September 15, 2026

Dear Professors Breza, Viviano, and Chandrasekhar,

I am writing to apply to the Workshop on Development Economics and Networks with the paper *Coworker Networks and Labor Misallocation*, joint with Lorenzo Aldeco Leo (Banco de México) and Juan Pablo Arredondo Ambriz (University of San Francisco).

Jason Kerwin, who taught me development at Minnesota, told us that the best research questions come from real world experience, so I would like to share a little bit about the story of how I became interested in the topic and how the paper has unfolded.

I have been working with networks since before I began to study economics. As an undergraduate computer science major at the University of Oklahoma, I spent two summers working at the University of Tennessee Health Science Center developing a visualization tool in Python (GeneNetwork) for the relationships between physical traits in mice brains. My software engineering skills have recently come in handy; our codebase for this paper is over 6000 lines of R spread over 40 files on the Bank of Mexico EconLab server.

After college, I worked directly on the US/Mexico border in a migrant shelter in Tijuana, where I began to learn about networks of a different sort and the important role that they play in migration decisions. Many years later, I worked for two years in El Paso. There I was able to visit an export manufacturing plant in Ciudad Juárez and chat with the workers, who had found the job through social connections. I began to think about how these connections work and their impact.

I brought this question to my doctoral coursework in Applied Economics at the University of Minnesota. In the labor sequence, I read papers on the role of referrals in the labor market, culminating in a paper proposal for Alan Benson's Personnel Economics class. He has agreed to be my recommender for this conference (bensona@umn.edu). As I looked for data, I first tried the Mexican Census. My Second Year Paper "Where You Go Depends on Who You Know?" is a demography exercise with five waves of the Mexican Census tracing out migration corridors.

Along the way, Chiang Tai Hsieh visited our department for a seminar, and in our one-on-one meeting, he told me, "you need the IMSS data for your question." He also introduced me to his well-known paper on misallocation. A year later, through another former master's student at our department, José Nuño Ledesma, I connected with the EconLab at the Bank of Mexico and successfully submitted a project proposal to study the role of coworker networks in job search outcomes. My initial proposal only included partial-equilibrium effects that leverage the displaced-worker identification strategy first proposed by Glitz (2017). I was assigned to work with Lorenzo Aldeco, a labor economist from the Bank who works on the spatial dimensions of labor markets.

In my job market year (2024-25), I continued work on the project. Through an RCT in Peru, I met a talented Mexican master's student, Juan Pablo Arendondo Ambriz. He needed a project for his master's thesis at the University of San Franisco, so with the Bank's permission, we wrote him in the data access contract and he completed the first version of the analysis as his master's project in May 2025. Shortly after I accepted a job offer at Marquette University.

This spring 2026, I began presenting the paper, and Lorenzo and I extended the analysis with an AKM decomposition to explore the general equilibrium effects of networks and think more deeply about the implications of networks on misallocation. I went to Matt Jackson's Network Science in Economics conference in April. Chatting with him and others at the conference helped me think more about the potential negative welfare effects of networks at the aggregate level, especially in light of his work on the origins and consequences of racial inequality in the US.

We have worked on the paper over the summer. The attached draft contains approved export results from August 8, 2026: the initial partial-equilibrium results using the displaced coworker design with and without closing firm fixed effects. It also includes the first AKM decomposition results. We have continued the analysis since then. My data access cycle ends on October 9. By that time, we hope to have another set of results that improves on these results in two important ways.

On the partial equilibrium side, we examine the role of variation in network size among the cohort of displaced workers to continue to unpack the difference in the results from the specifications with and without closing firm fixed effects. Networks appear to work quite differently in cases where they are small and cases where they are large. Here we contribute to a question about "network congestion" first raised by Lori Beaman. We also build the pre-displacement network several different ways--removing immediate coworkers and weighting coworkers by tenure--to get deeper insight into the types of connections that are actually helpful.

On the general equilibrium size, we have a first pass at some descriptives that quantify the present level of misallocation in the Mexican labor market and a counterfactual. This work is in line with Hsieh and Klenow (2009), the figures of marginal product dispersion that they use to motivate their paper in addition to the counterfactual exercise that they use in the end of the paper. We ask the question: in an ideal world, how would assortative matching work in a labor market? We have short-run results (matching the best workers to the best firms) that hold firm size constant and medium- and long-run results that allow firms to exit the market.

With these results in hand, I believe that our paper can make an important contribution to the development-misallocation literature, as we are in conversation with the Chandrasekhar (2020) NBER working paper as well as the upcoming chapter in the sixth volume of the Handbook on Labor Economics by Berguist, Lakshari and Verhoogen. In fact, we have submitted the paper as well to the upcoming conference at Columbia, "New Thinking in Industrial Policy."

As I explain in my research statement, as a junior faculty member, I would like to continue research on networks and misallocation in both of my fields: agriculture and labor. This workshop provides an opportunity to connect with senior scholars that will not only improve this paper but also motivate future work in this area.

Thank you for considering my application to this workshop. I look forward to hearing from you.

Stephen Pitts SJ, PhD
Wade Visiting Jesuit Assistant Professor
Department of Economics
stephen.pitts@marquette.edu

Stephen Pitts, S.J.

Wade Jesuit Visiting Assistant Professor of Economics, Marquette University

Affiliate, Collaborative for Econometrics and Integrated Development Studies, University of Notre Dame

Department of Economics, Marquette University
O'Brien Hall, 1530 W. Wisconsin Ave.
Milwaukee, WI 53233

stephen.pitts@marquette.edu
spittssj.github.io

Academic Appointments

Wade Jesuit Visiting Assistant Professor of Economics, Marquette University 2025–present

Affiliate, Collaborative for Econometrics and Integrated Development Studies (CEIDS), Kellogg Institute for International Studies, University of Notre Dame 2023–present

Education

Ph.D., Applied Economics, University of Minnesota–Twin Cities
Fields: Agricultural Development, Labor Economics. *Ph.D. minor:* GIScience 2025

M.S., International and Development Economics, University of San Francisco 2018

Th.M., Jesuit School of Theology of Santa Clara University *(Ethics and Social Theory)* 2018

M.Div., Jesuit School of Theology of Santa Clara University 2017

M.A., Philosophy, Loyola University Chicago 2011

B.S., Computer Science *(summa cum laude)*, University of Oklahoma 2006

B.A., Mathematics *(Japanese minor)*, University of Oklahoma 2006

Research Fields

Agricultural Economics; Development Economics; Labor Economics; Migration; Social Networks; Latin America; Catholic Social Teaching.

Publications

Peer-Reviewed Articles

Pitts, Stephen, Chris M. Boyd, and Grant X. Storer (2025). "Unpacking Side-Selling: Experimental Evidence from Rural Mexico." *Agricultural Economics* 56 (6): 1119–1149. doi:10.1111/agec.70051

Book Chapters

Pitts, Stephen (2023). "The Role of the Farmer and Their Cooperative in Supply Chain Governance: A Latin American Small Producer Perspective." In *Handbook of Research on Cooperatives and Mutuals*, eds. Michael Boland and Matthew Eliot. Edward Elgar Publishing, pp. 172–184. doi:10.4337/9781802202618.00019

Pitts, Stephen (2019). "Value Chain Integration as an Alternative to Fair Trade for Chiapas Coffee Farmers." In *Entrepreneurship and Development in the 21st Century*, eds. Sergio Bruno and Cole Scanlon. Emerald Publishing, pp. 103–138. doi:10.1108/978-1-78973-233-720191007

Invited Book Reviews

Pitts, Stephen (2024). "Review of *Research Ethics in Applied Economics: A Practical Guide*." *Faith & Economics* 83, Fall 2024.

Pitts, Stephen (2016). "Review of *The Age of Sustainable Development*." *Conversations on Jesuit Higher Education* 49, September 2016.

Working Papers

"Cooperative Membership as Partial Climate Adaptation: Transitory Withdrawal and Permanent Exit" (with Grant X. Storer and Jesse Anttila-Hughes). *Draft available.*

"Coworker Networks and Labor Misallocation" (with Lorenzo Aldeco Leo and Juan Pablo Arredondo Ambriz). *Revising.*

"Where You Go Depends on Who You Know: Social Networks as Determinants of Mexican Internal Migration." *Draft available.*

"Sweet and Timely Income: The Effect of Honey Production in Reducing Food Insecurity of Coffee Producers" (with Chris M. Boyd, Grant X. Storer, and Jesse Anttila-Hughes). *Draft available.*

Research in Progress

"The Causal Impacts of Geographic Targeting on Local Poverty Reduction: Evidence from Mexico" (with Alejandro Estefan). *Data analysis in progress.*

"Later, Not Lower: Price Rigidity and the Risk Smallholders Bear" (with Jesse Anttila-Hughes). *Data analysis in progress.*

"Socio-Emotional Skills in Peruvian Technical Education" (with Paul Glewwe and Gabe Velez). *Research design in progress.*

Grants and Funding

Marquette University, Center for Applied Economics Summer Stipend ($4,000)	2026
Marquette University, Center for International Education Course Development Grant ($2,000)	2026
Catholic Community Foundation of Minnesota ($10,000)	2024–26
University of Minnesota, Agricultural Experiment Station Summer Stipend ($6,300)	2024
Ralph K. Morris Foundation ($2,500)	2023–24
University of Minnesota, Department of Applied Economics Travel Grant ($1,000)	2023
University of Minnesota, CIFAP Travel Grant ($1,000)	2023
University of Minnesota, Center for International Food and Agricultural Policy ($25,000)	2022–23
Santa Clara University, Ignatian Center for Jesuit Education ($1,500; $1,000)	2022
University of Minnesota, Office of the Provost Summer Research Stipend ($6,300)	2021, 2022
Society of Jesus, U.S. Central and Southern Province — Social Ministries Grant ($1,100)	2017
Santa Clara University, Ignatian Center for Jesuit Education ($500)	2017
University of San Francisco, Department of Economics Field Research Grant ($1,500)	2017
Santa Clara University, Markkula Center for Applied Ethics Travel Grant ($750)	2015

Honors, Awards, and Fellowships

University of Minnesota, Applied Economics Department TA Award (1000-level courses)	2021–22
University of Minnesota, Dr. Dale C. Dahl and Jean Kinsey Graduate Fellowship	2020–21
University of San Francisco, Department Merit Scholarship ($2,500; $1,000)	2016–18
University of San Francisco, McDermott Scholarship (full tuition)	2015–17

Invited and Conference Presentations

2027 Virginia Tech (Economics, scheduled).

2026 Agricultural and Applied Economics Association (AAEA) Annual Meeting (×2); Network Science and Economics Conference; Centre for the Study of African Economies (CSAE) Conference; Midwest Economics Association (MEA) / Society of Labor Economists (SOLE) Annual Meeting; University of Wisconsin–Madison (Development Workshop); University of Wisconsin–Milwaukee (Applied Micro Workshop, 1, 1 scheduled); Colorado State University; Tec de Monterrey (Mexico City); Universidad del Pacífico (Lima); University of Minnesota (Applied Economics, scheduled).

2025 Georgetown University (Center for Latin American Studies); Loyola University Chicago.

2024 Boston College (Applied Micro Seminar); Fordham University.

2023 AAEA Annual Meeting (poster ×2); Association for Economic Research of Indigenous Peoples (virtual); University of Minnesota (Applied Economics Student Seminar); University of San Francisco (virtual).

Teaching

Instructor of Record (undergraduate)

Department of Economics, Marquette University:

Principles of Microeconomics	Fall 2025, Spring 2026, Fall 2026, Spring 2027 (scheduled)
Development Economics (also cross-listed at the master's level)	Spring 2026
Economics of Immigration	Spring 2027 (scheduled)

Department of Applied Economics, University of Minnesota:

Applications of Excel in Economics and Management	Fall 2023
Microeconomics of International Development	Spring 2024

St. Ignatius Institute, University of San Francisco:

Science and Religion	Spring 2015
Intimacy in a Virtual World	Fall 2015
Epic Stories	Spring 2016

Teaching Assistant

Department of Applied Economics, University of Minnesota:

Principles of Microeconomics (undergraduate)	Spring 2022
Econometric Analysis I–II (Ph.D.)	Fall 2021, Fall 2022
Econometric Analysis III–IV (Ph.D.)	Spring 2023

Advising and Mentoring

Marquette University

Ziggy Fuller-Engelmajer (M.S. Applied Economics) — directed professional project.

Md Anisur Rahman Mollah (M.S. Applied Economics) — supervised as graduate assistant.

Cara Goodwin (B.S. Economics) — supervised as teaching assistant.

University of San Francisco

Juan Pablo Arredondo Ambriz (M.S. Applied Economics) — supervised fieldwork, Piura, Peru.

Mallika Matharu (M.S. International Development Economics) — supervised fieldwork, Piura, Peru.

Brendon Wong (M.S. International Development Economics) — supervised fieldwork, Piura, Peru.

Grant Storer (M.S. International Development Economics) — supervised fieldwork, Chiapas, Mexico.

Melissa Sovik (M.S. International Studies) — supervised fieldwork, Chiapas, Mexico.

Paolo Bicchieri (M.S. International Studies) — supervised fieldwork, Chiapas, Mexico.

Santa Clara University & Loyola University Chicago

Chris Lane (B.A. Political Science) — supervised fieldwork, Chiapas, Mexico.

Calista Cardenas (B.A. Film and Digital Media) — supervised project, Chiapas, Mexico.

Professional Service

Refereeing

Production Economics section, AAEA Annual Meeting	2026
Agricultural Economics	2025
American Journal of Agricultural Economics	2022, 2023
Handbook of Research on Cooperatives and Mutuals	2022

Departmental

Undergraduate Committee, Department of Economics, Marquette University	2025–present
Priorities Committee, Department of Applied Economics, University of Minnesota	2022–23
Graduate Committee, Department of Applied Economics, University of Minnesota	2022–23

Public Engagement

Selected Media

Quoted in "Economists express concern about the poor as Supreme Court weighs tariffs," *OSV News*, 7 November 2025.

"The ICE raid on a Hyundai plant shows how hard it would be to quit globalization," *America Magazine*, 17 September 2025.

Profiled in "Jesuit Special Studies: Charting Paths of Learned Ministry," *Jesuits USA Central and Southern Province*, 4 March 2025.

Interviewed in "Catholic Social Teaching and Trump's Tariffs," *America Magazine*, 3 March 2025.

Invited Public Lectures

"What You Know Depends on Who You Know"

Marquette University (Wade Chair Lecture)	2026
John Carroll University (scheduled)	2026

"Seguimiento y levantamiento de datos en proyectos"
Regional Directorate of Education, Trujillo, Peru 2026
Jesuit Collaborative of the Jesuits in Peru (virtual) 2025

"What Is a Jesuit Approach to Economic Development?"
CREDO Economics and Catholic Social Teaching Workshop (virtual) 2023
Jesuit Scholarship in a Secular Age, Loyola University Chicago 2023

"Why Would a Jesuit Study Applied Economics?"
Jesuit Novitiate of St. Alberto Hurtado, St. Paul, MN 2023
Jesuit Friends and Alumni Network, St. Paul, MN 2022

"Overview of Economic Causes of U.S./Mexico Migration"
St. Catherine's University, St. Paul, MN 2024
Encuentro Project, El Paso, TX 2019–2021
Archdiocesan Peace & Justice Commission, St. Louis, MO 2019

"Relationships of Solidarity and Depth: Capeltic Coffee Cooperative"
Ignatian Family Teach-In for Justice, Washington, DC 2024
AJCU Conference on International Education (virtual) 2021
Pioneers 21, El Paso, TX 2020

Board Service

Ignatian Center for Jesuit Education, Santa Clara University 2021–2024
Built partnerships with Jesuit NGOs in Chiapas, Mexico and in Lima and Piura, Peru.
Supported their integration into undergraduate coursework.
Accompanied student and faculty immersions to both sites.

