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License to Broker: How Mobility Eliminates Gender Gaps in Network Advantage

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Abstract

Brokerage in intra-organizational networks is critical to performance, but women exhibit less brokerage in their social networks and receive lower performance returns to the brokerage they exhibit than men do. We uncover a condition under which the gender gaps in network advantage are entirely negated: mobility. When women move between units of the organization, they increase their brokerage more than mobile men do. Further, such mobility eliminates the gender gap in returns to brokerage. Using a rich dataset including the personnel records, monthly performance, and email communications of thousands of employees in a large financial institution, we find support for our arguments by comparing the networks and objective performance of those who changed jobs with matched non-movers prior to and following each job change. In probing why this might be the case, we find that women movers are more likely to maintain communication ties to colleagues from their previous roles and that these persistent ties give them a discernible and gender-role-congruent explanation for connecting otherwise disconnected units and benefiting from network brokerage. Our results illuminate important mechanisms by which social network dynamics and mobility affect gender inequality and performance in organizations.

Keywords: communication networks, intra-organizational mobility, gender, situational licenses, network brokerage

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Fundamental to the organizational literature on social networks is the concept of brokerage, in which connections that bridge formal or informal group boundaries—and the information that flows through them—are beneficial to important outcomes such as job performance and career advancement (Burt, 1992; Podolny and Baron, 1997; Kilduff and Brass, 2010; Ahuja, Soda, and Zaheer, 2011; Borgatti and Halgin, 2011; Tortoriello, Reagans, and McEvily, 2012). Despite the wide range of benefits that employees derive from brokerage, research has also substantiated challenges to establishing and retaining such bridging connections (Burt, 2001; Biancani, McFarland, and Dahlander, 2014; Jonczyk et al., 2016). Rather than encourage brokerage, both individual preferences and organizational design largely conspire to encourage employees to form densely connected ties among similar others who themselves are connected (Feld, 1981; Kossinets and Watts, 2009; Kovacs and Kleinbaum, 2019). Within an organization, employees' social networks tend to hew closely to their assigned formal roles, departments, and business units rather than cutting across the organization's functional groups and domains (Mehra, Kilduff, and Brass, 1998; Sasovova et al., 2010; Kleinbaum, Stuart, and Tushman, 2013; Lee, 2019). In addition to identifying these impediments to forming and maintaining bridging connections, network scholars are increasingly finding conditions that limit brokers' returns.

We classify these contingencies to the benefits of brokerage into two conceptual categories: suspicion of *brokerage itself* and apprehension about *certain individuals* occupying brokerage positions. In the first instance, regardless of who is acting as a bridge between disconnected social groups, the position itself, given its exploitative potential, can engender suspicion and, consequently, undermine others' willingness to share information and resources with brokers (Stovel, Golub, and Meyersson Milgrom, 2011; Stovel and Shaw, 2012; Aven and Hillmann, 2017; Iorio, 2022). As one who "plays conflicting demands and preferences against one another and builds value from their disunion," a broker can use their influence to sway decisions or outcomes and decide whose interests to serve (Burt, 2000a: 354; Xiao and Tsui, 2007). Accordingly, in such circumstances, anyone who is identified as a broker would be met with distrust, reducing their ability to profit from their network position. In the second case, with which we are primarily concerned here, broker positions are not equally advantageous to all employees who hold them. Rather, brokerage is associated with certain characteristics, such as agency or leadership, and women, whose identities do not stereotypically reflect those characteristics, seem to benefit from brokerage less consistently than men do (Burt, 1998; Brands and Kilduff, 2014; Brands, Menges, and Kilduff, 2015; Burt, Reagans, and Volvovsky, 2021). That is, women who occupy brokerage positions may suffer from an identity contradiction between their gender stereotype of communality, loyalty, and relational orientation and the brokerage associations of agency, leadership, and instrumentality (Eagly and Karau, 2002; Eagly, 2005; Brands and Kilduff, 2014; Burt, Reagans, and Volvovsky, 2021). Theories on expectation states and social roles contend that this misalignment creates a problem of perceived legitimacy in terms of women gaining influence or advantage over others, whereby others negatively evaluate women brokers because they are perceived to be illegitimate as brokers (Ridgeway, 2001; Eagly and Karau, 2002; Ridgeway and Correll, 2004).

Yet, expectation states theory also argues that for individuals whose characteristics are inconsistent with their stereotype, information specific to the individual's legitimacy—for example, details that contribute to women's perceived credibility as leaders—may be introduced to mitigate the negative ramifications of identity misalignment (Berger et al., 1992; Reskin and McBrier, 2000; Ridgeway, 2001). In other words, when women are penalized for behavior inconsistent with gender stereotypes, such as when women broker in social networks, the introduction of salient individual information may legitimate their behavior and, in turn, reduce associated penalties.

We propose that for women brokers, mobility offers such information. Social networks are not static; they must change dynamically in response to the evolving informal and formal structure of the organization. Because employees' intra-organizational social relationships predominantly reflect associations mandated by their formal roles, departments, and business units, mobility (i.e., changes in employees' formal position within the organization) often serves as a catalyst in reshaping their social networks (Podolny and Baron, 1997; Kleinbaum, 2012; Jonczyk et al., 2016). We contend that such formal job changes provide a salient explanation for network change. And when formal job changes induce brokerage in the informal network, the changes are particularly consequential for women because they provide a role-congruent explanation for women's brokerage. Although such mechanisms have yet to be studied in the context of brokerage, role-congruent information has been shown to mitigate differential returns due to violations of gender stereotypes across diverse settings (Bowles, Babcock, and Lai, 2007; Amanatullah and Morris, 2010; Bowles and Babcock, 2013; Lee and Huang, 2018). As such, we contend that mobility grants women license to broker.

In this study, we focus on intra-organizational mobility events, or moves whereby employees change jobs across business units within an organization—an empirical setting that provides several advantages. Focusing on internal mobility allows us to collect longitudinal data on a complete organizational network and to examine network dynamics before and after mobility. It also permits us to hold constant organizational characteristics, such as firm culture, performance metrics, and incentive systems. From a large U.S.-based financial institution that we call Big Bank, we obtained data consisting of all employees' demographic information, human resource records, and metadata of email communications. We focus on email communications to construct intra-organizational social networks, as prior work has shown that email data are an effective representation of communication networks between employees (e.g., Quintane and Kleinbaum, 2011; Kleinbaum, Stuart, and Tushman, 2013; Aven, 2015).

In addition, this setting allowed us to collect objective monthly sales performance data for all retail sales employees. While much prior work has relied on subjective performance evaluations, which research has shown are loosely coupled with objective performance (Castilla and Benard, 2010; Castilla, 2011), it remains unclear how the identity contradiction inherent in being a woman broker extends beyond subjective evaluations. Using email data coupled with objective monthly performance data, we employed a pre/post design to test our theoretical propositions on the differential effects of intra-organizational job changes on employees' brokerage and returns to brokerage for men and women. Our empirical context of intra-organizational mobility thus reduces concerns of confounds and unobservables that are common to inter-organizational mobility studies and allows us to test our arguments by using an objective performance criterion.

THEORY

Network Brokerage and Performance

Studies in organization theory and sociology have long recognized the importance of social networks for the information that flows through them and the social identity that they signal (Granovetter, 1973; Burt, 1992; Podolny and Baron, 1997; Kilduff and Brass, 2010; Ahuja, Soda, and Zaheer, 2011; Borgatti and Halgin, 2011). Social networks are informational conduits, and brokerage provides advantageous information access. By interacting with their network contacts, employees gain access to novel information, informal learning, advice and mentorship, gossip about the goings-on of an organization, social support, and friendship (Burt, 1992; Podolny and Baron, 1997; Walsh, Halgin, and Huang, 2018). And research has consistently shown that people whose networks give them access to these resources get evaluated more favorably and, consequently, get paid more and promoted faster, particularly in knowledge-based work (Burt, 1992, 2005).

While brokerage is generally found to produce positive outcomes via structural access to information, research has begun to suggest that others' perceptions of brokers might undercut the benefits of brokerage. For example, others may be suspicious of brokers' allegiances and commitments, leading to the deterioration of connections to brokers (Friedman and Podolny, 1992). By the same token, ideas proposed by individuals perceived to be brokers are less likely to be accepted (Iorio, 2022), and brokers may have difficulty obtaining information and resources from social connections (Stovel, Golub, and Meyersson Milgrom, 2011; Stovel and Shaw, 2012; Aven and Hillmann, 2017).

One category of people may be especially likely to be viewed as illegitimate when they occupy brokerage roles: women.[1] In general, brokers are seen to be agentic and instrumental middlemen (Simon, 1955; Burt, 1992; Padgett and Ansell, 1993; Brands and Kilduff, 2014; Brands and Mehra, 2019) who exert outsize influence over their peers (Sievers et al., 2022). Perhaps for this reason, those behaving as brokers may be perceived as leaders by their peers (Burt, Reagans, and Volvovsky, 2021). Just as tasks and roles become associated with gender, network positions, such as brokerage, may also become gender-typed (Berger et al., 1992; Reskin and McBrier, 2000; Ridgeway, 2001). In sum, while brokers may be advantaged structurally, they may also be prone to negative evaluations, especially when they are women.

Gender and Brokerage

Gender is among the most salient aspects of identity that people attend to when they encounter others (Turner et al., 1987; Martin and Slepian, 2020). Although social networks within organizations align with functional roles and task requirements to a significant degree, gender has also been shown to be

[1] Of course, there are many dimensions of identity that may be stereotyped. We focus our analysis here on gender, with only minimal exploration of its intersection with other dimensions, such as race.

an important determinant of network structure, beyond simple homophily effects (Brass, 1985; Ibarra, 1992; Mehra, Kilduff, and Brass, 1998; Kilduff and Brass, 2010; Singh, Hansen, and Podolny, 2010).

Research has long shown that women, rather than occupying brokerage positions, tend to be embedded in more structurally cohesive and homogenous networks, compared with men (Brass, 1985; Ibarra, 1992; Mehra, Kilduff, and Brass, 1998; Fang, Zhang, and Shaw, 2021; Woehler et al., 2021; Brands et al., 2022). Because of these differences in their social networks, women and men, on average, do not have equal access to information or other resources known to improve work outcomes. For instance, women are often excluded from participation in both formal and informal social networks in organizations dominated by men (Kanter, 1977; Antilla, 2002; Roth, 2006), which, in turn, may limit their knowledge about coworkers and clients (Groysberg, 2010). Women have been shown to have longer paths than men do for locating experts (Singh, Hansen, and Podolny, 2010), which potentially slows their ability to find solutions to organizational challenges. Thus, women are more likely than men to have network characteristics that are associated with low performance.

Further, when women are known to occupy brokerage positions, they do not benefit from these relationships as consistently as men do (Burt, 1992: 145; Brands and Kilduff, 2014; Brands, Menges, and Kilduff, 2015). One early result found that social networks rich in structural holes led to early promotions for men yet delayed promotion for women (Burt, 1998). Subsequent studies revealed that gendered stereotypes about brokerage in networks damaged the reputations of these women brokers (Brands and Kilduff, 2014) and reduced their perceived charisma (Brands, Menges, and Kilduff, 2015). In addition to facing the biases in others' social judgments, many women themselves feel anxious when they perceive their own networks to be structurally diverse (i.e., when they occupy brokerage positions) due to internalized expectations or the anticipation of negative evaluations by others; this anxiety, in turn, undermines women students' academic performance (Brands and Mehra, 2019).

Expectation states theory and research on gender stereotypes provide a helpful framework for understanding how the gender differences associated with brokerage come about. Society's gender roles entail expectations for men and women that prescribe how they *should* behave (Burgess and Borgida, 1999). In many Western societies, women are expected to be social experts who are warm, sensitive to others, and communal, whereas the expectations of men involve being ambitious, accomplished, agentic, and instrumental (Rudman et al., 2012). With few exceptions (e.g., Melin and Merluzzi, 2022), women who behave in role-incongruent ways—i.e., who behave like men—pay a penalty in the form of negative performance evaluation (Ridgeway, 2001; Eagly and Karau, 2002; Castilla and Benard, 2010; Elliott and Stead, 2017). More generally, wide-ranging research has shown that women shy away from stereotypically masculine behaviors, even when those behaviors also have communal elements (Akinola, Martin, and Phillips, 2018). Thus, the cost of gender-role incongruity comes both from audiences affected by stereotypical expectations and from women themselves, who may internalize such expectations (Correll, 2004; Barbulescu and Bidwell, 2013; Akinola, Martin, and Phillips, 2018; Brands and Mehra, 2019; Lee, Koval, and Lee, 2023). Stereotypes about women who broker not only affect others' subjective evaluations of women's performance but also influence their own behavior, which undermines their objective performance.

Building on this foundation, we argue that brokerage entails an identity contradiction for women because women are expected to be communal, loyal, and relationally oriented rather than agentic or instrumental (Eagly and Karau, 2002; Eagly, 2005). Although it is not clear that people build bridges intentionally, brokerage positions are nevertheless associated with masculine stereotypes in which brokers are perceived to be competent, agentic, and instrumental (Simon, 1955; Burt, 1992; Padgett and Ansell, 1993; Brands and Kilduff, 2014; Brands and Mehra, 2019). Absent other explanations of how they came to occupy brokerage positions, women professionals who do so appear incongruent with gender-role expectations. As a result of this incongruity, women's objective returns to brokerage are lower than those of men, although women brokers might still derive information benefits from brokerage compared to non-brokering women. Building on extant empirical evidence on gender and social networks, two premises undergird our contributions:

Premise 1: In general, women exhibit lower network brokerage than men do.
Premise 2: Women receive lower performance returns to brokerage than men do.

Mobility as a License to Broker

Importantly, expectation states theory contends that judgments of a behavior as role incongruent, whether others' judgments or one's own, could be altered by information that confers role-congruent legitimacy on that behavior. For example, in a group newly formed to determine the best military intervention, the suggestions of a sole woman might initially be ignored; however, if she is the only member of the group with military and combat experience, she might be more comfortable taking the lead. And if the other members are aware of that experience, they may be more likely to set aside their prior judgment and defer to her expertise. Similarly, the introduction of role-congruent explanations for behavior that had previously seemed role incongruent may lead others to revise their negative evaluations in that instance. That is, if a rationale is provided that aligns with gender stereotypes explaining a behavior or outcome, such as brokerage, diminished returns or inferior outcomes become far less likely.

Considering the dynamic interdependence between formal roles and social relationships within organizations, we focus on a situation that might foster network change and serve as a catalyst to brokerage: mobility. We contend that mobility provides opportunity to broker between previously disconnected groups along with a salient explanation as to why interaction patterns might change, which legitimizes brokerage roles for women.

Given the influence of formal structures on employees' social networks (Mehra, Kilduff, and Brass, 1998; Sasovova et al., 2010; Kleinbaum, Stuart, and Tushman, 2013; Jonczyk et al., 2016; Lee, 2019), mobility that changes employees' formal positions within the organization can serve as a catalyst in reshaping their social networks (Kleinbaum and Stuart, 2014). Following a job change, ties inevitably form between the mover and their new coworkers via increased interaction requirements imposed by the mover's new role (Jonczyk et al., 2016; Walsh, Halgin, and Huang, 2018). Conversely, ties to former colleagues with whom the mover is no longer required to interact may decay as a function of reduced opportunities and frequency of interactions (Roberts and Dunbar, 2015; Walsh, Halgin, and Huang, 2018) as well as increased physical distance (Burt, 2001; Lee, 2019). As Burt (2000b: 1) argued, "As much as [network] change is about adapting to the new, it is about detaching from the old." Formal workplace changes (e.g., work team reassignments) inform network arrangements, causing network structures to shift over time, an effect that suggests a more dynamic view of performance in organizations (Burt and Merluzzi, 2016). But even in the face of mobility, some ties persist (Kleinbaum, 2018).

Building on earlier network and gender research, we propose that the extent to which mobility leads to a brokerage position will vary by gender for two reasons. First, the network characteristics common to women that are conventionally considered disadvantageous may induce women to broker when they are faced with mobility. Women tend to be embedded in dense networks with few structural holes, and they are more likely to be surrounded by strong ties that share common contacts (Brass, 1985; Ibarra, 1992; Mehra, Kilduff, and Brass, 1998; Brands et al. 2022). Such ties have been found to be more resistant to decay than are weaker ties (Krackhardt, 1999; Burt, 2001; Dahlander and McFarland, 2013; Jonczyk et al., 2016; Kleinbaum, 2018). Their denser embeddedness will lead mobile women to retain ties to their prior colleagues to a greater degree than men do. Second, women and men differ in how they sustain their relationships in the face of mobility. When an employee leaves to join another, distant group and ties can be retained only through electronic communication, such as email instead of in-person interactions, women may be more likely than men to maintain such remote connections (Dunbar and Spoors, 1995; Roberts and Dunbar, 2015).

Because of women's strong ties embedded in cohesive social groups, coupled with their greater likelihood of maintaining distant relationships, on average they will, to a greater degree than men, maintain their relationships with former coworkers when they move. Moreover, to the extent that women must rely on "borrowed" social capital (Burt, 1998: 21), meaning the sponsorship or advocacy of others, more than men do (Kram, 1985), women may be particularly motivated to retain ties to their prior work group. Accordingly, we contend that after changing jobs, women will be more likely than men to retain ties to prior coworkers.

Persistent ties to prior coworkers, coupled with job mobility, create a situation that fosters brokerage, as women's relationships bridge their former group with their new one. Brokerage without explanation that corresponds with gender stereotypes may invoke suspicions in the minds of others (Brands, Menges, and Kilduff, 2015) and, thereby, anxiety or stress in women themselves (Brands and Mehra, 2019). But mobility provides a credible explanation for why interaction patterns might change and helps to legitimize the subsequent changes in women's networks. A woman's brokerage might be understood as a consequence of the job change rather than of her deliberately, strategically, or agentically cultivating a brokerage position to take advantage of others. Retaining ties to former colleagues, especially when these ties are embedded in the dense cluster of the old work group, may therefore signal communal motives rather than instrumental ones. Women's persistent ties to prior colleagues who do not seem to provide immediate instrumental benefits are congruent with gender-role expectations of warmth and communality. Therein lies the license to broker for mobile women.

While it is outside the scope of this research to demonstrate the psychological processes that permit women both to broker and to benefit from it following a move, we conjecture that mobility influences three distinct, relevant audiences. First, women's former colleagues expect them to maintain ties, and when they do, these colleagues continue sharing work-related insights with women movers. Former colleagues also recognize the imperative for movers to form new ties with their new teammates, giving rise to brokerage. Second, a mover's new colleagues might tend to penalize women brokers, but they make an allowance for movers who show the good social grace of retaining contact with former colleagues. And finally, women license themselves when the same legitimizing logic spares them from feeling anxious about being bridges between their new and old groups. Because all of these constituents play a role in granting women movers license to broker, we use the shorthand notation that mobility grants women license to broker.

Taken together, these observations lead us to expect,

Hypothesis 1: Mobility reduces or eliminates the gender gap in brokerage for women relative to men.

Hypothesis 2: Mobility reduces or eliminates the differential returns to brokerage on performance for women relative to men.

METHOD

Empirical Setting

In choosing an industry in which to study our research question, we sought to meet at least two criteria. First, testing the performance implications of professional network changes requires multiple observations of intra-organizational networks and measurable individual performance. Second, as understanding differences between men and women is central to our question, the setting needs sufficient representation of both women and men in the same positions to enable comparison. Additionally, we sought to minimize the impact of other factors that are known to affect movers' performance, such as the portability of teammates or clients (Groysberg, Lee, and Nanda, 2008; Groysberg and Lee, 2009). The retail banking industry meets all these criteria and offers a setting well suited to our research purposes.

