

Enforcement Spillovers and the Efficiency Costs of Means-Tested Transfers: Evidence from Workplace Networks

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Joan Vilá, Marcelo Bergolo y Guillermo Cruces*

Resumen

Las evaluaciones estándar de los programas de transferencias suelen asumir que la fiscalización de la elegibilidad afecta únicamente a los hogares directamente expuestos a la regla. En este trabajo, mostramos que este enfoque es incompleto cuando los eventos de suspensión transmiten información a través de redes laborales, induciendo a otros beneficiarios a ajustar su empleo formal e ingresos declarados. Estudiamos el programa de transferencias condicionadas AFAM-PE de Uruguay, en el cual la elegibilidad de ingresos se verifica mensualmente utilizando registros administrativos de ingresos formales, y los hogares que superan el umbral pueden perder el beneficio, mientras que la mayoría de los beneficiarios desconoce en gran medida la regla de ingreso. Vinculando registros del programa, historias laborales de la seguridad social e identificadores de empresas, construimos redes laborales definidas seis meses antes de la exposición de cada par (peer) a la fiscalización, e identificamos efectos de contagio (spillovers) mediante un diseño de regresión discontinua (fuzzy RD) que explota la variación cuasi-aleatoria generada por el umbral de ingresos del programa. Los compañeros de trabajo de pares que se ubican justo por encima del punto de corte reducen sus ingresos laborales registrados en un 20%, y la estimación fuzzy-RD correspondiente implica una caída de aproximadamente un tercio de los ingresos de base en respuesta a la suspensión de un par. Estos efectos surgen dentro de los dos meses y persisten durante al menos 12 meses. La respuesta opera casi en su totalidad en el margen extensivo del empleo formal, y la mayor parte de la caída refleja transiciones hacia el trabajo informal más que una salida de la fuerza laboral. Los efectos de contagio se concentran en empresas pequeñas y entre compañeros similares: son casi tres veces mayores cuando el par suspendido contactó activamente al servicio de atención del programa. Este patrón es consistente con la transmisión de información como canal principal. Incorporar estas respuestas indirectas reduce el MVPF estimado del programa de 0.53 a 0.36. Dado que el mismo canal de información opera en todos los programas con verificación de medios a través de registros de ingresos formales, las evaluaciones que solo contabilizan a los hogares directamente expuestos subestiman sistemáticamente el costo de la fiscalización.

Palabras clave

Asistencia social, verificación de ingresos, efectos de contagio de la fiscalización, efectos de pares, redes laborales, informalidad, MVPF

Clasificación JEL

H53, H75, I38, J13, J22, J46, O17

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Abstract

Standard welfare evaluations of means-tested transfers typically treat eligibility enforcement as affecting only households directly exposed to the rule. We show that this direct-only approach is incomplete when enforcement events transmit information through workplace networks, inducing other beneficiaries to adjust formal employment and reported earnings. We study Uruguay's AFAM-PE conditional cash transfer, in which household income eligibility is verified monthly using administrative records of formal earnings and households exceeding the threshold may lose benefits, while most beneficiaries are largely unaware of the income rule. Linking program records, social security employment histories, and firm identifiers, we build workplace networks defined six months before each peer's exposure to enforcement and identify spillovers using a fuzzy regression discontinuity design that exploits quasi-random variation in whether a peer crosses the income threshold. Coworkers of peers just above the cutoff reduce registered labor earnings by 20%, and the corresponding fuzzy-RD estimate implies a decline of about one-third of baseline earnings in response to a peer's suspension. These effects emerge within two months and persist for at least 12 months. The response operates almost entirely on the extensive margin of formal employment, and most of the decline reflects transitions to informal work rather than exit from the labor force. Spillovers are concentrated in small firms and among similar coworkers, are nearly three times larger when the suspended peer actively contacted the program's support service, and are absent among non-beneficiary coworkers. This pattern is consistent with information transmission as the main channel. Incorporating these indirect responses lowers the estimated MVPF of the program as implemented with enforcement from 0.53 to 0.36. Because the same information channel operates wherever means-tested programs are enforced through records of formal earnings, evaluations that count only directly exposed households systematically understate the cost of enforcement.

Keywords

social assistance, income-testing, enforcement spillovers, peer effects, workplace networks, informality, MVPF.

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Abstract

Standard welfare evaluations of means-tested transfers typically treat eligibility enforcement as affecting only households directly exposed to the rule. We show that this direct-only approach is incomplete when enforcement events transmit information through workplace networks, inducing other beneficiaries to adjust formal employment and reported earnings. We study Uruguay's AFAM-PE conditional cash transfer, in which household income eligibility is verified monthly using administrative records of formal earnings and households exceeding the threshold may lose benefits, while most beneficiaries are largely unaware of the income rule. Linking program records, social security employment histories, and firm identifiers, we build workplace networks defined six months before each peer's exposure to enforcement and identify spillovers using a fuzzy regression discontinuity design that exploits quasi-random variation in whether a peer crosses the income threshold. Coworkers of peers just above the cutoff reduce registered labor earnings by 20%, and the corresponding fuzzy-RD estimate implies a decline of about one-third of baseline earnings in response to a peer's suspension. These effects emerge within two months and persist for at least 12 months. The response operates almost entirely on the extensive margin of formal employment, and most of the decline reflects transitions to informal work rather than exit from the labor force. Spillovers are concentrated in small firms and among similar coworkers, are nearly three times larger when the suspended peer actively contacted the program's support service, and are absent among non-beneficiary coworkers. This pattern is consistent with information transmission as the main channel. Incorporating these indirect responses lowers the estimated MVPF of the program as implemented with enforcement from 0.53 to 0.36. Because the same information channel operates wherever means-tested programs are enforced through records of formal earnings, evaluations that count only directly exposed households systematically understate the cost of enforcement.

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1 Introduction

Welfare evaluations of means-tested transfer programs typically measure behavioral labor supply responses among households directly exposed to eligibility enforcement (Moffitt, 2002; Chan and Moffitt, 2018; Banerjee et al., 2017). This direct-effects approach is incomplete when the enforcement of one household’s eligibility changes the behavior of others who observe it. The fiscal and welfare cost of enforcement then depends not only on the sanctioned household’s response, but also on the indirect responses of those exposed to the sanction. We show that these indirect responses, operating through workplace networks, can be large enough to materially change a program’s welfare evaluation.

In settings where income eligibility is verified using administrative records of formal earnings, beneficiaries who exceed a threshold can lose their benefits. Through workplace interactions, these enforcement events can become visible to coworkers, who may transmit information to other beneficiaries facing similar income-reporting incentives. If beneficiaries imperfectly understand the income rule and its enforcement, a peer’s suspension can act as an informative shock that updates other beneficiary coworkers’ beliefs about the cost of formal earnings. Whether such information transmission generates labor market responses, and how these responses compare with the direct effects used in standard cost-benefit calculations, has not been quantified for means-tested transfer programs, despite its importance for welfare evaluation (Dahl and Loken, 2024). The question is most consequential in settings where formal and informal employment coexist and workers can adjust along the formality margin (Levy and Schady, 2013; Ulyssea, 2018; Bergolo and Cruces, 2021), a setting that characterizes many means-tested programs in middle- and low-income countries.

We answer this question using Uruguay’s AFAM-PE, a conditional cash transfer for low-income families with children. Two features of the program are central to our design. First, income eligibility is verified monthly using administrative records of formal earnings, and households whose verifiable per capita income exceeds a threshold may lose benefits. Second, survey evidence shows that beneficiaries are largely unaware of the income rule even though they know the health and school-attendance requirements, creating scope for learning from observed enforcement events. We link administrative records on AFAM-PE participation, so-

cial security employment histories, and firm identifiers, define workplace networks six months before each peer’s exposure to enforcement, and identify spillovers on coworkers using a fuzzy regression discontinuity design that exploits quasi-random variation in whether a peer crosses the income threshold.¹

We find substantial spillover effects of income-eligibility enforcement on coworkers’ labor market behavior. Coworkers of peers just above the income cutoff reduce their registered labor earnings by 20% relative to those just below, equivalent to about one-third of baseline earnings when scaled by the first stage. The effects emerge within two months and persist for at least 12 months. The response operates almost entirely on the extensive margin of formal employment, and evidence suggests that most of the decline in formal employment reflects transitions to informal work rather than exit from the labor force. This formal-to-informal margin is the response one would expect from an information shock about a rule that monitors only verifiable formal earnings (Kleven et al., 2011), and it implies that the spillover partly reflects strategic earnings concealment rather than pure labor supply withdrawal.