Professional Experience

Associate Pastor, Sacred Heart Church, El Paso, TX 2018–2020
Associate Pastor, Jesuit Mission of Bachajón, Chiapas, Mexico Summer 2018
Clinical Pastoral Education, Children's Hospital of San Antonio, San Antonio, TX Summer 2016
Research Associate, Jesuit Social Research Institute, New Orleans, LA Summer 2015
Mathematics Faculty, Jesuit College Preparatory, Dallas, TX 2011–2014
Volunteer, Adachi International Academy, Tokyo, Japan Summers 2010, 2013
Volunteer, Casa de los Pobres, Tijuana, B.C., Mexico Spring 2008

Languages and Technical Skills

Languages: English (native), Spanish (proficient), Japanese (advanced), Portuguese (intermediate).
Computing: R, Stata, Python, SQL, GIS, JavaScript.

Society of Jesus

Member, U.S. Central and Southern Province of the Society of Jesus (Jesuits).
Entrance 2006
First Vows 2008
Priestly Ordination 2017

References

Available upon request.

PhD Course List (University of Minnesota 2020-2025)

Fields: Agricultural Development and Labor Economics

PhD Minor: GIScience

Fall 2020 APEC 8001 Consumer Choice APEC 8002 Production and Uncertainty APEC 8211 Econometric Analysis I APEC 8212 Econometric Analysis II FNRM 5131 GIS for Natural Resources	**Spring 2021** APEC 8003 Game Theory APEC 8004 Social Choice and Welfare APEC 8213 Econometric Analysis III APEC 8214 Econometric Analysis IV GIS 5577 Spatial Database Design
Fall 2021 APEC 8501 Labor Economics I APEC 8502 Labor Economics II APEC 8903 PhD Qualifying Paper Seminar I APEC 8991 Independent Study: Cooperatives	**Spring 2022** APEC 8401: Ag Markets & Policy APEC 8402: Info and Behavioral Econ APEC 8703: Trade and Development III APEC 8901: PhD Qualifying Paper Seminar II SOC 8890: Adv Topics in Research Methods Birth, Death, Migration
Fall 2022 ECON 8105: Macroeconomic Theory ECON 8106: Macroeconomic Theory GEOG 5543: Advanced Geocomputing	**Spring 2023** FNRM 5462: Advanced Remote Sensing & Geospatial Analysis HRIR 8801 Core Seminar: Economic Analysis (Personnel Economics)

Statement of Motivation and Research Interests

Current research. I study two sets of decisions that shape economic mobility in Latin America: how smallholder farmers engage with agricultural markets, and how workers find their way into formal-sector employment. Networks turn out to be the common structure. In Chiapas I study indigenous coffee producers organized into cooperatives — who joins, how membership changes household welfare, and what happens when those relationships break down, using a household survey and lab-in-the-field experiment our team collected together with twenty years of producer records from the cooperative itself. In Mexico's formal labor market, I study how workers reach employers, using harmonized census data and the confidential employer–employee records of the Banco de México EconLab. A third project in Peru examines the interplay of public and private vocational training schools on the local market for education.

This paper. *Coworker Networks and Labor Misallocation* (with Lorenzo Aldeco Leo and Juan Pablo Arredondo Ambriz) asks whether coworker networks allocate workers to the firms where they are most productive or ration access to the firms that pay the most. Comparing workers displaced by the same firm closure, a better-employed network raises the wage at the next job and does not shorten time out of formal employment; decomposing that wage shows the premium running through the firm component and not at all through the match component. Networks here look like an access technology rather than a matching one.

Why this workshop.
These initial projects with networks have exposed me to the opportunities and challenges of this area and additional training to complement what I learned at Minnesota.
1) I've built networks from primary data collection, cooperative administrative data, and matched employer-employee data. I'd like to learn more about the forefront of network analysis and better techniques to collect network data.
2) My initial work--both the third chapter of my dissertation on the way that networks mediate drought response in a coffee cooperative and the first draft of this paper--uses the canonical linear-in-means model of Bramoulée (2009) with all of its incumbent identification challenges. With better network data, I'd like to explore newer techniques that borrow from network science.
3) Moreover, my training up until recently in the wake of the "credibility revolution" has emphasized causal inference, partial equilibrium effects, and atheoretical regressions. I'd like to learn more about structural models to think about general equilibrium effects and market structures.

Two future projects.
The training from this workshop would benefit not only the present paper but also two others.
1) I'd like to revise my Second Year Paper, which is a demography exercise on Mexican internal migration, to use a spatial model in the flavor of Morten et. al (2025) that leverages the well-known Mexican CCT Progresa as an incentive shift to ask whether CCTs in general help or hurt misallocation. That paper would speak to a new literature that examines the long-run general equilibrium effects of CCTs instead of just the short-run partial equilibrium effects.

2) I'd like to use my deep connections and experience in Latin America to "scale up" the small-scale work that I've done in conjunction with agricultural cooperatives that has examined market failures. In particular, I'd like to move up the supply chain to examine the role of networks at the country-level in the flavor of Macchiavello et. al (2015) or Hansman et. al (2020).

Coworker Networks and Labor Misallocation

Stephen M. Pitts* Lorenzo Aldeco Leo†
Juan Pablo Arredondo Ambriz‡

September 15, 2026

*Department of Economics, Marquette University. Corresponding author. Email: stephen.pitts@marquette.edu.

†General Directorate of Research, Banco de México. Email: lorenzo.aldeco@banxico.org.mx.

‡Department of Economics, University of San Francisco. Email: jarredondoambriz@dons.usfca.edu.

Abstract

In low- and middle-income labor markets, most jobs are found through personal contacts rather than through formal intermediaries. Where formal credentials are scarce, informality is widespread, and employers screen at high cost, coworker and kinship networks become the dominant technology allocating workers to firms. Whether that technology raises aggregate productivity is unresolved, and the stakes are first-order: misallocation of labor across firms is a leading account of the productivity gap between rich and poor countries. Networks may route workers toward the firms where they are most productive, or they may ration access to the firms that pay the most, redistributing rents without creating them.

We distinguish these two possibilities using matched employer–employee records covering the universe of Mexican formal-sector workers. The frictions that define this setting are the ones central to development economics: roughly half of employment is informal, formal signaling is thin, returns to tenure are steep, and referral-based hiring is the dominant allocation mechanism. What Mexico adds is full-population administrative data that make the question testable. We use firm closures between June 2014 and May 2015 as the source of displacement, and compare workers displaced from the same closing firm who differ in the share of their former coworkers employed at the moment of displacement.

Within the closing firm, holding constant the firm, municipality, industry, and month of displacement along with the worker's age, gender, years of formal-sector experience, and pre-displacement wage, a one-standard-deviation increase in this network employment rate raises the wage at the next job by 1.5 percent of the mean pre-displacement wage. It does not shorten the time out of formal employment. The large duration effect that appears when workers from different closing firms are pooled does not survive a within-firm comparison.

To ask where the wage premium comes from, we decompose the new job's wage into worker, firm, and match components using a two-way fixed-effects model estimated on the full mover panel, and regress each component on network quality, pooling across closing firms. The premium runs through the worker component, which we read as homophily rather than a network effect, and through the firm component. It does not run through the match component, which carries an economically negligible share of the premium. Better-connected workers land in firms that pay everyone more; they do not land in firms they fit especially well.

Coworker networks in this setting therefore act as an access technology rather than a matching technology. They move their members up the distribution of firm pay premia without improving the assignment of workers to firms,

redistributing the rents from firm-premium dispersion rather than reducing it. The policy implication is uncomfortable: interventions that substitute for network access, such as public employment services, job boards, and credential portability, redistribute opportunity but should not be expected to raise aggregate match quality on their own.

JEL codes: J31, J63, J64, O15, O17, Z13

1 Introduction

Misallocation of labor across firms is a first-order source of the productivity gap between rich and poor countries (Hsieh & Klenow, 2009). At the same time, social networks play a prominent role in the assignment of workers to firms in less-developed settings, raising the question of whether they help or hinder the process of firms finding matching workers and the quality of those matches. These informal institutions may help route workers toward the firms where they are more productive, or constrain them to jobs within their network independently of fit. The question is sharpest where the conditions that make networks the dominant allocation technology — thin formal signaling, widespread informality, costly screening — are most pronounced, as they are in the labor market we study.

We ask whether coworker networks in a developing labor market allocate workers to the firms where they are most productive, or ration access to the firms that pay the most. Three challenges make the question difficult to answer, and each motivates one element of our design. First, network quality is not assigned: workers whose former colleagues are employed tend to be higher-ability workers from better firms, so a raw comparison of the well- and poorly-connected confounds the network with the worker and with the origin firm. Second, the two answers are observationally equivalent in a wage. A worker who earns more at the next job may have found a firm where they are especially productive, or may simply have entered a firm that pays everyone more; the wage alone cannot tell these apart. Third, an allocation claim is a claim about where a worker landed relative to where they could have landed, which requires observing the full set of firms a worker could have moved to, not a single employer or sector.

The design that follows meets each in turn: a comparison of workers displaced by the same firm closure for the first, a decomposition of the new job's wage into worker, firm, and match components for the second, and full-population administrative data for the third.

Mexico provides an ideal setting to study this question. Networks are the dominant channel through which jobs are found: 57 percent of employed workers in the 2019 national labor force survey (ENOE) report having learned about their current job through family, friends, or acquaintances. About half of all employment is informal, formal signaling is thin, and returns to tenure are steep, so a broken match is costly. Our data are the monthly matched employer-employee records of the Mexican Social Security Institute (IMSS), which cover the near universe of formal-sector workers. Importantly, this data allows us to follow workers across job changes.

We define a worker's network as the set of past and present coworkers. We measure it along three dimensions — its size, the longest overlap with any member, and the share of its members formally employed in the month of displacement — and the last of these, the network employment rate, is our measure of network quality, following the intuition that job openings are more easily observed by the currently employed and likely to be shared among acquaintances (Calvó-Armengol & Jackson, 2004).

Our identification strategy leverages firm closures as an exogenous shock that releases workers into the labor market. We build pre-displacement networks of former coworkers for workers affected by these closures and follow them post-displacement to examine the effect of network quality on post-displacement employment outcomes: unemployment spell duration and post-displacement wage. The identifying assumption

is that job losses due to the firm closures are orthogonal to unobserved worker or firm characteristics. To support this claim, we provide a number of descriptive statistics pre- and post- displacement that compare our cohort of workers and the closing firms to the overall distribution of workers and firms in the Mexican labor market. In particular, we focus on firms with 6 to 50 employees, where coworkers can credibly know each other, in the period between June 2014 and May 2015. We exclude closures in which the workforce moved together to a new firm. At the worker level, we require at least 36 months of formal employment over the past five years (2010-2014), so that a pre-displacement network exists. In total, our cohort is 37,860 workers observed to return to formal employment within 24 months.

Our main specification adds closing-firm fixed effects: it compares two workers laid off by the same firm, in the same municipality, industry, and month, of the same age, gender, pre-displacement wage, and years of formal-sector experience, who differ in the employment rate of their former-coworker network. This specification controls for unobserved heterogeneity among firms that could be correlated with the reason for firm closure or the mechanism by which affected workers matched to the firm in the first place. Even with these fixed effects, co-displaced workers differ in network quality largely because 64 percent of them also worked elsewhere in the three years before the closure and carry contacts their colleagues do not (Appendix Figure 7). Those pre-closure histories provide a source of identifying variation.

Within closing firms, a better-employed network improves a worker's post-displacement salary, but does not reduce the duration of unemployment spell. A one-unit increase in the network employment rate increases the daily wage at the next job by 21.5 pesos;

one standard deviation of the rate, 0.16, is worth about 3.4 pesos per day, or 1.5 percent of the mean pre-displacement wage. We do not find an effect on the length of the unemployment spell. We compare these results to another specification without closing-firm fixed effects. Across closing firms, a one-standard-deviation increase in the network employment rate is associated with an unemployment spell about 22 days shorter, but that estimate does not survive the within-firm comparison and we do not interpret it. Our results run contrary to previous evidence from Europe, where the duration effect is the robust finding and the wage effect the mixed one.

Next we examine whether the wage gain reflects improved matching or the sorting of better-connected workers into better-paying firms. A network can act on the new job's wage on two margins: the match margin, by locating an idiosyncratically better pairing, or the access margin, by routing a worker of given type toward a firm that pays a higher premium to everyone it employs. Thus, we estimate an AKM wage decomposition (Abowd et al., 1999) on the full IMSS mover panel, split each re-employed worker's new-job wage into worker, firm, and match components, and regress each component on network quality, pooling across closing firms. The premium runs through the worker component — the larger share, which is the homophily selection just described — and through the firm component: better-connected workers land in firms that pay everyone more. The match component does not move with network quality at all.

Thus we conclude that coworker networks in this setting act as an access technology rather than a matching technology. They move their members up the distribution of firm pay premia without improving the assignment of workers to firms. Reading

that premium dispersion as the labor-market counterpart of the marginal-product dispersion that Hsieh and Klenow (2009) measure across plants is an interpretive step — pay premia can reflect rent sharing or compensating differentials as well as productivity — we argue that the network redistributes the rents from dispersion rather than reducing it. From a policy perspective, this result suggests that interventions that extend network-like access to the unconnected redistribute who reaches high-premium firms; on this evidence, they should not be expected to raise aggregate match quality on their own.

The paper contributes to three literatures. First, it contributes to the literature on networks in economic development, which Breza et al. (2019) review and which has largely treated networks as channels for information and as devices for insuring risk. The closest antecedents on labor market assignment study referrals inside a single firm or an experimental hiring setting — Beaman and Magruder (2012) in Kolkata and Heath (2018) in Bangladeshi garment factories — or migrant networks abroad (Munshi, 2003); none observes the full set of firms a worker could move to, which is what an allocation claim requires. We supply the first economy-wide, administrative evidence on coworker networks in a developing labor market, and we use it to test one prediction of network-based hiring models (Chandrasekhar et al., 2020; Munshi & Rosenzweig, 2016) in full-population data: that the network premium runs through access to higher-paying firms rather than through better matches.

Second, it contributes to a literature that builds coworker networks using administrative data from high-income countries — Italy (Cingano & Rosolia, 2012), Germany (Glitz, 2017), Austria (Saygin et al., 2021), Sweden (Eliason et al., 2023), and Portugal

(Garcia-Louzao & Silva, 2024). Across those five studies, the unemployment-duration effect of a better-employed network is robust and the wage effect is mixed (Section 6.4). Eliason et al. (2023) is closest to us in method, combining displacement with AKM worker and establishment effects. We bring the design to a labor market with roughly 50 percent informality and few formal signaling mechanisms (Donovan & Schoellman, 2023), and we take one further step: decomposing the wage effect into worker, firm, and match components, which is what separates access to better firms from better matching.

Third, it contributes to a literature that uses two-way fixed-effects models to characterize particular labor markets — the approach Card et al. (2013) established for West Germany — including Mexico's. Pérez Pérez and Nuño-Ledesma (2024) estimate worker and firm effects in the same IMSS records and decompose the variance of wages into worker, firm, and sorting components; Pérez Pérez et al. (2026) use the same machinery to study how local labor-market size and informality shape matching. That work asks how much of Mexican wage dispersion each component accounts for. We put the components to a different use, treating each as an outcome and asking what moves a worker's position in the firm-premium distribution. This turns a descriptive decomposition into a test of an allocative mechanism, and it makes the network the object that generates the sorting rather than a residual correlate of it.