We investigate the effects of brokerage on performance and, more important, the effects of intra-organizational mobility for women and men in the retail sales department in a large U.S.-based financial institution that we call Big Bank. Big Bank is organized into four large departments: retail sales, asset management, corporate and institutional banking, and mortgages. We collected data from the retail sales department because it provides several advantages for examining the effects of gender and network dynamics on post-move performance. Intra-organizational networks play an important role in determining the performance of retail sales employees in financial institutions such as Big Bank. Retail sales employees generally work with customers who drop into local branches, and they address customers' needs on site. Although managing relationships with clients can be important to achieving sales performance advantages in some marketplaces (Beatty et al., 1996), that is not the case in our setting because retail banking clients perceive the products and services offered by different retail banks as interchangeable (Škudienė and Šlepikaitė, 2013). Retail banking clients generally go to the most convenient location and rarely stay loyal to a salesperson who has moved. Employees' performance in this setting thus depends less on long-term relationships with clients and more on skills such as identifying customers' needs, providing sufficient information on products, and tailoring approaches to customers' individual circumstances, all of which could benefit from information about products and services garnered from coworkers.

Key to our choice of this empirical setting, intra-organizational mobility at Big Bank allows us to get traction on the effect of mobility on networks and performance. To fill a job vacancy, hiring managers post open positions online along with job descriptions and characteristics of ideal applicants. They interview both internal and external candidates to evaluate their suitability and select the one most qualified for the position. An internal mover could change jobs in one of two ways: they could change their job function (moving, for example, from mortgage lending to retail banking) either within or across business units; or they could stay in the same job function but change business units, which necessarily implies that they join a new work group. We focus on the latter case, in which the mover's job requirements and work tasks remain the same (e.g., selling financial products to retail customers), but their coworkers and job setting change. Although such movers need to develop relationships with their new colleagues, prior colleagues with whom they have shared greater trust would remain invaluable. During a period when movers are building trust and rapport with their new colleagues, trusted former colleagues may be the most useful sources of information about the products and services of Big Bank, which is key to performance. Hence, such former colleagues serve as useful conduits of information and support (Dokko, Kane, and Tortoriello, 2014; Godart, Shipilov, and Claes, 2014), rather than being a burden (Gargiulo and Benassi, 2000; Dokko and Jiang, 2017) as movers adapt to their new roles.

This setting also provides an objective and comparable measure of individual performance on a monthly basis. Retail sales employees specialize in providing personal financial tools and products to consumers and small businesses. They work independently to sell similar products to local customers, and at the end of each month Big Bank calculates their individual monthly sales as a performance metric. This monthly calculation of total sales value provides a regular and objective measure of each employee's performance. This objective measure allows us to investigate individual performance without concerns of interdependent tasks or work group confounds (e.g., Argote, Aven, and Kush, 2018). It also mitigates concerns articulated in prior research (e.g., Walsh, Halgin, and Huang, 2018; Brands and Mehra, 2019) that subjective measures of performance, such as self, peer, or supervisor evaluations, may suffer from evaluation bias. An objective measure of performance is essential to answer our research question, and as movers change jobs within the same organization, their past networks and performance information are both readily available in our dataset.

Finally, we interviewed two human resource executives at Big Bank and learned that retail sales employees rely heavily on email communication throughout their work activities to share job-related information, including product details and sales strategies. Big Bank allowed us to collect anonymized metadata of email exchanges among all employees. The use of email communication affords a behavioral measure of social interactions in organizations that is less prone to the biases that often affect self-reported data (Bernard, Killworth, and Sailer, 1982), and existing evidence indicates that email communication provides a reliable proxy for other communciation media (Quintane and Kleinbaum, 2011; Kleinbaum, Stuart, and Tushman, 2013; Aven, 2015).

Data

Big Bank's retail sales department comprises 2,850 unique branches, or business units, across 36 regions, and while the majority of business units (71.5 percent; $N = 2{,}039$) consist of one work group, some have more than one (max. = 5, mean = 1.30). We obtained access to three sources of data from Big Bank: (1) individuals' monthly retail sales records, which consist of monthly observations of total sales value in dollars for each employee; (2) anonymized email metadata (including sender ID, receiver ID, message size, and timestamp) for internal emails of all Big Bank employees during the observation period; and (3) data on employees' demographic characteristics, which include gender, race, age, job, organizational tenure, job role tenure, work group assignment, and business unit location.

Sampling strategy. To appropriately test the premises and our subsequent hypotheses, we focused on different subsets of the retail sales population. To test our two premises that in general, women are less likely than men to occupy brokerage positions and that when they do, their performance returns are lower than men's, we relied on monthly observations for all retail sales employees. The data consist of 121,457 complete person–month observations, ranging from November 2014 to April 2016 for 12,889 retail sales employees.

Hypotheses 1 and 2 argue that mobility closes the gender gaps in both brokerage and the returns to brokerage on performance. To test these hypotheses, we focused on 11,254 retail salespeople with the job title ''platform retail sales associate,'' who represent 87 percent of the retail sales employees in our setting.[2] Focusing on holders of a single job title minimizes confounds that may affect performance and how it is evaluated, such as functional role changes or promotion to management. We centered the analyses on platform retail sales associates who experienced internal mobility at Big Bank.

Of course, internal mobility is not random at Big Bank or other similar organizations (e.g., Keller, 2018), so comparing movers with employees who may never qualify for such a move raises questions about potential endogeneity. To compare the performance of movers and the returns to brokerage before and after job mobility, ideally one would have random assignment of mobility. Short of that, information on both the movers and all the candidates who had considered or been considered for the same positions within the organization would be useful, yet no such data exist (or even could exist, since some consideration occurred only in people's minds). Following Rogan and Sorenson (2014), we constructed a mover-control sample with observations on internal movers, to address this issue and test Hypotheses 1 and 2.[3] Specifically, we constructed a control sample—

[2] Our data include newcomers who joined in the middle of the observation window; about 10 percent of platform sales associates changed business units.

[3] As an alternative approach, we constructed a mover-only sample with which we compared movers after mobility with themselves before the mobility by controlling for individual fixed effects. All results show consistent patterns and are reported in Online Appendix Tables A1 and A2.

employees who were observationally equivalent to the movers yet remained in their roles—matched with the case sample of observed internal movers: 1,146 employees with the title "platform retail sales associate" who made exactly one job change within the retail sales department during our observation period. We constructed our control sample by using coarsened exact matching (CEM) (Iacus, King, and Porro, 2012). Such matching allowed us to pair each mover with observationally equivalent employees who did not move. For every individual who moved in *Month t*, we used the HR records in *Month t* to choose matched controls whose job title, age, gender, years of experience at Big Bank, years of experience on the job, primary market of focus, job level, and average categorical performance in the financial quarter prior to *Month t* were the same as the mover's. We assumed their network and performance dynamics were indistinguishable from the mover's before *Month t* and that differences occurring after *Month t* were therefore likely to result from mobility.

Most of our matching variables are categorical (i.e., gender, market of focus, job level, performance in categories), so the matching of these characteristics of the case to control samples was exact. We have three continuous variables (age, years of experience in Big Bank, years of experience on the job) for which the matching was coarsened. The matching procedure and density plots of these variables are provided in Online Appendix Figures A1 and A2, respectively; note that the balance of the distributions of these variables between movers and non-movers improves significantly after matching. The final matched sample has 60,824 mover–month observations, encompassing 841 movers. Observations on movers in their moving months were matched with characteristics of non-movers in the same month, resulting in 5,363 equivalent non-mover–month observations. Because we did not operationalize a one-to-one matching in the CEM process, it is possible for multiple movers to be grouped with non-movers together under a single matching ID. We created 807 unique matching IDs, with 522 women matched groups and 285 men matched groups. In all the models, we controlled for "matching cohort" by including matching ID fixed effects, thus comparing movers to their matched non-movers.

We excluded 305 movers for whom we were not able to match on any non-movers. Our results remain robust when 1,076 movers were matched based on categorical variables and the unmatched variables (age, years of experience in Big Bank and on the job) were controlled; see Online Appendix Tables A3, A4, and A5.

Although we took advantage of the longitudinal nature of our data and relied on the match-control sample in our main analyses, there are still, as in many mobility studies, reasons to be concerned about endogeneity introduced by unobserved heterogeneity. Specifically, the underlying reasons that employees might choose to change jobs may be driving both their social networking behavior and their post-move performance. Moreover, it is possible that unobservable differences between women and men movers exist that may underlie both their mobility and their post-move differences. We attempted to address this endogeneity concern by using a subsample of movers whose decision to move was not their own: Big Bank closed their units due to an industry-wide shift to mobile banking and the associated reduction in consumer demand for in-person service. Here, the reason for moving is likely exogenous to the employees (but not, of course, to Big Bank) and should not be correlated with their gender or their social networks. This analysis required a greatly diminished subsample (N = 127 movers, plus N = 793 matched non-movers). With this subsample, we ran the same set of analyses that we will present in Tables 4, 5, and 6; all findings remain robust. We report the subsample results in Online Appendix Tables A6, A7, and A8.

Key Variables

Women. This variable represents the employee's gender and was set to 1 for employees recorded as women and 0 for men. Gender was self-reported to Big Bank and was time-invariant in our data. Employees could provide one of three responses for gender: woman, man, or decline to identify. As we are interested in gender differences, we excluded from our analyses employees who declined to identify their gender. (In the first month of data collection, 99.85 percent of all retail salespeople self-identified their gender as either man or woman; this percentage remained stable in subsequent months.)

Mover. We set *Mover* to 1 for each employee who experienced internal mobility during our observation period and to 0 otherwise. For any given employee, this variable does not change over time. This binary variable allowed us to model the contrast between movers and their matched non-movers in the mover-control sample.

Post move. We captured the timing of the mobility event by setting *Post move* to 1 in the month the employee moved from one job to another and in all subsequent months and to 0 in the months before the move. *Post move* focuses on modeling the before–after comparison. Non-movers who were not matched to any mover were excluded from our analyses. For non-movers who were matched to movers and selected into the mover-control sample, *Post move* is set to the same value as that of the matched mover, indicating the month when that person's move occurred. Thus, we can model the effect of mobility by observing the differences between these non-moving employees and their matched movers when *Post move* takes the value of 1.

An alternative approach to the binary *Post move* is to calculate *Time since move* by counting the number of months that passed since the internal mobility occurred. *Time since move* focuses on modeling how the initial effects of mobility change over time. As our theory focuses on the changes after versus before mobility, we report all the main analyses with *Post move*. When we explored how the initial effects of mobility change over time, all results remained robust, as shown in Appendix Table A9.

Measuring Networks at Big Bank

To create a conservative representation of the intra-organizational communication network, we limited our analyses to one-to-one emails within the organization, excluding all one-to-many emails or emails sent to and received from external parties. Given that our performance metrics were captured monthly and calculated on the last day of each month, we counted the number of emails sent and received within each pair of employees by the same day every month. Thus, we constructed *directed* and *weighted* intra-organizational networks for each calendar month. As the number of days in a month varies, we included monthly fixed effects in all models. This approach has been shown to reliably quantify intra-organizational network data collected over time (Kleinbaum, Stuart, and Tushman, 2013; Aven, 2015).

Brokerage. The dependent variable for Premise 1 and Hypothesis 1, *Brokerage*, is measured as the multiplicative inverse of the square root of Burt's *Network constraint* measure. *Network constraint* is commonly used to measure network cohesion around an individual (Burt, 1992). Conceptually, *Network constraint* calculates the level of concentration of contacts who are also connected, as the sum of constraint posed by each of the contacts in the network (detailed in Burt, 1992). Monotonically transforming the *Network constraint* measure to a *Brokerage* measure facilitates interpretation of the results by reducing skewness and obtaining a direct rather than an inverse measure (Kleinbaum, 2018). Our results remain robust if we use *Betweenness centrality* instead.

Individual sales performance. The dependent variable for Premise 2 and Hypothesis 2, *Individual sales performance*, is a continuous variable that measures the dollar value of products and services that an employee sold during each calendar month. This variable provides an objective measure of salespeople's productivity and is a key metric through which Big Bank evaluated individual and business unit performance. To account for the right-skewed distribution of *Individual sales performance*, we log-transformed it. The effects should then be interpreted as a percentage change because the models estimate the odds ratio of geometric mean of *Individual sales performance* in the log scale.

Network size and *New contacts.* These two variables are controls that address concerns that employees' new ties, rather than bridging ties, could be underlying the brokerage effect. *Network size* in each month counts the total number of email recipients in the individual's network during that calendar month; this variable helps us to account for an employee's overall communication activity. *New contacts* counts the total number of contacts in the network each month who were not contacted by the focal sender in the prior three months. We log-transformed both variables to account for their right-skewed distributions. To avoid modeling issues with multicollinearity, our models include only the variable total number of *New contacts* because it is correlated with *Network size* ($r > 0.70$, $p < 0.01$, as will be shown in Tables 1A and 1B), but the results also hold when we instead control for *Network size* and when we drop both controls.

Control Variables

In our analyses, we either matched upon or controlled for variables that may contribute to variations in outcomes. We controlled for or matched on characteristics of employees that may affect how employees' networks evolve in organizations, including their *Age*, *Organizational tenure* (in years), and *Job role tenure* (in years). To account for the contextual differences among business units, we controlled for units' time-varying characteristics, including *Size*, *Average organizational tenure*, *Average role tenure* (in the prior financial quarter), *Business unit hierarchical depth* (total number of managerial layers from business unit manager to frontline employee), and *Proportion of men*. We also controlled for the *Unit average performance* (logged) in the prior quarter, to account for the effects of peers' performance on individuals, and for *Unit communication density*, which calculates the ratio of observed communication exchanges among employees in a business unit to the total number of possible communication ties.

Modeling Strategy

Linear regressions for premises. To test Premise 1 and identify the effect of *Gender* on *Brokerage* over time, we ran linear regressions to estimate whether *Gender* is significantly associated with *Brokerage*. As *Gender* does not change for any employee in our sample, we ran regressions with varying intercepts for job levels, business units, and employees nested in business units (Bates et al., 2015). In our setting, allowing the intercepts to vary by employee is important for modeling the gender effect on brokerage since, aside from gender, formal roles and individual differences generate variations in behavior (Burt, 1992; Sasovova et al., 2010). Across all models, we also included monthly fixed effects to account for possible temporal variation and market fixed effects to absorb the regional market differences.

We controlled for individual demographic variables in addition to gender, including their *Age*, *Job level* (formal organizational rank), *Organizational tenure* (in years), and *Job role tenure* (in years). We also controlled for characteristics of the business units to account for the contextual differences among employees, including *Unit size*, *Average organizational tenure*, *Average role tenure*, *Business unit hierarchical depth*, the *Proportion of men*, *Unit average performance* (prior quarter), and *Unit communication density*.

To test Premise 2 and identify the effect of gendered returns of brokerage on individual performance, we estimated the interaction effect of *Gender* × *Brokerage* on *Individual sales performance* in the subsequent month. For a robustness check, we report a model including individual fixed effects. Doing so enables us to deactivate the effect of gender on brokerage and focus on the within-person performance return of brokerage. All results are consistent.

Triple differences analyses for Hypotheses 1 and 2. To estimate the effects of mobility and how it allows women to derive returns from brokerage in the form of post-move performance, we adopted a differences-in-differences-in-differences (triple differences) approach (e.g., Rogan and Sorenson, 2014). Ideally, when the treatment (internal mobility in our case) is randomly assigned, we can interpret the estimated effects as causal rather than correlational, but voluntary job changes within an organization do not occur at random. The basic differences-in-differences (diff-in-diff) analysis examines the outcomes of individuals who are exposed to a treatment (in our case, the internal movers) and the outcomes of those not exposed to the treatment (the control group of non-movers) and allows the comparison of trends between the two groups pre- and post-treatment. With this approach, in our context we compared the trajectories of movers with a matched set of non-movers (i.e., the observationally equivalent employees who did not move). The diff-in-diff analysis in essence compares the average outcome in the treatment group (movers) to the average outcome in the control group (non-movers), thereby eliminating confound effects arising from stable differences between groups and from the trend.

Next, we applied an additional differencing into the diff-in-diff estimator to purge our results of factors correlated with gender, being a mover, and before and after internal mobility, resulting in a triple differences approach. This approach can be understood as a two-layer analysis. We first compared movers to non-movers to estimate the effect of mobility on the change in performance from the pre-move period to the post-move period. We then estimated the diff-in-diff for women and men, to compare the effect sizes of performance by gender. In other words, how does the performance trajectory of women movers compare with that of similar women who did not move? And how does the performance trajectory of men movers compare with that of similar men who did not move? The triple differences analysis represents differences between these differences to provide an estimate of the gendered effects on brokerage or performance, conditional on mobility. The analyses help to net out endogenous factors from variations in the effects of intra-organizational mobility as a function of gender.

RESULTS

Of retail sales employees in our sample, 65.5 percent are women, which corresponds roughly to the proportion of women in the mover sample (61.7 percent). The slight under-representation of women as movers is consistent with the observation that men at Big Bank are more likely than women to change jobs or to leave Big Bank. Because it is plausible that proximate job changes entail fewer challenges than distant job changes do, we explore the association between gender and moving distance. We find that women and men movers did not differ in the distances that they moved between jobs ($p > 0.1$), nor were women more likely than men to stay within the same city or state. Hence, it is unlikely that any gender-based difference in moving distance confounded the gender differences we report later. These coefficients are reported in Online Appendix Table A10. We report descriptive statistics for all retail sales employees and the correlation matrix in Table 1A and descriptive statistics for the mover-control sample in Table 1B. In comparing the observed means and standard deviations between Tables 1A and 1B, we find that the movers were younger, had less organizational and job experience, and worked in smaller business units, compared to the average employee at Big Bank. These differences between movers and non-movers support our approach of matching the movers with their observationally equivalent non-movers; the variables were either controlled or balanced by coarsened exact matching.

Gender and Brokerage

Table 2 presents results for Premise 1, whereby we examine the relationship between gender and brokerage in the person–month observations of all retail sales employees at Big Bank. We estimated the models with random intercepts for individuals nested within business units. Such models account for the non-independence of observations from the same individual and for the fact that individuals working in the same business unit might be more homogenous than those working at different locations. Standard errors are hence clustered by employees nested in business units.