Several pieces of evidence point to information transmission as the channel. Spillovers are concentrated in small firms and among similar coworkers, where workplace interactions are more frequent and substantive. They are nearly three times larger when the suspended peer actively contacted the program’s support service to inquire about the suspension. They are stronger among coworkers whose own household income places them closer to the threshold, where information about enforcement is more valuable. Non-beneficiary coworkers matched on observable characteristics show no significant responses, a pattern more consistent with information transmission than with social influence or common firm shocks. These labor market responses feed back into program outcomes. Coworkers exposed to a suspended peer become 9 percentage points (p.p.) less likely to be suspended themselves in subsequent months (14 p.p. in the 2SLS estimation), reflecting the decline in their verifiable family income.

¹This empirical setting allows us to address the challenges typically associated with studying spillovers in social assistance programs (Dahl, 2020). First, coworkers constitute a naturally occurring peer group (e.g., De Giorgi et al. 2019). Second, quasi-exogenous variation in peers’ exposure to income-eligibility enforcement (while beneficiary coworkers are not directly exposed to the cutoff) helps address identification concerns related to correlated unobservables. Finally, because workplace networks are defined before enforcement occurs, the design also mitigates concerns about endogenous group membership (Manski, 1993).

To assess the welfare consequences of these responses, we use the marginal value of public funds (MVPF) framework (Hendren and Sprung-Keyser, 2020; Finkelstein and Hendren, 2020). Considering only direct effects, the MVPF is 0.53, close to existing estimates for Uruguay’s AFAM-PE (Bergolo and Cruces, 2021). The indirect responses are economically meaningful: by lowering the estimated MVPF from 0.53 to 0.36, spillover-induced fiscal externalities show that analyses focused only on direct responses substantially understate the program’s efficiency costs. Importantly, our MVPF estimate captures the value of the AFAM-PE as implemented with enforcement, where enforcement events are constitutive of how the program operates, and spillovers are the behavioral responses to the program in a limited-information environment. From a policy perspective, better communication could reduce learning-driven spillovers and move the MVPF closer to the direct-effects benchmark of 0.53, although the exact effect would depend on how information changes beneficiaries’ behavior. At the same time, it could also make the income threshold more salient and increase avoidance responses among beneficiaries. Communication should therefore be viewed as a policy instrument whose effects on both compliance and avoidance need to be evaluated rather than assumed.

Our study of workplace spillovers and the analysis of the enforcement of means-tested transfers contribute to three related literatures. First, the literature on behavioral responses to means-tested transfers (Moffitt, 2002; Chan and Moffitt, 2018; Banerjee et al., 2017; Garganta and Gasparini, 2015; Bergolo and Cruces, 2021) has focused on the direct effects of eligibility mechanisms; we show that these capture only part of the total labor market distortion induced by enforcement. Second, the study is also directly connected to the literature on spillovers in social programs. While this literature often focuses on how social interactions affect program take-up across neighborhoods (e.g., Aizer and Currie 2004; Bobba and Gignoux 2019), families (e.g., Dahl and Gielen 2021), and workplace networks (e.g., Dahl et al. 2014; Mora-Garcia and Rau 2023), we show that spillovers from social programs also extend to labor market and earnings-reporting behavior, with important welfare implications. Our paper is close to Gerard et al. (2024), who study the spillover effects of a cash transfer program in Brazil on local labor markets and economic activity. Unlike their paper, which focuses on individuals not directly affected by the program, our study examines spillovers

transmitted through workplace interactions among program beneficiaries. Our analysis is also related to [Jinkins et al. \(2025\)](#), who show that welfare benefit cuts spill over to neighbors’ criminal behavior and raise the MVPF of welfare programs by 20%. We document a related pattern in a different network and outcome margin: indirect responses can materially change the welfare evaluation of transfer programs. Third, our study is linked to the literature on enforcement and compliance spillovers in taxation and social programs ([Rinke and Traxler, 2011](#); [Pomeranz, 2015](#); [Kleven et al., 2011](#); [Drago et al., 2020](#)). The closest related works on social programs are [Brollo et al. \(2020\)](#), who study learning spillovers in compliance with school conditionalities, and [Chetty et al. \(2013\)](#), who study neighborhood knowledge of the EITC schedule. We contribute to this type of analysis by covering income-eligibility enforcement and the labor-market and earnings-reporting responses to it, as well as the role of workplace networks in these enforcement externalities.

The remainder of the paper proceeds as follows. Section 2 describes the AFAM-PE program and the information environment that beneficiaries face. Section 3 details the identification strategy, data, and econometric specification. Section 4 presents the main empirical results: the spillover effect of a peer’s benefit suspension on coworkers’ registered earnings and formal employment, and the downstream effect on coworkers’ own compliance with the income rule. Section 5 then quantifies the welfare implications of these spillovers using the marginal value of public funds framework, after estimating the direct response of exposed peers that serves as the benchmark for a standard direct-only evaluation. Section 6 examines why these spillover responses arise, presenting evidence on workplace interaction, information transmission, and alternative channels. Section 7 concludes.

2 Institutional Setting: Income Testing, Enforcement, and Information

2.1 Uruguay and the AFAM-PE Program

Uruguay is an upper-middle-income country with a long-established social protection system. Access to most social insurance benefits depends on formal employment, defined as

employment registered with the Social Security Administration (SSA), with employers and employees paying the corresponding taxes and contributions. Formal workers are entitled to benefits such as health insurance, unemployment insurance, and pensions. During the period we study, however, around 20% of workers were informally employed. Informality can arise either because workers are employed by unregistered firms or because formal firms hire some workers off the books (Ulyssea, 2018). In addition, some registered workers may receive part of their compensation informally, through “payments under the table” (Feinmann et al., 2025). This coexistence of formal and informal work is central to our setting, since AFAM-PE eligibility is monitored using only verifiable formal income.

The AFAM-PE program (in Spanish, *Asignaciones Familiares del Plan de Equidad*, Law 18.227) is a means-tested cash transfer program for low-income families with children, implemented in Uruguay in 2008. The program includes health and school attendance requirements for children in beneficiary households and represents one of the main income-support programs for families with children. In 2018, it covered almost 380,000 children under age 18, or 44% of all children in that age group, with a budget of about 0.3% of GDP.

2.2 Eligibility Rules, the Income Notch, Monitoring and Sanctions

Eligibility for AFAM-PE is based on two criteria. First, households must pass a proxy means test, based on a poverty score constructed from socioeconomic characteristics reported at application. This score is typically computed only when households apply and is not continuously updated during program participation.² Second, households must satisfy an income test. The relevant income concept includes registered labor earnings, as reported by employers to tax and social security authorities, and pension income observed in administrative records. We refer to this measure as verifiable income because it corresponds to the income sources that program authorities can observe and monitor automatically. Informal earnings are reported at application but are not part of the continuous income-eligibility checks.

The income threshold is defined in per capita terms and varies with family size. A household remains eligible as long as its verifiable monthly per capita income is below the relevant

²The score is based on a model predicting whether a household belongs to the bottom quintile of per capita income, using socioeconomic variables from the application form; see Amarante and Vigorito (2011) and Bergolo and Cruces (2021) and the discussion of the institutional context in Appendix A.

threshold. This feature is central to our research design for three reasons. First, income testing is the criterion used by the administration to monitor eligibility during participation. Second, income eligibility is checked monthly using updated administrative records. Third, the income threshold creates a notch in the household budget set: benefits are not gradually withdrawn as earnings rise, but may be suspended when verifiable income exceeds the threshold. This generates incentives to adjust formal labor supply or earnings reporting.

Benefit amounts depend on the number of children in the household, their school level, and disability status. During 2008–2012, the transfer was relatively generous: for the average AFAM household, it represented about 68% of the minimum wage and 48% of the poverty line (Bergolo and Cruces, 2021). Importantly, conditional on remaining below the income threshold, the benefit amount does not depend on household earnings. Thus, beneficiaries can increase formal income without any reduction in benefits up to the cutoff, but crossing the income threshold may trigger a discrete loss of benefits.³

The AFAM-PE administration conducts automatic monthly checks of income eligibility. When a household’s verifiable monthly per capita income exceeds the threshold, the household receives a sanction. The first sanction is a benefit suspension, which can last up to three months. During this period, income verification continues, and households can automatically re-enter the program if their updated verifiable income falls below the threshold. If the household remains noncompliant for more than three consecutive months, participation is terminated and the household must reapply to receive benefits again. Households can also be sanctioned for failing to comply with health or school attendance requirements, but income noncompliance is the most relevant sanction for our empirical analysis.