The paper proceeds as follows. Section 2 sets out the Mexican setting; the sections that follow take up the theory, the design, the data, the within-firm results, and the decomposition in the order of the argument above. We conclude with policy recommendations and a description of additional analysis in progress.

2 Background

This section places Mexico's formal labor market in the context of the broader literature on developing-country labor markets and motivates our focus on coworker networks. Mexico is, by most measures, a typical low- and middle-income country (LMIC) economy: roughly half of all workers earn their living outside the formal sector, the formal sector itself displays high turnover and steep returns to tenure, and informal information channels — including referrals from friends, family, and former coworkers — play a large role in how workers find jobs. This section sets out the three features of that setting on which the paper's argument rests, and then explains why coworker networks in particular are the right object of study.

2.1 The Setting

Breza and Kaur (2025) argue that labor markets in low- and middle-income countries are characterized by two pervasive failures: high rates of involuntary unemployment and frequent turnover. Mexico exhibits both. What matters for this paper, however, is narrower than a general portrait of the labor market, and rests on three features of it.

The first is that informal channels dominate hiring. Figure 1 reports self-reported job-finding methods from the extended ENOE 2019 Q1 questionnaire. Panel a shows that 57.1% of currently-employed workers found their job through a family member, friend, or acquaintance — more than three times the share who went directly to the workplace (17.5%) and an order of magnitude more than the public employment

service (0.2%) or the internet (4.4%). Panel b shows a sharp education gradient: 62% of workers with no schooling or primary education found their job through personal connections, falling to 50% for upper-secondary and 40% for tertiary-educated workers. The level and the gradient are consistent with the cross-country evidence in Barnett-Howell and Mobarak (2021) that informal networks substitute for formal information channels where the latter are weakest.

The second is that the formal channels networks substitute for are thin. About half of Mexican employment is informal, schooling certification is sparse, on-the-job training carries more of the signal than in continental European labor markets, and public intermediation is negligible — the 0.2% above. Donovan and Schoellman (2023) document the associated pattern of short job spells and high separation rates across a wide set of developing countries. A worker displaced from a formal job in this setting has few credible ways to signal quality to an employer who does not already know them.

The third is the feature that makes *access* worth asking about at all. Where a worker lands is a large part of what they earn: in the two-way fixed-effects decomposition reported in Section 4, firm effects account for roughly a third of the variance of log daily wages in the Mexican formal sector, and worker and firm effects are positively correlated. Firms therefore pay persistently different wages to comparable workers, and a mechanism that determines which firm a worker reaches is a mechanism that determines a substantial part of their pay. That is the premise the rest of the paper tests, and it is why the question is about assignment to firms rather than about the speed of job-finding.

2.2 Why Coworker Networks?

The theoretical and empirical literature on labor market networks has used many different definitions of a worker's relevant social network: people from the same village (Munshi, 2003), the same neighborhood (Schmutte, 2015), friendship ties (Beaman & Magruder, 2012), the same country of origin (Beaman, 2012), or the same ethnic minority (Dustmann et al., 2016). Each definition has strengths in particular settings, but each also requires data — on residence, on social ties, on ethnicity — that is rarely available at scale in LMICs.

We follow a more recent strand of the literature in defining a worker's network as the set of people with whom that worker has been formally employed in the past (Cingano & Rosolia, 2012; Eliason et al., 2023; Garcia-Louzao & Silva, 2024; Glitz, 2017; Saygin et al., 2021). The coworker definition has three advantages in our context. First, it can be constructed entirely from administrative employer–employee records, which means it scales to the full universe of Mexican formal-sector workers. Former coworkers are also the contacts best placed to act on a worker's behalf: they know what the worker can do, and they are inside firms that hire people like them. Those are two different things — knowing about an opening and being able to get someone through the door — and separating them is what this paper does. Third, it has a clear behavioral interpretation: a worker's coworker network at the moment of displacement reflects accumulated occupational, industrial, and geographic experience that is plausibly predetermined relative to the displacement event itself.

The cost of the coworker definition is that it omits ties formed outside formal employment — through family, neighborhood, or schooling — which may be especially

important in an LMIC setting with high informality. We return to this limitation in Section 8.

3 Theory

We organize the theory around a single question: when a coworker network raises the wage a displaced worker earns at the next job, is it because the network found the worker a firm they fit especially well, or because it got them into a firm that pays everyone more? The two answers have different implications for the allocation of labor, and a wage decomposition with worker, firm, and match components makes them separately testable. We first explain why the employment rate of a worker's former coworkers measures the quality of the network, then set out the decomposition, and then state the predictions that Sections 6 and 7 take to the data.

3.1 Why the Employment Rate of the Network Measures Its Quality

In the model of Calvó-Armengol and Jackson (2004), agents learn about vacancies primarily through their social contacts. Employed contacts who hear about positions they cannot use themselves pass the information along; unemployed contacts have nothing to pass. A displaced worker whose former coworkers are employed therefore receives more leads, and better ones, than a worker whose former coworkers are themselves searching. The share of the network that is employed at the moment of displacement — the network employment rate, ER — is the natural summary of this

quality, and it is the measure used throughout the coworker-network literature in high-income countries (Cingano & Rosolia, 2012; Eliason et al., 2023; Garcia-Louzao & Silva, 2024; Glitz, 2017; Saygin et al., 2021). The model's own predictions are that a higher ER shortens the unemployment spell and, because more offers can be compared, raises the accepted wage. We test both, but the wage is the object we then decompose.

3.2 Two Margins in a Wage Decomposition

Let the log wage of worker i at firm j be

$$w_{ij} = \theta_i + \psi_j + m_{ij} + x_i\beta + \varepsilon_{ij},$$

where θ_i is the worker's portable pay premium, ψ_j the premium the firm pays to every worker it employs, and m_{ij} a match-specific component that is specific to the pair (Abowd et al., 1999). Labor misallocation, in the sense of Hsieh and Klenow (2009), is dispersion in marginal products that worker and firm fundamentals do not account for; in this equation its labor-market counterpart is the dispersion of ψ_j across firms and the assignment of workers to it. Reading ψ_j that way is an interpretive step rather than an identity: a firm premium can reflect rent sharing, monopsony power, or a compensating differential as well as productivity, and we return to the point in Section 8.

A network can act on the new job's wage through two margins. On the *match* margin it locates an idiosyncratically better pairing: m_{ij} is higher than the worker

would have drawn without the network. On the *access* margin it routes a worker of given type θ_i toward a firm of higher ψ_j: the worker earns more because the firm pays everyone more. The two margins differ in what they do to allocation. A network that raises match quality moves workers toward firms where they are more productive, which reduces misallocation. A network that only moves workers up the firm-premium distribution redistributes rents among workers of the same type without changing where output is produced; it is an access technology, not a matching technology.

3.3 Two Hypotheses

The *information* view, which follows from the arrival-rate model and from referral models in which an employer learns more about a referred candidate (Dustmann et al., 2016; Montgomery, 1991), predicts that networks act on the match margin. Referrals compress the employer's uncertainty about the pair, so the starting wage is higher because the employer discounts less for uncertainty, and the match component should rise with network quality. In the canonical learning model of Jovanovic (1979), better initial information also implies flatter wage–tenure profiles and faster early separations among the rare bad matches, which we leave to future work with a longer post-displacement panel.

The *access* view predicts that networks act on the sorting margin. Vacancies at high-premium firms are rationed, and a contact inside such a firm is a way past the queue (Chandrasekhar et al., 2020). On this view the network raises the firm component of the new job's wage and leaves the match component unchanged; the same worker earns more because of where they landed, not because of how well they

fit. Two corollaries follow. First, because coworker networks are homophilous in worker type, part of any raw network premium is selection: high-θ_i workers have high-θ contacts. Second, the local benefit has a global cost. When network members compete for the same rationed openings, the marginal value of a network falls as it grows (Chandrasekhar et al., 2020), and an economy in which access rather than fit governs assignment can lock workers into connected firms independent of productivity (Munshi & Rosenzweig, 2016). A word on terminology, since *sorting* is used in this literature for two different things: the access margin above is about which firm a given worker reaches, whereas *assortative sorting* is the correlation between worker and firm effects in the labor market as a whole. The two are connected — a network that routes high-type workers to high-premium firms strengthens assortative sorting — but they are not the same quantity, and we reserve *sorting* for the second. Whether network-based hiring aggravates or attenuates assortative sorting is an open empirical question: Eliason et al. (2023) find in Swedish data that connections let low-premium firms reach high-type workers, reducing sorting, while the homophily argument points the other way.

3.4 Predictions Taken to the Data

The two views agree that a higher network employment rate raises the new job's wage among workers displaced from the same firm. They disagree on the decomposition of that effect. If networks act through information and match quality, the match component of the new job's wage should rise with ER. If they act through access, the firm component should rise with ER and the match component should not move; the

worker component may rise with ER through homophily, which we read as selection rather than a network effect. The Mexican setting sharpens the stakes: about half of employment is informal, formal signaling is scarce, and Puggioni et al. (2022), working in the same IMSS data, document that workers who exit formal employment take three or more years to recover their pre-exit *earnings* — a horizon that includes the months spent outside formal employment, not only the pay of the job eventually found. A network that improves matches would raise the pay of that job; a network that only redistributes access would change which workers get the better-paying vacancies without changing how many there are. Section 6 establishes the within-firm wage effect and Section 7 decomposes it.

4 Data

4.1 Data Sources

Labor market analysis in developing countries often faces challenges due to informality, high turnover, and data fragmentation. To overcome these obstacles, this study uses confidential matched employer-employee records from the **Mexican Social Security Institute (IMSS)**. The IMSS is Mexico's social security institute, which administers health insurance, pensions, and other benefits for private-sector formal employees; employers must register each worker and report a contribution wage every month, and those filings are the records used here. They cover formal-sector workers between 2005 and 2024.

This dataset was obtained through a data-sharing collaboration with the EconLab

at Banco de México, and includes the following variables at the monthly level:

- Employee ID (unique and time-invariant)
- Company ID
- Municipality and state of employment
- Sector of employment
- Gender
- Type of employment (permanent vs. temporary)
- Daily wage (*salario base de cotización*, the salary base on which IMSS contributions are computed)

The size and scope of the dataset are substantial: it covers approximately 12 million individuals in 2005, expanding to 20 million by 2015, allowing for dynamic analyses of employment spells, job mobility, and wage growth. A key strength of the dataset is that it tracks workers and companies over time, capturing job transitions and long-term employment histories that are rarely observed in developing country contexts.

The two analyses in this paper draw different windows from this panel, and for different reasons. The displacement design uses 2010–2014 to build each worker's pre-displacement network, June 2014 to May 2015 for the closures themselves, and the 24 months after each closure for outcomes. The AKM decomposition of Section 7 instead uses a 2013–2016 window, wide enough to bracket the displacement window

and dense enough in job-to-job moves to link firms into a single connected set, which is what identifies firm effects. Neither window is a different dataset; each is a cut of the same records, taken to suit a different estimator.

4.2 Sample Construction and Data Cleaning

The unit of observation is the **individual worker**. Each observation represents a formally employed person displaced from a closing company between June 2014 and May 2015. Workers are followed monthly, before and after displacement.

To build the analytical sample, we focus on workers displaced due to company closures that occurred between **June 2014 and May 2015**. Using monthly IMSS records, we identified companies that ceased operations in each rolling month-to-month window — for example, companies active in June but absent in July, July but absent in August, and so on. This dynamic window allowed for a precise classification of closure events and ensured accurate timing of job displacement.

Several additional filters were applied to refine the sample:

- **Company closures**: Companies were considered closed if they disappeared from the registry in a given month and had no identifiable mass movement of employees (i.e., less than 30% of workers moved to the same destination company, to avoid counting mergers or acquisitions).
- **Company size restriction**: The sample was restricted to companies with 6–50 employees to increase the likelihood that coworkers personally knew each other, which is critical for constructing meaningful social networks.

- **Employment history**: Workers needed to have at least 36 months of formal employment between 2010 and 2014 to be included in the analysis, ensuring a sufficient work history for defining their professional networks.

- **Network definition**: Each worker's social network was constructed as the set of coworkers with whom they overlapped in employment for at least one month at the same company during the 2010–2014 pre-closure period.

A correction to the closure filter. The mass-transfer rule stated above is the rule we intend and the rule we report. In preparing this draft we found that our implementation of it was too permissive, in two respects. It retained a closing firm whenever *some* destination absorbed less than 30 percent of that firm's departing workers, rather than dropping the firm whenever *any* destination absorbed more than 30 percent; and it silently discarded closures from which no worker was re-employed in the month immediately following. The first error admitted a bloc of firms that had in substance re-registered under a new identifier, carrying their workforce with them. The second removed from the sample precisely those workers with the longest spells outside formal employment, and so understated non-employment throughout.

We have rebuilt the sample under the correct rule and re-estimated the paper on it. The within-closing-firm wage result — the central estimate of this paper, and the one the displacement design actually identifies — is unchanged by the correction. The pooled cross-firm duration estimate does not survive it, and the cross-firm estimates reported below should accordingly be read as descriptive rather than causal. Because results leaving the Banco de México research enclave must clear a disclosure review

that the corrected estimates have not yet completed, the tables in this draft report the cleared figures from the earlier build; affected quantities are flagged where they appear, and the corrected sample will be reported in the next version. A second and much smaller correction belongs in the same place: the main-job rule above was not applied when the estimates reported here were produced, so for a worker registered with two employers in the month of re-employment the wage taken as the outcome depended on the order of the underlying records. This affects only workers with simultaneous registrations, a small minority of the re-employed; the corrected sample will report the share.

4.3 Key Variables

The complete dataset includes 43,905 employees displaced during the June 2014–May 2015 period, of whom 37,860 (27,865 men and 9,995 women) were observed to find formal re-employment within a 24-month window following displacement; the analysis sample is these 37,860 workers. The following are the main variables[1] used in the analysis:

- **Age**: Age of the worker at the time of displacement.
- **Wage at displacement**: Daily wage (the IMSS contribution base) in the final month of employment before the company closed; it enters the regressions in hundreds of pesos.
- **Longest coworker overlap**: The longest number of months the worker shared

[1] Sample distributions appear in Appendix Figures 9a–9d.

an employer with any member of the network, a proxy for tie strength.

- **Years in formal sector**: Total years of formal sector experience prior to displacement.
- **Network size (NS)**: Number of unique coworkers from 2010–2014.
- **Employment rate (ER)**: Share of a worker's network who are employed at the time of displacement.
- **Unemployment duration**: Number of months until reemployment in the formal sector.
- **Wage at re-employment**: Daily wage in the first month of the first formal job after the closure. A worker registered with two employers in that month contributes the higher-paying registration, which is the same main-job rule we apply when identifying the first post-displacement employer.

Two constructions are worth stating explicitly: network size enters the regressions in logs, to address its skewness, and unemployment duration is the number of months until a return to formal-sector employment after displacement.

4.4 Descriptive Statistics

Table 2 reports descriptive statistics for the continuous variables in the sample, and Table 3 its composition by industry and displacement month. Figure 2 shows the proportion of individuals by the number of months they remained unemployed; the distribution is concentrated at short unemployment durations, with most individuals

exiting unemployment within the first few months. The dashed vertical line marks the sample mean of 2.45 months. Appendix Figure 6 shows the distribution of the other outcome, the daily wage at the first post-displacement job. Figure 3 reports the sample distributions of network size and the network employment rate, and Appendix Figure 9d reports the distribution of the longest coworker overlap.