Table 1A. Descriptive Statistics of Retail Salespeople at Big Bank (at Individual-Month Level, N = 121,457)

	Mean.	Std.	Min.	Max.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
1 Age (logged, years)	3.53	0.32	2.89	4.30															
2 Gender (women = 1, men = 0)	0.67	0.47	0	1	0.15•														
3 Org tenure (years)	4.85	7.33	0	49.8	0.54•	0.18•													
4 Job tenure (years)	1.15	1.26	0	14.9	0.34•	−0.04•	0.39•												
5 Growth market (binary)	0.24	0.43	0	1	0.01•	−0.07•	−0.08•	−0.01											
6 Unit size (logged)	2.29	0.63	0.69	7.56	0.07•	−0.08•	0.1•	0.14•	−0.06•										
7 Unit average org tenure	5.79	3.79	0.03	30.15	0.26•	0.13•	0.45•	0.19•	−0.22•	0.31•									
8 Unit average job tenure	1.97	1.17	0.01	9.93	0.18•	0.06•	0.26•	0.26•	−0.13•	0.29•	0.71•								
9 Within-unit proportion of men	0.32	0.2	0	1	−0.13•	−0.43•	−0.17•	−0.02•	0.19•	0.12•	−0.33•	−0.19•							
10 Unit hierarchical depth	5.80	3.55	1	25	0.08•	−0.07•	0.10•	0.16•	−0.05•	0.9•	0.35•	0.36•	0.09•						
11 Log average unit performance	10.58	0.87	0	13.71	0.12•	−0.02•	0.01•	0.0	0.05•	0.02•	0.01•	0.08•	0.02	0.07•					
12 Within-unit communication density	0.35	0.14	0.01	1	−0.09•	0.05•	−0.11•	−0.11•	0.02•	−0.61•	−0.28•	−0.30•	−0.07•	−0.57•	−0.15•				
13 Brokerage	2.36	0.66	0.89	10	0.19•	−0.15•	0.16•	0.32•	0.09•	0.21•	0.06•	0.08•	0.14•	0.22•	0.09•	−0.20•			
14 New contacts (logged)	1.92	0.92	0	7.77	0.17•	−0.11•	0.16•	0.28•	0.01	0.21•	0.08•	0.08•	0.05•	0.22•	0.05•	−0.05•	0.60•		
15 Network size (logged)	2.65	0.96	0	7.8	0.19•	−0.1•	0.19•	0.33•	−0.03•	0.22•	0.11•	0.11•	0.04•	0.23•	0.24•	0.01•	0.61•	0.87•	
16 Individual sales performance (logged)	9.70	2.75	0	14.02	0.2•	−0.02•	0.24•	0.28•	0.01•	0.06•	0.11•	0.11•	−0.02•	0.10•	0.48•	−0.01•	0.29•	0.34•	0.45•

• $p < 0.05$ (two-tailed tests)

Table 1B. Descriptive Statistics of Internal Movers and Their Matched Non-Movers (at Individual-Month Level, N = 60,824)

		Mean	Std.	Min.	Max.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
1	Age (logged, years)	3.52	0.32	2.89	4.29															
2	Gender (women = 1, men = 0)	0.69	0.44	0	1	0.18•														
3	Org tenure (years)	4.75	7.32	0.3	46.7	0.56•	0.20•													
4	Job tenure (years)	1.12	1.01	0	13.2	0.35•	0.0	0.38•												
5	Growth market (binary)	0.28	0.45	0	1	−0.02•	−0.08•	−0.1•	−0.04•											
6	Unit size (logged)	2.21	0.59	0.69	7.56	0.01•	−0.02•	0.09•	0.06•	−0.07•										
7	Unit average org tenure	5.41	3.66	0.03	26.9	0.29•	0.18•	0.48•	0.18•	−0.21•	0.29•									
8	Unit average job tenure	1.91	1.13	0.01	9.93	0.2•	0.12•	0.27•	0.25•	−0.15•	0.26•	0.71•								
9	Within-unit proportion of men	0.32	0.21	0	1	−0.16•	−0.45•	−0.19•	−0.04•	0.19•	0.09•	−0.36•	−0.23•							
10	Unit hierarchical depth	5.34	3.31	1	25	0.04•	0	0.1•	0.06•	−0.06•	0.89•	0.35•	0.36•	0.04•						
11	Log average unit performance	10.58	0.81	0	13.33	0.01	−0.01•	0	0	−0.01•	0	0.01	0.02	0.02•	0					
12	Within-unit communication density	0.36	0.14	0	1	−0.05•	0	−0.11•	−0.07•	0.01•	−0.57•	−0.26•	−0.28•	−0.05•	−0.54•	0				
13	Brokerage	2.38	0.61	0.89	7.19	0.14•	−0.21•	0.12•	0.24•	0.14•	0.12•	−0.02	0.02•	0.18•	0.13•	0.01	−0.14•			
14	New contacts (logged)	1.93	0.83	0	7.02	0.14•	−0.09•	0.14•	0.19•	0.02•	0.14•	0.04•	0.04•	0.05•	0.15•	0.03	0.02•	0.52•		
15	Network size (logged)	3.36	0.64	0	7.04	0.17•	−0.13•	0.16•	0.29•	0.04•	0.14•	0	0.03•	0.12•	0.14•	0.04	0.07•	0.63•	0.73•	
16	Individual sales performance (logged)	9.66	2.36	0	14.02	0.23•	−0.07•	0.23•	0.29•	0.02•	0.04•	0.10•	0.10•	0.02	0.08•	0.01•	−0.01•	0.25•	0.25•	0.38•

• $p < 0.05$ (two-tailed tests)

Table 2. Effects of Gender on Network Brokerage*

	Brokerage (t)				
	(1)	(2)	(3)	(4)	(5)
Women	– 0.106•••	– 0.086•••	– 0.113•••	– 0.059•••	– 0.078•••
	(0.009)	(0.008)	(0.008)	(0.008)	(0.008)
New contacts (logged)		0.249•••	0.246•••	0.254•••	0.251•••
		(0.001)	(0.001)	(0.001)	(0.001)
Age (years, logged)			0.124•••		0.108•••
			(0.013)		(0.013)
Org tenure (years)			0.005•••		0.006•••
			(0.001)		(0.001)
Job tenure (years)			0.048•••		0.049•••
			(0.003)		(0.003)
Growth market (binary)				0.133•••	0.128•••
				(0.008)	(0.008)
Unit size (logged)				– 0.043•••	– 0.045•••
				(0.007)	(0.007)
Average org tenure				0.004•••	– 0.001
				(0.001)	(0.001)
Average job tenure				– 0.002	– 0.007•
				(0.003)	(0.003)
Proportion of men				0.115•••	0.121•••
				(0.012)	(0.012)
Unit hierarchical depth				0.004•••	0.004•••
				(0.001)	(0.001)
Log average unit performance (t–4 to t–1)				– 0.001	– 0.001
				(0.001)	(0.001)
Within-unit communication density				– 0.360•••	– 0.359•••
				(0.013)	(0.013)
Constant	2.865•••	2.078•••	1.608•••	2.180•••	1.792•••
	(0.731)	(0.275)	(0.270)	(0.153)	(0.569)
Observations	121,457	121,457	121,457	121,457	121,457
Log likelihood	– 67,111.77	– 54,344.55	– 53,929.76	– 53,801.94	– 53,365.79

• $p < .05$; •• $p < .01$; ••• $p < .001$ (two-tailed tests)
* All models include job and month fixed effects. All models include business unit and individual nested in business-unit random intercepts.

Across all the models in Table 2, we find a significant gender effect, indicating that women exhibit lower network brokerage than men do. Model 5 of Table 2 includes all control variables and shows that women's brokerage is 0.078 lower than that of men's on average, which is about 0.11 standard deviation. The effects we show here are consistent with findings reported by prior studies (Fang, Zhang, and Shaw, 2021; Brands et al., 2022).[4] Across all analyses, Premise 1 is supported.

[4] While the observed gender differences may seem small, note that compared with network survey data, email data are heavily structured by the task requirements of the formal organization, which should reduce the magnitude of gender differences. In Online Appendix Tables A11 and A12, we explored an alternative measure of *Brokerage* by excluding all email communications between supervisors and subordinates and between subordinates reporting to the same supervisor. This adjustment results in a larger gender gap, highlighting the presence of gender differences in semi-formal and informal communications.

Table 3. Effects of Gender and Brokerage on Individual Sales Performance*

	Individual Sales Performance (logged, t+1)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Brokerage	0.481•••	0.449•••	0.288•••	0.240•••	0.297•••	0.250•••	0.187•••
	(0.012)	(0.020)	(0.021)	(0.021)	(0.021)	(0.021)	(0.026)
Women		0.256•••	0.164•	0.014	0.201••	– 0.005	
		(0.069)	(0.068)	(0.066)	(0.069)	(0.067)	
Women × Brokerage		– 0.106•••	– 0.108•••	– 0.091•••	– 0.106•••	– 0.127•••	– 0.093•••
		(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.031)
New contacts (logged)			0.227•••	0.225•••	0.220•••	0.218•••	0.171•••
			(0.008)	(0.008)	(0.008)	(0.008)	(0.009)
Age (years, logged)				0.437•••		0.429•••	
				(0.060)		(0.060)	
Org tenure (years)				0.066•••		0.070•••	
				(0.003)		(0.003)	
Job tenure (years)				0.329•••		0.322•••	
				(0.013)		(0.013)	
Growth market (binary)					0.054	0.059	
					(0.040)	(0.037)	
Unit size (logged)					– 0.059	– 0.103••	– 0.090•
					(0.035)	(0.034)	(0.045)
Average org tenure					0.019•••	– 0.017•••	– 0.019••
					(0.005)	(0.005)	(0.007)
Average job tenure					0.062•••	0.044•••	0.002
					(0.013)	(0.013)	(0.017)
Proportion of men					– 0.163••	– 0.141•	– 0.089
					(0.056)	(0.055)	(0.072)
Unit hierarchical depth					0.008	0.015••	0.059•••
					(0.005)	(0.005)	(0.006)
Log average unit performance					0.006	0.005	0.015•
(t–4 to t–1)					(0.005)	(0.005)	(0.006)
Within-unit communication					0.445•••	0.457•••	0.441•••
density					(0.063)	(0.062)	(0.075)
Constant	10.865•••	10.692•••	10.612•••	8.847•••	10.308•••	8.861•••	
	(0.776)	(0.777)	(0.773)	(0.812)	(0.777)	(0.817)	
Observations	109,234	109,234	109,234	109,234	109,234	109,234	109,234
Individual fixed effects	No	No	No	No	No	No	Yes
Log likelihood/R^2	– 212,333.5	– 212,329.7	– 211,927.7	– 210,933.0	– 211,860.8	– 210,800.3	0.112

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

* All models include job and month fixed effects. Models 1–6 include business unit and individual nested in business-unit random intercepts. Variables that are collinear with individual fixed effects are excluded from Model 7. The effects of *Brokerage* and its interaction with gender are robust to controls for network variables that may correlate with *Brokerage*, such as *Network size*.

Gender and Return to Brokerage

Table 3 presents results for Premise 2. As in Table 2, standard errors are clustered by employees, who are nested in business units. As shown in all models, we find that all else being equal, employees with higher brokerage exhibit significantly higher individual sales performance than do peers with lower brokerage, which is consistent with prior research on social networks in organizations (e.g., Burt, 1992; Kilduff and Brass, 2010; Tortoriello, Reagans, and McEvily, 2012). However, the negative, significant interaction effect of *Women* × *Brokerage* indicates that women receive lower performance returns to brokerage than men do across all the models. In Model 6, with individual random intercepts and control variables, the positive association between brokerage and individual sales performance is nearly twice as strong for men ($\beta_{Brokerage} = 0.250$, $p < 0.01$) as it is for women ($\beta_{Women \times Brokerage} = -0.127$, $p < 0.01$). A similar pattern occurs in Model 7 with individual fixed effects included: the positive association between brokerage and individual sales performance is twice as strong for men ($\beta_{Brokerage} = 0.187$, $p < 0.01$) as it is for women ($\beta_{Women \times Brokerage} = -0.093$, $p < 0.01$). To contextualize this effect size, an increase by one standard deviation in brokerage from the grand mean is associated with a 13.1 percent increase in individual sales performance for men but just a 5.9 percent increase in individual sales performance for women. These findings confirm that the benefits that women obtain from network brokerage are significantly lower than those that men receive. Taken together, our results support Premise 2 and promote the generalizability of prior research findings that, in general, women earn lower returns to brokerage than men do.

Mobility, Gender, and Brokerage

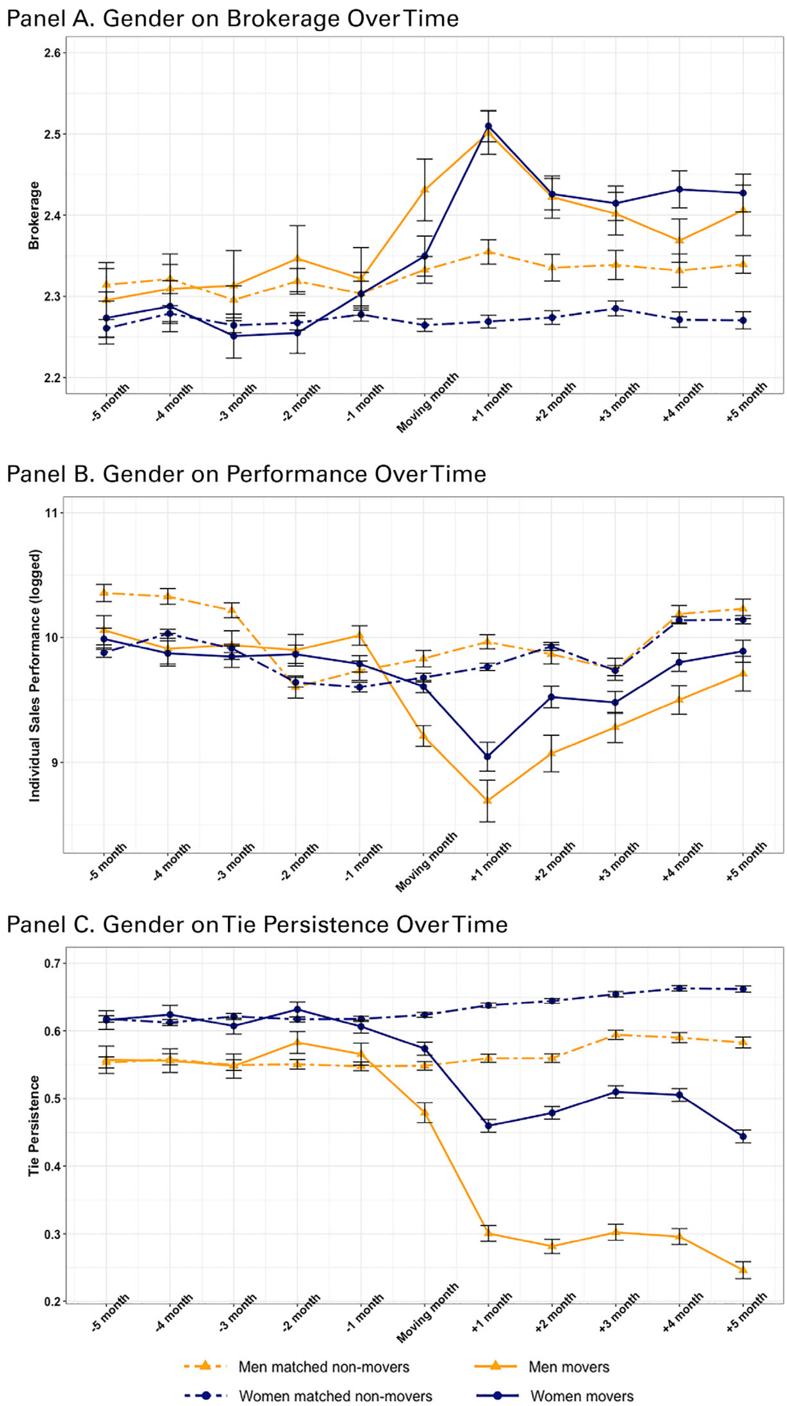
We next estimate the effect of mobility on brokerage with the mover-control sample. To facilitate interpretation, we present a visualization of the raw data from the matched sample in Figure 1. Panel A shows that for both movers and their matched non-movers, women exhibit visibly lower brokerage before mobility than men do, consistent with Premise 1. Such gender difference persists only for the matched non-movers (dashed lines); the gender difference in brokerage disappears for movers after a move (solid lines).

To account for gender, we use within–between or hybrid models (Allison, 2009; Bell and Jones, 2015; Long, 2020), which allow us to calculate difference-in-differences estimates for both men and women employees. At the same time, we control for fixed effects related to matching IDs or within matched groups. The main effect of *Women* is estimated between matched groups. The second-order difference-in-differences estimator *Mover* × *Post move* is estimated within matched groups. And the interactions *Women* × *Mover* and *Women* × *Post move* are estimated by the cross-level interactions. Through the within–between estimations, we gauge the impact across genders while adjusting for variations between matched movers and non-movers. Results on *Brokerage* are reported in Table 4.

Model 1 in Table 4 includes our key covariates; Model 2 additionally includes each individual's total number of new contacts and business unit–level controls. Consistent with the models in Table 2, as is shown in Model 2, women exhibit lower brokerage than men do ($\beta_{Women} = -0.062$, $p < 0.01$) by about 0.10 standard deviation. The within-matched-group effects of *Mover*, *Post move*, and the *Mover* × *Post move* diff-in-diff estimator indicate that among men, brokerage does not vary significantly between movers and matched non-movers or over time ($\beta_{Mover \times Post\ move} = 0.014$, $p = 0.50$).[5]

[5] By comparison to Model 2 in Online Appendix Table A14, which controls for *Network size* where the interaction is significant, the contrast, together with the effect of *Women* × *Mover* × *Post move* that was positive and significant in both models, suggests that brokerage of women movers cannot be fully explained by changes in *New contacts*. For average men movers, mobility increases brokerage via their increases in *New contacts*.

Figure 1. Visualized Results*



* In all panels, we plot the means and standard errors of the means summarized within matching IDs and months, between gender.

Table 4. Effects of Mobility and Gender on Brokerage*

	Brokerage (t)					
	(1)	(2)	(3) Before Move	(4) After Move	(5) Women (522 MIDs)	(6) Men (285 MIDs)
Within-Matched-Group Effects						
Mover	0.001	0.003	0.001	0.019	0.013	0.016
	(0.019)	(0.017)	(0.017)	(0.013)	(0.007)	(0.011)
Post move	0.010	0.008			0.017••	0.004
	(0.014)	(0.012)			(0.005)	(0.009)
Mover × Post move	0.039	0.014			0.084•••	– 0.004
	(0.022)	(0.021)			(0.015)	(0.024)
Cross-Group Interactions with Gender						
Women	– 0.150•••	– 0.062••	– 0.104•••	– 0.060•		
	(0.020)	(0.023)	(0.023)	(0.023)		
Women × Mover	– 0.021	– 0.037	– 0.029	0.048••		
	(0.022)	(0.020)	(0.020)	(0.016)		
Women × Post move	– 0.009	0.003				
	(0.016)	(0.014)				
Women × Mover × Post move	0.095•••	0.082••				
	(0.028)	(0.025)				
Within-Matched-Group Effects of Control Variables						
Growth market (binary)		0.125•••	0.152•••	0.094		
		(0.035)	(0.042)	(0.077)		
New contacts (logged)		0.287•••	0.294•••	0.277•••	0.281•••	0.305•••
		(0.003)	(0.004)	(0.004)	(0.003)	(0.005)
Unit size (logged)		– 0.061•••	– 0.112•••	– 0.012	– 0.093•••	0.022
		(0.008)	(0.012)	(0.011)	(0.010)	(0.017)
Average org tenure		– 0.001	0.001	– 0.002	– 0.001	0.001
		(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Average job tenure		– 0.001	– 0.002	0.003	– 0.001	– 0.001
		(0.003)	(0.004)	(0.004)	(0.003)	(0.006)
Proportion of men		0.075•••	0.071•••	0.077•••	0.110•••	0.058•
		(0.012)	(0.017)	(0.017)	(0.014)	(0.023)
Unit hierarchical depth		0.010•••	0.016•••	0.003	0.007•••	0.015•••
		(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
Log average unit performance (t–4 to t–1)		0.001	– 0.001	0.001	0.001	0.001
		(0.002)	(0.003)	(0.003)	(0.003)	(0.005)
Within-unit communication density		– 0.534•••	– 0.582•••	– 0.474•••	– 0.622•••	– 0.266•••
		(0.017)	(0.024)	(0.025)	(0.020)	(0.036)
Mean Differences Between Matched Groups						
Mover	– 0.037	– 0.048	– 0.028	– 0.105••		
	(0.050)	(0.041)	(0.038)	(0.040)		
Post move	– 0.146••	– 0.148•••				
	(0.047)	(0.038)				
Constant	1.824•••	1.607•••	1.949•••	1.787•••		
	(0.263)	(0.346)	(0.271)	(0.293)		
Control variables	No	Yes	Yes	Yes	No	No
Observations	60,824	60,824	31,513	29,311	42,352	18,472
Log likelihood/R^2	– 44,889.66	– 38,552.40	– 19,916.78	– 18,887.98	0.443	0.538

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

* Matching ID fixed effects are included in all the models. We focus on the effects within matching IDs and the interaction between gender and key variables across matching IDs. Given that the IDs are generated through coarsened exact matching, between-matched-group control effects are not meaningful for our research questions. Therefore, while we have accounted for these control variable differences across matched groups in Models 2–4, we have opted to omit these coefficients due to space limitations. Models 5 and 6 are fixed-effect models; mean differences between matched groups are not included in the estimations. Variables that are collinear with matching ID fixed effects are excluded from Models 5 and 6.

