two-stage labor market experiment design



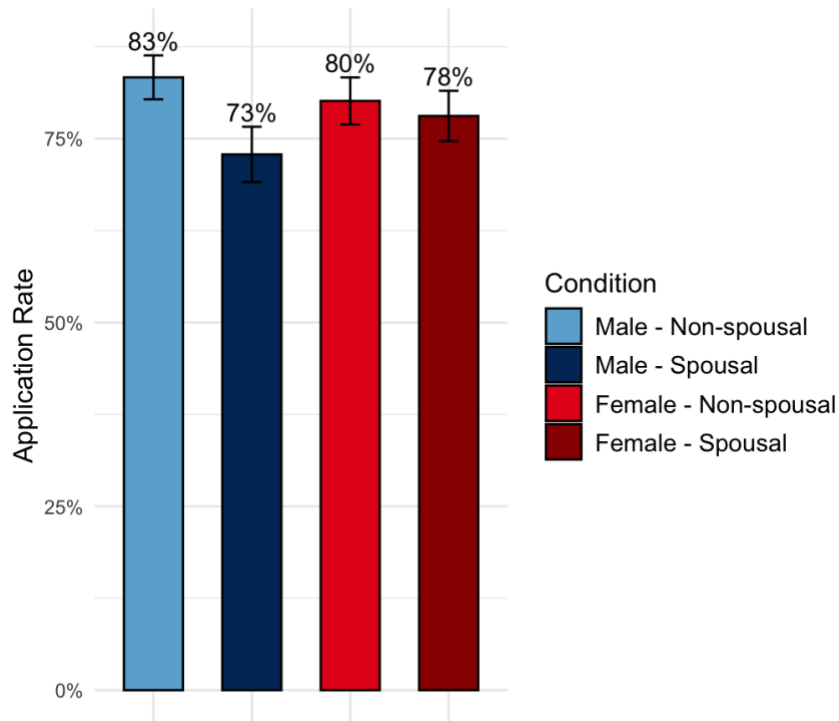
[\[Back to text\]](#)

**Figure 2A:** The spousal treatment reduces the application rate.



[\[Back to text\]](#)

**Figure 2B:** Male-leading spousal treatment results in a larger drop in application rate than female-leading spousal treatment.



[\[Back to text\]](#)

**APPENDIX A:**

**Table A1.** Descriptive Statistics and Correlations

	<b>Variable</b>	<b>Mean</b>	<b>SD</b>	<b>1</b>	<b>2</b>	<b>3</b>	<b>4</b>	<b>5</b>	<b>6</b>
1	Age of startup (months)	41.219	30.188	1.000					
2	Hired first employee	0.020	0.140	-0.029	1.000				
3	Male lead entrepreneur	0.619	0.486	0.016	0.020	1.000			
4	Solo entrepreneur	0.572	0.495	-0.006	-0.032	-0.077	1.000		
5	Spousal team	0.232	0.422	0.006	0.005	-0.040	-0.636	1.000	
6	Non-spousal team	0.195	0.397	0.001	0.034	0.139	-0.570	-0.271	1.000
7	Gender diversity of team	0.143	0.223	0.019	0.017	-0.047	-0.741	0.858	0.011
8	Average age of team	44.189	11.688	0.070	-0.026	-0.039	0.039	-0.013	-0.034
9	Average education of team	3.330	1.009	0.006	0.021	-0.059	0.004	0.002	-0.007
10	Avg. paid work experience of team	21.078	11.096	0.047	-0.019	0.037	0.062	-0.025	-0.050
11	Avg. no. of startups created before	1.003	1.612	-0.012	0.000	0.049	0.038	-0.069	0.027
12	Avg. managerial experience of team	10.672	8.881	0.040	0.006	0.059	-0.016	-0.035	0.057
13	Avg. industry experience of team	9.307	9.721	0.065	-0.009	0.110	0.099	-0.088	-0.029
14	Achieved revenue in first year	0.450	0.498	0.134	0.010	-0.042	0.004	0.062	-0.072
	<b>Variable</b>	<b>7</b>	<b>8</b>	<b>9</b>	<b>10</b>	<b>11</b>	<b>12</b>	<b>13</b>	
7	Gender diversity of team	1.000							
8	Average age of team	0.010	1.000						
9	Average education of team	0.009	0.228	1.000					
10	Avg. paid work experience of team	-0.009	0.801	0.136	1.000				
11	Avg. no. of startups created before	-0.046	0.190	0.167	0.214	1.000			
12	Avg. managerial experience of team	0.003	0.605	0.223	0.626	0.317	1.000		
13	Avg. industry experience of team	-0.054	0.328	0.050	0.359	0.063	0.355	1.000	
14	Achieved revenue in first year	0.038	-0.031	0.017	-0.021	0.065	-0.003	-0.042	