2.3 Beneficiary Information and Scope for Learning

A key feature of the institutional setting is that communication about the income rule is limited. Beneficiary households receive no warning before benefits are suspended, and official communications do not clearly state the reason for noncompliance or how to remedy the situation. Official websites provide information on school attendance and health condition-

³The AFAM-PE program shares this feature with most cash transfer programs in Latin American countries, though not with welfare programs in developed countries, where benefits are reduced as beneficiaries’ earnings increase.

alities, but little detail on the income test, how verifiable income is constructed, or how the rule is enforced. Survey evidence is consistent with this opacity: in the AFAM-PE Follow-Up Survey,⁴ 61% of respondents knew about school or health conditionalities, but only 9% reported knowing that eligibility was subject to other requirements, including the income requirement. Moreover, 29% believed that AFAM-PE eligibility did not entail any requirements. This limited knowledge creates scope for enforcement events to transmit information to other beneficiaries.

Administrative records show that income eligibility was actively enforced during the period of analysis. Appendix Figure A.1 reports monthly suspensions between 2009 and 2015. Suspensions occur in most months, affecting between 0.5% and 6% of beneficiary households. Income-related suspensions constitute a sizable and persistent component of overall benefit suspensions, indicating that the income requirement was actively monitored during the study period, although other program requirements also generated a non-negligible share of suspensions.

3 Research Design

We study spillover effects arising from the enforcement of AFAM-PE income-eligibility requirements on beneficiaries and their families within workplace networks. This section describes the source of variation used for identification, the data and sample construction, the econometric approach, and the main threats to identification.

3.1 Identification: Quasi-random Eligibility

To identify spillover effects of income-eligibility enforcement on beneficiaries' coworkers, the ideal experiment would randomly assign beneficiary peers to experience enforcement—specifically, to have their benefits suspended—or not. This would allow us to estimate the causal effect of peers' exposure to eligibility enforcement on coworkers' labor market behavior.

⁴The survey, *AFAM-PE Follow-Up Survey (2011–2013)*, was designed and implemented by researchers at the Instituto de Economía (IECON) of the Universidad de la República (UDELAR), in collaboration with MIDES and researchers at the Institute of Statistics and at the Department of Sociology at UDELAR (Amarante and Vigorito, 2011). Data from this survey were used in previous research by some of the authors of this article (see, e.g., Bergolo and Cruces 2021).

In our setting, identification instead leverages a discontinuity generated by the AFAM-PE income-eligibility rule.⁵

During program participation, AFAM-PE families must keep their verifiable monthly per capita household income below a specified threshold to remain eligible. This rule creates a discontinuous change in the probability of benefit suspension at the eligibility cutoff (see Figure 1). Beneficiaries whose household verifiable income is slightly below the threshold remain eligible for AFAM-PE, whereas those whose income is slightly above it face a higher probability of suspension. We argue that this arbitrary threshold generates quasi-random variation in eligibility status within a narrow window around the cutoff, which we use as an instrument for peers' benefit suspension.

Panel (a) of Figure 1 illustrates the identification strategy. It plots the probability of benefit suspension against household verifiable income Z . The dashed vertical line denotes the income-eligibility threshold $\bar{Z}$. Beneficiaries with household income at or below $\bar{Z}$ are income-eligible, whereas those with income above $\bar{Z}$ are income-ineligible. Thus, eligibility status changes discontinuously at the threshold. Our quasi-random variation comes from beneficiaries whose income lies within a narrow window around the cutoff, $[\bar{Z} - \Delta, \bar{Z} + \Delta]$, where 2Δ denotes the window size.

To see how we use this variation, consider a group of AFAM-PE beneficiaries working in the same firm. Some beneficiaries, whom we label *peers*, have household income within the window around the threshold at event time τ , and therefore face quasi-random eligibility. Other beneficiaries in the same firm, whom we label *coworkers*, have household income below $\bar{Z} - \Delta$ at τ and are therefore eligible without being directly exposed to the cutoff. We use the quasi-random variation in peers' eligibility within the window as an instrument for peers' exposure to income-eligibility enforcement. This local variation allows us to estimate, using an RD approach, the effect of peers' enforcement experience on the labor market behavior of coworkers located farther from the cutoff.

A technical challenge in this setting is that a coworker may be linked to multiple peers within the same firm. This many-to-one structure complicates the assignment of a single

⁵Among others, [Dahl et al. \(2014\)](#) and [Mora-Garcia and Rau \(2023\)](#) exploit quasi-random variation in group assignment to analyze peer effects in program participation, while [Dahl and Gielen \(2021\)](#) follows this strategy to estimate intergenerational spillovers in disability insurance.

running variable and instrument in the RD design, especially when a coworker is connected to peers on both sides of the income-eligibility threshold. To avoid this ambiguity, we follow an approach similar to previous studies (e.g., [Dahl et al., 2014](#)) and restrict our main analysis to firms in which exactly one peer falls within the income window.

3.2 Data and Workplace Networks

This study combines four administrative data sources from Uruguay, linked through a unique masked individual identifier (*Cédula de Identidad*).⁶ We complement these records with a follow-up survey of AFAM-PE beneficiaries. The linked data allow us to identify program beneficiaries, reconstruct workplace networks, measure labor market outcomes, observe program suspensions, and study whether information about enforcement diffuses through beneficiary interactions.

AFAM-PE Administrative Records. We use monthly administrative records for the universe of AFAM-PE beneficiary households and their members over 2008–2022. These records include socio-demographic information collected at application, household characteristics, the poverty score used for program targeting, benefit amounts, the family’s verifiable monthly per capita income while eligible, and the threshold used to determine income eligibility. They also identify monthly compliance status, benefit suspensions and terminations, including the reason for noncompliance. We use these data to define the universe of beneficiaries, construct predetermined household and individual covariates, identify benefit suspensions due to income noncompliance, and build the main analysis sample described below.

Social Security Records. We link AFAM-PE records to monthly employer–employee data from Uruguay’s Social Security Administration (SSA, *Banco de Previsión Social*) for 1997–2014. These records cover the universe of registered workers and contain worker-firm employment histories, including monthly earnings, hours and days worked, contract characteristics, hiring and separation dates, and firm identifiers. We use these data to construct workplace networks, define peers and coworkers within firms, and measure the main labor

⁶This ID is issued at birth and corresponds to the main national identification number used for tax, social security records, and other public services. We access a masked number that enables us to link the databases without revealing individual identities.

market outcomes. Total labor earnings are defined as the sum of all registered labor-related income in a month. Formal employment is an indicator for positive registered labor earnings, so zeros include unemployment, inactivity, retirement, or informal employment.

Income Tax Records. We also use individual-level administrative records from Uruguay’s Tax Authority (*Dirección General Impositiva*, DGI), covering income tax filings and third-party reports for the population of registered workers from 2009 to 2016. Together with SSA records, these data allow us to reconstruct each household’s verifiable monthly per capita income, which is the running variable in our RD design. We construct this measure using the administrative protocol applied by AFAM-PE authorities, rather than relying directly on AFAM-PE records, because the latter only report income while households remain active beneficiaries. For details on the construction of verifiable income and the implications of measurement error, see Appendix I.

Beneficiary Support Service Records. To study information-seeking behavior after enforcement, we use records from MIDES’ Beneficiary Support Service (BSS), which cover the universe of inquiries received between 2011 and 2014. These records identify the date, channel, and topic of each contact with the support service. We use them to measure whether suspended beneficiaries contacted MIDES after losing benefits, which provides a proxy for information acquisition about AFAM-PE rules and enforcement.

AFAM Follow-Up Survey. Finally, we complement the administrative records with the AFAM follow-up survey, implemented between September 2011 and February 2013 for a sample of beneficiary households near the poverty-score eligibility threshold. The survey contains detailed household and individual information, including labor market participation outside registered employment. We use it to assess whether reductions in formal employment reflect transitions into informal work or exits from the labor force.