Next, we shift our attention to the effect of the interaction between *Gender* and the diff-in-diff estimator, *Mover* × *Post move*, on *Brokerage*. This triple-difference estimator, the three-way interaction *Women* × *Mover* × *Post move*, permits estimation of a gender difference in the extent to which movers increase their brokerage after mobility. The significantly positive three-way interaction shows that mobility affects brokerage differently for men and women ($\beta_{Women \times Mover \times Post\ move} = 0.095$, $p < 0.001$). When we include control variables in Model 2, the results remain consistent ($\beta_{Women \times Mover \times Post\ move} = 0.082$, $p < 0.01$). Despite a main effect that women exhibit lower network brokerage than men do overall, mobile women exhibit greater increases in brokerage than men do after they move, bringing the brokerage of women movers on par with that of men. That is, the negative main effect of gender on brokerage ($\beta_{Women} = -0.062$, $p < 0.01$) is eliminated following mobility ($\beta_{Women \times Mover \times Post\ move} + \beta_{Women \times Mover} + \beta_{Women \times Post\ move} + \beta_{Women} = -0.014$).

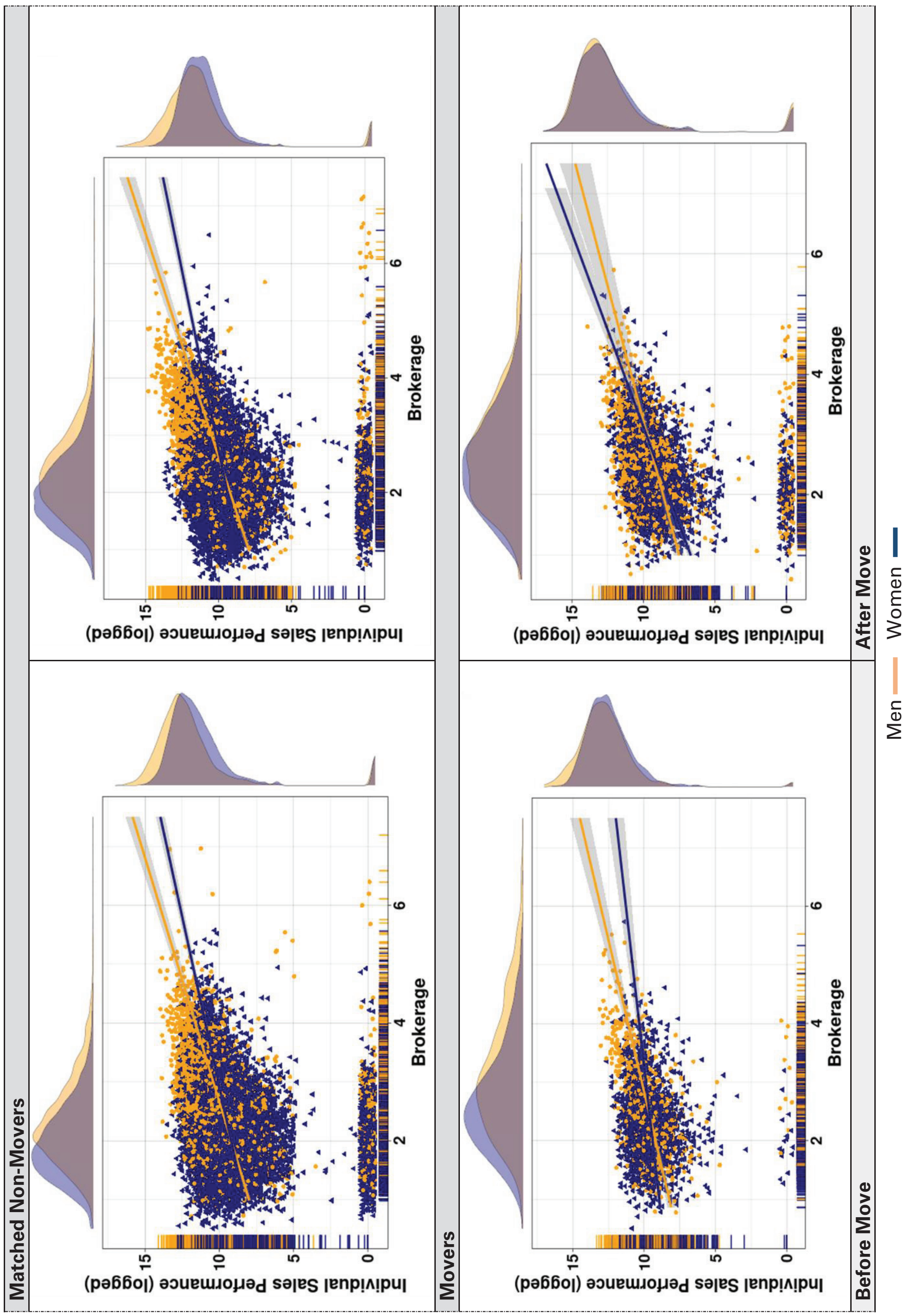
In Models 3 and 4, we drop the triple-difference estimator and, instead, separately examine gender effects in subsamples before and after mobility events. As shown in Model 3, although women exhibit lower brokerage than men do ($\beta_{Women} = -0.104$, $p < 0.01$), the effect of *Women* × *Mover* is not significant before mobility occurs ($\beta_{Women \times Mover} = -0.029$, $p = 0.15$). The statistically insignificant coefficient confirms the validity of CEM: there is no significant difference between movers and matched non-movers before the movers change jobs. Model 4 reports the effect of *Gender* × *Mover* after moving. Consistent with the results of Models 1 and 2, the negative effect of *Gender* ($\beta_{Women} = -0.060$; $p = 0.01$) remains for non-movers but is wiped out for women movers ($\beta_{Women \times Mover} = 0.048$; $p < 0.01$). The results suggest that after mobility, the gender gap in brokerage is eliminated ($\beta_{Women \times Mover} + \beta_{Women} = -0.012$).

Although the cross-level three-way interaction is a powerful tool to model gender differences, it assumes that the underlying data for men and women have homogenous distributions (Aiken and West, 1991). In Models 5 and 6, we further test the robustness of our results by dropping the triple-difference estimator and, instead, examining the two-way difference-in-differences estimators separately for women and men. Model 5 reports the effect of *Mover* × *Post move* for women ($\beta_{Mover \times Post\ move} = 0.084$, $p < 0.001$). Model 6 reports its effect for men ($\beta_{Mover \times Post\ move} = -0.004$, $p = 0.87$). Across all the models, we find consistent evidence that women exhibit significantly lower brokerage before mobility than men do, consistent with Premise 1. But the gender difference persists only for the matched non-movers. Among movers, the gender difference in brokerage disappears after their move. We thus find strong support for Hypothesis 1.

Mobility, Gender, and Returns to Brokerage

In Hypothesis 2, we argue that mobility will eliminate the gender gap in returns to brokerage. Testing this hypothesis entails several complications. To facilitate interpretation, we present visualizations of the raw data of performance in Figure 1, Panel B and Figure 2. Among both movers and their matched non-movers, women benefit less from brokerage than men do before the move. Such gender difference in returns to brokerage (in terms of performance), visualized by the slope between the x-axis and y-axis in Figure 2, persists for the matched non-movers but is eliminated for movers once they move.

Figure 2. The Scattered Plots Between Brokerage and Performance (Standard Errors in the Shade)



To parallel the analyses in Table 3, we would confirm the differential performance return to brokerage in this mover-control sample and then test a mobility effect such that the above differential returns to brokerage on performance are eliminated after the movers change their jobs. A full model of this proposed effect, therefore, would necessitate a four-way interaction: *Women* × *Mover* × *Post move* × *Brokerage*. For simplicity, we first split the data into pre-move and post-move subsamples. We provide separate analyses for the three-way *Women* × *Mover* × *Brokerage* interaction on *Individual sales performance* before and after intra-organizational job changes in Table 5. According to our theoretical prediction, we do not expect mobility (or being a mover later) to have any effect on the relationship between *Women* × *Brokerage* and *Individual sales performance* before the movers change jobs. After movers change their jobs, however, we expect them and their match-control counterparts to derive differential benefits from brokerage: the performance return of brokerage for women movers should be equivalent to, if not greater than, that for men (both movers and non-movers). Models in Table 5 confirm our predictions.

Models 1 and 2 in Table 5 present the estimated effects of *Women* × *Mover* × *Brokerage* on *Individual sales performance* prior to internal mobility. We find that, consistent with results in Table 3, brokerage benefits performance but that in the pre-move subsample, women experience significantly lower returns from brokerage than men do ($\beta_{Women \times Brokerage} = -0.464$; $p < 0.001$), as shown in Model 1. Models 3 and 4 in Table 5 present the estimated effects of *Women* × *Mover* × *Brokerage* on *Individual sales performance* after mobility. While women still experience significantly lower returns to brokerage in general ($\beta_{Women \times Brokerage} = -0.361$; $p < 0.001$), after mobility the positive and significant interaction of *Women* × *Mover* × *Brokerage* ($\beta_{Women \times Mover \times Brokerage} = 0.389$, $p < 0.001$) in Model 3 suggests that mobility eliminates the negative pattern for women movers and enables them to benefit from brokerage as much as their men counterparts do ($\beta_{Women \times Brokerage} + \beta_{Women \times Mover \times Brokerage} = 0.028$). Results in Models 1 and 3 remain robust after we included control variables in Model 2 and Model 4, respectively. To contextualize this effect size in Model 4, all else equal, after mobility, an increase by one standard deviation in brokerage from the grand mean is associated with the following increases in individual sales performance: 27.4 percent for men movers, 33.9 percent for men non-movers, 6.0 percent for women non-movers, and 40.1 percent for women movers.

To statistically test whether the mobility effect such as the above differential return to brokerage on performance is eliminated after the movers change their jobs, we estimate a four-way interaction *Women* × *Mover* × *Post move* × *Brokerage*, despite the risk of overfitting the model (Aiken and West, 1991; Dawson and Richter, 2006). We report models using this four-way interaction in Table 6. Models 1 and 2 in Table 6 report our key covariates. The main effect of *Women* on *Individual sales performance* and interactions with gender were estimated between matched groups. The second-order diff-in-diff estimators *Mover* × *Post move, Mover* × *Brokerage*, and *Post move* × *Brokerage* and the third-order estimator *Mover* × *Post move* × *Brokerage* were all estimated within matched groups. Here we focus our interpretation on Model 2, which included control variables.

When we hold brokerage constant, the negative and significant effect of the *Mover* × *Post move* diff-in-diff estimator suggests that in general, movers exhibit significant performance disruption compared with their observationally

Table 5. Effects of Mobility and Gendered Return of Brokerage*

	Individual Sales Performance (logged, t+1)			
	Before Move		After Move	
	(1)	(2)	(3)	(4)
Within-Matched-Group Effects				
Mover	0.158•	0.131	– 0.003	– 0.019
	(0.076)	(0.076)	(0.053)	(0.053)
Brokerage	0.622•••	0.509•••	0.534•••	0.479•••
	(0.042)	(0.043)	(0.040)	(0.041)
Mover × Brokerage	0.071	0.069	– 0.066	– 0.082
	(0.156)	(0.156)	(0.114)	(0.114)
Cross-Group Interactions with Gender				
Women	0.221•	– 0.134	0.240••	– 0.049
	(0.111)	(0.140)	(0.083)	(0.099)
Women × Mover	– 0.002	0.013	0.136•	0.126
	(0.090)	(0.089)	(0.065)	(0.065)
Women × Brokerage	– 0.464•••	– 0.436•••	– 0.361•••	– 0.384•••
	(0.049)	(0.049)	(0.047)	(0.047)
Women × Mover × Brokerage	– 0.368	– 0.327	0.389••	0.458•••
	(0.190)	(0.189)	(0.142)	(0.141)
Within-Matched-Group Effects of Control Variables				
Growth market (binary)		– 0.060		0.311
		(0.179)		(0.309)
New contacts (logged)		0.158•••		0.138•••
		(0.017)		(0.016)
Unit size (logged)		– 0.230•••		– 0.562•••
		(0.052)		(0.046)
Average org tenure		0.020•••		0.013•
		(0.005)		(0.005)
Average job tenure		0.077•••		0.032•
		(0.015)		(0.016)
Proportion of men		0.154•		0.170•
		(0.069)		(0.068)
Unit hierarchical depth		0.055•••		0.081•••
		(0.009)		(0.009)
Log average unit performance (t–4 to t–1)		0.019		0.036••
		(0.014)		(0.014)
Within-unit communication density		0.846•••		0.186
		(0.102)		(0.100)
Mean Differences Between Matched Groups				
Mover	0.320	– 0.041	– 0.777•••	– 1.022•••
	(0.230)	(0.215)	(0.188)	(0.170)
Brokerage	1.518•••	0.933•••	1.056•••	0.507•••
	(0.191)	(0.213)	(0.139)	(0.152)
Constant	2.017•••	– 0.388	4.193•••	5.511•••
	(0.619)	(1.632)	(0.595)	(1.295)
Control variables	No	Yes	No	Yes
Observations	31,513	31,513	29,311	29,311
Log likelihood	– 65,814.30	– 65,609.16	– 59,842.97	– 59,624.29

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

* Matching ID fixed effects are included in all the models. Standard errors are clustered by matching ID. We focus on the effects within matching IDs and the interaction between gender across matched groups. Between-matched-group control effects were included in Models 2 and 4 but not reported due to space limitations.

Table 6. Effects of Mobility and Gendered Return of Brokerage (Continued)*

	Individual Sales Performance (logged, t+1)			
	(1) Full Interaction Model	(2) Interaction Model with Controls	(3) Women (522 MIDs)	(4) Men (285 MIDs)
Within-Matched-Group Effects				
Mover	0.053	0.033	0.056	–0.045
	(0.045)	(0.044)	(0.029)	(0.049)
Post move	0.145	0.158	0.041	–0.033
	(0.080)	(0.080)	(0.022)	(0.042)
Brokerage	0.588$^{\bullet\bullet\bullet}$	0.501$^{\bullet\bullet\bullet}$	0.120$^{\bullet\bullet\bullet}$	0.511$^{\bullet\bullet\bullet}$
	(0.029)	(0.030)	(0.020)	(0.036)
Mover × Post move	–0.421$^{\bullet\bullet\bullet}$	–0.396$^{\bullet\bullet\bullet}$	–0.310$^{\bullet\bullet\bullet}$	–0.492$^{\bullet\bullet\bullet}$
	(0.098)	(0.097)	(0.063)	(0.107)
Mover × Brokerage	–0.045	–0.050	0.105	–0.073
	(0.091)	(0.091)	(0.063)	(0.100)
Post move × Brokerage	–0.098	–0.072	–0.055	–0.105
	(0.063)	(0.063)	(0.039)	(0.070)
Mover × Post move × Brokerage	–0.012	–0.020	0.383$^{\bullet\bullet}$	0.113
	(0.187)	(0.187)	(0.129)	(0.200)
Cross-Group Interactions with Gender				
Women	0.114	–0.068		
	(0.067)	(0.080)		
Women × Mover	0.077	0.077		
	(0.054)	(0.054)		
Women × Post move	0.005	–0.004		
	(0.097)	(0.097)		
Women × Brokerage	–0.420$^{\bullet\bullet\bullet}$	–0.415$^{\bullet\bullet\bullet}$		
	(0.034)	(0.034)		
Women × Mover × Post move	0.266$^{\bullet}$	0.230$^{\bullet}$		
	(0.117)	(0.115)		
Women × Mover × Brokerage	0.073	0.126		
	(0.112)	(0.111)		
Women × Post move × Brokerage	0.053	0.026		
	(0.074)	(0.074)		
Women × Mover × Post move × Brokerage	0.481$^{\bullet}$	0.506$^{\bullet}$		
	(0.232)	(0.231)		
Mean Differences Between Matched Groups				
Mover	0.300	–0.134		
	(0.168)	(0.149)		
Post Move	–1.810$^{\bullet\bullet\bullet}$	–1.692$^{\bullet\bullet\bullet}$		
	(0.157)	(0.138)		
Brokerage	0.806$^{\bullet\bullet\bullet}$	0.045		
	(0.118)	(0.128)		
Constant	1.648	3.031$^{\bullet}$		
	(0.909)	(1.281)		
Control variables (within matched groups)	No	Yes	Yes	Yes
Control variables (between matched groups)	No	Yes	No	No
Observations	60,824	60,824	42,352	18,472
Log likelihood/R^2	–125,632.80	–125,215.94	0.350	0.383

$^{\bullet}$ $p < .05$; $^{\bullet\bullet}$ $p < .01$; $^{\bullet\bullet\bullet}$ $p < .001$ (two-tailed tests)

* Matching ID fixed effects are included in all the models. Standard errors are clustered by matching ID. Within-matched-group control variable estimations are included in Models 2–4, and between-matched-group control variables are included in Model 2; coefficients are omitted due to space limitations.

equivalent counterparts who have not moved ($\beta_{Mover \times Post\ move} = -0.396$, $p < 0.001$). Mobility decreases individual sales performance by about 32.7 percent for movers, regardless of gender. Within each matched group, brokerage exhibits a significant and positive association with individual sales performance in general ($\beta_{Brokerage} = 0.501$, $p < 0.001$). For men, this association between brokerage and individual sales performance does not vary significantly between movers and matched non-movers ($\beta_{Mover \times Brokerage} = -0.050$, $p = 0.58$) or over time ($\beta_{Post\ move \times Brokerage} = -0.072$, $p = 0.25$ and $\beta_{Mover \times Post\ move \times Brokerage} = -0.020$, $p = 0.91$). Accounting for the cross-level interactions between *Gender* and the diff-in-diff estimators, we find that women experience significantly lower returns from brokerage than men do ($\beta_{Women \times Brokerage} = -0.415$, $p < 0.001$). Such gender difference persists for women movers prior to mobility ($\beta_{Women \times Mover \times Brokerage} = 0.126$, $p = 0.26$) and for women non-movers ($\beta_{Women \times Post\ move \times Brokerage} = 0.026$, $p = 0.73$). The substantial positive coefficient from the four-way interaction ($\beta_{Women \times Mover \times Post\ move \times Brokerage} = 0.506$, $p = 0.03$) offsets the negative return associated with women brokers ($\beta_{Women \times Mover \times Post\,move \times Brokerage} + \beta_{Women \times Mover \times Brokerage} + \beta_{Women \times Post\,move \times Brokerage} + \beta_{Women \times Brokerage} = 0.243$).

To provide robustness checks on our estimations, we also report estimators for the three-way *Mover* × *Post move* × *Brokerage* interaction on *Individual sales performance* for men and women separately. In Models 3 and 4 in Table 6, we drop the four-way interaction and, instead, estimate models separately for women and men within matching IDs/matched groups. Model 3 reports the effect of *Mover* × *Post move* × *Brokerage* for women ($\beta_{Mover \times Post\ move \times Brokerage} = 0.383$, $p < 0.01$). Model 4 reports its effect for men ($\beta_{Mover \times Post\ move \times Brokerage} = 0.113$, $p = 0.57$). To contextualize this effect size, as shown in Model 3, a one-standard-deviation increase in brokerage from the grand mean is associated with a 14.7 percent increase in performance for women movers prior to mobility, and a 40.1 percent increase for women movers following mobility. And as shown in Model 4, a one-standard-deviation increase in brokerage from the grand mean is associated with the following increases in individual sales performance for men: 30.6 percent for movers prior to mobility and 31.3 percent for movers following mobility. Mobility eliminates the gender gap in returns to brokerage. Taken together, our analyses provide strong support for Hypothesis 2.

Exploring Network Dynamics Underlying Women's Brokerage

This section provides empirical analyses exploring the network dynamics underlying women mover's brokerage. We anticipate that movers will vary in their retention of social relations with prior colleagues following mobility, which we term *Tie persistence*, and we propose that women movers tend to exhibit a higher proportion of persistent ties than their men counterparts do. These persistent ties to prior colleagues permit women movers to hold a brokerage position that comes about in a way that is consistent with gender stereotypes.

We measure tie persistence as the degree to which mobile individuals preserve prior network ties. Specifically, for a mover, tie persistence measures the proportion of the mover's email contacts from their prior business unit with whom they stay in touch following the move. For each month, we measure the extent to which an individual's network has changed, by comparing the current network in *Month t* with the networks in Months *t–1*, *t–2*, and *t–3*, and by calculating the ratio of persistent contacts—the ones that had at least one email exchange with the focal individual over the past three months—to total contacts.[6] Regardless of whether mobility has occurred, an employee's network involves dynamic updates. For non-movers and for movers before they move, tie persistence captures the baseline propensity to retain the contacts in one's local network over the same three-month window. The measure of tie persistence is based on changes in outgoing ties (i.e., emails sent by the focal employee). Focusing on outgoing ties permits us to capture the sender's networking behavior rather than incoming emails or reciprocal emails, over which the focal employee has less control. We present a visualization of tie persistence in Figure 1, Panel C. After mobility, both men and women retained fewer ties to prior colleagues overall, but women exhibited higher tie persistence to former colleagues than men did.