N=4671 Startup-month observations

[\[Back to text\]](#)

## APPENDIX B:

**Table B1:** Balance table of candidates who accepted the initial invitation but varied in application completion during the second stage: applied versus didn't apply (Revelio Data and Experimental Data)

	Applied Obs	Applied Mean	Didn't Apply Obs	Didn't Apply Mean	Difference
Male	412	0.43	453	0.45	-0.014
Lower than bachelor's degree	412	0.034	453	0.051	-0.017
Bachelor's degree	412	0.58	453	0.65	-0.069 *
Graduate degree	412	0.31	453	0.19	0.114 ***
Bachelor or higher degree	412	0.89	453	0.84	0.045+
Num.Obs.	865 <sup>22</sup>				

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Robust standard errors in parentheses.

[\[Back to text\]](#)

**Table B2:** Balance table for all survey entries by treatment in the male-leading sample: male-leading non-spousal cofounders (control group) versus male-leading spousal cofounders (treatment group)

	Control Obs	Control Mean	Treatment Obs	Treatment Mean	Difference
Application	156	0.83	140	0.73	-0.10*
Survey Progress (in %)	156	92.70	140	89.98	-2.72
Survey Duration (in seconds)	156	3208.34	140	3990.86	782.52
Number of observations	296				
Sample	M-led				

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Robust standard errors in parentheses.

[\[Back to text\]](#)

**Table B3:** Balance table for all survey entries by treatment in the female-leading sample: female-leading non-spousal cofounders (control group) versus female-leading spousal cofounders (treatment group)

	Control Obs	Control Mean	Treatment Obs	Treatment Mean	Difference
Application	156	0.80	146	0.78	-0.02
Survey Progress (in %)	156	91.53	146	92.7	1.17
Survey Duration (in seconds)	156	6682.09	146	4808.34	-1873.75
Number of observations	302				
Sample	F-led				

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Robust standard errors in parentheses.

[\[Back to text\]](#)

<sup>22</sup> This analysis draws on a third-party dataset from Revelio, which compiles publicly accessible information on gender and education based on self-reported data from LinkedIn user profiles. This is also consistent with ethical standards, which require that candidate information be collected only with their consent. The data are available for 865 out of 933 candidates who expressed interest in applying, including 453 of the 462 candidates who did not apply and 412 of the 471 candidates who did apply.

**Table B4:** Applicant demographics and attributes by treatment in the full sample: non-spousal cofounders (control group) versus spousal cofounders (treatment group)