We construct workplace networks linking AFAM-PE beneficiaries who are close to the income-eligibility threshold to other AFAM-PE beneficiaries employed in the same firm.⁷ We

⁷Firms are identified using the legal employer tax identifier available from SSA records. A firm may therefore operate one or multiple physical establishments, implying that coworkers are initially defined at the employer level rather than the establishment level. Appendix B discusses this issue in detail. Additional heterogeneity analyses show that spillover responses are concentrated among single-establishment firms, where direct workplace interactions between firms’ workers are more likely.

refer to the former as *peers* and to the latter as *coworkers*. Appendix B describes the sample construction in detail.

The peer sample includes adult beneficiaries whose household verifiable per capita income first falls within a 20% window around the eligibility threshold between 2009 and 2013. We define this first crossing of the income window as the event. To ensure that workplace links are predetermined, we keep only peers who were continuously employed at the same firm for at least six months before the event and had no prior benefit suspension or termination.

Coworkers are adult AFAM-PE beneficiaries employed in the same firm as the peer before the event, but whose household income is sufficiently below the eligibility threshold and therefore not directly exposed to the cutoff. We also exclude beneficiary coworkers whose households had been suspended before the event. Because a coworker may be linked to multiple peers, our baseline sample restricts attention to firms with exactly one peer in the income window at the relevant event.⁸ This restriction assigns each coworker a unique peer, running variable, and instrument. We then follow peers and coworkers in administrative labor market records for 12 months before and after the event.

The final analysis sample includes 5,136 peers and 8,568 coworkers, distributed across 4,154 firms. Appendix Table B.1 reports descriptive statistics.

3.3 Fuzzy Regression Discontinuity Design

We use a fuzzy RD design that exploits quasi-random variation in peers' income eligibility for households located close to the eligibility cutoff. This allows us to estimate the spillover effects of peers' exposure to income-eligibility enforcement—specifically, benefit suspension for noncompliance—on the behavior of coworkers and their families, whose household income is farther from the cutoff.

We implement the fuzzy RD design using the following two-equation system:

$$Y_{iFt} = \alpha + \gamma PeerSS_{kF\tau} + \beta_1 f(z_{k\tau}) + \beta_2 f(z_{k\tau})1[z_{k\tau} > 0] + x_k + v_{iFt} \quad (1)$$

⁸This restriction ensures a unique assignment of coworkers to peers and therefore to a unique running variable and instrument. The estimated effects should therefore be interpreted as local responses within workplace networks with a uniquely identified exposed peer. Appendix B provides additional details on sample construction and discusses the implications of these restrictions.

$$PeerSS_{kF\tau} = \mu + \delta 1[z_{k\tau} > 0] + \lambda_1 f(z_{k\tau}) + \lambda_2 f(z_{k\tau}) 1[z_{k\tau} > 0] + x_k + \varepsilon_{kF}. \quad (2)$$

Equation (1) relates coworkers' outcomes to whether their peer received a benefit suspension, controlling flexibly for the peer's distance to the cutoff, while equation (2) captures the discontinuity in the likelihood of the peers' benefit suspension at the eligibility cutoff. Specifically, Y_{iFt} is the outcome of interest for AFAM-PE beneficiary i , a coworker employed in firm F , measured t months after event τ . The event is the month in which peer k 's household verifiable income first falls within the window around the eligibility threshold. In both equations, $z_{k\tau}$ denotes peer k 's standardized household verifiable monthly per capita income relative to the cutoff, measured at τ , where peer k works in the same firm F as individual i . The function $f(z_{k\tau})$ controls for the running variable, x_k is a set of predetermined controls, and v_{iFt} is an error term.⁹ The variable $PeerSS_{kF\tau}$ equals one if peer k received a benefit suspension at τ , and zero otherwise. The coefficient γ in equation (1) captures the effect of a peer's benefit suspension due to income noncompliance on coworkers' outcomes.

Because peers' benefit suspension is not randomly assigned, $PeerSS_{kF\tau}$ is instrumented using $1[z_{k\tau} > 0]$, as specified in the first-stage equation (2). This indicator equals one if peer k 's household income is above the eligibility cutoff within the income window, and zero otherwise. The estimation of δ captures the jump in peers' benefit suspension at the cutoff. As shown in Section 3.4, this discontinuity in the income-eligibility rule strongly predicts benefit suspension. The 2SLS estimate of γ can therefore be interpreted as the local average treatment effect of a peer's benefit suspension on coworkers' outcomes, under the standard fuzzy RD assumptions. The key identifying assumption is that beneficiary households cannot precisely manipulate verifiable monthly per capita income around the cutoff. Under this assumption, variation in income eligibility near the threshold is *as good as* random.

We also estimate a reduced-form specification that relates coworkers' outcomes directly

⁹We include the following controls for the peer group: age, education level, and gender of the family member who submitted the AFAM-PE application form, family size, number of children, place of residence, and event-time fixed effects. All these variables, except for the event-time fixed effects, are measured at the time the household applied to the program. In Appendix Section E, we show that results are robust to excluding these controls.

to peers' income eligibility:

$$Y_{iFt} = \omega + \pi 1[z_{k\tau} > 0] + \kappa_1 f(z_{k\tau}) + \kappa_2 f(z_{k\tau}) 1[z_{k\tau} > 0] + x_k + u_{iFt}. \quad (3)$$

The reduced-form estimate of π captures the intention-to-treat effect of peers' income-ineligibility status on coworkers' outcomes.

It is worth noting that, compared to the estimates from the reduced-form equation (3)—which only require the absence of precise manipulation of the running variable—the fuzzy RD design relies on two additional assumptions to consistently estimate the spillover effects of experienced income-eligibility enforcement (Imbens and Lemieux, 2008; Lee and Lemieux, 2010). First, we must assume that the assignment of eligibility status to peers affects coworkers' outcomes only through its effect on the peer's benefit suspension, and not through any other channel. This condition is analogous to the *exclusion restriction* in the instrumental variables framework. Second, we assume *monotonicity*, which in our context means that if a peer with verifiable per capita income Z is not suspended when the cutoff is c , then they would also not be suspended under any higher cutoff $c' > c$. Conversely, if a peer is suspended when the cutoff is c , they must be suspended under any lower cutoff $c' < c$.

Our main outcomes are coworkers' labor market outcomes: total registered labor income, labor income conditional on positive formal earnings (on working, for short), and formal employment. The second outcome captures the intensive margin, while the third captures the extensive margin. We also examine labor market outcomes for adult members of coworkers' households and household-level outcomes, including benefit suspension and household verifiable per capita income. In the baseline specification, outcomes are measured six months after the event τ (i.e., $t = 6$), except when we analyze dynamic effects.

In estimating the RD models, we follow Imbens and Lemieux (2008) and Calonico et al. (2019) and use local linear regressions fitted separately on each side of the cutoff. Following Calonico et al. (2014), we report Mean Squared Error (MSE)-optimal coefficients and standard errors, but rely on bias-corrected estimates for p -values. We implement data-driven bandwidth selection following Calonico et al. (2019), using MSE-optimal bandwidths and triangular kernel weights, so that observations closer to the cutoff receive greater weight.

We cluster standard errors at the firm level, allowing for arbitrary serial correlation in errors among coworkers within a firm over time. Appendix Section E shows that our baseline results are robust to alternative RD implementation choices.

3.4 Relevance of the Instrument and Threats to Identification

First Stage. We begin by verifying the relevance of our instrument by estimating the first-stage equation (2). Panel (b) of Figure 1 presents the graphical evidence, plotting the probability of benefit suspension against the distance to the eligibility threshold for the peer sample. The figure is restricted to the optimal bandwidth following Calonico et al. (2019). Each point represents the average suspension rate in 15 equally-spaced bins on each side of the threshold. This estimate is obtained in the peer sample, in which each peer appears once. The figure shows a sharp 51 p.p. increase in the probability of suspension at the eligibility threshold, statistically significant at the 1% level.¹⁰

Although this confirms the relevance of the instrument, Figure 1 also shows that enforcement is not perfectly sharp: about 20% of households below the cutoff experienced a suspension, while roughly 30% of those above the cutoff did not. Several factors can explain this pattern. First, as discussed in Section 3.2, we do not directly observe the family’s verifiable income—the running variable—in the administrative data. Instead, we reconstruct it using the protocol and administrative records provided by the authorities, which introduces measurement error, as discussed in Appendix Section I. Appendix Figure I.3 provides an indirect validation exercise showing the absence of a discontinuity in prediction errors at the eligibility threshold, which suggests that the imputation procedure is unlikely to generate systematic discontinuities around the cutoff. Second, partial enforcement may reflect administrative delays in applying the income-eligibility rule. Although income eligibility was intended to be monitored monthly, authorities may not always have had up-to-date information on registered labor earnings, especially during the initial implementation period. Finally, some suspensions below the cutoff may reflect discrepancies between the reconstructed income measure used in the analysis and the information available to AFAM-PE administrators during eligibility

¹⁰Appendix Figure E.1 shows that this result is robust to placebo eligibility thresholds, alternative estimation windows, different combinations of optimization algorithms, kernel functions, and polynomial degrees, and different donut radii.

verification. Such discrepancies may arise from administrative timing differences or residual measurement error.