Results on gender, mobility, and tie persistence are reported in Table 7. Model 1 includes our key covariates, and Model 2 includes control variables. The negative and significant effect of the *Mover* × *Post move* diff-in-diff estimator suggests that in general, movers exhibit significant network reconstruction compared to the matched non-movers ($\beta_{Mover \times Post\ move} = -0.324$, $p < 0.001$). The three-way interaction *Women* × *Mover* × *Post move* permits estimation of a gender difference between matched groups. The significantly positive three-way interaction shows that men and women differ significantly in maintaining ties to prior colleagues after mobility ($\beta_{Women \times Mover \times Post\ move} = 0.155$, $p < 0.001$). Movers on average experience a significant post-move decrease of tie persistence. After a move, men movers' tie persistence dropped by 30.0 percentage points compared to that of men non-movers, whereas women movers' tie persistence dropped by 15.7 percentage points compared to that of women non-movers, which is 14.5 percentage points less than men movers. Models 3 and 4 in Table 7 provide consistent estimates of these effects with women and men subsamples.

We proceed to estimate the relationship between mobility, tie persistence, and brokerage and report the results in Table 8. Model 1 includes our key covariates, and Model 2 further includes control variables. As shown in Model 2, tie persistence in general negatively relates to brokerage ($\beta_{Tie\ persistence} = -0.300$, $p < 0.001$) when we control for *Network size*, consistent with prior evidence that persistent ties are associated with clusters and that bridging

[6] Our challenge was to select a time frame that was long enough to capture the majority of communication contacts but short enough to exclude inactive contacts that occurred only circumstantially. We explain the procedure as follows: to determine an appropriate time frame, for each mover, we composed a list of communication contacts (senders and/or receivers) and calculated the pairwise time intervals between emails of the mover and each of their contacts. Then we calculated *Communication intervals*, which is the average such time interval by each mover and their respective contacts. *Communication intervals* varies between 0 and 75.72 days, with a mean of 8.23 days and a standard deviation of 4.77 days. In other words, if a mover were to send or receive a second message to or from a contact, this would occur, on average, within 75.72 days of the first message. Therefore, we based our moving time window for observing tie persistence on three months (rounding up 75.72 to 3 months because other variables were monthly based), allowing us to capture all recurring email-exchanging contacts with a time frame appropriate for these data.

Table 7. Effects of Mobility and Gender on Tie Persistence*

	Tie Persistence (t)			
	(1)	(2)	(3) Women (522 MIDs)	(4) Men (285 MIDs)
Within-Matched-Group Effects				
Mover	0.005	0.024•••	0.005	0.011
	(0.008)	(0.006)	(0.004)	(0.006)
Post move	0.048•••	0.066•••	0.043•••	0.046•••
	(0.005)	(0.006)	(0.002)	(0.004)
Mover × Post move	– 0.311•••	– 0.324•••	– 0.162•••	– 0.308•••
	(0.010)	(0.008)	(0.005)	(0.008)
Cross-Group Interactions with Gender				
Women	0.073•••	0.066•••		
	(0.006)	(0.007)		
Women × Mover	– 0.015	– 0.012		
	(0.009)	(0.008)		
Women × Post move	– 0.007	– 0.010		
	(0.006)	(0.007)		
Women × Mover × Post move	0.140•••	0.155•••		
	(0.012)	(0.010)		
Within-Matched-Group Effects of Control Variables				
Growth market (binary)		0.018		
		(0.013)		
New contacts (logged)		– 0.172•••	– 0.175•••	– 0.162•••
		(0.001)	(0.001)	(0.002)
Unit size (logged)		0.053•••	0.051•••	0.055•••
		(0.003)	(0.004)	(0.006)
Average org tenure		0.001•	0.001	0.002•
		(0.000)	(0.001)	(0.001)
Average job tenure		0.002•	0.004•••	– 0.004
		(0.001)	(0.001)	(0.002)
Proportion of men		0.005	0.004	0.008
		(0.004)	(0.005)	(0.009)
Unit hierarchical depth		0.004•••	0.005•••	0.002
		(0.001)	(0.001)	(0.001)
Log average unit performance (t–4 to t–1)		– 0.001	0.001	– 0.006••
		(0.001)	(0.001)	(0.002)
Within-unit communication density		0.380•••	0.390•••	0.366•••
		(0.006)	(0.008)	(0.014)
Mean Differences Between Matched Groups				
Mover	0.025•	– 0.002		
	(0.012)	(0.010)		
Post move	– 0.164•••	– 0.145•••		
	(0.011)	(0.009)		
Constant	0.321•••	0.012		
	(0.057)	(0.081)		
Control variables	No	Yes		
Observations	60,824	60,824	42,352	18,472
Log likelihood/R^2	7,564.56	20,830.63	0.422	0.471

• $p < .05$; •• $p < .01$; ••• $p < .001$ (two-tailed tests)
* Matching ID fixed effects are included in all the models. Standard errors are clustered by matching ID. We focus on the effects within matching IDs and the interaction between gender and the variables across matched groups. Between-matched-group control effects were included in Model 2 but not reported due to space limitations. Models 3 and 4 are fixed effect models. Mean differences between matched groups are not included in the estimations. Variables that are collinear with matching ID fixed effects are excluded from Models 3 and 4.

Table 8. Effects of Mobility and Tie Persistence on Brokerage*

	Brokerage (t)			
	(1)	(2)	(3) Women (522 MIDs)	(4) Men (285 MIDs)
Within-Matched-Group Effects				
Tie persistence	– 0.341•••	– 0.300•••	– 0.331•••	– 0.209•••
	(0.010)	(0.009)	(0.010)	(0.018)
Mover	0.015•	– 0.006	– 0.009	0.004
	(0.007)	(0.006)	(0.007)	(0.012)
Post move	0.024•••	0.005	0.009	– 0.004
	(0.005)	(0.005)	(0.005)	(0.010)
Mover × Post move	0.071•••	0.086•••	0.106•••	0.055•
	(0.015)	(0.014)	(0.016)	(0.028)
Tie persistence × Mover	0.019	0.042	0.013	0.082
	(0.031)	(0.028)	(0.034)	(0.052)
Tie persistence × Post move	– 0.146•••	0.003	0.007	– 0.016
	(0.021)	(0.019)	(0.022)	(0.039)
Tie persistence × Mover × Post move	0.825•••	0.522•••	0.484••	0.480•••
	(0.062)	(0.057)	(0.070)	(0.102)
Within-Matched-Group Effects of Control Variables				
Growth market (binary)		0.130•••		
		(0.034)		
Network size (logged)		0.282•••	0.270•••	0.315•••
		(0.003)	(0.003)	(0.005)
Unit size (logged)		– 0.077•••	– 0.104•••	– 0.004
		(0.008)	(0.010)	(0.017)
Average org tenure		– 0.001	– 0.001	– 0.001
		(0.001)	(0.001)	(0.002)
Average job tenure		– 0.001	– 0.001	0.001
		(0.003)	(0.003)	(0.006)
Proportion of men employees		0.058•••	0.095•••	0.033
		(0.012)	(0.014)	(0.023)
Unit hierarchical depth		0.010•••	0.007•••	0.016•••
		(0.002)	(0.002)	(0.003)
Log average unit performance (t–4 to t–1)		0.001	0.001	0.004
		(0.002)	(0.003)	(0.005)
Within-unit communication density		– 0.632•••	– 0.695•••	– 0.434•••
		(0.017)	(0.020)	(0.036)
Mean Differences Between Matched Groups				
Women	– 0.086•••	0.009		
	(0.023)	(0.025)		
Mover	– 0.055	– 0.131••		
	(0.049)	(0.046)		
Post move	– 0.228•••	– 0.125••		
	(0.054)	(0.045)		
Constant	1.984•••	2.576•••		
	(0.264)	(0.353)		
Control variables	No	Yes	No	No
Observations	60,824	60,824	42,352	18,472
Log likelihood/R^2	– 44,372.59	– 38,847.26	0.437	0.546

• $p < .05$; •• $p < .01$; ••• $p < .001$ (two-tailed tests)
* Matching ID fixed effects are included in all the models. Standard errors are clustered by matching ID. Models 3 and 4 are fixed effect models; mean differences between matched groups are not included in the estimations. Variables that are collinear with matching ID fixed effects are excluded from Models 3 and 4.

connections are less likely to be sustained (Gulati, 1995; Kleinbaum, 2018).[7] In the same model, we examine the effect of the interaction between tie persistence and the diff-in-diff estimator, *Mover* × *Post move*, on brokerage. The significantly positive three-way interaction *Tie persistence* × *Mover* × *Post move* shows that tie persistence increases brokerage for movers only after they make intra-organizational job changes and not in any other circumstance ($\beta_{Tie\ persistence \times Mover \times Post\ move} = 0.522$, $p < 0.001$). Models 3 and 4 in Table 8 provide consistent estimations of these effects with women and men subsamples.

Persistent ties to prior coworkers, coupled with job mobility, create a situation that fosters brokerage, as women's relationships bridge their former groups with their new ones. Upon mobility, conventionally disadvantageous network characteristics provide advantages for women. Notably, these network patterns may not be a choice for women. Networks are co-constructed, and therefore, network differences indicate not only the focal individual's own preferences. Instead, several emergent processes, such as exclusion, avoidance, or (conversely) selective retention, may conspire to yield network differences along gender lines.

DISCUSSION AND CONCLUSION

In this field study of a large financial services firm, we examined gender and the evolution of social networks and performance for retail bankers. As in prior research at the intersection of gender and social networks, we found that in general—and consistent with gender-role stereotypes—women exhibited less brokerage in their social networks and received lower performance returns to that brokerage than men did. But we also uncovered a contingency under which women's network disadvantage is entirely negated: mobility. Our evidence is consistent with the licensing effects of mobility: women who move between units of the organization increase their brokerage more than do men who move, and these mobility events eliminate the gender gap in returns to brokerage. By giving women a discernable and gender-role-congruent explanation for connecting otherwise disconnected units in their organization, mobility grants women the license to broker.

Our findings, coupled with a growing body of evidence, indicate that by minimizing or alleviating the penalties associated with transgressing certain gender stereotypes, situational licenses may enable women to engage in behaviors that might otherwise be perceived as counter-stereotypical. It is therefore possible that just as mobility helps women to legitimize their networking behaviors and take advantage of brokerage positions in the setting of our study, other organizational situations might provide similar licensing opportunities for women or other stereotyped groups (e.g., Lluent, 2022). Provided that social networks are inherently relational, we believe that exploring other forms of licensing for women's organizational networks might prove a promising avenue for future research.

With regard to men, our most surprising result is that compared to women, they are more likely to overlook the value of retaining old ties, instead quickly

[7] We control for *Network size* here instead of *New contacts* because *Tie persistence* and *New contacts* correlate highly with one another ($r = -0.46$, $p < 0.05$).

re-forming their networks around the task requirements of their new roles. While this may be functional in the short term, it cedes both information and social support from trusted colleagues that may still be useful. Of course, research shows that dormant ties can be easily rekindled (Levin, Walter, and Murnighan, 2011), so the men's haste to adapt to new circumstances need not have long-lasting repercussions for them. But our results suggest that even in the short run, persistent ties to past colleagues can yield significant benefits to movers.

Our results also imply an HR policy that firms could potentially use to promote greater gender equity: more internal mobility for both men and women. Mobility does come at a cost for organizations, as it disrupts existing and potentially effective working relationships, requires the formation of new ties, leads to the decay of (some) otherwise fruitful ties, and may be disruptive to employees' personal and professional lives. But it also entails significant benefits. It encourages the formation of new social ties, leading people to diversify and refresh their networks—something that, emerging evidence suggests, is increasingly difficult to do in the post-COVID-19 world of virtual and hybrid work (Yang et al., 2021). And our results suggest that mobility can reduce or eliminate gender gaps in both network brokerage and individual returns to brokerage, through the Pareto-optimal outcome of helping women movers without hurting men movers or those who do not move. Moreover, while our research highlights that mobility offers women a pathway to realize the advantages of brokerage, we think it is crucial that the onus should not fall on women to correct organizational inequities. Of course, given the significant potential for unintended consequences of such a policy, in which the costs of mobility might outweigh its benefits, future research should employ randomized control trials before definitively concluding that mobility does (or does not) promote gender equity.

This research offers four main contributions to the literature. First, our attempt to investigate how women's and men's network dynamics differ, particularly in response to mobility, builds on and extends research that links social network and gender differences. Scholars studying gender differences increasingly acknowledge the critical role that social networks and social interactions play in organizations. In line with prior findings, women in our context tended to build embedded networks that limited opportunities to broker, whereas the men's networks were characterized by far more brokerage. Accordingly, as evidence of the benefits of brokerage in organizations mounts, women are likely to experience challenges associated with the "super strong and sticky" (Krackhardt, 1998: 21) social relationships within which they tend to embed (Brass, 1985; Ibarra, 1992). And beyond the structural patterns that can limit women's ability to broker, their own and others' perceptions that brokering is stereotypically male may also undermine women's willingness to broker (Brands and Kilduff, 2014; Brands and Mehra, 2019). By demonstrating that women are more likely than men to maintain social ties following an intra-organizational job change, our work shows an important contingency through which women may benefit from brokerage without penalty. Shining a light on women's greater propensity to retain network ties following mobility, we also add to the growing body of research that suggests alternative network arrangements by which women might find outcomes similar to or better than those of men (Ody-Brasier and Fernandez-Mateo, 2017; Yang, Chawla, and Uzzi, 2019; Obukhova and Kleinbaum, 2022).

Our work also identifies and helps to address two notable challenges in generalizing from earlier experiments (e.g., Correll, 2004; Akinola, Martin, and Phillips, 2018; Brands and Mehra, 2019) to work settings and, in doing so, offers methodological diversity that validates and extends prior research. First, in workplaces, network connections and positions may be difficult to discern or ascertain (Bernard, Killworth, and Sailer, 1982; Casciaro, 1998). That is, because determining network configurations and who is a broker is not easy, it is unclear to what extent employees' negative perceptions of brokers will affect brokers' performance. Second, while subjective evaluations of individuals are common in organizations (e.g., performance reviews, 360-degree feedback), they are only loosely coupled with objective performance (Castilla and Benard, 2010; Castilla, 2011); many employee outcomes are based on objective, independent outcomes that may not be directly affected by stereotypes. In other words, brokerage confers an informational advantage, which may persist regardless of subjective discounting for objective employee outcomes. In such instances, certain brokers may be viewed negatively but still experience improved performance from the informational advantages of brokerage. Through our empirical approach, which is based on objective email networks and sales performance, this study promotes the generalizability of prior research.

Second, our investigation contributes to the discussion of how gender-role stereotypes interact with characteristics of networks to affect performance outcomes and, more important, how organizations can combat such stereotyping. According to the expectation states theory and other gender-stereotype literature, women who are seen as violating prescribed gender roles elicit sanctions (Berger et al, 1992; Ridgeway, 2001; Eagly and Karau, 2002; Eagly, 2005). Because brokerage is stereotypically ascribed to men rather than women (Barbulescu and Bidwell, 2013; Brands et al., 2022), the research on subjective performance evaluation and leadership has documented prejudice against women as brokers in organizational networks (Brands and Kilduff, 2014; Brands, Menges, and Kilduff, 2015).[8] We extend this research by providing evidence that the penalty for women who broker persists even in objective measures of performance. More important, we show a contingent context in which women can benefit from brokerage: mobility grants women a gender-role-congruent license to occupy a brokerage position, which neutralizes the gender penalty for brokering and enables them to leverage bridging relationships to their prior colleagues. Future research, possibly including experimental examination of mechanisms, should more fully explore precisely why building brokerage positions through the combination of mobility and tie maintenance provokes less of a gender discount.

Third and related, although early research assumed away gender differences, more recently network scholars have documented substantial descriptive network differences between men and women (see Woehler et al., 2021, Brands et al., 2022, and Ertug et al., 2022 for reviews). Compared to men, women tend to have smaller (Dunbar and Spoors, 1995) and less-diverse networks (Brass,

[8] In recent, related research, Iorio (2022) found that brokers perform best when they are not perceived as brokers because those perceived to be brokers are viewed as less trustworthy. We suspect—and theories of gender stereotyping would predict—that this effect would be larger for women than for men, but Iorio does not find this effect. We leave it to future work to further explore this issue.

1985; Ibarra, 1992; Mehra, Kilduff, and Brass, 1998; Singh, Hansen, and Podolny, 2010), which tend to be weaker in providing career-related information, resources, and opportunities compared with those of their men counterparts (Ibarra, 1992; Ody-Brasier and Fernandez-Mateo, 2017). Our findings corroborate these gendered patterns in organizational networks. Despite ample evidence underscoring gendered network differences (Burt, 1992; Ibarra, 1992; Singh, Hansen, and Podolny, 2010), only recently have researchers begun to investigate how women might benefit from their networks (Ody-Brasier and Fernandez-Mateo, 2017; Yang, Chawla, and Uzzi, 2019; Obukhova and Kleinbaum, 2022). Our research identifies mobility as one condition that converts women's network disadvantage to an advantage. Put simply, the very network structures that constrain women's performance in the cross-section seem to benefit them when they move within an organization. This conclusion underscores the need for more-dynamic analyses of organizational networks.

Lastly, this study speaks to previous work on mobility and organizational social networks. A long line of research has examined the role of networks for job changes and work performance (Burt, 1992; Podolny and Baron, 1997; Mehra, Kilduff, and Brass, 1998; Singh, Hansen, and Podolny, 2010; Borgatti and Halgin, 2011; Tortoriello, Reagans, and McEvily, 2012). Heeding calls for increased focus on network dynamics (Ahuja, Soda, and Zaheer, 2011), our research examines how mobility affects networks and how these network changes are conditioned on the employee's network characteristics prior to the move. While the preponderance of network scholarship has focused on tie formation, we also contribute to the growing literature on tie decay (Burt, 2001; Dahlander and McFarland, 2013; Jonczyk et al., 2016; Kleinbaum, 2018) and substantiate findings that women are more willing to maintain distant ties than men are (Dunbar and Spoors, 1995; Roberts and Dunbar, 2015).

Another important result of our work indicates that people who retain contact with their prior colleagues, even as they build relationships with new colleagues, perform better and that women tend to retain such contacts more than men do. But enacting this strategy requires at least a temporary increase in the size of one's network. Some scholars have assumed that forming new ties requires the release of old ones in order to free up network carrying capacity (Roberts et al., 2009). Other research has explored individual differences in network size owing to heterogeneous networking strategies (Bensaou, Galunic, and Jonczyk-Sédès, 2014) or to cognitive abilities and demographic attributes, including gender (Dunbar, 2008). Our results suggest an intriguing alternative: that although people's network capacity is not infinite, neither does it have hard and fast bounds. Rather, the cognitive resources that we devote to our social networks (Smith et al., 2020) may vary over time, and after a move might be a time when we devote relatively more cognitive resources to building our new network while also maintaining at least some old relationships. The degree to which such intertemporal variation occurs might also vary across people. We leave it to future research to more fully explore the role of temporal dynamics and individual differences in network cognition and network carrying capacity.

Despite these contributions, our work entailed methodological trade-offs that present opportunities for further investigation. Although our data come from a large sample of employees from throughout the United States, which provides an array of methodological benefits, we study network dynamics following job mobility within a single organization. Focusing our empirical work within a single firm enabled us to gather a wide range of rich data and to limit confounding factors. Although we believe that the organization we studied is representative of many knowledge-intensive firms, we are cautious in generalizing our findings beyond our sample.