4.5 Pre-Displacement Wages and Sample Comparability

The identifying assumption is that the closure, not the worker, is what ends the job. Two features of the pre-period support it. Figure 4(a) plots the mean daily wage of the June 2014 cohort in each of the 36 months before the closure. The series rises gradually — by about 7 percent in nominal terms over three years, which is less than cumulative inflation over the same window — and then declines modestly over the final months before the firm disappears. That terminal decline is what one expects of firms approaching failure, and it is the one respect in which displaced workers are not on an ordinary trajectory; it is small relative to the post-displacement wage changes the paper estimates, and it is common to all workers at a given closing firm, so the closing-firm fixed effect absorbs it. What the figure does not show is a sharp pre-displacement collapse, which would indicate that the workers, rather than the firm, were already in trouble.

Figure 4(b) asks whether workers at closing firms are a selected slice of the small-firm workforce. It compares the distribution of the wage at displacement in the cohort against all IMSS workers employed at firms of the same 6–50-employee size class in June 2014. The two distributions track each other over the whole support, and

their p45–p55 bands overlap (125–144 in the cohort against 119–140 size-matched). Workers displaced by closure are close to ordinary small-firm workers on pay before the event.

Both panels are computed on the June 2014 cohort alone rather than the pooled twelve-cohort sample, and on the pre-correction sample discussed above.

4.6 The Wage Structure of the Formal Sector

A second measurement step underlies Section 7. We estimate the two-way fixed-effects wage model of Abowd et al. (1999) on a 2013–2016 window of the IMSS panel — 323 million person-month observations on 13.0 million workers and 981,939 firms in the largest connected set — chosen to bracket the displacement window with enough mover density on either side to connect firms, and narrower than the 2005–2024 panel of Section 4 because the estimator requires every firm to be linked through workers who move, with a cubic in age and year effects as controls:

$$w_{it} = \underbrace{\theta_i}_{\text{worker}} + \underbrace{\psi_{J(i,t)}}_{\text{firm}} + \underbrace{m_{iJ}}_{\text{match}} + x_{it}\beta + \varepsilon_{it}. \tag{1}$$

Here θ_i is a worker's portable pay premium, $\psi_{J(i,t)}$ the premium that firm $J(i,t)$ pays to every worker it employs, and m_{iJ} the residual match-specific component. The model fits the data well ($R^2 = 0.916$ against a standard deviation of log wages of 0.743), and worker and firm effects are positively correlated (0.19), evidence of positive assortative sorting in the Mexican formal sector. Table 6 reports the variance decomposition: worker effects account for 44 percent of the variance of log wages,

firm effects for 34 percent, and the sorting covariance for 15 percent. These shares are close to those Pérez Pérez and Nuño-Ledesma (2024) report for the same records over an earlier window, which is a useful check: our decomposition is not an outlier, and what is new here is the use we put the components to rather than the components themselves. The 34 percent is an upper bound, for reasons set out in Section 8.

5 Empirical Framework

5.1 The Identification Problem

The central challenge in estimating the effect of coworker networks on post-displacement outcomes is that network quality and job outcomes are simultaneously determined and both depend on unobserved worker characteristics. A worker's ability, motivation, occupational specialization, and willingness to relocate jointly shape both the people the worker has previously worked with — and thus the employment status of those contacts at any given moment — and the worker's own re-employment prospects after a job loss. The relationship between network quality and job outcomes therefore runs in both directions, and an unobserved worker characteristic — summarized as ability — potentially drives both.

Compounding this confounding, job-seekers vary widely in ways that the analyst typically cannot observe: new labor-market entrants differ from experienced workers in their network exposure; voluntary leavers differ from involuntarily-separated workers in their reservation wage and search intensity; and geographically mobile workers differ from those with strong local ties in the geographic scope of their search. These

groups also differ in unobservable characteristics — motivation, risk tolerance, family responsibilities — that simultaneously affect both network formation and search outcomes.

5.2 Research Design: Displaced Workers from Establishment Closures

To address these concerns we follow the now-standard research design developed in Cingano and Rosolia (2012), Glitz (2017), Saygin et al. (2021), Eliason et al. (2023), and Garcia-Louzao and Silva (2024). We restrict attention to workers who lose their jobs through the closure of their employing establishment, dropping closures that resemble mergers or mass transfers (Section 4). Establishment closures are plausibly exogenous to individual worker performance, particularly under our 6–50-employee size restriction — a large firm may close because of a single underperforming worker, but a small firm rarely does. Conditional on the closure event, the cross-sectional variation in network characteristics among the displaced workers identifies the effect of those characteristics on subsequent outcomes.

A second feature of our design is that networks are *predetermined* relative to the displacement event. We construct each worker's coworker network from formal employment overlaps during 2010–2014, while the displacement window opens in June 2014 and the post-displacement outcomes accumulate through 2017. This timing means that the realization of a worker's network employment rate at the displacement date depends on the labor market trajectories of the worker's former coworkers, which

the worker does not control.

A third feature is that we compare workers who differ in their networks but are otherwise observationally similar. Specifically, we condition on age, gender, pre-displacement wage, and years in the formal sector, and we include fixed effects for industry, municipality, and displacement month or, in our central specification, for the closing firm itself. The identifying assumption is that, conditional on these controls and fixed effects, variation in coworker-network characteristics is unrelated to unobserved determinants of post-displacement outcomes.

5.3 Model 1: Post-Displacement Outcomes

Our first specification tests the arrival-rate prediction of Section 3: stronger coworker networks should shorten unemployment spells and, plausibly, raise post-displacement wages by giving workers access to more and better-matched job offers. Letting i index displaced workers and t index the displacement month, we estimate

$$Y_{it} = \alpha + \beta_1 \, \mathrm{ER}_{it} + \beta_2 \, \log(\mathrm{NS}_{it}) + \beta_3 \, \mathrm{Overlap}_{it} + \gamma' \mathbf{X}_{it} + \delta_m + \tau_t + \phi_j + \epsilon_{it}, \quad (2)$$

where Y_{it} is alternately the number of months i remains unemployed after displacement and the daily wage at the first formal-sector job after displacement. The three network regressors capture distinct aspects of the network: ER_{it}, the share of i's past coworkers who are formally employed in month t, proxies network *quality*; $\log(\mathrm{NS}_{it})$, the log of the number of past coworkers, proxies network *size*; and $\mathrm{Overlap}_{it}$, the longest period of shared employment between i and any network member, proxies tie *strength*.

The control vector $\mathbf{X}_{it}$ contains age, gender, pre-displacement wage, and years in the formal sector. The fixed effects absorb time-invariant unobserved heterogeneity at the level of municipality (δ_m), displacement month (τ_t), and industry (ϕ_j); because municipality is nested within state, a separate state effect would be redundant. All specifications are estimated by OLS. Standard errors are heteroskedasticity-robust (HC1) in the pooled specification and clustered by closing firm in the within-firm specification of equation (3), which is the one we rely on. The primary parameter of interest is β_1.

Equation (2) compares workers displaced from different closing firms. Our central specification tightens the comparison to workers displaced from the *same* firm by replacing the three group effects with a closing-firm fixed effect λ_f:

$$Y_{if} = \beta_1 \,\mathrm{ER}_i + \beta_2 \,\log(\mathrm{NS}_i) + \beta_3 \,\mathrm{Overlap}_i + \gamma' \mathbf{X}_i + \lambda_f + \epsilon_{if}. \tag{3}$$

The fixed effect absorbs everything common to the workers displaced together — the firm's municipality, industry, and month of closure, its wage level, and whatever shock brought it down — so that β_1 is identified from co-displaced workers who differ in the quality of their networks. Because the closing firms have 6 to 50 employees, each fixed effect is estimated from a handful of workers, and we cluster standard errors by closing firm. Within a closing firm the network measures move together, because co-displaced workers share most of their former coworkers; the within-firm variation comes from the 64 percent of displaced workers who also worked elsewhere during the three years before the closure and therefore carry contacts their colleagues do not

(Appendix Figure 7). Equation (3) therefore identifies the effect of network quality well and the effects of network size and of interactions with size poorly; we return to this in Section 6.

5.4 Model 2: Wage Components

Our second specification asks which margin the within-firm wage effect operates on. We estimate the two-way fixed-effects wage model of Abowd et al. (1999) on the full IMSS mover panel for 2013–2016, which yields for every worker a portable pay premium $\hat{\theta}_i$ and for every firm in the connected set a premium $\hat{\psi}_j$ paid to all its workers. For each displaced worker who is re-employed we take the log wage at the first post-displacement job and split it into the worker effect $\hat{\theta}_i$, the destination-firm effect $\hat{\psi}_{J(i,\text{post})}$, the age–year index $x_{it}\hat{\beta}$, and the match residual $\hat{m}_{iJ} = w_{it} - \hat{\theta}_i - \hat{\psi}_{J(i,\text{post})} - x_{it}\hat{\beta}$. We regress each component, and the log wage itself, on the same network measures as equation (2), with age, gender, and years in the formal sector as controls:

$$c_i^k = \beta_1^k \,\text{ER}_i + \beta_2^k \,\log(\text{NS}_i) + \beta_3^k \,\text{Overlap}_i + \gamma^{k\prime}\mathbf{X}_i + \delta_m + \tau_t + \phi_j + e_i^k, \qquad (4)$$

for $c^k \in \{w,\ \hat{\theta},\ \hat{\psi},\ x\hat{\beta},\ \hat{m}\}$, with municipality ($\delta_m$), displacement-month ($\tau_t$), and industry ($\phi_j$) fixed effects. The control vector here is not identical to that of equation (2): the pre-displacement wage, which enters Model 1, is not among the controls in the estimates reported below. Because the components sum to the log wage, the coefficients β_1^k sum to the total effect of ER on the log wage, so each one measures

the share of the network premium that runs through that channel. The information view predicts a positive coefficient on the match residual; the access view predicts a positive coefficient on the firm component and none on the match residual. The connected set is also what makes the third design element of Section 1 operational: a destination firm's $\hat{\psi}$ is a position in the distribution of premia across all 981,939 connected firms, which is the closest observable counterpart to the set of firms a displaced worker could have moved to. The sample is the 37,483 displaced workers for whom both effects are estimated; every displaced worker in the analysis sample is, by construction, a mover in the AKM panel. The results are in Section 7. The components are estimated rather than observed, which has consequences for both the firm-premium variance and the standard errors; we take them up in Section 8.

5.5 Limitations

Two caveats temper the causal interpretation of our estimates. First, although establishment closures provide an exogenous separation, unobserved individual traits such as motivation or risk tolerance may still correlate with both network composition and search outcomes; the rich fixed-effect structure addresses but does not eliminate this concern. Second, the IMSS data observe only formal-sector ties, so workers with rich informal networks — through family, neighborhood, or informal-sector employment — will appear in our data with measured networks that understate their true social capital. The closing-firm fixed effects of equation (3) address the first concern at the firm level; we discuss extensions that address both, including the use of mass layoffs as an instrument, in Section 8.3.

6 Results

6.1 Within the Closing Firm

Our central estimates compare workers displaced by the same firm closure. Table 4 reports equation (3), which carries a closing-firm fixed effect and clusters standard errors by closing firm. Because that fixed effect absorbs everything the co-displaced share (Section 5), the coefficient on the network employment rate (ER), the share of a displaced worker's former coworkers who are formally employed in the month of displacement, is identified from co-displaced workers who differ in the quality of their networks.

The two outcomes part ways. The wage effect is present and precise: a unit increase in ER raises the daily wage at the next job by 21.5 pesos (column 3; 23.4 pesos in the parsimonious column 4), so one standard deviation of ER is worth about 3.4 pesos per day, or 1.5 percent of the 231-peso mean pre-displacement wage, with a clustered standard error of 7.3. It does not depend on the other two network measures. The duration effect is absent: the ER coefficient is -0.03 in column (1) and -0.04 in column (2), each with a standard error of 0.16 and indistinguishable from zero. Within the closing firm, a better-employed network changes the job a worker lands, not how quickly they land it.

This is the result the rest of the paper builds on, and Section 7 asks where the wage gain comes from. The comparison is between co-displaced workers alike in age, gender, years of formal-sector experience, and pre-displacement wage, whose former colleagues happen to be more or less employed when the firm closes. What it does

not absorb is the pre-closure employment history that generates the difference in network quality, and Section 7 is where that selection appears.

6.2 Comparison: Pooling Across Closing Firms

The value of the within-firm comparison is clearest in what happens without it. Table 5 reports OLS estimates of equation (2), which pools workers across closing firms and absorbs municipality, industry, and displacement-month effects in place of the firm itself.

Column (1) shows a large negative coefficient on ER: pooled across closing firms, a better-employed network is associated with a markedly shorter spell out of formal employment. We report the estimate but do not interpret it, for two reasons given below. Columns (3) and (4) show that ER is also positively associated with the wage at the first post-displacement job: a one-unit increase raises the daily wage by 49.6 pesos, so one standard deviation of ER is worth about 7.9 pesos per day, or 3.4 percent of the mean pre-displacement wage.[2]

The wage estimate is of the same sign and order of magnitude as its within-firm counterpart. The duration estimate has no within-firm counterpart at all, and it is the estimate most exposed to the closure definition: the filter correction described in Section 4 removes firms that re-registered carrying their workforce, whose workers by construction combine near-zero spells with near-complete network employment. A coefficient that does not survive the within-firm comparison and is sensitive to the

[2] This is a cross-firm estimate, and is among the quantities affected by the closure-filter correction described in Section 4; it does not survive the correction. The within-firm estimates are unaffected.

sample definition is not one to build on. We treat the wage effect as the causal object and do not interpret the pooled duration result.

It is worth noting what its absence within the firm implies. A duration effect is the prediction of the information channel — a better-employed network delivers vacancy news faster, so the worker finds a job sooner. Its absence once the closing firm is held fixed is the first indication, before any decomposition, that information is not the margin the network operates on.

The log of network size is not statistically significant for either outcome here, so within this specification the number of contacts matters less than whether those contacts are employed; Section 7 shows that in the pooled log specification network size does carry a wage effect, and that it runs through the firm component. The longest coworker overlap, our proxy for tie strength, is significant but small for unemployment duration and insignificant for wages. The controls behave as expected: older workers earn more; women earn 3.6 pesos less per day and have slightly shorter spells; a higher pre-displacement wage (entered in hundreds of pesos) predicts both faster re-employment and higher earnings; and more years in the formal sector predict slightly lower post-displacement wages.

6.3 Splits, Interactions, and Robustness

Appendix Tables 8 and 9 split the within-firm specification by the closing firm's sector and region, and Appendix Table 10 adds the interaction of the network employment rate with log network size. None of the three identifies what it is asked to. Every sector and region estimate lies within two standard errors of the pooled within-firm estimate

of 21.5 pesos and no two are distinguishable from each other, so the splits neither establish nor rule out heterogeneity; the point estimates are largest in manufacturing and in the center of the country, which is suggestive and no more. The size interaction is positive on duration in the cross-firm specification, which would be consistent with members of a large network competing for the same openings, but it is not identified within the closing firm, where co-displaced workers share most of their network by construction. We report all three for completeness and build on none of them.

Dropping network size and tie strength from the specification (columns 2 and 4 of Tables 5 and 4) leaves the network employment rate coefficient nearly unchanged in both specifications, so the headline result is not an artifact of collinearity among the three network regressors.