Manipulation. The validity of an RD design requires that peers cannot precisely manipulate the running variable, which in our setting is the household’s verifiable per capita income. Such manipulation could occur, for instance, if household members employed formally adjusted their registered labor earnings to avoid benefit suspension. Although beneficiaries have incentives to adjust their earnings, precise manipulation is unlikely because authorities do not disclose either the exact formula used to construct household verifiable per capita income or the level of the eligibility threshold. Appendix C presents evidence against precise manipulation of the running variable. The distribution of household verifiable per capita income is smooth around the cutoff, and continuity tests based on Cattaneo et al. (2018) and McCrary (2008) do not reject continuity at the threshold. We also find no systematic discontinuities in predetermined peer or coworker characteristics at the cutoff. These results support the validity of the RD design.

Correlated shocks. To consistently estimate spillover effects using the fuzzy RD approach, we must assume that coworkers are affected only through their peers’ exposure to income-eligibility enforcement. A potential concern is that correlated workplace shocks may affect peers and coworkers simultaneously. For example, a positive firm-level earnings shock could make peers more likely to exceed the income threshold while also increasing coworkers’ earnings or probability of formal employment. Such a shock would generate a positive correlation between peers’ benefit suspension and coworkers’ labor market outcomes, rather than the negative relationship we observe. A mean-reverting firm-level shock, however, could generate a negative correlation and therefore confound our estimates.

To assess this concern, Appendix Table C.3 implements a placebo test using non-AFAM-PE coworkers employed in the same firms as the peers. If our estimates were driven by firm-level shocks, similar effects should appear among these workers. The estimated effects on total labor income, labor income conditional on working, and formal employment are small and statistically insignificant. These results suggest that the main spillover estimates are unlikely to be explained by correlated workplace shocks.

Endogenous group membership. A final concern is that workers are not randomly as-

signed to firms or coworker groups. This could bias peer-effect estimates if group membership were itself related to the enforcement shock. In our setting, however, workplace networks are defined before the peer’s exposure to income-eligibility enforcement. Thus, exposure is assigned within predetermined networks, and post-event changes in group membership cannot determine initial exposure. If such changes occur after the peer’s suspension, they are either unrelated to the enforcement shock or part of the behavioral response to it.

4 Main Effects of Eligibility Enforcement

This section presents the main results in two steps. Section 4.1 documents the spillover effect of a peer’s benefit suspension on coworkers’ registered labor earnings and formal employment, including the dynamics of the response and its decomposition into formal-to-informal transitions. Section 4.2 then traces these labor market adjustments through to coworkers’ own future compliance with the AFAM-PE income rule. We defer the direct response of the suspended peers themselves to Section 5, where it serves as the benchmark for the welfare analysis.

4.1 Spillover Effects on Coworkers’ Registered Earnings and Formal Employment

Coworkers of peers just above the income-eligibility cutoff reduce their total registered labor income by 20% relative to coworkers of peers just below the cutoff, measured six months after the event. The 2SLS estimate implies a decline of roughly one-third of baseline earnings in response to a peer’s benefit suspension. The response operates almost entirely on the extensive margin of formal employment and persists for at least 12 months. We document each of these results in turn and then assess whether the decline reflects reduced labor supply or transitions into informal employment.

Graphical Evidence. Figure 2 presents graphical evidence on the reduced-form (ITT) effects of peers’ income eligibility on coworkers’ labor market outcomes: total labor income (Panel (a)), positive labor income (Panel (b)), and the probability of formal employment

(Panel (c)). In this and all subsequent RD figures, the sample is restricted to the optimal bandwidth selected following [Calonico et al. \(2019\)](#). The running variable—peers’ household verifiable per capita income—is normalized to zero at the eligibility threshold. In each figure, the vertical dotted gray line marks the cutoff separating income-eligible peers from income-ineligible peers. The blue and red circles plot local averages for 10 equally-spaced bins on each side of the threshold, together with separate linear fits to illustrate the size of the discontinuity at the cutoff.¹¹ Each figure also reports the RD point estimate, the optimal bandwidth, and the p -value for the continuity test at the threshold. These graphical estimates correspond to the reduced-form results reported in the tables. The local polynomial regressions are estimated using equation (3) and the default technical options described in Section 3.3.

Figure 2, Panel (a), plots coworkers’ total labor income as a function of peers’ normalized household verifiable income. The figure shows a large and statistically significant discontinuity at the eligibility cutoff. Panels (b) and (c) examine whether this effect operates through intensive- or extensive-margin responses, respectively. Panel (b) shows no evidence of a discontinuity in labor income conditional on working. In contrast, Panel (c) shows a sharp decline in coworkers’ probability of formal employment when their peer’s income is just above, rather than just below, the eligibility threshold. This pattern indicates that the spillover effect operates mainly through the extensive margin of formal employment.

Regression Evidence. Table 1 reports the main fuzzy RD results of the effect of peers’ income-eligibility enforcement on coworkers’ labor market behavior. We use the same technical choices as in the RD plots. This and subsequent regression tables report three panels. Panel A presents the local average treatment effects (LATE), obtained from the 2SLS estimation of equation 1. These second-stage estimates rescale the reduced-form (ITT) effects reported in Panel B—and depicted in Figure 2—by the first-stage estimates reported in Panel C, based on equation 2. Across specifications, the first-stage estimates are around 56 p.p., indicating a sizable increase in the probability of benefit suspension due to noncompliance with the family income requirement. This estimate is obtained in the coworker sample, in which each peer–coworker link constitutes one observation; peers with more beneficiary coworkers

¹¹The number of bins is chosen arbitrarily, following the recommendations for visual inference in RD designs discussed in [Korting et al. \(2023\)](#).

therefore receive proportionally greater weight, which shifts the effective composition of the estimation sample near the cutoff relative to the peer sample underlying Figure 1 (51 p.p.).

Column (1), Panel B, shows that coworkers' total registered labor income falls by UY\$1,941 in the reduced-form specification. This effect is statistically significant and represents a 20% reduction relative to the mean of the outcome for the comparison group: coworkers of income-eligible peers whose household verifiable per capita income is close to the cutoff. Panel A reports the corresponding 2SLS estimate, indicating a peer effect of UY\$3,307 that is statistically significant at the 5% level (p -value = 0.023). Thus, coworkers reduce total registered labor income by UY\$3,307 six months after their peer is subject to a benefit suspension. This effect is large relative to the baseline mean.

Column (2) shows a small and statistically insignificant decline in formal labor income among coworkers who remain employed (UY\$134, or 1.2% relative to the comparison group; p -value = 0.931). The corresponding 2SLS estimate is also small and imprecise. In contrast, Column (3) shows a negative and highly statistically significant reduced-form effect on formal employment: the probability of being formally employed falls by 16 p.p., or about 18.7% relative to the reference group (p -value = 0.001). This implies a peer effect estimate of approximately 29 p.p., which is large relative to the baseline mean.

Taken together, these results indicate substantial spillover effects on coworkers' registered labor income in response to peers' exposure to AFAM-PE income-eligibility enforcement. These effects are driven primarily by a large decline in formal employment.

Robustness Checks. Appendix E shows that the main spillover results are robust to alternative specifications and standard RD implementation choices. The estimates are qualitatively similar when we exclude predetermined controls and when we cluster standard errors at the household rather than firm level. The reduced-form effects are also robust to placebo eligibility thresholds, alternative bandwidths, donut RD specifications, and alternative combinations of kernel functions, polynomial orders, and bandwidth-selection procedures. Across these checks, the effects on total labor income and formal employment remain negative and statistically significant, while the intensive-margin estimates remain close to zero.