Our work is also suggestive but far from definitive about the role of mobility in labor markets outside of a focal organization. Labor market scholars have documented the benefits for inter-organizational movers of dropping prior ties and forming ties with current coworkers (Gargiulo and Benassi, 2000; Groysberg and Lee, 2009), hinting at the benefits associated with adaptability into new social contexts. Related, portable social relations or pre-existing social relations with future colleagues facilitate movers' transition to a new firm (Fernandez, Castilla, and Moore, 2000; Broschak, 2004; Castilla, 2005; Somaya, Williamson, and Lorinkova, 2008; Groysberg and Lee, 2009; Carnahan and Somaya, 2013). However, recent work has shown that women's attempts to learn new roles could be negatively evaluated because of gender stereotypes (Lee, Koval, and Lee, 2023). We speculate that our results would speak to the conditions under which prior ties might help with new roles. We think that our findings would likely apply to situations in which skills are transferable from one job to the next. To permit movers to benefit from brokerage positions that connect otherwise disconnected social cliques, there must be opportunities through which individuals can learn from distant knowledge and transform it into improved outcomes. We leave it for future research to definitively identify whether inter-firm mobility follows patterns similar to those of intra-firm mobility.

Our results are based on the email exchanges of the women and men in our sample rather than on their own or others' perceptions of their networks. Although this approach provides a large sample of objective, unbiased, longitudinal network data, we cannot directly speak to individuals' perceptions—either of gender-role expectations or of network position—nor can we identify how others' awareness and perceptions affect the benefits that women movers obtain. Moreover, although email exchanges provide several methodological benefits—longitudinality, temporal granularity, and comprehensiveness foremost among them—it is impossible to derive measures of relational quality from such data. Accordingly, future research should directly measure the perceptions of employees who move and the quality of their intra-organizational relationships, and further explore whether brokerage via ties to former colleagues is perceived as role congruent for women.

Finally, our theory builds on rich theorizing about the communal, relational stereotype about women that prevails in much of Western society. Although much extant research is quick to speculate that results regarding gender might also apply to race, the facile assumption that race should be similar fails to engage seriously with that theoretical foundation. Not only are stereotypes about different racial groups categorically different from the gender stereotypes upon which our theory rests, but the full implications of the intersectionality of race and gender are not well understood: stereotypes about White women differ significantly from stereotypes about Black or Asian women, for example. Empirically, our data do not show significant evidence of similar patterns in the networks and performance of members of racial minority groups, but they do suggest interesting patterns regarding intersectionality.[9] We therefore urge caution in generalizing our findings to other groups and call on future research to improve our understanding of how these dynamics affect different groups.

Although our research points to mobility as one avenue by which women gain license to connect disconnected peers and, in doing so, to benefit from structural brokerage, we decry the fact that gender stereotypes still prevail to the point that women need such license. We urge scholars to continue to explore the ways in which diverse people both build and benefit from networks differently. At the same time, we implore the world of practice to move ever faster in the direction of equity and inclusion so that women will need no such license to benefit from networks rich in brokerage in the same way that men do.

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Supplementary Material

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[9] Gender and racial minorities (specifically, Asian women, Black/African American women, and Hispanic/Latina women) who have changed jobs show less performance disruption (yet only significant at $p = 0.1$ level) than other women or men of the same races do. We have also observed larger coefficients (although not significant) of *Post move* on *Tie persistence* for these movers. We do not find support for significant changes of brokerage or returns to brokerage associated with race. Overall, we do not find significant differences across racial groups or any robust effects of intersectionality.

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License to Broker: How Mobility Eliminates Gender Gaps in Network Advantage
Online Appendix

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Robustness checks with within-person approaches. We conducted robustness checks using a within-person approach to assess the effect of mobility on individual performance. This approach allows us to control for time-invariant differences between individuals, such as unobserved variations in ability. In the within-person fixed effect model, we compared each mover with their own past records (in terms of communication networks and performance) and estimated whether the variation of communication networks (i.e., *Brokerage*) and the returns to *Brokerage* on *Individual sales performance* can be explained by *Post Move x Gender*. Across all models, we included monthly fixed effects to account for possible time-specific variations, as well as business unit variables to control for factors or regional differences that may impact performance.

In Tables A1 and A2, we report fixed-effect models with additional control variables, including the characteristics of the employee's business unit (such as size and proportion of men) and the work group's network characteristics (i.e., communication density and total level of hierarchies) to account for the temporal variation within the business units. All results are consistent with those reported in the main manuscript and support both hypotheses.

Coarsened exact matching procedure. The procedure of coarsened exact matching (CEM) is depicted in Figure A1. CEM allowed us to pair each mover in the month of moving with observationally equivalent employees that could have moved internally but did not. For every individual who moved in month *t*, we took the HR records in *month t* and chose "matched controls" who had the same job title, age, gender, years of experience at Big Bank, years of experience on the job, primary market of focus, job level, and average categorical performance in the financial quarter prior to month *t* as the mover. Hence, we were able to construct a "control sample" of matched employees who were observationally equivalent to the movers in the months of moving, but who did not move. As shown in Figure A2, the balance of the distributions of the continuous variables—i.e., age, years of experience at Big Bank, years of experience on the job—between movers and non-movers improves significantly after matching.

Alternative mover-control sample with all movers included. As an alternative to the coarsened exact matching analysis, in which we matched movers and non-movers based on some continuous variables, we adopted an "exact matching" based only on categorical variables (i.e., gender, market of focus, job level, categorical performance rating). In this way, most mover-months (N = 1,076) were matched with non-mover-month observations (N = 10,863). Individual characteristics, such as age and job experience, were controlled instead of being matched. Using this sample and controlling for these additional variables, we replicated the analyses and report the results in Tables A3, A4, and A5; the results support Hypotheses 1 and 2, consistent with the results reported in the main analysis.

Subsample analyses focusing on business units that were closed. As mentioned in the main manuscript, it is unlikely that networks could be created with the intention of influencing mobility and performance. However, to further explore this issue, we considered a subsample of employees who experienced involuntary job mobility due to external factors. Specifically, we focused on employees who changed jobs internally following the closure of their prior business units. This analysis required a greatly reduced sample size (N = 127 moved employees, plus N = 793 control observations, identified through the same coarsened exact matching procedure described above). These employees were moved due to external forces, which allows us to examine the effect of network characteristics on mobility and performance in a context where the creation of networks was unlikely to be strategically motivated.

Upon closure of business units, employees had the option to either move within Big Bank or leave the organization, based on their personal preferences. We confirmed with HR executives that these choices were voluntary. Using the subsample of employees who moved within Big Bank due to closed business units, we conducted the same set of analyses to test our main hypotheses H1 and H2. The four-way interaction in Table A8 Models (1) and (2) are only significant at $p = 0.08$. Other findings remained robust. The results are reported in Tables A6, A7, and A8.

Exploring the gender-brokerage relationship as "time since move" increases. We report the analyses on *Brokerage* over time in Table A9, with the variable "*Time since move*".

We calculate "*Time since move*" by counting the number of months that went by since the job changes occur. *Post Move* focuses on modelling the before-after comparisons whereas *Time since move* focuses on modelling how the initial effects of mobility changes over time.

As are shown in Table A9, the effect of *Gender* and *Mobility* remain robust after including interactions with *Time Since Move*. As is in Model 2, consistent with our theory on the licensing effect associated with mobility, we found that women who moved had a greater increase in brokerage compared to men after they moved ($\beta_{Mover \times Post\ move \times Women}$= 0.106, $p < 0.01$). Although this increase decreases slightly over time as women movers become more embedded, the positive effect on their networks persists for a relatively long period ($\beta_{Mover \times Post\ move \times Women}$= −0.007, $p < 0.05$). Further research is needed to fully understand the dynamics of individual differences in sustaining brokerage positions.

Exploring gender and internal moves at Big Bank. We examine the role of gender in determining mobility within Big Bank and report the results in Table A10. In this analysis, we aim to explore whether women and men exhibit different patterns of mobility within our specific empirical setting.

In this set of analyses, we include all the employees in the retail sales department at Big Bank in our sample. With this all-employee sample, we run multi-level logistic regressions to estimate the effect of individual network characteristics on the individual's probability of *Internal move* (which equals 1 if the employee left the current working business unit and moved to a new business unit within Big Bank, and is otherwise 0 for the employees who remained in current positions), *Promotion* (which equals 1 if the employee's formal rank at Big Bank increased and is otherwise 0 for the employees who remained in current positions), *Attrition* (which equals 1 when the focal employee left Big Bank and is otherwise 0 for the employees who remained in current positions), *Same-city move* (which equals 1 if the employee left the current working business unit and moved to a new business unit within Big Bank and in the same city, and is otherwise 0 for the employees who remained in current positions) and *Same-state move* (which equals 1 if the employee left the current working business unit and moved to a new business unit within Big Bank and in the same state, and is otherwise 0 for the employees who remained in current positions). The main independent variable of focus in this set of analyses is the *Gender* of the individuals. Notably, the variables coding various types of job changes are not mutually exclusive. We separate *Same-city (state) move* and *Internal move* to estimate if there is any gender differences across city boundaries.

The results are reported in Table A10. Consistent with the numbers we observe in our main sample, men are more likely to make job changes within the same organization and between organizations. Notably, gender does not significantly affect a focal employee's likelihood of getting promoted, moving within same city or state.

Alternative measure of *Brokerage*. Workplace email communication is complex in that it encapsulates both formal and informal communication networks. To capture this complexity, we have adjusted the level of "formal communication" considered when constructing our organizational networks for our Brokerage measures. Networks that include all emails largely mirror formal organizational structures like departments and locations, while also incorporating the informal interactions occurring within these formal boundaries (Tushman and Nadler, 1978; Kleinbaum, Stuart, and Tushman, 2013). In this analysis, we aimed to uncover "informal networks" that are not defined by formal task requirements. To identify such "informal networks," we excluded all emails between supervisors and subordinates, as well as between subordinates who reported to the same supervisor, which we assume here to be induced by the formal organizational structure. The remaining connections likely represent informal social ties, shared interests, or other unofficial relationships that operate independently of formal organizational guidelines. This approach provides us with an approximation of the informal networks in operation.

As an alternative approach, we calculate *Brokerage* based on these "informal networks." The two measures are highly correlated (r = 0.81). The effects of gender and mobility on this construct are reported in Tables A11 and A12.

Robustness checks controlling for network size. *Network size* in each month counts the total number of email recipients in the individual's network during that calendar month; this variable helps us to account for an employee's overall communication activity. All results reported in Tables 4-6 are repeated controlling for *Network size* instead of *New contacts*. The models are reported in Tables A13, A14, and A15.

Exploring total number of new contacts. *New contacts* is the count of contacts in the network of that month who were not contacted by the focal sender in the prior three months. We expect to observe gender differences in *tie persistence* rather than *new contacts*.

Results on *gender*, *mobility*, and *new contacts* are reported in Table A16. Similar with Table 7, the positive and significant effect of the *Mover* $\times$ *Post move* diff-in-diff estimator suggests that in general, movers exhibit significant network reconstruction than the matched non-movers ($\beta_{Mover \times Post\ move}$= 0.066, p = 0.02). The three-way interaction *Women* $\times$ *Mover* $\times$ *Post move*, permits estimation of a gender difference between the matched groups. The insignificantly three-way interaction shows men and women do not differ significantly in building "new" contacts after mobility ($\beta_{Women \times Mover \times Post\ move}$ = 0.057, p = 0.15).

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Table A1: Within-Mover Analyses: Effects of Mobility and Gender on Brokerage

DV:	*Brokerage*				
	(1)	(2)	(3)	(4)	(5)
Post move	0.168***	0.132***	0.104***	−0.178***	−0.162***
	(0.011)	(0.020)	(0.019)	(0.029)	(0.028)
Post move × Women		0.053*	0.049*	0.023	0.015
		(0.024)	(0.023)	(0.016)	(0.017)
Network size (logged)			0.235***		0.224***
			(0.007)		(0.007)
Unit size (logged)			−0.013		−0.008
			(0.023)		(0.023)
Average org tenure			0.009***		0.010***
			(0.003)		(0.003)
Average job tenure			−0.021*		−0.023**
			(0.008)		(0.008)
Proportion of men			0.100**		0.094*
			(0.037)		(0.037)
Unit hierarchical depth			−0.004		−0.003
			(0.002)		(0.002)
Average unit performance (logged, prior quarter)			−0.019*		−0.021**
			(0.008)		(0.008)
Within-unit communication Density			−0.263***		−0.234***
			(0.044)		(0.044)
Tie persistence				−0.601***	−0.482***
				(0.038)	(0.036)
Post move × Tie persistence				0.558***	0.502***
				(0.047)	(0.044)
Observations	11,876	11,876	11,876	11,876	11,876
R^2	0.059	0.076	0.153	0.099	0.172

*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$ (two-tailed tests)
All models include mover and month fixed effects.

Table A2: Within-Mover Analyses: Effects of Mobility and Gendered Return of Brokerage

	Individual Sales Performance (t+1)				
	(1)	(2)	(3)	(4)	(5)
Post move	−0.285***	−0.367***	−0.122***	−0.158***	−0.104***
	(0.035)	(0.035)	(0.033)	(0.032)	(0.031)
Brokerage	0.299***	0.326***	0.338**	0.435***	0.488**
	(0.059)	(0.061)	(0.124)	(0.121)	(0.110)
Women					0.119
					(0.134)
Post move × Women	0.148	0.172	0.136	0.152	0.172
	(0.112)	(0.117)	(0.114)	(0.144)	(0.139)
Post move × Brokerage	0.074	0.227**	0.164	0.160	0.142
	(0.076)	(0.084)	(0.129)	(0.126)	(0.124)
Women × Brokerage	−0.217*	−0.453**	−0.557***	−0.579***	−0.489***
	(0.106)	(0.145)	(0.145)	(0.142)	(0.129)
Post move × Brokerage × Women		0.318*	0.598***	0.660***	0.653***
		(0.154)	(0.155)	(0.151)	(0.149)
Post move ×			0.016	0.044	0.098
New contacts (logged)			(0.066)	(0.065)	(0.065)
Job level change				−0.527***	−0.529***
				(0.043)	(0.071)
Working group change				−0.335***	−0.387***
				(0.059)	(0.061)
New contacts (logged)			0.127*	0.119*	0.202***
			(0.056)	(0.055)	(0.055)
Unit size (logged)				−0.205*	−0.295*
				(0.103)	(0.099)
Average org tenure				0.011	0.016
				(0.009)	(0.011)
Average job tenure				0.023	0.046
				(0.026)	(0.033)
Proportion of men				0.131	0.129
				(0.174)	(0.154)
Unit hierarchical depth				−0.008	−0.012
				(0.019)	(0.017)
Average unit performance				0.069*	0.014
(logged, prior quarter)				(0.038)	(0.033)
Within-unit communication density				−0.500***	−0.536***
				(0.106)	(0.100)
Constant					6.311***
					(0.498)
Observations	11,876	11,876	11,876	11,876	11,876
R^2	0.135	0.137	0.166	0.181	0.186
Mover Fixed Effects	Yes	Yes	Yes	Yes	No

*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$ (two-tailed tests).

All models include month fixed effects.

Table A3: Effects of Mobility and Gender on Brokerage (Alternative Matching Sample)

	Brokerage (t)					
	(1)	(2)	(3) Before Move	(4) After Move	(5) Women (629 MIDs)	(6) Men (365 MIDs)
Within-Matched-Group Effects						
Mover	0.033*	0.023	0.029	−0.012	0.004	0.002
	(0.016)	(0.014)	(0.015)	(0.012)	(0.006)	(0.010)
Post move	0.001	0.012			0.005	0.012
	(0.010)	(0.009)			(0.004)	(0.007)
Mover × Post move	−0.040	−0.033			0.032*	−0.011
	(0.020)	(0.018)			(0.013)	(0.020)
Cross-Group Interactions with Gender						
Women	−0.163***	−0.072***	−0.080***	−0.091***		
	(0.015)	(0.018)	(0.018)	(0.019)		
Women × mover	−0.019	−0.027	−0.031	0.034*		
	(0.019)	(0.017)	(0.017)	(0.014)		
Women × Post move	−0.012	−0.009				
	(0.012)	(0.011)				
Women × Mover × Post move	0.064**	0.063**				
	(0.025)	(0.022)				
Within-Matched-Group Effects of Control Variables						
Age		0.001**	0.001***	0.001**		
		(0.000)	(0.000)	(0.000)		
Org tenure		0.001*	0.002*	0.000		
		(0.000)	(0.000)	(0.000)		
Job tenure		0.053***	0.056***	0.049***		
		(0.002)	(0.003)	(0.003)		
Growth market (binary)		0.179**	0.166***	0.257***		
		(0.030)	(0.034)	(0.074)		
New contacts (logged)		0.301***	0.302***	0.298***	0.300***	0.302***
		(0.002)	(0.003)	(0.003)	(0.002)	(0.002)
Unit size (logged)		−0.177***	−0.201***	−0.153***	−0.202**	−0.116***
		(0.007)	(0.009)	(0.010)	(0.008)	(0.013)
Average org tenure		0.002**	0.004***	−0.000	0.002*	−0.003
		(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Average job tenure		−0.014***	−0.020***	−0.009**	−0.013***	−0.014**
		(0.002)	(0.003)	(0.003)	(0.002)	(0.005)
Proportion of men		0.090***	0.110***	0.066***	0.096***	0.121***
		(0.008)	(0.012)	(0.013)	(0.010)	(0.017)
Unit hierarchical depth		0.022***	0.025***	0.019***	0.019***	0.031***
		(0.001)	(0.002)	(0.002)	(0.001)	(0.002)
Log average unit performance		−0.002	−0.002	−0.003	−0.001	−0.007*
(t-4 to t-1)		(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
Within-unit communication		−0.664***	−0.696***	−0.632***	−0.758***	−0.388***
density		(0.013)	(0.018)	(0.020)	(0.015)	(0.027)
Mean Differences between Matched Groups						
Mover	−0.200***	−0.120**	−0.120**	−0.106**		
	(0.054)	(0.044)	(0.046)	(0.034)		
Post move	0.044	−0.030				
	(0.062)	(0.048)				
Constant	1.923***	2.199***	2.308***	2.466***		
	(0.291)	(0.510)	(0.427)	(0.374)		
Control variables	No	Yes	Yes	Yes	No	No
Observations	109,127	109,127	59,357	49,770	81,978	27,149
Log Likelihood/R^2	−83,594.47	−70,467.06	−38,453.37	−32,286.23	0.422	0.470

*p < 0.05; **p < 0.01; ***p < 0.001 (two-tailed tests).
Matching ID fixed effects are included in all the models. Standard errors are clustered by Matching iD.

Table A4: Effects of Mobility and Gendered Return of Brokerage (Alternative Matching Sample)

	Individual Sales Performance (t+1)			
	Before Move		After Move	
	(1)	(2)	(3)	(4)
Within-Matched-Group Effects				
Mover	0.227***	0.112	−0.376***	−0.155**
	(0.066)	(0.061)	(0.053)	(0.051)
Brokerage	0.952***	0.598***	1.005***	0.742***
	(0.031)	(0.030)	(0.035)	(0.035)
Mover × Brokerage	0.067	0.206	−0.102	−0.006
	(0.118)	(0.110)	(0.115)	(0.110)
Cross-Group Interactions with Gender				
Women	0.192***	−0.252***	0.144**	−0.131
	(0.048)	(0.067)	(0.054)	(0.071)
Women × Mover	−0.066	0.006	−0.011	0.044
	(0.077)	(0.074)	(0.065)	(0.063)
Women × Brokerage	−0.499***	−0.437***	−0.597***	−0.590***
	(0.036)	(0.035)	(0.041)	(0.040)
Women × Mover × Brokerage	−0.223	−0.265	0.599***	0.583***
	(0.147)	(0.137)	(0.139)	(0.133)
Within-Matched-Group Effects of Control Variables				
Age		0.003**		0.003***
		(0.001)		(0.001)
Org tenure		0.026***		0.026***
		(0.002)		(0.002)
Job tenure		0.593***		0.448***
		(0.012)		(0.013)
Growth market (binary)		−0.001		−0.059
		(0.143)		(0.320)
New contacts (logged)		0.272***		0.224***
		(0.012)		(0.013)
Unit size (logged)		−0.240***		−0.540***
		(0.040)		(0.041)
Average org tenure		0.010**		0.006
		(0.004)		(0.004)
Average job tenure		0.018		0.055***
		(0.012)		(0.013)
Proportion of men		−0.092		−0.040
		(0.049)		(0.054)
Unit hierarchical depth		0.104***		0.136***
		(0.007)		(0.007)
Log average unit performance		−0.031***		−0.027**
(t-4 to t-1)		(0.009)		(0.010)
Within-unit communication density		0.924***		0.124
		(0.077)		(0.087)
Mean Differences between Matched Groups				
Mover	0.242	−0.003	−1.203***	−1.150***
	(0.179)	(0.160)	(0.131)	(0.127)
Brokerage	0.476***	0.181	0.544***	0.366**
	(0.102)	(0.115)	(0.108)	(0.119)
Constant	9.213***	9.406**	8.184***	9.455***
	(0.540)	(1.582)	(0.427)	(1.442)
Control variables	No	Yes	No	Yes
Observations	59,357	59,357	49,770	49,770
Log Likelihood	−126,161.40	−123,341.37	−106,566.67	−104,820.78

*p < 0.05; **p < 0.01; ***p < 0.001 (two-tailed tests).
Matching ID fixed effects are included in all the models. Standard errors are clustered by Matching ID.