6.4 Comparison with the Developed-Country Literature

Our within-firm design is the one used by Cingano and Rosolia (2012), who exploit variation among workers displaced by the same firm and absorb shared shocks with firm fixed effects. They find that a one-standard-deviation increase in the network employment rate shortens unemployment by about 8 percent in Italian administrative data. We do not find a within-firm duration effect in Mexico; the large cross-firm effect we do find is the one that a within-firm comparison removes. Like Cingano and Rosolia, we find that the employment rate of the network is a far more powerful predictor than its size. That comparison holds across closing firms; within the closing firm, as Section 5 notes, network size is poorly identified because co-displaced workers share most of their network, so we do not read it as a within-firm ranking of the two

measures.

Country	Main finding on coworker networks	Study
Italy	Better-employed networks shorten unemployment	Cingano & Rosolia (2012)
Germany	Shorter unemployment; no robust wage effect	Glitz (2017)
Austria	Easier moves between industries and regions	Saygin et al. (2021)
Sweden	Connections reduce sorting between high- and low-wage firms	Eliason et al. (2023)
Portugal	Shorter unemployment, higher initial wages, longer tenure	Garcia-Louzao & Silva (2024)

Table 1: Coworker-network effects on labor market outcomes in five developed-country studies. The unemployment-duration effect is robust across settings; the wage effect is mixed.

The wage result is where the Mexican evidence is distinctive. Table 1 summarizes the five developed-country studies that use our design. Across them, the wage effect of coworker networks is mixed: Cingano and Rosolia (2012) did not estimate it; Glitz (2017) finds an OLS wage effect that disappears under instrumental variables; Saygin et al. (2021) find no wage effect; Eliason et al. (2023) interpret their findings as evidence that networks reduce sorting between high- and low-wage firms; and Garcia-Louzao and Silva (2024) find a small initial wage premium that fades with tenure. Against this backdrop, a within-firm wage effect that survives closing-firm fixed effects and clustering is the finding to explain. Section 7 asks whether it reflects a better match or access to a better-paying firm.

7 Where the Wage Premium Comes From

7.1 Decomposing the Network Wage Premium

Section 5 sets out the decomposition; this section reports it. Figure 5 shows the same decomposition without a functional form: binned means of each component across

twenty equal-count bins of ER. The worker and firm effects are flat across the bottom two-thirds of the ER distribution and climb steeply in the top third, while the match residual stays flat and economically negligible. The network premium is therefore concentrated among the best-connected quarter of displaced workers, and even there it takes the form of access to higher-paying firms rather than better fit.

Before reading the shares, it is worth being explicit about what is being decomposed, because it is not the coefficient of Section 6. These regressions are pooled across closing firms and the dependent variables are in logs, where Section 6 estimated a within-firm effect on the wage in levels. Both differences push the total up: pooling reintroduces the between-firm variation the closing-firm effect removes, and a log specification weights proportional gains, which are largest for the low-wage workers whose absolute gains are smallest. A unit increase in ER raises the post-displacement log wage by 0.97 (column 1), about 0.16 log points per standard deviation. That is larger in proportional terms than the within-firm level estimate of Section 6, but the two are not comparable and the distance between them is a property of the two specifications rather than a finding about networks. What Section 7 takes from this table is therefore the *composition* of the premium, not its size: the shares are what the argument uses, and they are what the within-firm counterpart, once estimated, is expected to preserve. The larger share of that premium is the worker effect (column 2): workers with better-employed networks are workers with higher portable pay premia. This is selection through homophily rather than a causal effect of the network, and we state it as such. The remainder is the firm effect (column 3): better-connected workers land in firms that pay a higher premium to everyone they employ. We report the

split qualitatively because it is a cross-firm quantity: the division between the worker and firm channels moves under the closure-filter correction described in Section 4, while the absence of a match effect does not. The match component (column 5) carries essentially none of the premium: the point estimate is -0.03 against a total of 0.97, so it is economically negligible, though it is precisely enough estimated to be statistically distinguishable from zero. Dropping network size and tie strength from the regressions moves the ER coefficients only marginally (to 0.95, 0.53, 0.42, and -0.04).

The same table shows that network size and tie strength, which are indistinguishable from zero in the within-firm level specification, are positive in the pooled log specification and load almost entirely on the firm component: a log point of network size carries 0.018 on the log wage and 0.018 on the firm effect, with nothing on the worker effect. That is the access reading again, on a different margin — more contacts reach more firms rather than better matches. These are cross-firm quantities, so we do not build on them.

7.2 Reading

Networks, on this evidence, do not find workers the firms they fit especially well; they get them into firms that pay more. That is the access-rationing pattern rather than the matching pattern — the local-benefit, global-cost logic of Chandrasekhar et al. (2020); compare Munshi and Rosenzweig (2016).

It also answers, for this setting, the question Section 3 left open about the direction in which networks move assortative matching. Eliason et al. (2023) find that in

Sweden connections let low-premium firms reach high-type workers, so that networks *reduce* sorting between high- and low-wage firms. Here the network employment rate raises the worker component and the firm component together (columns 2 and 3): better-connected workers are higher-type workers who also land at higher-premium firms. In Mexico, on this evidence, coworker networks *reinforce* assortative sorting rather than loosening it. The same mechanism that is egalitarian in a labor market with dense formal intermediation is stratifying in one without it.

Combined with the positive sorting in the panel as a whole, the result says that coworker networks redistribute workers' positions in the firm-premium distribution by connection without shrinking that distribution. Two caveats apply. The components are generated regressors, so the reported standard errors are provisional pending a leave-out correction; and the firm effects come from an uncorrected AKM, so the firm channel should be read as an upper bound.

8 Conclusion

This paper asks whether coworker networks in a developing labor market allocate workers to the firms where they are most productive or ration access to the firms that pay the most. Using confidential matched employer–employee records from the Mexican Social Security Institute (IMSS) and firm closures between June 2014 and May 2015 as the source of displacement, we compare workers displaced from the same firm who differ in the share of their former coworkers who are employed. Within the closing firm, a better-employed network raises the wage at the next job: one

standard deviation of the network employment rate is worth about 3.4 pesos per day, or 1.5 percent of the mean pre-displacement wage. It does not shorten the spell out of formal employment; the large duration effect that appears when workers from different closing firms are pooled disappears once they are compared within the firm. The number of former coworkers and the strength of the ties matter little once the employment rate is held fixed.

Decomposing the new job's wage into worker, firm, and match components with a two-way fixed-effects model estimated on the full IMSS mover panel gives the answer to the opening question. The larger share of the network wage premium is the worker component — better-connected workers are higher-type workers, which is homophily rather than a causal effect of the network. The remainder is the firm component: better-connected workers land in firms that pay everyone more. The match component carries an economically negligible share of the premium, and the whole premium is concentrated in the best-connected quarter of displaced workers. Networks, on this evidence, get their members into better-paying firms; they do not find them the firms they fit especially well.

These findings extend the coworker-network evidence from high-income countries to a much higher-informality setting, but they change its interpretation. The European studies establish that displaced workers' former colleagues help them find work and, in some settings, earn more; ours is the first economy-wide administrative evidence in a developing labor market, and it identifies the channel. The channel is access, not matching. That reading connects the network literature to the misallocation literature: firm-premium dispersion is the labor-market analog of the marginal-product dispersion

that Hsieh and Klenow (2009) measure across plants — an interpretive step, since a firm premium may reflect rent sharing or monopsony as much as productivity — and, on that reading, a mechanism that moves connected workers up the distribution without improving fit redistributes the rents from dispersion rather than reducing it. It is also the access-rationing pattern of Chandrasekhar et al. (2020): a private benefit to the connected that need not be an aggregate gain.

8.1 Policy Implications

The first implication is about where the gains come from, and it is uncomfortable. The network premium in this setting is not created; it is captured. Firms pay persistently different wages to comparable workers, and what a well-employed network does is move its members up that distribution without changing its shape. If the premium ran through the match component — if connected workers landed in firms where they were especially productive — then extending network-like access to the unconnected would raise output, and the case for intervention would be an efficiency case. It does not. On the evidence here, a policy that gave every displaced worker the reach of a well-connected one would change who enters high-premium firms and not how many such positions exist. That is a distributional gain, and it may well be one worth having, since the workers without connections are not a random subset. It is not an aggregate productivity gain, and a misallocation argument for the policy would need the match result to have come out the other way.

The second is about which instruments are the right ones, and here the decomposition is informative in a way that outcome data alone is not. Carranza and

McKenzie (2024), reviewing active labor market programs in low- and middle-income countries, organize the evidence around the question of what a program must supply to substitute for a missing network. Our results narrow the answer by elimination. The mechanism is not information about vacancies: a public job board reproduces the arrival-rate function of a network, and within the closing firm a better-employed network does not shorten the time a worker spends out of formal employment. Nor is it a credible signal of general quality, which a certification program would supply, since the premium does not run through the worker component in a way that a certificate could carry — that component is selection, not something the network confers. What a connection supplies is entry to a firm whose vacancies are rationed. The substitute has to reproduce entry, which points to intermediaries holding standing relationships with employers rather than to programs that post openings or credential workers.

This also gives program evaluation a sharper test than placement or earnings. If an intermediary works through the channel identified here, the wage gain of a placed worker should show up in the firm component of their wage — they should land somewhere that pays everyone more — and not in the match component. A program that raises earnings through better matching and one that raises them through better access look identical in an earnings regression and imply different things about whether the gains scale when the program does. The decomposition used in this paper distinguishes them, and can be run on the administrative records that most such programs already generate.

One instrument we would not draw from these results is an extension of referral bonuses. Paying firms to solicit referrals from beyond their current workforce operates

through exactly the channel the paper identifies as rationing: it widens the set of people with a connection rather than weakening the role of connections in who gets in. Whether that is desirable depends on who acquires the new connections, which is a distributional question and not one this paper answers.

8.2 Limitations

Four limitations should be acknowledged. First, the worker and firm effects come from an uncorrected two-way fixed-effects model, so the firm-premium variance is an upper bound — limited mobility between firms biases it upward, as Bonhomme et al. (2026) set out — and the standard errors of the component regressions treat generated regressors as data. Second, the worker component of the network premium is selection through homophily, and we do not claim it as a network effect; only the firm component is the network at work. Third, the IMSS data observe only the formal sector, so workers who move into informal employment, or who maintain rich informal-sector ties, appear with measured networks that understate their social capital and with re-employment that we do not observe. Fourth, the design uses a single displacement year and identifies the effect of network quality well but the effect of network size poorly, because workers displaced together share most of their network. Fifth, every outcome is conditional on returning to formal employment within 24 months, so the paper is silent on the extensive margin — whether a better-employed network makes a return more likely at all. That margin matters here: the closure-filter correction of Section 4 raises the share of displaced workers who never reappear, and what we can say is that the network premium is a placement effect rather than

displacement insurance. It changes the job a worker lands, not how fast they land it and not, on this evidence, whether they land one.

The bottom line is direct: in Mexico's formal labor market, who you know — specifically, who among your former coworkers is employed when you lose your job — shapes how well your next job pays, and it does so by getting you into a better-paying firm rather than into a better match. Labor market institutions that reproduce that access for workers who lack it are, on the evidence presented here, the margin on which policy can act — with the caveat that extending access redistributes the rents of firm-premium dispersion rather than, by itself, reducing that dispersion.

8.3 Next Steps

Five extensions are under way.

First, the decomposition of Section 7 is estimated across closing firms. Re-estimating it with a closing-firm fixed effect, on the components already in hand, would make the composition of the network premium an identified object rather than a descriptive one, and would let Sections 6 and 7 report and decompose the same total rather than a level effect and a log effect on different samples.

Second, the worker and firm effects come from a plain two-way fixed-effects estimator, which overstates the variance of firm effects in a panel with limited mobility between firms (Bonhomme et al., 2026). We are implementing the leave-out correction of Kline et al. (2020) on the mover panel and will report corrected variance components alongside the uncorrected ones, together with standard errors for the component regressions that do not treat the estimated components as data.

Third, the closure filter described in Section 4 has been corrected and the analysis re-estimated on the corrected sample. The within-firm wage result is unchanged by the correction; the pooled estimates, and the division of the premium between the worker and firm channels, move. A future version will report the corrected sample throughout, once those estimates have completed the disclosure review that results leaving the research enclave require.

Fourth, the network employment rate is one of several defensible ways to summarize the employment status of a worker's former colleagues, and the estimates should not depend on which one is chosen. We are rebuilding it under alternative definitions — excluding colleagues displaced by the same closure, weighting contacts by the length of shared employment, and requiring a minimum overlap before a former coworker counts as a contact — and will report the wage coefficient under each. The measure is also estimated with error, which attenuates its coefficient, and the error is likely to be larger for workers with few former colleagues, where a handful of contacts determines the rate. Splitting each worker's network at random into halves and instrumenting the rate computed on one half with the rate computed on the other identifies the reliability of the measure and bounds that attenuation. This matters for more than precision: if reliability rises with network size, an attenuation gradient is observationally equivalent to the interaction between network quality and network size that we report as suggestive in Section 6, and the two need to be told apart before either is interpreted.

Fifth, this paper takes the dispersion of firm pay premia as given and asks who reaches the top of it. The companion question is where that dispersion comes from,

and how much of it represents misallocation rather than compensating differences: whether observably identical workers are paid differently within narrow local labor market by industry cells, as the labor-market counterpart of the within-industry marginal-product dispersion that Hsieh and Klenow (2009) measure across plants, and that Bloom et al. (2026) document for Mexican firms. Because the IMSS data cover only formal employment, magnitudes there will be conditional on formality, a boundary Alvarez and Ruane (2024) show is neither small nor stable in Mexico.

Acknowledgments

We thank Marc Bellemare, Alan Benson, Nick Jolly, and Robizon Khubulashvili, as well as participants at the UW–Milwaukee Applied Micro Workshop, the SOLE sessions at the 2026 MEA Meetings, and the Colorado State University Economics Seminar for valuable comments and discussion.

The data was accessed through the EconLab at Banco de México. The EconLab collected and processed the data as part of its effort to promote evidence-based research and foster ties between Banco de México's research staff and the academic community. Inquiries regarding the terms under which the data can be accessed should be directed to econlab@banxico.org.mx.