	<b>Control Obs</b>	<b>Control Mean</b>	<b>Treatment Obs</b>	<b>Treatment Mean</b>	<b>Difference</b>
Age	252	22.80	222	23.54	0.74 *
Male	259	0.46	229	0.37	-0.10 *
<b>Education</b>					
High School Diploma	260	0.08	230	0.05	-0.02
Bachelor's degree	260	0.52	230	0.47	-0.05
Postgraduate/Master's degree	260	0.32	230	0.36	0.05
College degree	260	0.09	230	0.12	0.03
<b>Ethnicity</b>					
Asian	253	0.55	227	0.52	-0.03
Hispanic or Latino	253	0.11	227	0.14	0.03
Black or African American	253	0.10	227	0.10	-0.01
White	253	0.19	227	0.22	0.03
<b>Marital Status</b>					
Single	254	0.93	226	0.92	-0.01
<b>Location</b>					
New York City	312	0.34	286	0.35	0.01
California	312	0.24	286	0.24	0.00
Illinois	312	0.20	286	0.19	-0.01
Texas	312	0.22	286	0.22	0.00
<b>Employment status</b>					
Employed part-time	261	0.26	231	0.26	-0.01
Unemployed, actively seeking	261	0.49	231	0.49	-0.01
Employed full-time	261	0.23	231	0.24	0.02
Unemployed, not actively seeking	261	0.02	231	0.01	-0.01
Current monthly salary (\$)	225	2253.89	199	4251.22	1997.33 *
Expected monthly salary (\$)	280	6495.44	250	6051.28	-444.16
Full-time experience (in years)	237	1.58	214	1.76	0.18
Full-time in startups (in years)	240	0.64	213	0.55	-0.08
<b>Preferred workplace arrangement</b>					
Hybrid	280	0.24	250	0.30	0.06
Fully on-site	280	0.10	250	0.07	-0.02
Fully remote	280	0.30	250	0.24	-0.06
Number of observations	598				
Sample	Full				

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

[\[Back to text\]](#)

**Table B5:** Applicant demographics and attributes by treatment in the male-leading sample: male-leading non-spousal cofounders (control group) versus male-leading spousal cofounders (treatment group)

	<b>Control Obs</b>	<b>Control Mean</b>	<b>Treatment Obs</b>	<b>Treatment Mean</b>	<b>Difference</b>
Age	128	22.76	105	23.18	0.42
Male	132	0.48	110	0.36	-0.11 +
<b>Education</b>					
High School Diploma	132	0.07	110	0.05	-0.02
Bachelor's Degree	132	0.56	110	0.51	-0.05
Postgraduate/Master's degree	132	0.27	110	0.31	0.04
College degree	132	0.09	110	0.14	0.05
<b>Ethnicity</b>					
Asian	129	0.52	109	0.53	0.01
Hispanic or Latino	129	0.11	109	0.14	0.03
Black or African American	129	0.11	109	0.10	-0.01
White	129	0.20	109	0.19	-0.01
<b>Marital status</b>					
Single	131	0.93	108	0.90	-0.03
<b>Location</b>					
New York City	156	0.35	140	0.34	-0.02
California	156	0.23	140	0.25	0.02
Illinois	156	0.19	140	0.19	0.00
Texas	156	0.22	140	0.22	0.00
<b>Employment status</b>					
Employed part-time	132	0.27	110	0.22	-0.05
Unemployed, actively seeking	132	0.48	110	0.49	0.01
Employed full-time	132	0.23	110	0.27	0.04
Unemployed, not actively seeking	132	0.02	110	0.02	0.00
Current monthly salary (\$)	113	2585.54	91	5531.43	2945.89
Expected monthly salary (\$)	142	6673.27	118	5922.47	-750.8
Full-time experience (in years)	118	1.60	99	1.63	0.02
Full-time in startups (in years)	121	0.55	100	0.52	-0.03
<b>Preferred workplace arrangement</b>					
Hybrid	142	0.22	118	0.33	0.11 *
Fully on-site	142	0.09	118	0.07	-0.02
Fully remote	142	0.27	118	0.24	-0.03
Number of observations	296				
Sample	M-lead				

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

**Table B6:** Applicant demographics and attributes by treatment in the female-leading sample: female-leading non-spousal cofounders (control group) versus female-leading spousal cofounders (treatment group)