Effects over Time. The results above focus on spillover effects on coworkers during the

first six months after the event, defined as the month in which the peer’s household verifiable income first falls within the income-eligibility window. Our data also allow us to examine how these labor market responses evolve over time. For beneficiary coworkers, we construct average outcomes from the event month up to each horizon k , for $k = 1, \dots, 12$. We then estimate Equation 3 separately at each horizon and plot the resulting estimates against event time, with $t = 0$ denoting the event month. Because outcomes are defined cumulatively, each estimated coefficient should be interpreted as the cumulative reduced-form (dynamic) effect up to that horizon. Figure 3, Panels (a) through (c), reports results for coworkers’ total labor income, labor income conditional on working, and formal employment, respectively.

Figure 3 reveals several patterns. First, peers’ exposure to income-eligibility enforcement has no significant effect on any outcome in the months before the event. Although this is consistent with the RD design, it also provides reassuring evidence in support of the identification strategy. Second, Panel (a) shows that coworkers exposed to income-ineligible peers experience an immediate decline in total labor income after the event, relative to coworkers exposed to income-eligible peers. This gap is statistically significant and widens over time, reaching its maximum within the first six months after the event. It then stabilizes at a similar magnitude in subsequent months, at approximately 14% relative to the comparison group. Third, Panels (b) and (c) show that the dynamics of total labor income are driven mainly by changes in formal employment. Panel (b) provides no evidence of an effect on labor income conditional on working. In contrast, Panel (c) shows a sharp decline in the probability of formal employment among coworkers exposed to income-ineligible peers. During the first six months after the event, the magnitude of this effect is similar to that for total labor income—about 13% relative to the comparison group—and it remains stable from month 7 onward.

Overall, the dynamic analysis shows that the spillover effects of peers’ income-eligibility enforcement on total registered labor income persist over time, and that these effects are driven primarily by changes in formal employment.

Labor Supply and Informal Employment. The main results show a negative and persistent effect of peers’ income-eligibility enforcement on coworkers’ formal labor income and employment. We next assess whether this decline reflects a reduction in overall labor supply

or a shift from formal to informal employment.

As a first step, we examine whether peers' income-eligibility enforcement affects coworkers' working time in their formal jobs, measured by days and hours worked in the SSA records. Appendix Table D.1 shows no statistically significant effects on either days or hours worked, for all coworkers or for those who remain formally employed. This is consistent with the null effects on the intensive margin of formal employment documented above.

As a second step, we assess whether the decline in formal employment reflects an increase in non-employment or a shift toward informal employment. The SSA records do not allow us to distinguish between these two margins of adjustment, since an exit from registered employment may correspond either to non-employment or to a transition into informal work. To address this limitation, we use information from the AFAM-PE Follow-Up Survey, conducted between 2011 and 2013, which covers the post-event period for a subset of our main sample ("matched survey subsample") and includes self-reported measures of registered employment, informal (unregistered) employment, unemployment, and labor market non-participation for each adult household member.¹²

Appendix Table D.2 shows that the matched survey subsample reproduces the main pattern observed in the administrative data: the decline in total registered labor income is driven primarily by reductions in formal employment. Self-reported survey outcomes provide complementary evidence on the adjustment mechanism and are consistent with a reallocation toward informal employment rather than exit from the labor force. Six months after the event, the probability of informal employment increases by 16 p.p. (p -value = 0.043), while the estimated change in non-employment is smaller and statistically insignificant. Although the matched-survey design does not allow us to track individual transitions directly, these results provide suggestive evidence that the decline in formal employment is driven mainly by movements into informal work rather than exits from the labor force.

¹²We use an exact matching procedure to select one worker from the AFAM-PE Follow-Up Survey for each coworker in the main sample. For details on the matching procedure, see Appendix D.

4.2 Spillover Effects on Compliance with Income Eligibility

The results so far show that AFAM-PE beneficiary coworkers reduce their total registered labor income when peers in the same workplace are exposed to income-eligibility enforcement. This response is consistent with the idea that beneficiaries adjust reported earnings in response to income-testing incentives in order to preserve program eligibility. We next examine whether these labor market responses translate into future changes in coworkers' compliance with AFAM-PE income-eligibility rules.

Figure 4 provides graphical evidence on the reduced-form effects of peers' income eligibility on coworkers' future compliance with AFAM-PE income rules. Panel (a) shows the effect on household verifiable per capita income, while Panel (b) shows the effect on the likelihood of benefit suspension due to income noncompliance. Panel (a) shows a clear decline in household verifiable per capita income for coworkers exposed to income-ineligible *vs.* income-eligible peers six months earlier. The estimated discontinuity is UY\$435, implying a 16.3% reduction relative to the comparison group, and is statistically significant at conventional levels (p -value = 0.025). Panel (b) shows a corresponding decline in the likelihood of benefit suspension. The estimated effect is approximately 9 p.p. and statistically significant (p -value = 0.005), representing a large decline relative to the comparison group.

Table 2 reports the corresponding fuzzy RD estimates. Panel A, columns (1) and (2), shows sizable peer effects on household verifiable per capita income and the likelihood of benefit suspension, obtained by rescaling the reduced-form estimates in Panel B by the first-stage estimates in Panel C. For example, coworkers exposed to income-ineligible peers are 14 p.p. less likely to be suspended from AFAM-PE six months after the event (p -value = 0.018).

To examine how these effects evolve over time, Figure 4, Panels (c) and (d), plots the dynamic (cumulative) reduced-form estimates for compliance outcomes at different event-time horizons around the event. Before the event, the estimates for household verifiable per capita income are close to zero and statistically insignificant, as expected. After the event, coworkers exposed to income-ineligible peers experience a sharp and statistically significant decline in household verifiable per capita income, which remains negative throughout the period analyzed. We also find a decline in the likelihood of benefit suspension, although the

dynamic pattern differs from that for verifiable income. Specifically, the effect on benefit suspension becomes increasingly negative over time, at least through month 10 after the event. Panel (d) does not report pre-event estimates because, by construction, none of the coworkers in the sample had experienced a benefit suspension before their peers were exposed to eligibility enforcement.

The evidence above shows that peers’ income-eligibility enforcement reduces coworkers’ household verifiable per capita income and, as a result, lowers their likelihood of future benefit suspension. However, this decline could reflect either a reduction in total formal household income or a change in reported household size. Appendix Figure D.1 and Table 2 show no significant change in household size, indicating that the decline in household verifiable per capita income is not driven by changes in the reported number of household members.

Overall, the findings in this section show that exposure to income-ineligible peers leads to a significant and persistent decline in coworkers’ household verifiable per capita income, thereby reducing their likelihood of future benefit suspension. The preceding results establish that eligibility enforcement affects not only directly exposed peers but also their coworkers, and that these spillover responses are large enough to feed back into program participation. We next ask whether they are large enough to materially change the program’s welfare evaluation.

5 Welfare Implications

To assess the welfare implications of the spillover effects documented in Section 4, we apply the marginal value of public funds (MVPF) framework (Hendren and Sprung-Keyser, 2020; Finkelstein and Hendren, 2020). Our MVPF estimate captures the value of AFAM-PE as implemented with income-eligibility enforcement: enforcement events are integral to how the program operates, and spillovers reflect beneficiaries’ behavioral responses in a limited-information environment. Specifically, this measure compares beneficiaries’ willingness to pay with the fiscal costs of income-eligibility enforcement, including both direct effects and behavioral spillovers. We proceed in two steps. First, we estimate the direct response of exposed peers, which provides the benchmark fiscal externality for an evaluation focused

only on directly sanctioned households. We then incorporate coworkers’ spillover responses and show that they raise the implied efficiency cost by roughly the same order of magnitude as the direct effects.

5.1 Direct Behavioral Responses of Exposed Peers

To compute the direct behavioral responses of exposed peers, we rely on the main fuzzy RD specification using the peer sample. Because coworkers’ spillover responses are expected to follow peers’ own adjustments to enforcement, we focus on peers’ labor market outcomes during the first three months after the event. Existing evidence for Uruguay shows that AFAM-PE participation has direct effects on formal labor market participation on the order of 6 p.p., about 13% (Bergolo and Cruces, 2021), and we next assess whether comparable direct effects emerge in our setting.