References

Abowd, J. M., Kramarz, F., & Margolis, D. N. (1999). High Wage Workers and High Wage Firms. *Econometrica*, *67*(2), 251–333. Retrieved February 5, 2025, from https://www.jstor.org/stable/2999586

Alvarez, J. A., & Ruane, C. (2024). Informality and aggregate productivity: The case of Mexico. *European Economic Review*, *167*, 104791. https://doi.org/10.1016/j.euroecorev.2024.104791

Barnett-Howell, Z., & Mobarak, A. M. (2021). Social networks analysis in agricultural economies. In *Handbook of Agricultural Economics* (pp. 4613–4652, Vol. 5). Elsevier. https://doi.org/10.1016/bs.hesagr.2021.10.010

Beaman, L. (2012). Social Networks and the Dynamics of Labour Market Outcomes: Evidence from Refugees Resettled in the U.S. *The Review of Economic Studies*, *79*(1), 128–161. https://doi.org/10.1093/restud/rdr017

Beaman, L., & Magruder, J. (2012). Who Gets the Job Referral? Evidence from a Social Networks Experiment. *American Economic Review*, *102*(7), 3574–3593. https://doi.org/10.1257/aer.102.7.3574

Bloom, N., Iacovone, L., Pereira-López, M., & Van Reenen, J. (2026). Management and Misallocation in Mexico. *American Economic Journal: Macroeconomics*, *18*(2), 429–456. https://doi.org/10.1257/mac.20220034

Bonhomme, S., Lamadon, T., & Manresa, E. (2026). A Users' Guide to Uncovering Worker and Firm Effects: The ABC of AKM. *Journal of Economic Perspectives*, *40*(2), 195–214. https://doi.org/10.1257/jep.20231372

Breza, E., Chandrasekhar, A., Golub, B., & Parvathaneni, A. (2019). Networks in economic development. *Oxford Review of Economic Policy*, *35*(4), 678–721. https://doi.org/10.1093/oxrep/grz025

Breza, E., & Kaur, S. (2025). Labor Markets in Developing Countries. *Annual Review of Economics*, *17*, 747–776. https://doi.org/10.1146/annurev-economics-091124-051650

Calvó-Armengol, A., & Jackson, M. O. (2004). The Effects of Social Networks on Employment and Inequality. *American Economic Review*, *94*(3), 426–454. https://doi.org/10.1257/0002828041464542

Card, D., Heining, J., & Kline, P. (2013). Workplace Heterogeneity and the Rise of West German Wage Inequality*. *The Quarterly Journal of Economics*, *128*(3), 967–1015. https://doi.org/10.1093/qje/qjt006

Carranza, E., & McKenzie, D. (2024). Job Training and Job Search Assistance Policies in Developing Countries. *Journal of Economic Perspectives*, *38*(1), 221–244. https://doi.org/10.1257/jep.38.1.221

Chandrasekhar, A., Morten, M., & Peter, A. (2020, February). *Network-Based Hiring: Local Benefits; Global Costs* (w26806). National Bureau of Economic Research. Cambridge, MA. https://doi.org/10.3386/w26806

Cingano, F., & Rosolia, A. (2012). People I Know: Job Search and Social Networks. *Journal of Labor Economics*, *30*(2), 291–332. https://doi.org/10.1086/663357

Donovan, K., & Schoellman, T. (2023). The role of labor market frictions in structural transformation*. *Oxford Development Studies*, *51*(4), 362–374. https://doi.org/10.1080/13600818.2023.2276702

Dustmann, C., Glitz, A., Schönberg, U., & Brücker, H. (2016). Referral-based Job Search Networks. *The Review of Economic Studies*, *83*(2), 514–546. https://doi.org/10.1093/restud/rdv045

Eliason, M., Hensvik, L., Kramarz, F., & Skans, O. N. (2023). Social connections and the sorting of workers to firms. *Journal of Econometrics*, *233*(2), 468–506. https://doi.org/10.1016/j.jeconom.2021.12.018

Garcia-Louzao, J., & Silva, M. (2024). Coworker networks and the labor market outcomes of displaced workers: Evidence from Portugal. *Industrial Relations: A Journal of Economy and Society*, *63*(3), 389–413. https://doi.org/10.1111/irel.12348

Glitz, A. (2017). Coworker networks in the labour market. *Labour Economics*, *44*, 218–230. https://doi.org/10.1016/j.labeco.2016.12.006

Heath, R. (2018). Why Do Firms Hire Using Referrals? Evidence from Bangladeshi Garment Factories. *Journal of Political Economy*, *126*(4), 1691–1746. https://doi.org/10.1086/697903

Hsieh, C.-T., & Klenow, P. J. (2009). Misallocation and Manufacturing TFP in China and India. *The Quarterly Journal of Economics*, *124*(4), 1403–1448. https://doi.org/10.1162/qjec.2009.124.4.1403

Jovanovic, B. (1979). Job Matching and the Theory of Turnover. *Journal of Political Economy*, *87*, 972–990. https://doi.org/10.1086/260808

Kline, P., Saggio, R., & Sølvsten, M. (2020). Leave-Out Estimation of Variance Components. *Econometrica*, *88*(5), 1859–1898. https://doi.org/10.3982/ECTA16410

Montgomery, J. D. (1991). Social Networks and Labor-Market Outcomes: Toward an Economic Analysis. *The American Economic Review*, *81*(5), 1408–1418. Retrieved April 18, 2023, from https://www.jstor.org/stable/2006929

Munshi, K. (2003). Networks in the Modern Economy: Mexican Migrants in the U. S. Labor Market*. *The Quarterly Journal of Economics*, *118*(2), 549–599. https://doi.org/10.1162/003355303321675455

Munshi, K., & Rosenzweig, M. (2016). Networks and Misallocation: Insurance, Migration, and the Rural-Urban Wage Gap. *American Economic Review*, *106*(1), 46–98. https://doi.org/10.1257/aer.20131365

Pérez Pérez, J., Meléndez, J., & Nuño-Ledesma, J. G. (2026). Matching and Local Labor Market Size in Mexico: The Role of Informality.

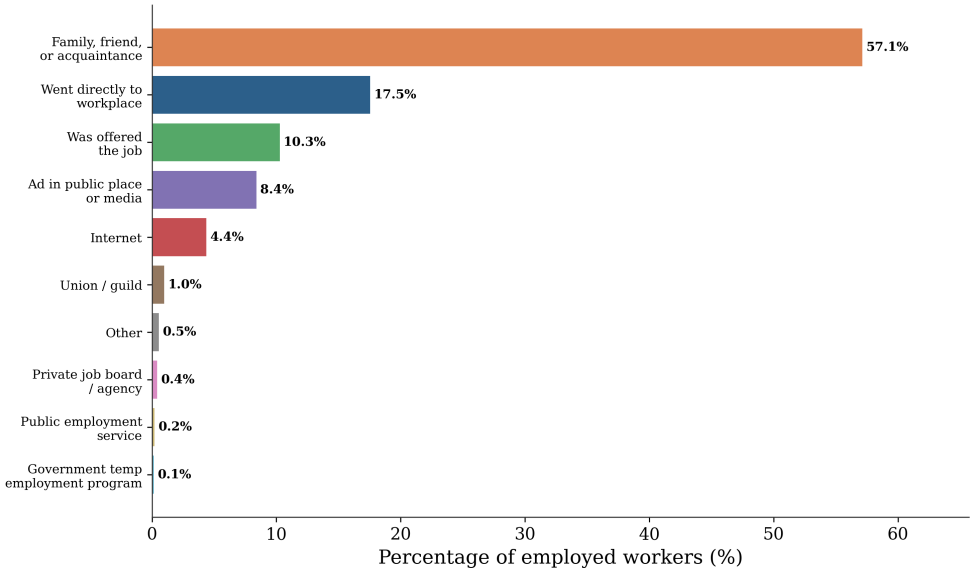
Pérez Pérez, J., & Nuño-Ledesma, J. G. (2024). Workers, Workplaces, Sorting, and Wage Dispersion in Mexico. *Economía*, *23*(1). https://doi.org/10.31389/eco.424

Puggioni, D., Calderón, M., Cebreros Zurita, A., Fernández Bujanda, L., Inguanzo González, J. A., & Jaume, D. (2022). Inequality, income dynamics, and worker transitions: The case of Mexico. *Quantitative Economics*, *13*(4), 1669–1705. https://doi.org/10.3982/QE1863

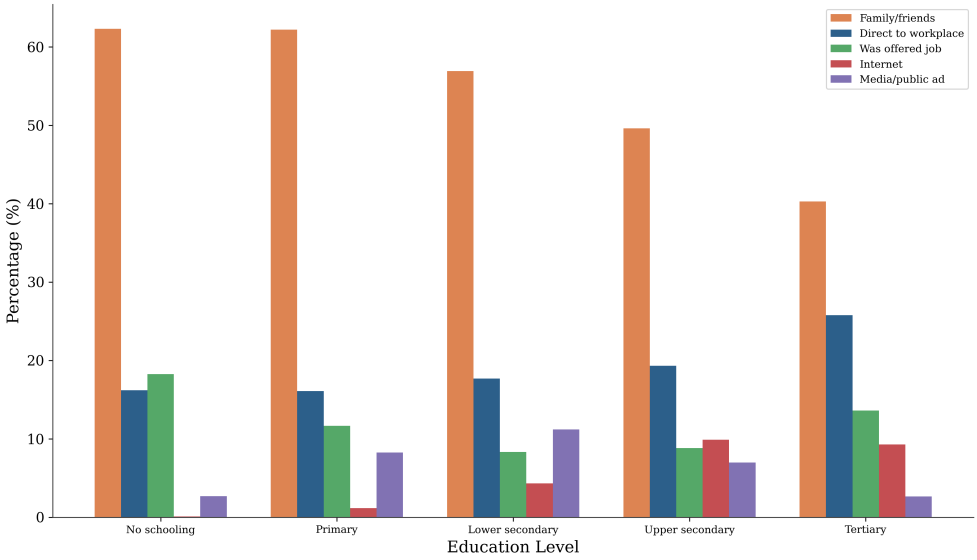
Saygin, P. O., Weber, A., & Weynandt, M. A. (2021). Coworkers, Networks, and Job-Search Outcomes among Displaced Workers. *ILR Review*, *74*(1), 95–130. https://doi.org/10.1177/0019793919881988

Schmutte, I. M. (2015). Job Referral Networks and the Determination of Earnings in Local Labor Markets. *Journal of Labor Economics*, *33*(1), 1–32. https://doi.org/10.1086/677389

Exhibits



(a) Overall shares



(b) By education level

Figure 1: How currently-employed Mexican workers found their job, by self-reported method. Personal connections (family, friends, or acquaintances) are the leading channel overall and in every education group. Source: authors' calculations from raw INEGI ENOE 2019 Q1 microdata, extended questionnaire (working-age 15–64).

	N	Mean	SD	Q1	n	Q3	n
Age	37,860	39.69	10.31	31.00	1,385	47.00	897
Wage at displacement	37,860	230.96	277.91	91.60	0	236.02	0
Years in formal sector	37,860	19.00	9.87	11.00	1,302	26.00	1,072
Employment rate	37,860	0.56	0.16	0.46	0	0.67	0

Table 2: Descriptive statistics for selected continuous variables. Q1 and Q3 are the 25th and 75th percentiles; the n column beside each counts the workers whose value equals the reported percentile exactly (0 where the percentile is interpolated between observations), as required by the EconLab disclosure review. Minimum, maximum and median are not reported for the same reason. The corresponding distributions appear in Figures 9a–9d.

Industry	Frequency	Percent
Servicios empresas	12,427	32.82
Construccion	9,234	24.39
Comercio	6,923	18.29
Ind. Transformacion	5,022	13.26
Transporte	2,452	6.48
Servicios Sociales	851	2.25
Ind. Extractivas	446	1.18
Agricultura	411	1.09
Ind. Electrica	94	0.25

Displacement Date	Frequency	Percent
June 2014	3,081	8.14
July 2014	2,758	7.28
August 2014	2,547	6.73
September 2014	3,133	8.28
October 2014	3,266	8.63
November 2014	2,426	6.41
December 2014	5,765	15.23
January 2015	2,512	6.63
February 2015	2,942	7.77
March 2015	2,510	6.63
April 2015	3,469	9.16
May 2015	3,451	9.12

Table 3: Sample composition by industry of the closing firm and by displacement month. Industry follows the IMSS nine-division classification and the division names are reproduced in the original Spanish as they appear in the administrative data: *Servicios empresas* (business services), *Construccion* (construction), *Comercio* (commerce), *Ind. Transformacion* (manufacturing), *Transporte* (transport), *Servicios Sociales* (social services), *Ind. Extractivas* (mining and extraction), *Agricultura* (agriculture), *Ind. Electrica* (electricity). Months run from June 2014 to May 2015.

Dependent Variables:	Months unemployed		Post-displacement wage	
Model:	(1)	(2)	(3)	(4)
Variables				
Network employment rate	-0.03	-0.04	21.5***	23.4***
	(0.16)	(0.16)	(7.3)	(7.2)
Log (network size)	-0.02*		0.95*	
	(0.01)		(0.57)	
Longest coworker overlap (months)	-0.001		0.009	
	(0.001)		(0.06)	
Age	0.006**	0.006**	0.22*	0.19*
	(0.003)	(0.002)	(0.11)	(0.11)
Female	-0.007	-0.006	-1.6	-1.7
	(0.04)	(0.04)	(1.8)	(1.8)
Wage at displacement (100 MXN)	-0.02**	-0.02**	86.2***	86.1***
	(0.008)	(0.008)	(1.4)	(1.4)
Years in formal sector	-0.003	-0.004	-0.07	-0.06
	(0.003)	(0.003)	(0.11)	(0.11)
Fixed-effects				
Closing firm	Yes	Yes	Yes	Yes
Fit statistics				
Observations	37,860	37,860	37,860	37,860
R^2	0.55234	0.55230	0.86918	0.86916

Clustered (Closing firm) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 4: Network quality and post-displacement outcomes within the closing firm — the paper's principal specification. OLS estimates of equation (3): months out of formal employment in columns (1)–(2), the daily wage at the first post-displacement job in (3)–(4); columns (2) and (4) drop network size and tie strength. Coefficients are identified from workers displaced by the same firm. Standard errors clustered by closing firm; wage at displacement in hundreds of pesos. These within-firm estimates are stable under the sample correction of Section 4.

Dependent Variables:	Months unemployed		Post-displacement wage	
Model:	(1)	(2)	(3)	(4)
Variables				
Network employment rate	-4.6***	-4.5***	49.6***	49.2***
	(0.12)	(0.12)	(4.9)	(4.8)
Log (network size)	0.01		-0.57	
	(0.01)		(0.44)	
Longest coworker overlap (months)	-0.009***		-0.06	
	(0.001)		(0.05)	
Age	-0.001	-0.005*	0.42***	0.42***
	(0.003)	(0.003)	(0.12)	(0.11)
Female	-0.14***	-0.17***	-3.6**	-3.7**
	(0.04)	(0.04)	(1.6)	(1.6)
Wage at displacement (100 MXN)	-0.03***	-0.04***	85.2***	85.1***
	(0.005)	(0.005)	(0.72)	(0.71)
Years in formal sector	0.004	0.004	-0.24*	-0.24*
	(0.003)	(0.003)	(0.12)	(0.12)
Fixed-effects				
Industry FE (pre)	Yes	Yes	Yes	Yes
Municipality FE (pre)	Yes	Yes	Yes	Yes
Displacement-month FE	Yes	Yes	Yes	Yes
Fit statistics				
Observations	37,860	37,860	37,860	37,860
R^2	0.13211	0.12990	0.78623	0.78621

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Mean wage at displacement is 231 MXN $\sim$ 11.7 USD; the wage-at-displacement control enters in hundreds of pesos.
Mean unemployment duration is 2.45 months.

Table 5: Network quality and post-displacement outcomes, pooled across closing firms. OLS estimates of equation (2), with municipality, industry and displacement-month effects in place of the closing-firm effect; heteroskedasticity-robust (HC1) standard errors. These estimates are descriptive: they compare workers displaced by different firms, and the duration coefficient in particular is sensitive to the closure definition discussed in Section 4. The within-firm counterpart is Table 4.