	<b>Control Obs</b>	<b>Control Mean</b>	<b>Treatment Obs</b>	<b>Treatment Mean</b>	<b>Difference</b>
Age	124	22.84	117	23.86	1.02 *
Male	127	0.45	119	0.37	-0.08
<b>Education</b>					
High School Diploma	128	0.09	120	0.06	-0.03
Bachelor's Degree	128	0.47	120	0.43	-0.04
Postgraduate/Master's degree	128	0.36	120	0.41	0.05
College degree	128	0.09	120	0.10	0.010
<b>Ethnicity</b>					
Asian	124	0.58	118	0.51	-0.07
Hispanic or Latino	124	0.11	118	0.14	0.02
Black or African American	124	0.10	118	0.09	0.00
White	124	0.18	118	0.24	0.06
<b>Marital status</b>					
Single	123	0.92	118	0.93	0.01
<b>Location</b>					
New York City	156	0.33	146	0.37	0.04
California	156	0.24	146	0.23	-0.02
Illinois	156	0.20	146	0.18	-0.02
Texas	156	0.22	146	0.23	0
<b>Employment status</b>					
Employed part-time	129	0.26	121	0.29	0.03
Unemployed, actively seeking	129	0.51	121	0.49	-0.02
Employed full-time	129	0.22	121	0.21	0.00
Unemployed, not actively seeking	129	0.02	121	0.01	-0.01
Current monthly salary (\$)	112	1919.29	108	3172.53	1253.24 *
Expected monthly salary (\$)	138	6312.46	132	6166.44	-146.02
Full-time experience (in years)	119	1.55	115	1.87	0.32
Full-time in startups (in years)	119	0.72	113	0.58	-0.14
<b>Preferred workplace arrangement</b>					
Hybrid	138	0.27	132	0.27	0.00
Fully on-site	138	0.10	132	0.08	-0.03
Fully remote	138	0.34	132	0.25	-0.09
Number of observations	302				
Sample	F-lead				

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

## **APPENDIX C: Four Treatment Conditions**

### **Manipulation 1 M-S**

#### **Message from the Founder**

Hi there,

I am Hugh Smith, and I oversee the daily operations of our startup. After graduating from the University of Pennsylvania, I devoted my career to digital marketing—particularly in the areas of live commerce and content production. This journey has taught me the importance of pairing creativity with strategy to deliver campaigns that truly resonate.

I cofounded this company alongside my wife, Elise Smith. Together, we bring a shared understanding of how social media and business advertising can shape meaningful connections between brands and their audiences. While I lead day-to-day execution, our vision and commitment are built hand-in-hand.

Warmly,

**Hugh Smith, with my wife Elise Smith**

---

### **Manipulation 2 F-S**

#### **Message from the Founder**

Hi there,

I am Elise Smith, and I oversee the daily operations of our startup. After graduating from the University of Pennsylvania, I devoted my career to digital marketing—particularly in the areas of live commerce and content production. This journey has taught me the importance of pairing creativity with strategy to deliver campaigns that truly resonate.

I cofounded this company alongside my husband, Hugh Smith. Together, we bring a shared understanding of how social media and business advertising can shape meaningful connections between brands and their audiences. While I lead day-to-day execution, our vision and commitment are built hand-in-hand.

Warmly,

**Elise Smith, with my husband Hugh Smith**

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### **Manipulation 3 M-N**

#### **Message from the Founder**

Hi there,

I am Hugh Smith, and I oversee the daily operations of our startup. After graduating from the University of Pennsylvania, I devoted my career to digital marketing—particularly in the areas of live commerce and content production. This journey has taught me the importance of pairing creativity with strategy to deliver campaigns that truly resonate.

I cofounded this company alongside my colleague, Elise Anderson. Together, we bring a shared understanding of how social media and business advertising can shape meaningful connections between brands and their audiences. While I lead day-to-day execution, our vision and commitment are built hand-in-hand.