Figure H.1 presents graphical evidence of the reduced-form effects of peers’ income eligibility on their labor market outcomes, based on equation 3. Panel (a) reports effects on total labor income, Panel (b) on labor income conditional on working, and Panel (c) on formal employment. The results show a statistically significant decline in peers’ total labor income three months after the event. In contrast to the pattern documented for coworkers, peers’ direct response is concentrated mainly along the intensive margin: labor income conditional on working declines, while the reduction in formal employment is smaller and not statistically significant.

Table H.1 reports the corresponding estimates for the same set of outcomes. Column (1), Panel B, shows that peers’ total registered labor income falls by UY\$1,544 in the reduced-form specification. This effect is statistically significant and represents a 13% decline relative to the mean of the outcome for the reference group. As shown in Column (1), Panel A, the corresponding 2SLS direct-effect estimate implies a reduction of UY\$3,757, which is very similar in magnitude to the spillover effect estimated for coworkers. The decline is driven primarily by adjustments along the intensive margin, with labor income conditional on working falling by UY\$3,617 in the 2SLS estimate.

One feature of these direct-effect estimates is worth noting. In contrast to the spillover effects on coworkers, which operate primarily through the extensive margin of formal em-

ployment, peers’ direct responses are concentrated along the intensive margin. This pattern may seem surprising, since previous studies of cash transfer social programs have found that direct labor market responses often operate through formal employment (e.g., [Garganta and Gasparini 2015](#); [Bergolo and Cruces 2021](#)). A possible explanation is that peers face an actual suspension and are located close to the eligibility threshold. For them, relatively small adjustments in reported formal earnings may be sufficient to restore eligibility while preserving the formal job. Coworkers, in contrast, learn indirectly about enforcement risk and may face greater uncertainty about the relevant threshold and income definition. Under such uncertainty, exiting formal employment may be a more effective way to reduce verifiable income. This interpretation is also consistent with the evidence shown below, that spillover effects are stronger among coworkers with weaker attachment to formal employment.

5.2 Incorporating Spillovers into the Marginal Value of Public Funds

With direct effects on peers in hand, we now combine them with the spillover effects on coworkers estimated in Section 4 to compute the fiscal externalities induced by income-eligibility enforcement events. We compare beneficiaries’ willingness to pay (WTP) for transfers, proxied by the cash value of the transfer, with the total fiscal costs generated by these events, including both direct fiscal expenditures and behavioral fiscal externalities. Following the MVPF framework ([Hendren and Sprung-Keyser, 2020](#); [Finkelstein and Hendren, 2020](#)), this exercise allows us to summarize the welfare implications of enforcement-induced behavioral responses. It is worth noting that our MVPF calculations are normalized per eligibility-enforcement event—that is, per peer suspension—and should therefore be interpreted as the welfare consequences of AFAM-PE with enforcement under the information environment observed during the study period.

Table 4 summarizes the components of our MVPF-based welfare exercise. Panel A reports each component, while Panel B reports welfare calculations under alternative assumptions regarding spillover-induced fiscal externalities. Following previous MVPF estimates for transfer programs (e.g., [Hendren and Sprung-Keyser 2020](#); [Bergolo and Cruces 2021](#)), we assume that beneficiaries’ WTP for the cash transfer equals its mechanical cost: on average, recipients value 1 UYU transferred at 1 UYU. Under this assumption, efficiency costs arise from fiscal

externalities generated by behavioral responses. The transfer amount used as the benchmark willingness-to-pay equals the average AFAM benefit amount at the time of the event, UY\$1,358 (row 1 of Table 4).

In our setting, fiscal externalities arise through two main channels. First, eligibility enforcement reduces payroll tax revenues by lowering formal employment and formal labor income among both peers (direct effects) and coworkers (spillover effects). Because our estimates capture declines in total formal labor income—combining extensive- and intensive-margin responses—they allow us to quantify the associated loss in social security contributions. Specifically, income-eligibility enforcement reduces peers’ total labor income by UY\$3,757 and exposed coworkers’ labor income by UY\$3,307, as reported in rows 2 and 3 of Table 4. The coworker estimate corresponds to the average response of a single exposed coworker. To obtain the aggregate fiscal consequence of an enforcement event, we multiply this individual response by the average number of exposed beneficiary coworkers per peer, $\overline{N^{cow}} = 1.668$, which captures the network scale through which the enforcement shock propagates.¹³ Assuming a payroll tax rate of 32%,¹⁴ the implied fiscal externality from reduced formal labor income equals $(3,757 \times 0.32) + \overline{N^{cow}} \times (3,307 \times 0.32)$.

Second, behavioral responses may increase program expenditures by allowing some beneficiaries who would otherwise have lost eligibility to continue receiving the transfer. We quantify this channel using the estimated decline in the probability that coworkers experience a benefit suspension after the event. The reduction in household verifiable income makes coworkers 14 p.p. less likely to be suspended. Under the same WTP assumption used throughout the analysis, these preserved transfers also generate additional WTP of equal magnitude. This treatment follows a local-envelope interpretation in which the value of preserving eligibility is approximately equal to the transfer amount received. This calculation abstracts from effects on other tax bases, such as income from alternative sources or consumption, and from additional program-related expenditures, including administrative costs.

¹³As a robustness exercise, we report alternative calculations using different values of N^{cow} , the number of exposed beneficiary coworkers per peer. In addition to the sample average, we consider $N^{cow} = 1$ and $N^{cow} = 2$, which proportionally scale spillover fiscal externalities and eligibility-preservation costs. The corresponding welfare estimates are 0.407 and 0.344, respectively.

¹⁴This rate incorporates average social security contributions from both employees and employers, including payments to the pension and health insurance systems.

Overall, fiscal externalities amount to UY\$3,284, or approximately 2.4 times the mechanical cost of the transfer (row 8 of Table 4). Approximately two-thirds of this amount is driven by coworkers’ spillover responses. Ignoring spillovers yields an MVPF estimate of 0.53 (95% CI: [0.33,0.98]), close to previous estimates for Uruguay’s AFAM-PE (0.61 on average; Bergolo and Cruces (2021)) and within the broader range of transfer-program estimates reported in Hendren and Sprung-Keyser (2020). Once spillover responses are incorporated, the estimated MVPF falls to 0.36 (95% CI: [0.22,0.60]).¹⁵ Under a more conservative specification excluding the WTP associated with preserved eligibility, the estimate falls further to 0.29 (95% CI: [0.19,0.49]).

The MVPF estimates should be interpreted with at least two considerations in mind. First, they apply to the limited-information environment during our period of analysis, in which enforcement events transmit information through workplace networks and induce behavioral responses among other beneficiaries. More broadly, the results highlight the importance of accounting for indirect fiscal effects and information frictions when evaluating means-tested transfer programs and the enforcement of their eligibility rules. Second, the efficiency-cost estimate is a short-run approximation: it abstracts from potential medium- and long-run effects on adults’ income trajectories and beneficiary children, which may be important for a complete welfare assessment. Incorporating these longer-term effects is important because long-run gains may offset, at least partly, the efficiency losses from short-run behavioral responses.

The size of the spillover-induced fiscal externality raises a natural question: why do coworkers who remain eligible adjust their behavior when a peer is suspended? The remainder of the paper examines the channels through which a peer’s suspension transmits, and the patterns of who responds.

¹⁵The baseline specification combines the direct labor-market response of peers measured three months after the enforcement event with spillover responses measured six months after the event. As a robustness exercise, we harmonize the time horizon by replacing the three-month direct effect with the corresponding six-month estimate. Using the six-month direct effect on formal labor income (UY\$2,049 instead of UY\$3,757) increases the direct MVPF from 0.53 to 0.67 and the spillover-adjusted MVPF from 0.36 to 0.41. The qualitative conclusions remain unchanged.

6 Mechanisms

We organize the evidence around three questions. First, do the patterns of who responds, and where, line up with workplace interaction as the channel through which a peer’s suspension transmits? Second, is the content being transmitted information about the income rule and its enforcement? Third, can we rule out alternative explanations, in particular social influence and firm-level adjustments to a peer’s suspension?

6.1 Workplace Interaction and Peer Similarity

If spillovers operate through coworker interaction, the response should be concentrated where such interaction is most likely and where the coworker has the practical ability to adjust formal employment. We present three sets of results consistent with this pattern. First, spillover effects are larger among peer-coworker pairs that share predetermined socioeconomic characteristics. Second, they are larger in small firms and where AFAM-PE beneficiaries make up a larger share of the workforce. Third, they are larger among coworkers whose own job features make adjustment along the formal margin easier. We report reduced-form RD estimates from Table F.1 in the main text; the corresponding fuzzy RD estimates appear in Appendix G.