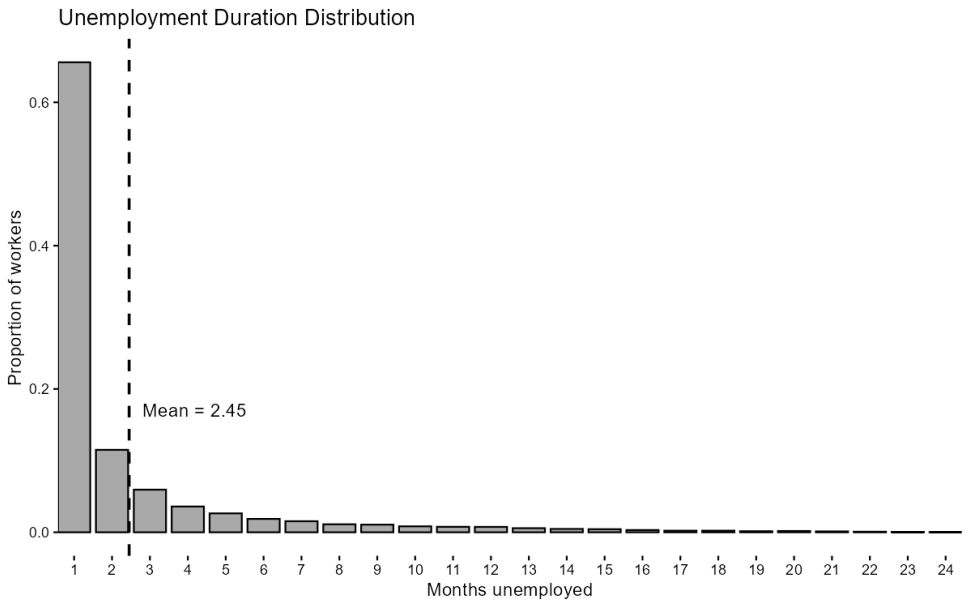
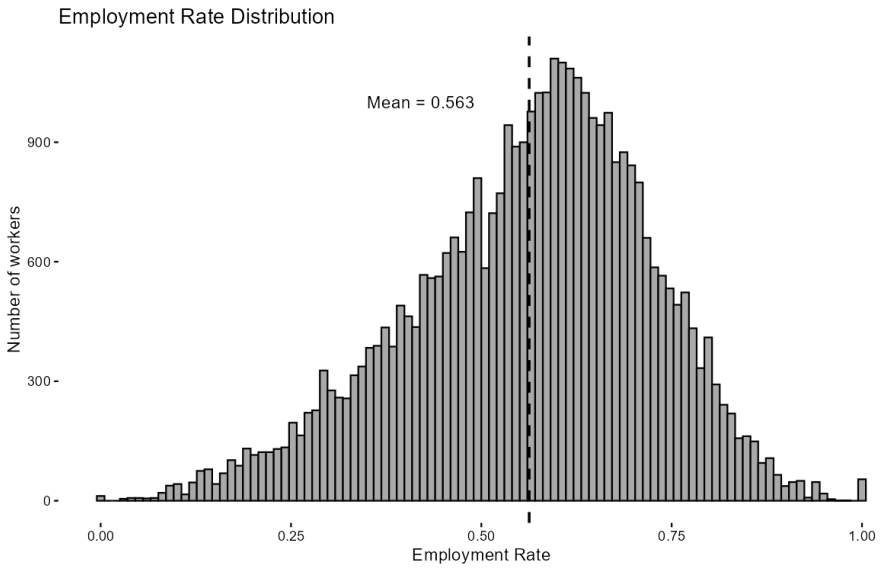
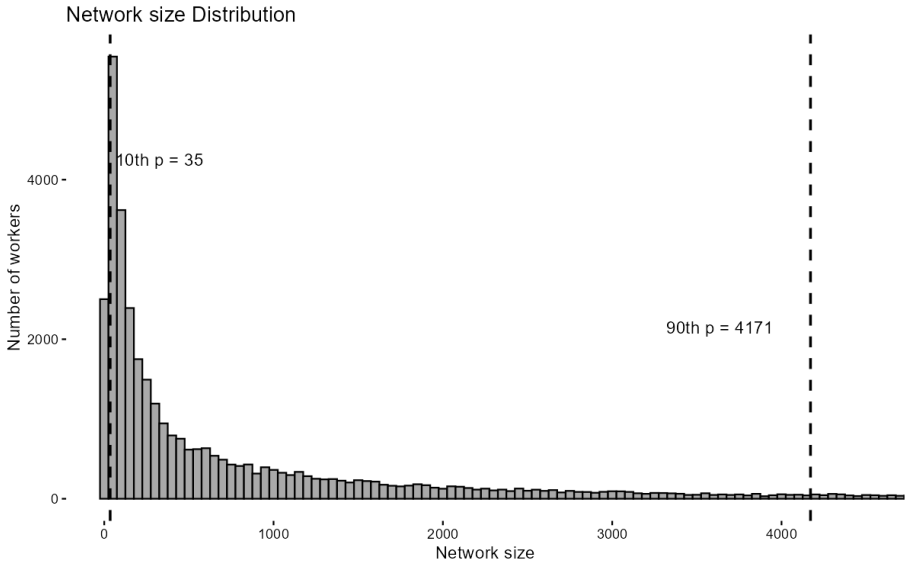


Figure 2: Unemployment duration. The figure reports the distribution of completed unemployment durations for the analysis sample; the dashed line marks the sample mean (2.45 months). Two features of the sample shape it. The sample is conditioned on returning to formal employment within 24 months, so workers who never return are excluded and the horizontal axis begins at one month rather than zero. The closure filter described in Section 4 also dropped closures from which no worker was re-employed in the following month, which removed some of the longest spells; the corrected sample has a substantially higher share of workers never re-employed.

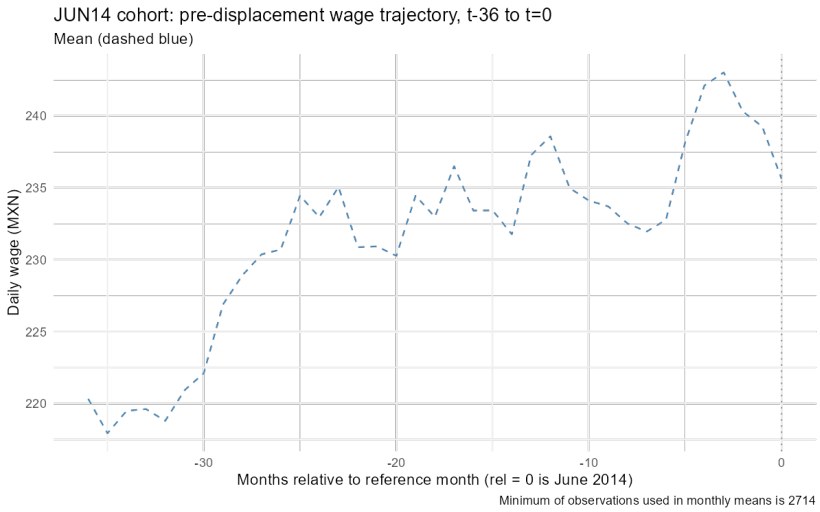


(a) Network employment rate

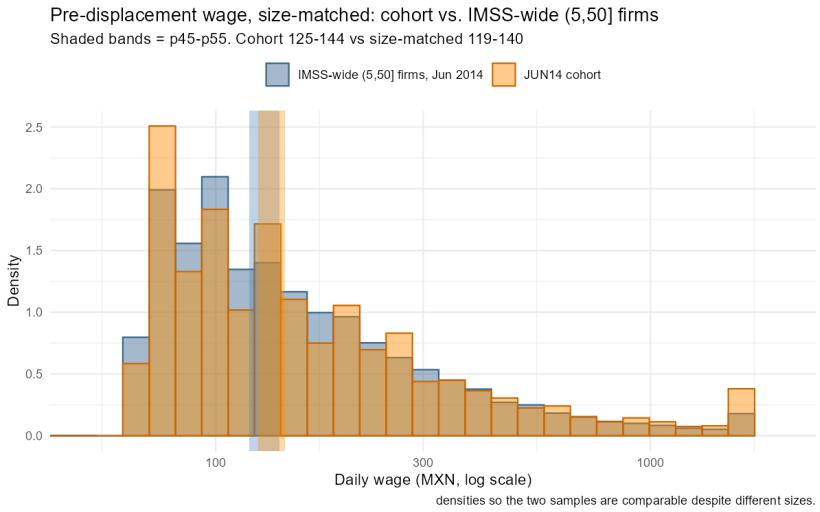


(b) Network size

Figure 3: Sample distributions of the two network measures, for the 37,860 displaced workers of the analysis sample. Panel (a) is the share of a worker's former coworkers who are formally employed in the month of displacement, the paper's measure of network quality; the dashed line marks the sample mean of 0.563. Panel (b) is the number of distinct former coworkers; its horizontal axis is truncated near 4,700, just above the 90th percentile of 4,171, so roughly the top tenth of the distribution falls outside the plotted range.



(a) Pre-displacement wage path, $t - 36$ to $t = 0$



(b) Wage at displacement, cohort versus size-matched IMSS

Figure 4: Pre-displacement wages for the June 2014 displacement cohort. Panel (a) plots the mean nominal daily wage in each month from 36 months before the closure to the reference month (monthly means rest on at least 2,714 workers). The series drifts up by about 7 percent over three years — less than cumulative inflation — and turns down modestly in the final months, consistent with firms trimming pay as they approach failure; there is no sharp pre-displacement collapse. Panel (b) compares the wage at displacement in the cohort against all IMSS workers at firms of the same 6–50-employee size class in June 2014; bands are p45–p55 (cohort 125–144, size-matched 119–140). The distributions are close over the whole support. Both panels are for the June 2014 cohort only, not the pooled sample of Table 2, and rest on the pre-correction sample described in Section 4.

Component	Share of $\mathrm{var}(\log w)$	SD
Worker effects, $\mathrm{var}(\theta)$	0.436	0.491
Firm effects, $\mathrm{var}(\psi)$	0.340	0.433
Sorting, $2\,\mathrm{cov}(\theta, \psi)$	0.147	—
Age and year index, $\mathrm{var}(x\beta)$	0.039	0.147
$2\,\mathrm{cov}(\theta, x\beta)$	−0.066	—
$2\,\mathrm{cov}(\psi, x\beta)$	0.021	—
Residual	0.084	0.216
Total	1.001	

Table 6: Variance decomposition of log daily wages from the two-way fixed-effects model, equation (1), estimated on the largest connected set of the IMSS mover panel for 2013–2016 (323 million person-month observations; 13.0 million workers; 981,939 firms). Shares sum to one up to rounding; the residual share of 0.084 is the complement of $R^2 = 0.916$. Standard deviation of log wages 0.743; $\mathrm{corr}(\theta, \psi) = 0.19$. Plain AKM estimates without a leave-out correction, so the firm-effect share is an upper bound.

Dependent Variables:	Log wage (post)	Worker effect (pe)	Firm effect (fe)	Age/year index	Match quality (resid)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Network employment rate	0.9729***	0.5636***	0.4067***	0.0312***	-0.0286***
	(0.0261)	(0.0179)	(0.0154)	(0.0027)	(0.0075)
Log (network size)	0.0179***	0.0001	0.0177***	0.0046***	-0.0045***
	(0.0023)	(0.0016)	(0.0014)	(0.0003)	(0.0007)
Longest coworker overlap (months)	0.0063***	0.0040***	0.0023***	-3.44×10^{-6}	9.21×10^{-6}
	(0.0002)	(0.0002)	(0.0001)	(2.72×10^{-5})	(8.06×10^{-5})
Age	0.0005	0.0171***	-0.0014***	-0.0145***	-0.0007***
	(0.0006)	(0.0005)	(0.0004)	(7.71×10^{-5})	(0.0002)
Female	-0.0580***	-0.0344***	-0.0279***	0.0025***	0.0019
	(0.0087)	(0.0061)	(0.0051)	(0.0009)	(0.0026)
Years in formal sector	0.0018***	0.0002	0.0009**	0.0009***	-0.0001
	(0.0007)	(0.0005)	(0.0004)	(6.95×10^{-5})	(0.0002)
Fixed-effects					
Industry FE (pre)	Yes	Yes	Yes	Yes	Yes
Municipality FE (pre)	Yes	Yes	Yes	Yes	Yes
Displacement-month FE	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	37,483	37,483	37,483	37,483	37,483
R^2	0.20798	0.24212	0.24068	0.78889	0.04203
Within R^2	0.06145	0.17776	0.03261	0.77556	0.00273

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 7: Effect of network ER on the post-displacement log wage and its AKM components. Each column reports equation (4) with the named component as the dependent variable: the log wage at the first post-displacement job (1), the worker effect $\hat{\theta}$ (2), the destination-firm effect $\hat{\psi}$ (3), the age–year index (4), and the match residual (5). Because the components sum to the log wage, the ER coefficients in columns (2)–(5) sum to column (1). These regressions are pooled across closing firms, with municipality, industry, and displacement-month effects rather than a closing-firm effect, so they are the cross-firm counterpart of Table 5 and not of the within-firm specification of Table 4; they are descriptive of where the premium sits, not identified within the firm, and they rest on the pre-correction sample described in Section 4. $N = 37{,}483$ of the 37,860 displaced workers in the analysis sample: the remainder are those whose post-displacement firm falls outside the largest connected set of the mover panel, for which no firm effect is estimated. Heteroskedasticity-robust standard errors in parentheses; they treat the estimated components as data and are provisional pending a leave-out correction.

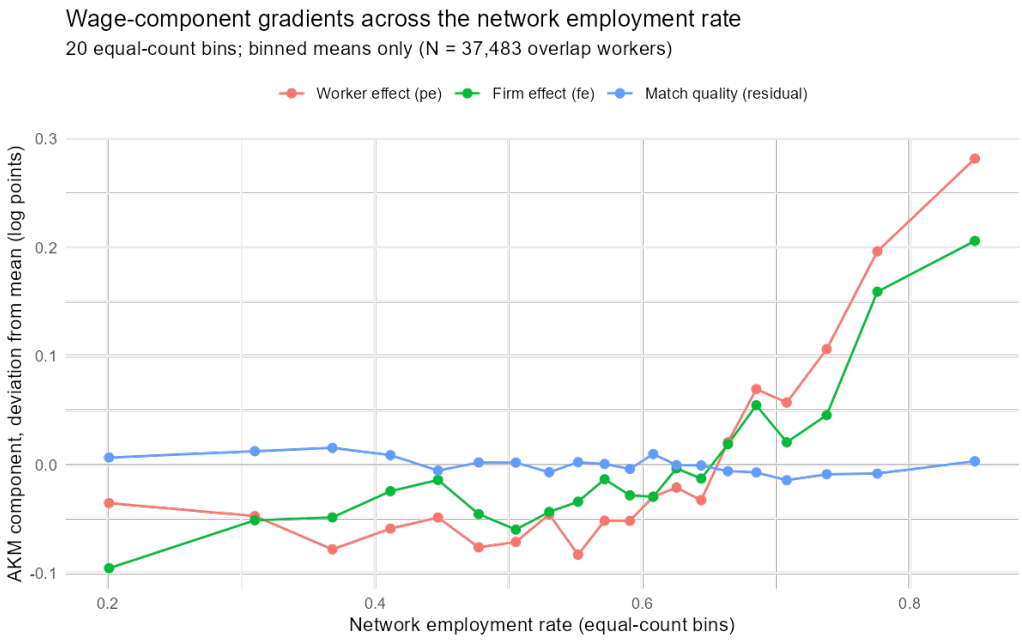


Figure 5: AKM components of the post-displacement wage by network employment rate. Binned means of the worker effect $\hat{\theta}$, the destination-firm effect $\hat{\psi}$, and the match residual $\hat{m}$ across twenty equal-count bins of ER (about 1,874 workers each), demeaned; no functional form imposed. Bins pool workers across closing firms. $N = 37{,}483$.