Warmly,

**Hugh Smith, with my colleague Elise Anderson**

---

### **Manipulation 4 F-N**

#### **Message from the Founder**

Hi there,

I am Elise Smith, and I oversee the daily operations of our startup. After graduating from the University of Pennsylvania, I devoted my career to digital marketing—particularly in the areas of live commerce and content production. This journey has taught me the importance of pairing creativity with strategy to deliver campaigns that truly resonate.

I cofounded this company alongside my colleague, Hugh Anderson. Together, we bring a shared understanding of how social media and business advertising can shape meaningful connections between brands and their audiences. While I lead day-to-day execution, our vision and commitment are built hand-in-hand.

Warmly,

**Elise Smith, with my colleague Hugh Anderson**

[\[Back to text\]](#)

## APPENDIX D:

**Table D1:** Balance table for all survey entries by treatment in the full sample: spousal cofounders (treatment group) versus non-spousal cofounders (the first control group) versus sibling cofounders (the second control group).

Variable	N	Spousal	Non-spousal	Difference	Sig	Sibling	Difference	Sig
Startup Attractiveness	1,190	3.39	3.54	-0.15	*	3.43	-0.04	
Willingness to Join	1,190	2.90	3.19	-0.29	***	3.08	-0.18	*
Professionalism	1,190	3.65	3.92	-0.27	***	3.67	-0.02	
Growth Potential	1,190	3.47	3.68	-0.21	**	3.58	-0.11	
Career Advancement Opportunities	1,190	3.18	3.43	-0.25	***	3.21	-0.03	
Startup Stability/Risks	1,190	2.99	3.13	-0.14		2.99	0.00	
Team Competence (Item 1)	1,190	3.38	3.63	-0.25	***	3.38	0.00	
Team Competence (Item 2)	1,190	3.53	3.77	-0.24	***	3.56	-0.03	
Boundary Violations (Item 1)	1,190	2.82	3.79	-0.97	***	3.01	-0.19	*
Boundary Violations (Item 2)	1,190	3.51	4.04	-0.53	***	3.64	-0.13	
Fairness & Ethics (Item 1)	1,190	3.28	3.80	-0.52	***	3.41	-0.13	
Fairness & Ethics (Item 2)	1,190	3.31	3.66	-0.35	***	3.41	-0.10	
Leading Founder's Competence	1,190	3.68	3.97	-0.29	***	3.77	-0.09	
Leading Founder's Competence	1,190	3.52	3.79	-0.27	***	3.53	-0.01	
Supporting Founder's Competence	1,190	3.48	3.84	-0.36	***	3.50	-0.02	
Supporting Founder's Competence	1,190	3.41	3.79	-0.38	***	3.48	-0.07	
Response Duration (Seconds)	1,190	756.67	764.92	-8.25		786.00	-29.33	
Age	1,190	43.67	42.44	1.23		42.67	1.00	
Annual Salary (Self-reported)	1,190	12,108.4	17,110.31	-5,001.85		11,291.0	817.42	
Years of Full-time Experience	1,190	19.46	18.85	0.61		26.30	-6.84	
Years of Startup Experience	1,190	2.30	2.60	-0.30		15.21	-12.91	
Risk Preference (1–10 Scale)	1,190	6.04	6.04	0.00		6.03	0.01	
Survey Type (1 = Marketing)	1,190	0.50	0.50	0.00		0.51	-0.01	
Employed Full-time (Dummy)	1,190	0.75	0.80	-0.05		0.76	-0.01	
Employed Part-time (Dummy)	1,190	0.16	0.14	0.02		0.18	-0.02	
Bachelor Degree (Dummy)	1,190	0.48	0.45	0.03		0.47	0.01	
Some College (Dummy)	1,190	0.20	0.18	0.02		0.18	0.02	
High School Degree (Dummy)	1,190	0.04	0.06	-0.02		0.05	-0.01	
Master's Degree (Dummy)	1,190	0.28	0.31	-0.03		0.30	-0.02	
Asian (Non-Hispanic) (Dummy)	1,190	0.09	0.11	-0.02		0.10	-0.01	
Black or African American	1,190	0.15	0.15	0.00		0.14	0.01	
Hispanic or Latino (Dummy)	1,190	0.07	0.09	-0.02		0.07	0.00	
White (Non-Hispanic) (Dummy)	1,190	0.67	0.63	0.04		0.65	0.02	
Married (Dummy)	1,190	0.49	0.48	0.01		0.52	-0.03	
Male (Dummy)	1,190	0.41	0.44	-0.03		0.42	-0.01	