Table A5: Effects of Mobility and Gender on Brokerage (Alternative Matching Sample, Continued)

	Individual Sales Performance (t+1)			
	(1) Full Interaction Model	(2) Interaction Model with Controls	(3) Women (522 MIDs)	(4) Men (285 MIDs)
Within-Matched-Group Effects				
Mover	−0.107*	−0.023	−0.041	−0.028
	(0.042)	(0.040)	(0.027)	(0.045)
Post move	0.039	0.056	0.013	−0.043
	(0.063)	(0.058)	(0.016)	(0.032)
Brokerage	0.978***	0.667***	0.158***	0.699***
	(0.023)	(0.023)	(0.015)	(0.028)
Mover × Post move	−0.592***	−0.244**	−0.285***	−0.345***
	(0.088)	(0.085)	(0.056)	(0.094)
Mover × Brokerage	0.049	0.155*	0.282***	0.124
	(0.077)	(0.074)	(0.054)	(0.082)
Post move × Brokerage	−0.028	−0.003	−0.047	−0.026
	(0.051)	(0.049)	(0.029)	(0.054)
Mover × Post move × Brokerage	0.100	0.125	0.652**	0.210
	(0.155)	(0.148)	(0.108)	(0.159)
Cross-Group Interactions with Gender				
Women	0.125***	−0.154**		
	(0.036)	(0.051)		
Women × Mover	−0.020	0.031		
	(0.050)	(0.048)		
Women × Post move	−0.076	−0.116		
	(0.077)	(0.071)		
Women × Brokerage	−0.545***	−0.510***		
	(0.027)	(0.026)		
Women × Mover × Post move	0.062	0.051		
	(0.106)	(0.100)		
Women × Mover × Brokerage	0.115	0.098		
	(0.096)	(0.092)		
Women × Post move × Brokerage	−0.046	−0.048		
	(0.059)	(0.057)		
Women × Mover × Post move × Brokerage	0.583**	0.540**		
	(0.195)	(0.187)		
Mean Differences between Matched Groups				
Mover	−0.222	−0.403**		
	(0.140)	(0.132)		
Post move	0.409**	0.298*		
	(0.154)	(0.139)		
Brokerage	0.385***	0.084		
	(0.078)	(0.092)		
Constant	12.410	11.003***		
	(0.736)	(1.511)		
Control Variables (within Matched Groups)	No	Yes	Yes	Yes
Control Variables (between Matched Groups)	No	Yes	No	No
Observations	109,127	109,127	81,978	27,149
Log Likelihood/R^2	−232,997.01	−228,489.71	0.261	0.290

*p < 0.05; **p < 0.01; ***p < 0.001 (two-tailed tests).
Matching ID fixed effects are included in all the models. Standard errors are clustered by Matching ID.

Table A6: Effects of Mobility and Gender on Brokerage (Closed Business Units)

	Brokerage (t)					
	(1)	(2)	(3) Before Move	(4) After Move	(5) Women (85 MIDs)	(6) Men (32 MIDs)
Within-Matched-Group Effects						
Mover	0.059	0.022	−0.025	0.007	0.052**	−0.014
	(0.054)	(0.049)	(0.052)	(0.039)	(0.017)	(0.034)
Post Move	0.025	−0.001			−0.013	−0.025
	(0.044)	(0.039)			(0.013)	(0.029)
Mover × Post move	−0.061	−0.038			0.155***	0.005
	(0.067)	(0.061)			(0.037)	(0.077)
Cross-Group Interactions with Gender						
Women	−0.056	0.071	0.023	−0.005		
	(0.053)	(0.063)	(0.077)	(0.056)		
Women × Mover	−0.061	−0.057	−0.030	0.134**		
	(0.060)	(0.055)	(0.057)	(0.044)		
Women × Post move	−0.062	0.028				
	(0.048)	(0.045)				
Women × Mover × Post move	0.245**	0.199**				
	(0.077)	(0.069)				
Within-Matched-Group Effects of Control Variables						
Growth market (binary)		0.045	0.039	0.065		
		(0.043)	(0.053)	(0.047)		
New contacts (logged)		0.307***	0.315***	0.294***	0.310***	0.294***
		(0.006)	(0.109)	(0.009)	(0.007)	(0.014)
Unit size (logged)		−0.149***	−0.206***	−0.081**	−0.126***	−0.261***
		(0.022)	(0.031)	(0.031)	(0.024)	(0.054)
Average org tenure		0.004*	0.004	0.005	0.005	0.023***
		(0.002)	(0.003)	(0.003)	(0.002)	(0.006)
Average job tenure		−0.013*	−0.012	−0.009	−0.011	−0.046**
		(0.005)	(0.007)	(0.008)	(0.006)	(0.016)
Proportion of men		0.080**	0.069	0.101*	0.092**	0.024
		(0.031)	(0.041)	(0.046)	(0.034)	(0.069)
Unit hierarchical depth		0.016***	0.025***	0.001	0.009*	0.040***
		(0.004)	(0.005)	(0.006)	(0.004)	(0.009)
Log average unit performance		0.003	−0.006	0.015+	0.014*	−0.067***
(t-4 to t-1)		(0.006)	(0.008)	(0.008)	(0.006)	(0.017)
Within-unit communication		−0.640***	−0.702***	−0.568***	−0.710***	−0.422***
Density		(0.042)	(0.058)	(0.064)	(0.048)	(0.091)
Mean Differences between Matched Groups						
Mover	−0.126	−0.146	0.074	−0.193		
	(0.122)	(0.102)	(0.097)	(0.104)		
Post move	0.037	−0.067				
	(0.112)	(0.093)				
Constant	2.532***	2.019*	1.674	2.282***		
	(0.676)	(0.942)	(0.956)	(0.656)		
Control variables	No	Yes	Yes	Yes	No	No
Observations	9,787	9,787	5,408	4,379	7,994	1,793
Log Likelihood/R^2	−7,140.02	−6,100.17	−3,409.07	−2,742.71	0.420	0.401

+ p < 0.1; *p < 0.05; **p < 0.01; ***p < 0.001 (two-tailed tests).
Matching ID fixed effects are included in all the models. Standard errors are clustered by Matching ID.

Table A7: Effects of Mobility and Gendered Return to Brokerage (Closed Business Units)

	Individual Sales Performance (t+1)			
	Before Move		After Move	
	(1)	(2)	(3)	(4)
Within-Matched-Group Effects				
Mover	0.397	0.378	0.012	0.014
	(0.203)	(0.205)	(0.140)	(0.140)
Brokerage	0.700***	0.572**	0.760***	0.691***
	(0.113)	(0.116)	(0.104)	(0.107)
Mover × Brokerage	0.469	0.477	−0.424	−0.405
	(0.459)	(0.458)	(0.315)	(0.316)
Cross-Group Interactions with Gender				
Women	0.477*	0.326	0.106	−0.189
	(0.239)	(0.309)	(0.194)	(0.218)
Women × Mover	−0.346	−0.335	0.168	0.162
	(0.227)	(0.227)	(0.161)	(0.161)
Women × Brokerage	−0.633***	−0.580***	−0.549***	−0.530***
	(0.125)	(0.125)	(0.118)	(0.118)
Women × Mover × Brokerage	−0.587	−0.584	0.835*	0.809*
	(0.497)	(0.500)	(0.358)	(0.357)
Within-Matched-Group Effects of Control Variables				
Growth market (binary)		0.087		0.472*
		(0.259)		(0.209)
New contacts (logged)		0.131***		0.103**
		(0.038)		(0.037)
Unit size (logged)		−0.103		0.190
		(0.120)		(0.106)
Average org tenure		0.011		0.011
		(0.010)		(0.010)
Average job tenure		0.038		0.033
		(0.028)		(0.030)
Proportion of men		−0.013		−0.215
		(0.156)		(0.161)
Unit hierarchical depth		0.056**		−0.007
		(0.021)		(0.021)
Log average unit performance		−0.044		−0.036
(t-4 to t-1)		(0.032)		(0.029)
Within-unit communication density		0.502*		0.646**
		(0.216)		(0.220)
Mean Differences between Matched Groups				
Mover	0.331	0.423	−1.672***	−1.464**
	(0.437)	(0.436)	(0.430)	(0.447)
Brokerage	1.497***	1.288**	1.226***	1.082*
	(0.422)	(0.478)	(0.350)	(0.432)
Constant	4.004**	−7.082	5.160***	−2.022
	(1.937)	(4.592)	(1.343)	(4.032)
Control variables	No	Yes	No	Yes
Observations	5,408	5,408	4,379	4,379
Log Likelihood	−10,642.62	−10,630.30	−8,257.05	−8,246.56

*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$ (two-tailed tests)

Matching ID fixed effects are included in all the models. Standard errors are clustered by Matching ID.

Table A8: Effects of Mobility and Gendered Return to Brokerage (Closed Business Units, Continued)

	Individual Sales Performance (t+1)			
	(1) Full Interaction Model	(2) Interaction Model with Controls	(3) Women (522 MIDs)	(4) Men (285 MIDs)
Within-Matched-Group Effects				
Mover	0.160	0.153	0.045	0.196
	(0.121)	(0.122)	(0.062)	(0.151)
Post move	0.365	0.356	0.032	−0.190
	(0.196)	(0.194)	(0.048)	(0.127)
Brokerage	0.756***	0.654***	0.098*	0.676***
	(0.078)	(0.080)	(0.041)	(0.107)
Mover × Post move	−0.049	−0.066	−0.252	−0.461
	(0.282)	(0.281)	(0.135)	(0.343)
Mover × Brokerage	−0.105	−0.072	0.120	−0.143
	(0.271)	(0.270)	(0.117)	(0.332)
Post move × Brokerage	−0.128	−0.085	0.099	−0.149
	(0.170)	(0.169)	(0.081)	(0.208)
Mover × Post move × Brokerage	−0.505	−0.498	0.616*	−0.055
	(0.569)	(0.568)	(0.242)	(0.684)
Cross-Group Interactions with Gender				
Women	0.247	0.302		
	(0.163)	(0.214)		
Women × Mover	−0.048	−0.049		
	(0.137)	(0.137)		
Women × Post move	−0.302	−0.288		
	(0.223)	(0.221)		
Women × Brokerage	−0.615***	−0.574***		
	(0.087)	(0.087)		
Women × Mover × Post move	−0.063	−0.051		
	(0.315)	(0.314)		
Women × Mover × Brokerage	0.196	0.174		
	(0.297)	(0.297)		
Women × Post move × Brokerage	0.206	0.179		
	(0.189)	(0.189)		
Women × Mover × Post move × Brokerage	1.094+	1.077+		
	(0.626)	(0.625)		
Mean Differences between Matched Groups				
Mover	−0.637	−0.535		
	(0.398)	(0.363)		
Post move	−1.434***	−1.550***		
	(0.364)	(0.333)		
Brokerage	1.314***	0.878*		
	(0.315)	(0.360)		
Constant	5.437*	−1.738		
	(2.363)	(3.466)		
Control variables (within Matched Groups)	No	Yes	Yes	Yes
Control variables (between Matched Groups)	No	Yes	No	No
Observations	9,787	9,787	7,994	1,763
Log Likelihood/R^2	−18,951.39	−18,918.24	0.361	0.360

+ $p < 0.1$; *$p < 0.05$; **$p < 0.01$; ***$p < 0.001$ (two-tailed tests).
Matching ID fixed effects are included in all the models. Standard errors are clustered by Matching ID.

Table A9: Effects of Mobility and Gender on Brokerage, Accounting for Time Since Move

	Brokerage (t)					
	(1)	(2)	(3) Before Move	(4) After Move	(5) Women (522 MIDs)	(6) Men (285 MIDs)
Within-Matched-Group Effects						
Mover	−0.001	0.002	0.001	0.013	0.013	0.016
	(0.018)	(0.017)	(0.017)	(0.025)	(0.007)	(0.011)
Post move	0.030*	0.023			0.017*	0.021
	(0.015)	(0.014)			(0.007)	(0.013)
Time since move	−0.006	0.003		0.003	−0.004	−0.003
	(0.007)	(0.006)		(0.006)	(0.005)	(0.007)
Mover × Post move	0.066*	0.008			0.130***	0.011
	(0.033)	(0.030)			(0.022)	(0.034)
Mover × Time since move	−0.006	−0.006		−0.004	−0.010*	−0.002
	(0.007)	(0.006)		(0.006)	(0.005)	(0.007)
Cross-Group Interactions with Gender						
Women	−0.146***	−0.056*	−0.104***	−0.065*		
	(0.019)	(0.023)	(0.023)	(0.026)		
Women × Mover	−0.027	−0.039	−0.029	0.070*		
	(0.022)	(0.020)	(0.020)	(0.031)		
Women × Post move	−0.023	−0.011				
	(0.017)	(0.015)				
Women × Time since move	0.003	0.003		0.001		
	(0.004)	(0.004)		(0.004)		
Women × Mover × Post move	0.119**	0.106**				
	(0.049)	(0.036)				
Women × Mover	−0.009*	−0.007*		−0.009*		
× Time since move	(0.004)	(0.003)		(0.005)		
Within-Matched-Group Effects of Control Variables						
Growth market (binary)		0.118**	0.152***	0.099		
		(0.034)	(0.042)	(0.077)		
New contacts (logged)		0.287***	0.294***	0.277***	0.281***	0.305***
		(0.003)	(0.004)	(0.004)	(0.003)	(0.005)
Unit size (logged)		−0.062***	−0.112***	−0.023	−0.104**	−0.006
		(0.008)	(0.013)	(0.012)	(0.010)	(0.017)
Average org tenure		−0.001	0.002	−0.002	−0.001	0.001
		(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Average job tenure		−0.001	−0.002	0.004	−0.001	0.001
		(0.003)	(0.004)	(0.004)	(0.003)	(0.006)
Proportion of men		0.077***	0.071•••	0.077***	0.110***	0.058*
		(0.012)	(0.016)	(0.017)	(0.014)	(0.023)
Unit hierarchical depth		0.010***	0.016•••	0.003	0.007***	0.014***
		(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
Log average unit performance		0.001	−0.001	0.001	0.001	0.001
(t-4 to t-1)		(0.002)	(0.003)	(0.003)	(0.003)	(0.005)
Within-unit communication		−0.534***	−0.582***	−0.470***	−0.622***	−0.264***
Density		(0.017)	(0.024)	(0.025)	(0.020)	(0.036)
Mean Differences between Matched Groups						
Mover	−0.033	−0.044	−0.028	−0.108**		
	(0.050)	(0.041)	(0.038)	(0.040)		
Post move	−0.217	−0.261				
	(0.190)	(0.155)				
Constant	1.792***	1.555***	1.949***	2.116***		
	(0.264)	(0.349)	(0.271)	(0.319)		
Control variables	No	Yes	Yes	Yes	No	No
Observations	60,824	60,824	31,513	29,311	42,352	18,472
Log Likelihood/R^2	−45,028.25	−38,693.91	−19,916.78	−18,883.76	0.440	0.540

*p < 0.05; **p < 0.01; ***p < 0.001 (two-tailed tests). Matching ID fixed effects are included in all the model

Table A10: Modeling the Antecedents of Intra-organizational Mobility

	Internal Mobility (t+1)	Promotion (t+1)	Moving Same City (t+1)	Moving Same State (t+1)	Attrition (t+1)
	(1)	(2)	(3)	(4)	(5)
Women	−0.140*	0.026	−0.059	−0.094	−0.091*
	(0.063)	(0.023)	(0.070)	(0.162)	(0.039)
Age	−0.014**	−0.001	−0.012***	−0.015	−0.017**
	(0.003)	(0.001)	(0.003)	(0.008)	(0.002)
Org tenure	−0.029***	−0.015***	−0.033***	−0.057*	−0.058***
	(0.007)	(0.002)	(0.008)	(0.022)	(0.006)
Job tenure	0.066*	0.084***	0.066*	0.190**	0.131***
	(0.026)	(0.012)	(0.028)	(0.070)	(0.020)
Related prior job	−0.158*	0.828***	−0.219***	−0.012	−0.060
	(0.071)	(0.024)	(0.073)	(0.172)	(0.042)
Network size (logged)	0.240***	−0.495***	0.092*	−0.372**	−0.286***
	(0.045)	(0.013)	(0.045)	(0.094)	(0.023)
Brokerage	0.172**	−0.010	0.243***	0.337*	−0.059
	(0.058)	(0.021)	(0.061)	(0.144)	(0.037)
Growth market	0.016	0.007	−0.014	0.090	0.038
	(0.065)	(0.024)	(0.072)	(0.161)	(0.040)
Unit size (logged)	−0.234*	1.216***	0.301*	−0.365	−0.022
	(0.114)	(0.036)	(0.132)	(0.304)	(0.072)
Average org tenure	0.016	0.002	0.020	−0.001	−0.009
	(0.012)	(0.004)	(0.013)	(0.032)	(0.008)
Average job tenure	0.047	0.045***	0.031	−0.051	−0.061*
	(0.034)	(0.013)	(0.039)	(0.097)	(0.024)
Proportion of men	0.105	0.170**	0.113	−0.155	−0.047
	(0.159)	(0.058)	(0.176)	(0.407)	(0.098)
Unit hierarchical depth	0.011	−0.258***	0.001	0.081	−0.003
	(0.020)	(0.007)	(0.022)	(0.050)	(0.012)
Log average unit performance (t-4 to t-1)	0.068*	0.068*	0.084*	0.164	−0.011
	(0.034)	(0.034)	(0.038)	(0.093)	(0.007)
Within-unit communication density	−0.068	0.271***	−0.074	0.151	0.019
	(0.260)	(0.046)	(0.143)	(0.327)	(0.080)
Constant	−5.938***	−0.699***	−5.632***	−7.450***	−1.916***
	(0.538)	(0.015)	(0.553)	(1.338)	(0.291)
Observations	109,234	109,234	109,234	109,234	109,234
Log Likelihood	−7,404.09	−36,623.75	−6,277.49	−1,492.80	−15,567.41

*p<0.05; **p<0.01; ***p<0.001 (two-tailed tests)
All models include job and month fixed effects.
All models include business unit and individual nested in business-unit random intercepts.