Appendix Exhibits

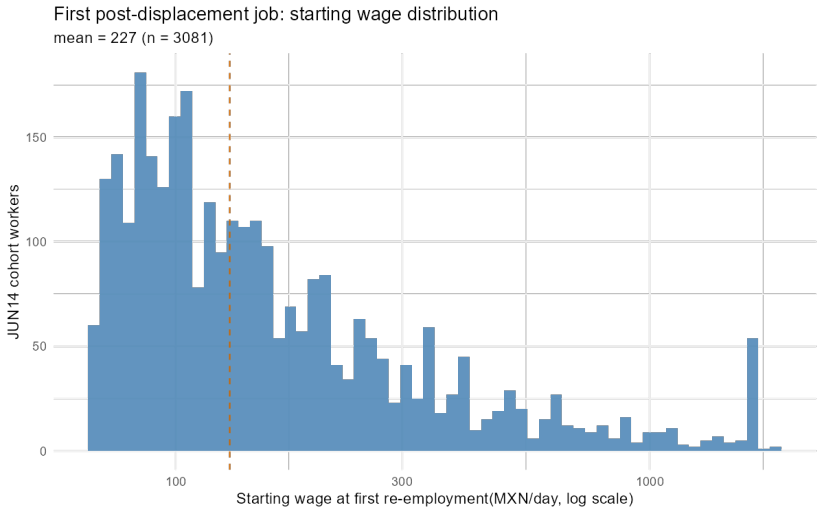


Figure 6: Distribution of the daily wage at the first post-displacement job, June 2014 displacement cohort ($n = 3{,}081$; mean 227 pesos, marked by the dashed line). The horizontal axis is on a log scale. The spike at the right edge is the IMSS contribution-base ceiling, which in 2014 was 1,682 pesos per day and top-codes the wage of workers above it. This is the outcome variable of the within-firm wage regressions in Section 6, for the one cohort whose distribution was exported. Pre-correction sample, see Section 4.

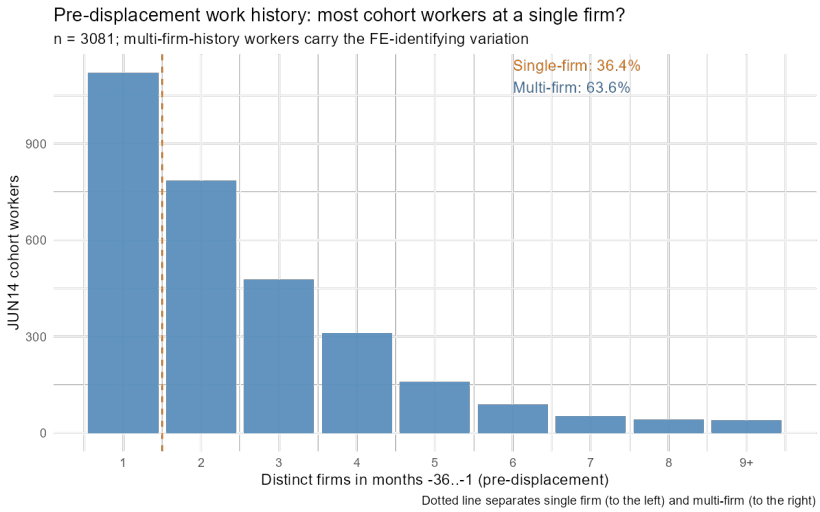
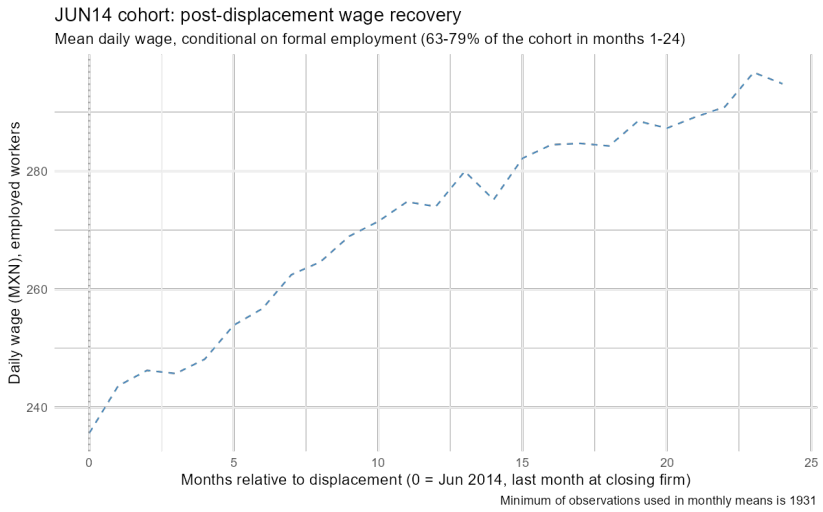
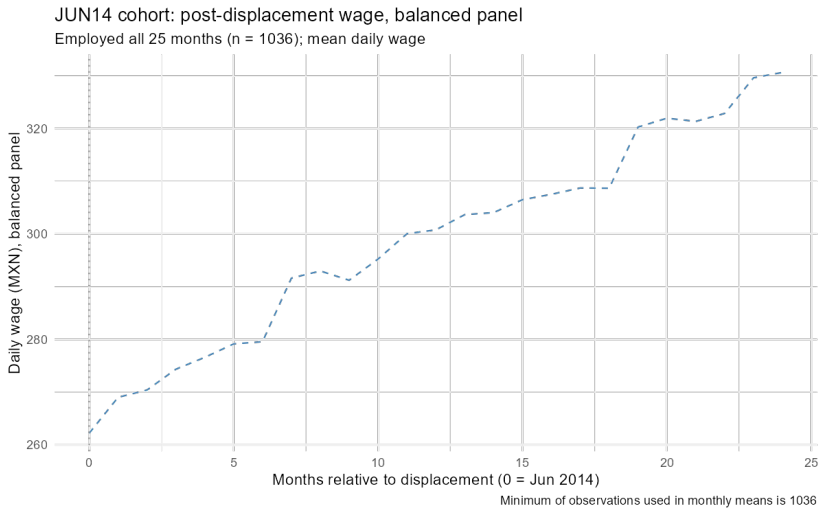


Figure 7: Number of distinct employers in the 36 months before displacement, June 2014 cohort (n = 3,081). 36.4 percent of displaced workers were at the closing firm alone over that window; the remaining 63.6 percent also worked elsewhere and therefore carry former coworkers the rest of their co-displaced colleagues do not. That second group is the source of the within-firm variation in network quality that identifies equation (3). Pre-correction sample, see Section 4.



(a) All workers observed in formal employment



(b) Balanced panel: workers employed in every month

Figure 8: Post-displacement wage path for the June 2014 displacement cohort. Panel (a) plots the mean daily wage of cohort members observed in formal employment in each month after the closure; between 63 and 79 percent of the cohort is employed in any given month, so the composition changes month to month and early re-entrants need not resemble later ones. Panel (b) restricts to the balanced panel of workers employed in every one of the 25 months ($n = 1{,}036$). In both, the mean wage of the re-employed exceeds its pre-displacement level within the first months and continues to rise. This is a wage concept conditional on formal employment; it is not inconsistent with the slower *earnings* recovery documented by Puggioni et al. (2022), which incorporates months spent outside formal employment and workers who never return. Both panels are for the June 2014 cohort only and rest on the pre-correction sample described in Section 4.

Dependent Variables:	Months unemployed					Post-displacement wage				
Sector	Full sample	Comercio	Manufactura	Otro	Servicios	Full sample	Comercio	Manufactura	Otro	Servicios
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Variables										
Network employment rate	-0.03	-0.09	-0.37	-0.47	0.64**	21.5***	2.2	44.6**	31.5**	7.5
	(0.16)	(0.34)	(0.40)	(0.30)	(0.26)	(7.3)	(18.3)	(22.1)	(12.3)	(11.3)
Fixed-effects										
Closing firm	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics										
Observations	37,860	6,923	5,022	12,637	13,278	37,860	6,923	5,022	12,637	13,278

Clustered (Closing firm) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 8: Network ER effect by industry of the closing firm, within the closing firm. Each column re-estimates equation (3) on the subsample of workers displaced from firms in the named sector; columns (1)–(5) use months unemployed and (6)–(10) the post-displacement daily wage as the outcome. "Otro" groups the remaining sectors, chiefly construction and transport. Only the ER coefficient is reported; the specification includes the full control set. Standard errors clustered by closing firm.

Dependent Variables:	Months unemployed					Post-displacement wage				
region	Full sample	Centro	Centro-Norte	Norte	Sur	Full sample	Centro	Centro-Norte	Norte	Sur
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Variables										
Network employment rate	-0.03	-0.12	0.23	0.10	-0.41	21.5***	31.0**	22.4*	18.4	-0.83
	(0.16)	(0.26)	(0.36)	(0.30)	(0.40)	(7.3)	(12.6)	(13.6)	(16.0)	(15.8)
Fixed-effects										
Closing firm	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics										
Observations	37,860	13,491	8,572	10,027	5,770	37,860	13,491	8,572	10,027	5,770

Clustered (Closing firm) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 9: Network ER effect by region of the closing firm, within the closing firm. Regions follow the Bank of Mexico's four-region division of the states. Same specification and reporting as Table 8.

Panel A. Cross-firm, eq. (2)

Dependent Variables:	Months unemployed	Post-displacement wage
Model:	(1)	(2)
Variables		
Network employment rate	-8.8***	29.9**
	(0.34)	(12.6)
Log (network size)	-0.47***	-2.9**
	(0.04)	(1.4)
Longest coworker overlap (months)	-0.008***	-0.06
	(0.001)	(0.05)
Age	0.0005	0.43***
	(0.003)	(0.12)
Female	-0.15***	-3.7**
	(0.04)	(1.6)
Wage at displacement (100 MXN)	-0.03***	85.2***
	(0.005)	(0.72)
Years in formal sector	0.003	-0.24*
	(0.003)	(0.12)
ER x Log(network size)	0.84***	4.0
	(0.06)	(2.5)
Fixed-effects		
Industry FE (pre)	Yes	Yes
Municipality FE (pre)	Yes	Yes
Displacement-month FE	Yes	Yes
Fit statistics		
Observations	37,860	37,860
R^2	0.13763	0.78624

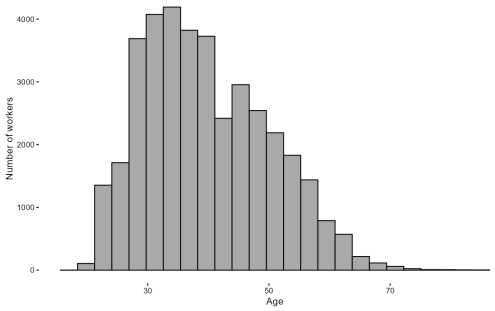
Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Panel B. Within the closing firm, eq. (3)

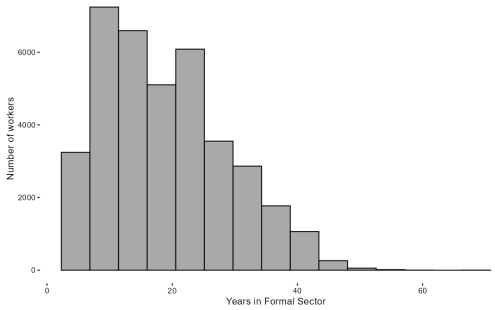
Dependent Variables:	Months unemployed	Post-displacement wage
Model:	(1)	(2)
Variables		
Network employment rate	-0.25	13.3
	(0.46)	(19.3)
Log (network size)	-0.04	0.08
	(0.05)	(2.0)
Longest coworker overlap (months)	-0.001	0.009
	(0.001)	(0.06)
Age	0.006**	0.22*
	(0.003)	(0.11)
Female	-0.007	-1.6
	(0.04)	(1.8)
Wage at displacement (100 MXN)	-0.02**	86.2***
	(0.008)	(1.4)
Years in formal sector	-0.003	-0.07
	(0.003)	(0.11)
ER x Log(network size)	0.04	1.5
	(0.08)	(3.3)
Fixed-effects		
Closing firm	Yes	Yes
Fit statistics		
Observations	37,860	37,860
R^2	0.55235	0.86918

Clustered (Closing firm) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

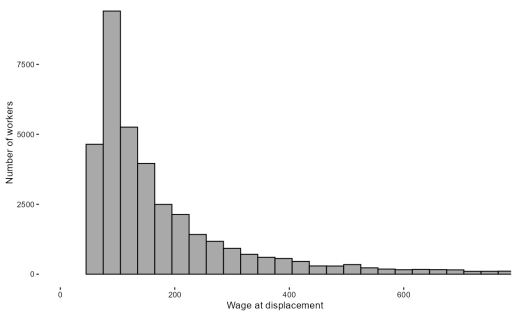
Table 10: Network ER interacted with log network size. Panel A adds the interaction to the cross-firm specification (heteroskedasticity-robust standard errors); Panel B to the closing-firm fixed-effects specification (standard errors clustered by closing firm). The wage-at-displacement control enters in hundreds of pesos.



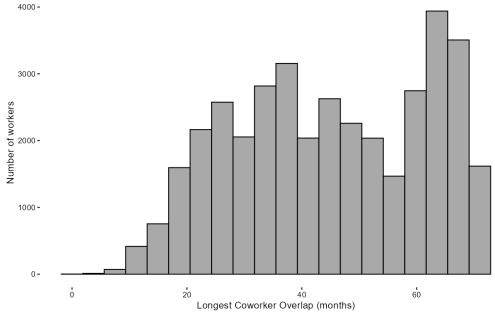
(a) Age at displacement



(b) Years in the formal sector



(c) Daily wage at displacement



(d) Longest coworker overlap

Figure 9: Sample distributions of the four worker characteristics that enter the regressions as controls, for the 37,860 displaced workers of the analysis sample. Panel (c) is truncated near 800 pesos; the IMSS contribution ceiling was 1,682 pesos per day in 2014, so a top-coded mass above the plotted range is not shown. Panel (d) heaps at 60 months, which is the length of the 2010–2014 window over which coworker overlap is measured and therefore a censoring point rather than a feature of tenure. Pre-correction sample, see Section 4.

Dependent Variables:	Months unemployed					Post-displacement wage				
Sector	Full sample	Comercio	Manufactura	Otro	Servicios	Full sample	Comercio	Manufactura	Otro	Servicios
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Variables										
Network employment rate	-4.6***	-3.9***	-4.3***	-4.1***	-4.2***	49.6***	-8.9	68.1***	66.7***	42.8***
	(0.12)	(0.30)	(0.30)	(0.25)	(0.20)	(4.9)	(12.7)	(13.5)	(9.3)	(8.9)
Fixed-effects										
Industry FE (pre)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE (pre)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Displacement-month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics										
Observations	37,860	6,923	5,022	12,637	13,278	37,860	6,923	5,022	12,637	13,278

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 11: Network ER effect by industry of the closing firm, cross-firm specification. Same splits as Table 8, estimated with equation (2) (municipality, industry, and displacement-month effects; heteroskedasticity-robust standard errors).

Dependent Variables:	Months unemployed					Post-displacement wage				
region	Full sample	Centro	Centro-Norte	Norte	Sur	Full sample	Centro	Centro-Norte	Norte	Sur
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Variables										
Network employment rate	-4.6***	-4.6***	-4.6***	-4.7***	-4.6***	49.6***	68.1***	24.1***	37.2***	42.8***
	(0.12)	(0.20)	(0.26)	(0.25)	(0.33)	(4.9)	(8.2)	(8.6)	(10.7)	(11.6)
Fixed-effects										
Industry FE (pre)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE (pre)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Displacement-month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics										
Observations	37,860	13,491	8,572	10,027	5,770	37,860	13,491	8,572	10,027	5,770

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 12: Network ER effect by region of the closing firm, cross-firm specification. Same splits as Table 9, estimated with equation (2).