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

[\[Back to text\]](#)

**Table D2: Online (Prolific) Experiment: The Effect of Spousal Treatment on Willingness to Join by Job Role (Marketing vs. Sales, Sibling Teams Excluded)**

	(1)	(2)	(3)	(4)	(5)	(6)
Sample	Full	M-led	F-led	Full	M-led	F-led
Model	ologit	ologit	ologit	ologit	ologit	ologit
Treatment	-0.425*	-0.697**	-0.170	-0.443*	-0.578*	-0.323
	(0.181)	(0.261)	(0.254)	(0.182)	(0.259)	(0.255)
Survey Type	Marketing	Marketing	Marketing	Sales	Sales	Sales
Num. Obs.	399	199	200	396	197	199

+  $p < 0.1$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Robust standard errors in parentheses.

[\[Back to text\]](#)

**Table D3:** Mediation Analysis (Full Sample): Spousal vs Non-spousal (excluding sibling teams), scales are combined in average.  
a-path: SEM (lavaan) mediator ~ spousal; b-path: SEM (lavaan) join ~ spousal + mediator. Indirect effect = a×b, estimated via nonparametric bootstrap (500 replications). Proportion mediated = (a×b)/total effect, where the total effect is from OLS join ~ spousal

<b>Mediator</b>	<b>a-path</b>	<b>b-path</b>	<b>Indirect effect</b>	<b>Direct effect</b>	<b>Total effect</b>	<b>Proportion mediated</b>	<b>p-value (indirect)</b>
<i>Boundary Violation</i>	-0.7498	0.5301	-0.3974	0.1055	-0.2919	1.3613	0.0000
<i>Fairness</i>	-0.4365	0.6259	-0.2732	-0.0188	-0.2919	0.9358	0.0000
<i>Supporting Founder</i>	-0.3689	0.5223	-0.1927	-0.0993	-0.2919	0.6599	0.0000
<i>Professionalism</i>	-0.2712	0.6333	-0.1717	-0.1202	-0.2919	0.5882	0.0000
<i>Team Competence</i>	-0.2492	0.6852	-0.1708	-0.1212	-0.2919	0.5850	0.0001
<i>Leading Founder</i>	-0.2761	0.6155	-0.1699	-0.1220	-0.2919	0.5821	0.0000
<i>Career Advancement</i>	-0.2448	0.5993	-0.1467	-0.1452	-0.2919	0.5025	0.0009
<i>Startup Growth Potential</i>	-0.2077	0.6799	-0.1412	-0.1507	-0.2919	0.4838	0.0010
<i>Employment Risks</i>	-0.1435	0.5730	-0.0822	-0.2097	-0.2919	0.2817	0.0553
Number of observations	795						

[\[Back to text\]](#)

**Table D4: Prolific Experiment: The Effect of Spousal Treatment on Willingness to Join: Sibling Teams**

	(1)	(2)	(3)	(4)
Sample	Full	Full	Full	Full
Model	ologit	OLS	ologit	OLS
Spousal (vs sibling)	-0.263* (0.128)	-0.174* (0.088)		
Sibling (vs. colleague)			0.141 (0.182)	0.085 (0.125)
Male-leading			0.182 (0.180)	0.143 (0.120)
Sibling × Male-leading			-0.590* (0.257)	-0.413* (0.173)
Constant		3.076*** (0.063)	3.122*** (0.089)	3.122*** (0.089)
Num.Obs.	793	793	792	792

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Robust standard errors in parentheses.

[\[Back to text\]](#)