Table A11: Effects of Gender on Network Brokerage (an Alternative Measure)

	Brokerage (t)				
	(1)	(2)	(3)	(4)	(5)
Women	−0.151***	−0.095***	−0.117***	−0.089***	−0.127***
	(0.011)	(0.009)	(0.009)	(0.009)	(0.008)
New contacts (logged)		0.296***	0.293***	0.299***	0.296***
		(0.002)	(0.002)	(0.002)	(0.002)
Age (years, logged)			0.141***		0.125***
			(0.015)		(0.014)
Org tenure (years)			0.007***		0.009**
			(0.001)		(0.001)
Job tenure (years)			0.051***		0.052***
			(0.003)		(0.003)
Growth market (binary)				0.144***	0.139***
				(0.009)	(0.009)
Unit size (logged)				−0.023***	−0.025**
				(0.008)	(0.008)
Average org tenure				0.001	−0.006***
				(0.001)	(0.001)
Average job tenure				−0.006	−0.011**
				(0.003)	(0.003)
Proportion of men				0.077***	0.083***
				(0.014)	(0.014)
Unit hierarchical depth				0.006**	0.006***
				(0.001)	(0.001)
Log average unit performance (t-4 to t-1)				−0.001	−0.001
				(0.001)	(0.001)
Within-unit communication density				−0.243***	−0.240***
				(0.015)	(0.015)
Constant	2.426***	2.186***	1.655*	2.223***	1.778*
	(0.013)	(0.804)	(0.065)	(0.064)	(0.818)
Observations	121,457	121,457	121,457	121,457	121,457
Log Likelihood	−76,011.49	−71,940.52	−71,516.20	−71,670.29	−71,193.71

**p*<0.05; ***p*<0.01; ****p*<0.001 (two-tailed tests).
All models include job and month fixed effects.
All models include business unit and individual nested in business-unit random intercepts.

Table A12: Effects of Mobility and Gender on Brokerage (an Alternative Measure)

	Brokerage (t)					
	(1)	(2)	(3) Before Move	(4) After Move	(5) Women (522 MIDs)	(6) Men (285 MIDs)
Within-Matched-Group Effects						
Mover	−0.019	−0.012	−0.013	−0.001	0.044***	0.001
	(0.022)	(0.020)	(0.021)	(0.016)	(0.008)	(0.013)
Post move	0.040*	0.038**			0.048***	0.026*
	(0.016)	(0.014)			(0.006)	(0.011)
Mover × Post move	0.038	0.008			0.086***	−0.027
	(0.027)	(0.025)			(0.018)	(0.029)
Cross-Group Interactions with Gender						
Women	−0.127***	−0.076**	−0.116***	−0.088***		
	(0.022)	(0.026)	(0.026)	(0.026)		
Women × mover	0.027	−0.001	0.006	0.105***		
	(0.026)	(0.020)	(0.024)	(0.019)		
Women × Post move	−0.014	−0.002				
	(0.018)	(0.016)				
Women × Mover × Post move	0.109***	0.104***				
	(0.033)	(0.030)				
Within-Matched-Group Effects of Control Variables						
Growth market (binary)		0.099*	0.122*	0.085		
		(0.041)	(0.050)	(0.091)		
New contacts (logged)		0.344***	0.350***	0.337***	0.339***	0.360***
		(0.003)	(0.004)	(0.004)	(0.003)	(0.006)
Unit size (logged)		−0.009	−0.044***	0.030*	−0.051***	0.113***
		(0.010)	(0.014)	(0.014)	(0.011)	(0.020)
Average org tenure		−0.002*	−0.001	−0.003**	−0.002*	−0.004
		(0.001)	(0.001)	(0.002)	(0.001)	(0.003)
Average job tenure		−0.020***	−0.018***	−0.023***	−0.023***	−0.006
		(0.003)	(0.004)	(0.005)	(0.003)	(0.008)
Proportion of men		0.029*	0.039*	0.022	0.027	0.104***
		(0.014)	(0.019)	(0.020)	(0.016)	(0.027)
Unit hierarchical depth		0.004*	0.010***	−0.003	0.005*	0.000
		(0.002)	(0.003)	(0.003)	(0.002)	(0.004)
Log average unit performance		−0.003	−0.006	−0.001	−0.003	−0.003
(t-4 to t-1)		(0.002)	(0.004)	(0.004)	(0.003)	(0.006)
Within-unit communication		−0.382***	−0.426***	−0.348***	−0.472***	−0.150***
Density		(0.017)	(0.024)	(0.025)	(0.020)	(0.036)
Mean Differences between Matched Groups						
Mover	−0.004	−0.062	−0.033	−0.092*		
	(0.056)	(0.045)	(0.042)	(0.044)		
Post move	−0.215***	−0.198***				
	(0.052)	(0.041)				
Constant	1.596***	2.010***	1.915***	2.283***		
	(0.294)	(0.396)	(0.304)	(0.325)		
Control variables	No	Yes	Yes	Yes	No	No
Observations	60,824	60,824	31,513	29,311	42,352	18,472
Log Likelihood/R^2	−55,186.40	−48,879.44	−25,286.21	−23,832.59	0.420	0.481

*p < 0.05; **p < 0.01; ***p < 0.001 (two-tailed tests). Matching ID fixed effects are included in all the models.

Table A13: Effects of Mobility and Gender on Brokerage, Controlling for Network Size

	Brokerage (t)					
	(1)	(2)	(3) Before Move	(4) After Move	(5) Women (522 MIDs)	(6) Men (285 MIDs)
Within-Matched-Group Effects						
Mover	0.001	−0.019	−0.025	0.031*	0.013	0.013
	(0.019)	(0.017)	(0.018)	(0.013)	(0.007)	(0.011)
Post move	0.010	−0.018			−0.005	−0.016
	(0.014)	(0.012)			(0.005)	(0.009)
Mover × Post move	0.039	0.052*			0.130***	0.059*
	(0.022)	(0.021)			(0.015)	(0.024)
Cross-Group Interactions with Gender						
Women	−0.150***	−0.071**	−0.112***	−0.068**		
	(0.020)	(0.023)	(0.024)	(0.024)		
Women × mover	−0.021	−0.033	−0.024	0.048**		
	(0.022)	(0.020)	(0.020)	(0.016)		
Women × Post move	−0.009	0.004				
	(0.016)	(0.014)				
Women × Mover × Post move	0.095***	0.077**				
	(0.028)	(0.025)				
Within-Matched-Group Effects of Control Variables						
Growth market (binary)		0.122**	0.137**	0.097		
		(0.035)	(0.043)	(0.078)		
Network size (logged)		0.284***	0.287***	0.279***	0.274***	0.317***
		(0.003)	(0.004)	(0.004)	(0.003)	(0.005)
Unit size (logged)		−0.085***	−0.146***	−0.032**	−0.116**	−0.009
		(0.008)	(0.013)	(0.012)	(0.010)	(0.017)
Average org tenure		−0.001	0.001	−0.002	−0.001	−0.001
		(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Average job tenure		−0.001	−0.003	0.003	−0.001	0.001
		(0.003)	(0.004)	(0.004)	(0.003)	(0.006)
Proportion of men		0.054***	0.058***	0.047**	0.091***	0.032
		(0.012)	(0.017)	(0.017)	(0.014)	(0.023)
Unit hierarchical depth		0.010***	0.017***	0.004	0.007***	0.016***
		(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
Log average unit performance (t-4 to t-1)		0.001	−0.001	−0.003	0.001	0.005
		(0.002)	(0.003)	(0.003)	(0.003)	(0.005)
Within-unit communication density		−0.698***	−0.758***	−0.623***	−0.776***	−0.477***
		(0.017)	(0.024)	(0.025)	(0.020)	(0.036)
Mean Differences between Matched Groups						
Mover	−0.037	−0.037	−0.003	−0.042		
	(0.050)	(0.042)	(0.038)	(0.040)		
Post move	−0.146**	−0.011				
	(0.047)	(0.040)				
Constant	1.824***	2.474***	2.799***	2.367***		
	(0.263)	(0.357)	(0.279)	(0.295)		
Control variables	No	Yes	Yes	Yes	No	No
Observations	60,824	60,824	31,513	29,311	42,352	18,472
Log Likelihood/R^2	−44,889.66	−39,314.66	−20,332.76	−19,212.45	0.421	0.540

*p < 0.05; **p < 0.01; ***p < 0.001 (two-tailed tests). Matching ID fixed effects are included in all the models.
We focus on the effects within Matching IDs and the interaction between gender and key variables across Matching IDs. Given that the IDs are generated through Coarsened Exact Matching, between-matched-group control effects are not meaningful for our research questions. Therefore, while we have accounted for these control variable differences across Matched Groups in Models (2)–(4), we have opted to omit these coefficients due to space limitations.
Models (5) and (6) are fixed-effect models, mean differences between Matched Groups are not included in the estimations.
Variables that are collinear with Matching ID fixed effects are excluded from Models (5) and (6).

Table A14: Effects of Mobility and Gendered Return of Brokerage, Controlling for Network Size

	Individual Sales Performance (t+1)			
	Before Move		After Move	
	(1)	(2)	(3)	(4)
Within-Matched-Group Effects				
Mover	0.158*	0.065	−0.003	−0.011
	(0.076)	(0.075)	(0.052)	(0.053)
Brokerage	0.622***	0.279***	0.534***	0.425***
	(0.042)	(0.043)	(0.040)	(0.041)
Mover × Brokerage	0.071	0.111	−0.066	−0.096
	(0.156)	(0.153)	(0.114)	(0.114)
Cross-Group Interactions with Gender				
Women	0.221*	−0.296*	0.240**	−0.106
	(0.111)	(0.120)	(0.083)	(0.092)
Women × Mover	−0.002	0.006	0.136*	0.119
	(0.090)	(0.088)	(0.065)	(0.065)
Women × Brokerage	−0.464***	−0.389***	−0.361***	−0.366***
	(0.049)	(0.049)	(0.047)	(0.047)
Women × Mover × Brokerage	−0.368	−0.340	0.389**	0.467***
	(0.190)	(0.186)	(0.142)	(0.141)
Within-Matched-Group Effects of Control Variables				
Growth market (binary)		−0.078		0.304
		(0.177)		(0.309)
Network size (logged)		0.535***		0.233***
		(0.017)		(0.017)
Unit size (logged)		−0.345***		−0.587***
		(0.052)		(0.046)
Average org tenure		0.018***		0.012*
		(0.005)		(0.005)
Average job tenure		0.071***		0.031
		(0.015)		(0.016)
Proportion of men		0.156*		0.149*
		(0.068)		(0.067)
Unit hierarchical depth		0.051***		0.080***
		(0.009)		(0.009)
Log average unit performance		0.019		0.038**
(t-4 to t-1)		(0.014)		(0.014)
Within-unit communication density		0.087		−0.032
		(0.102)		(0.102)
Mean Differences between Matched Groups				
Mover	0.320	−0.262	−0.777***	−0.788***
	(0.230)	(0.185)	(0.188)	(0.157)
Brokerage	1.518***	0.136	1.056***	0.160
	(0.192)	(0.182)	(0.139)	(0.140)
Constant	2.017***	7.067**	4.193***	8.246***
	(0.619)	(1.482)	(0.595)	(1.223)
Control variables	No	Yes	No	Yes
Observations	31,513	31,513	29,311	29,311
Log Likelihood	−65,814.30	−65,058.34	−59,842.97	−59,506.08

*p < 0.05; **p < 0.01; ***p < 0.001 (two-tailed tests).
Matching-ID fixed effects are included in all the models. Standard errors are clustered by Matching ID.
We focus on the effects within Matching IDs and the interaction between gender across Matched Groups. Between-Matched-Group control effects were included in Models (2) and (4) but not reported due to space limitations.

Table A15: Effects of Mobility and Gendered Return of Brokerage, Controlling for Network Size (Continued)

	Individual Sales Performance (t+1)			
	(1) Full Interaction Model	(2) Interaction Model with Controls	(3) Women (522 MIDs)	(4) Men (285 MIDs)
Within-Matched-Group Effects				
Mover	0.053	0.018	0.031	−0.057
	(0.045)	(0.044)	(0.029)	(0.049)
Post move	0.145	0.122	0.014	−0.066
	(0.080)	(0.078)	(0.022)	(0.042)
Brokerage	0.588***	0.353***	−0.006	0.326***
	(0.029)	(0.029)	(0.019)	(0.036)
Mover × Post move	−0.421***	−0.313**	−0.256***	−0.389***
	(0.098)	(0.097)	(0.062)	(0.107)
Mover × Brokerage	−0.045	−0.038	0.119	−0.056
	(0.091)	(0.090)	(0.062)	(0.099)
Post move × Brokerage	−0.098	−0.063	−0.069	−0.088
	(0.063)	(0.063)	(0.039)	(0.070)
Mover × Post move × Brokerage	−0.012	−0.064	0.383**	0.050
	(0.187)	(0.185)	(0.128)	(0.198)
Cross-Group Interactions with Gender				
Women	0.114	−0.119		
	(0.067)	(0.077)		
Women × Mover	0.077	0.066		
	(0.054)	(0.053)		
Women × Post move	0.005	0.002		
	(0.097)	(0.094)		
Women × Brokerage	−0.420***	−0.380***		
	(0.034)	(0.034)		
Women × Mover × Post move	0.266*	0.195		
	(0.117)	(0.115)		
Women × Mover × Brokerage	0.073	0.128		
	(0.112)	(0.110)		
Women × Post move × Brokerage	0.053	0.003		
	(0.074)	(0.074)		
Women × Mover × Post move × Brokerage	0.481*	0.542*		
	(0.232)	(0.229)		
Mean Differences between Matched Groups				
Mover	0.300	−0.128		
	(0.168)	(0.144)		
Post move	−1.810***	−1.268***		
	(0.157)	(0.138)		
Brokerage	0.806***	0.016		
	(0.118)	(0.121)		
Constant	1.648	6.689***		
	(0.909)	(1.270)		
Control variables (within Matched Groups)	No	Yes	Yes	Yes
Control variables (between Matched Groups)	No	Yes	No	No
Observations	60,824	60,824	42,352	18,472
Log Likelihood/R^2	−125,632.80	−124,766.54	0.361	0.400

*p < 0.05; **p < 0.01; ***p < 0.001 (two-tailed tests).
Matching-ID fixed effects are included in all the models. Standard errors are clustered by Matching ID.
Within-matched-group control variable estimations are included in Models (2)-(4); and between-matched-group control variables are included in Model (2); coefficients are omitted due to space limitations.

Table A16: Effects of Mobility and Gender on New Contacts

	New Contacts (t, logged)			
	(1)	(2)	(3) Women (522 MIDs)	(4) Men (285 MIDs)
Within-Matched-Group Effects				
Mover	0.016	0.042	0.061**	−0.013
	(0.027)	(0.026)	(0.022)	(0.034)
Post move	0.048***	0.056***	0.047***	0.043***
	(0.005)	(0.006)	(0.003)	(0.005)
Mover × Post move	0.087**	0.066*	0.089***	0.055*
	(0.032)	(0.023)	(0.017)	(0.027)
Cross-Group Interactions with Gender				
Women	0.016	0.005		
	(0.020)	(0.026)		
Women × Mover	0.069*	0.054		
	(0.032)	(0.031)		
Women × Post move	−0.031	−0.036*		
	(0.017)	(0.017)		
Women × Mover × Post move	0.035	0.057		
	(0.040)	(0.039)		
Within-Matched-Group Effects of Control Variables				
Growth market (binary)		0.088		
		(0.054)		
Unit size (logged)		0.106***	0.081***	0.172***
		(0.013)	(0.015)	(0.026)
Average org tenure		−0.005***	−0.005***	−0.005
		(0.001)	(0.002)	(0.004)
Average job tenure		0.009*	0.013**	−0.006
		(0.004)	(0.005)	(0.010)
Proportion of men		−0.031	−0.028	−0.016
		(0.019)	(0.022)	(0.036)
Unit hierarchical depth		0.027***	0.029***	.023***
		(0.002)	(0.003)	(0.005)
Average unit performance		0.006	0.010*	−0.010
(logged, prior quarter)		(0.004)	(0.004)	(0.008)
Within-unit communication density		0.467***	0.461***	0.482***
		(0.013)	(0.015)	(0.028)
Mean Differences between Matched Groups				
Mover	0.164**	0.078		
	(0.051)	(0.048)		
Post move	−0.128**	−0.094*		
	(0.047)	(0.044)		
Constant	1.065***	0.280		
	(0.265)	(0.398)		
Control variables	No	Yes		
Observations	60,824	60,824	42,352	18,472
Log Likelihood/R^2	−67,978.95	−67,149.28	0.220	0.292

*p < 0.05; **p < 0.01; ***p < 0.001 (two-tailed tests)

Matching ID fixed effects are included in all the models. Standard errors are clustered by Matching ID.

Figure A1: Coarsened Exact Matching (CEM) Procedure

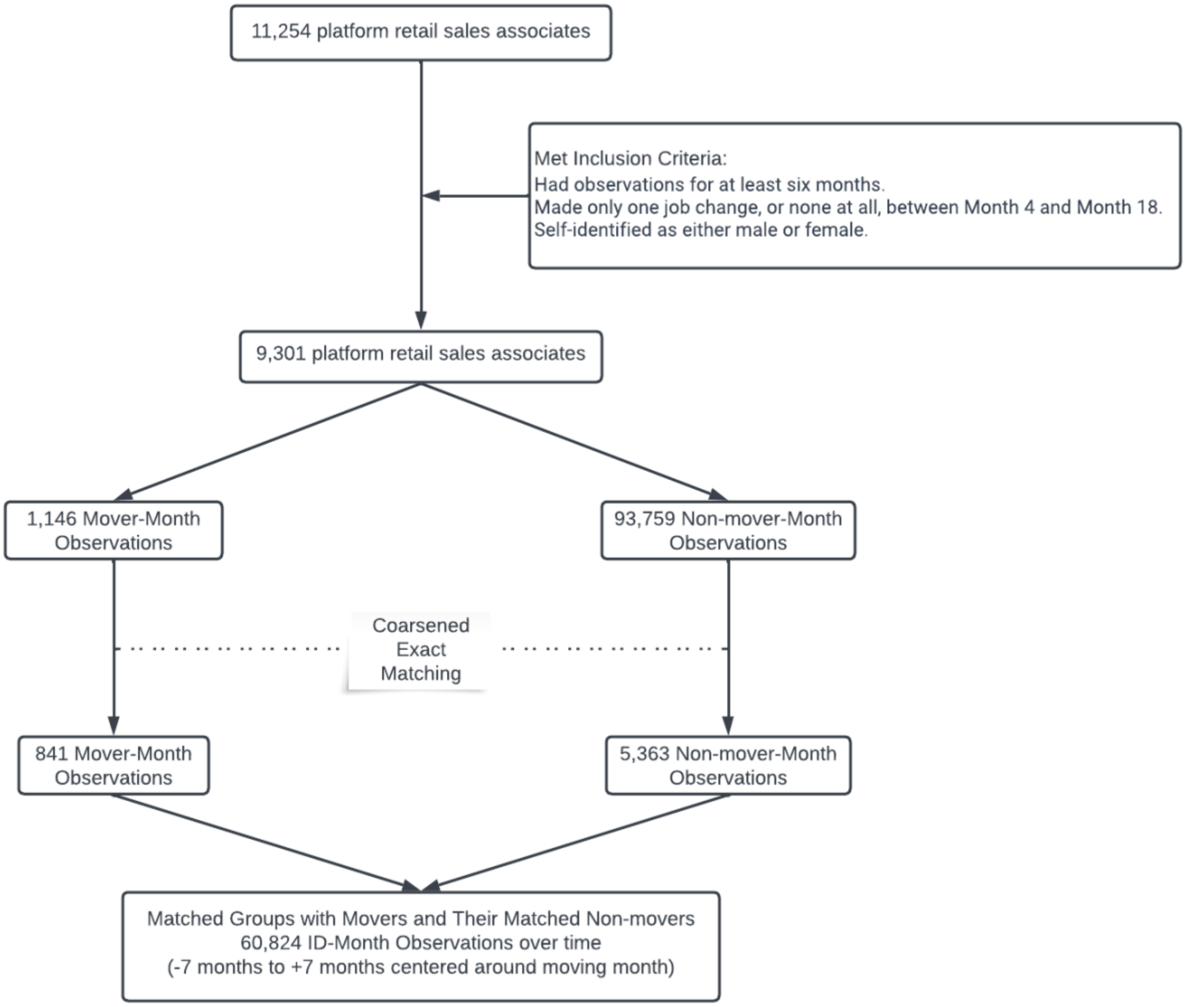

Figure A2: Balance of Continuous Variables Before and After CEM

Before Matching	After Matching