First, we examine whether spillover effects are stronger among coworkers who share socioeconomic characteristics with their peers and are therefore more likely to interact. The underlying hypothesis is that peer effects may be stronger among similar individuals because they are more likely to form close social ties, a pattern often referred to as homophily (e.g., McPherson et al. 2001). We re-estimate the main RD specification separately for coworkers who match their peers along four predetermined dimensions: earnings level, age group, region of residence, and gender.

Figure 5 summarizes the results. We first focus on coworkers who match their peers along all four socioeconomic characteristics defined above. Panels (a), (c), and (e) plot total registered labor income, labor income conditional on working, and formal employment, respectively, against the running variable. For this group, the reduced-form RD estimates for total registered labor income and formal employment are sizable and statistically significant;

Table [G.7](#) reports the corresponding 2SLS estimates. The estimated effects are more than twice as large as those in the full sample. In contrast, and consistent with the baseline results, we find no response along the intensive margin.

We then estimate effects for subsamples of peer-coworker pairs that match along different combinations of characteristics. For each outcome, Panels (b), (d), and (f) plot the reduced-form RD estimates and confidence intervals, along with the full-sample benchmark from Table [1](#), Panel B. Two patterns emerge from the results for total registered labor income and formal employment. First, spillover effects are particularly strong among coworkers who share the same gender as their peers, and weaker among those matched only on earnings. Second, the effects increase with the degree of peer-coworker similarity. For example, among coworkers who share at least two additional characteristics with their peers, exposure to income-ineligible peers approximately doubles the decline in registered labor income relative to the average effect in the full sample. These findings are consistent with homophily in workplace networks.

Next, we examine how spillover effects vary with network structure. Peer interactions may be more relevant in workplaces where coworkers interact more frequently and exchange information more intensively. This is likely to occur in small workplaces, where social and personal contacts tend to be stronger than in larger workplaces ([De Giorgi et al., 2019](#)). Similarly, workplace interactions may be more localized within firms operating a single establishment, where coworkers are more likely to share common spaces and daily interactions than in firms with multiple establishments. Peer effects may also be stronger when AFAM-PE beneficiaries represent a larger share of the firm’s workforce. Panels A and B of Table [3](#) report heterogeneous reduced-form estimates by firm establishment structure and firm size, while Panel C reports heterogeneity by the share of AFAM-PE beneficiaries in the workplace. Tables [G.1](#) to [G.6](#) report the corresponding fuzzy RD estimates. The results broadly support these hypotheses. Spillover responses are concentrated almost exclusively among coworkers employed in single-establishment firms, consistent with the idea that workplace proximity and repeated interactions facilitate the transmission of information and behavioral responses across coworkers. Coworker responses are notably weaker in larger firms, whereas labor market responses are stronger in workplaces with a higher share of AFAM-PE beneficiaries,

especially for total registered labor income and formal employment.

A third dimension concerns the coworker’s capacity to adjust formal employment. Previous evidence shows that shorter job tenure and more flexible labor relations are associated with weaker attachment to formal employment (Bosch and Maloney, 2010; Ulyssea, 2018). We split coworkers by job stability, classifying jobs as more stable when workers hold a permanent contract and receive a monthly wage rather than a temporary contract or daily pay, and by firm tenure, defined as below or above one year in the firm.¹⁶ Panels D and E of Table 3 show that spillover responses are concentrated among coworkers in low-stability jobs and among coworkers with shorter firm tenure. Although some effects remain statistically significant among high-stability workers, the estimated responses are generally smaller and less consistently significant across outcomes. The corresponding fuzzy RD estimates appear in Appendix Tables F.8 to F.11. The pattern is consistent with adjustment capacity rather than with peer interaction per se: coworkers respond when they have a realistic margin for adjustment.

For completeness, Panels A and B of Table F.1 also report heterogeneity by age and gender. The spillover response is somewhat larger among younger coworkers and among male coworkers, but the gender pattern in particular is sensitive to differences in household income position and network composition between male and female coworker samples, and we do not interpret it as informative about the mechanism. Appendix Section G.1 reports the corresponding fuzzy RD estimates and a fuller discussion.

6.2 Information Transmission

Section 6.1 established that spillovers concentrate where coworker interaction is more likely and where coworkers have the means to adjust. We now ask what is being transmitted. The strongest candidate in this setting is information about the existence and enforcement of the income rule. As documented in Section 2.3, only 9% of AFAM-PE beneficiaries report knowing about additional eligibility conditions, including the income requirement, and 29% believe the program has no requirements at all. A peer’s benefit suspension is plausibly the

¹⁶To define the stability category, we use two variables from the SSA records: the type of payment specified in the contract (monthly, daily, or commission-based) and the worker’s category (dependent employee, laborer, or seasonal employee).

first concrete signal many coworkers receive about how the rule operates. Information transmission among coworkers is a well-documented feature of workplace networks ([Montgomery, 1991](#); [Calvo-Armengol and Jackson, 2004](#)), including in contexts related to social program participation ([Dahl et al., 2014](#); [Mora-Garcia and Rau, 2023](#)).

Although we do not directly observe information transmission among coworkers, we can examine whether spillover effects vary with the information available to peers and coworkers after a peer’s exposure to income-eligibility enforcement. To do so, we use data from MIDES’ Beneficiary Support Service (BSS), which records whether beneficiaries contacted the support service with inquiries about AFAM-PE. We link these records to AFAM-PE administrative data to identify beneficiaries who contacted the BSS after a benefit suspension due to income noncompliance.

To explore whether information transmission may contribute to the observed spillovers, we re-estimate the RD specification separately for coworkers whose peers contacted the BSS support call center after a benefit suspension and for coworkers whose peers did not. Because contact with the BSS is itself a post-suspension outcome, this exercise should not be interpreted as identifying causal heterogeneity by information acquisition. Rather, it provides suggestive evidence regarding whether spillovers differ across groups with varying degrees of information exposure following the enforcement event. Figure 6 shows that spillover effects are stronger among coworkers whose peers contacted the BSS after suspension (Panel I) than among coworkers whose peers did not (Panel II). In particular, the reduced-form RD estimates indicate that the decline in coworkers’ total registered labor income at the cutoff is nearly three times larger when peers contacted the BSS than when they did not. This difference is visible in both the intensive and extensive margins. Appendix Tables G.8 and G.9 report the corresponding 2SLS estimates.

An alternative hypothesis is that coworkers themselves contact the BSS after learning from their peers about benefit suspensions due to income-eligibility enforcement. Appendix Figure G.1 examines this possibility. The figure plots the reduced-form RD estimate for the probability that a beneficiary coworker contacted the BSS to inquire about AFAM-PE. The estimate is positive, but statistically insignificant.

To further assess the role of information transmission and learning, we examine whether

coworkers’ labor market responses vary with the distance between their household verifiable income and the eligibility threshold. The intuition is that information about enforcement should be more valuable for coworkers whose household income places them closer to the cutoff, and who therefore face a higher risk of future suspension.

Appendix Figure G.2 tests this hypothesis by presenting reduced-form RD estimates for each labor market outcome across subgroups of coworkers located at different distances from the threshold. The results show substantially larger spillover effects for coworkers whose household income is closer to the cutoff—between 20% and 30% below the threshold, shown in Panel I—than for coworkers farther away from the cutoff—more than 30% below the threshold, shown in Panel II. This pattern is consistent with the idea that the value and relevance of information about enforcement shape behavioral responses. Appendix Tables G.10 and G.11 report the corresponding 2SLS estimates.

A related test exploits variation in coworkers’ ability to absorb and act on the information that a peer’s suspension conveys. Panel E of Table 3 reports spillover effects separately for coworkers with fewer than nine years of education and those with at least nine years, which corresponds to completion of lower secondary education. Coworkers with higher educational attainment reduce their total registered labor income by more than twice as much as those with lower attainment when exposed to an income-ineligible peer, and the gap is driven by an intensive-margin response rather than by a larger decline in formal employment. Appendix Tables F.6 and F.7 report the corresponding fuzzy RD estimates. The pattern that more educated coworkers respond more, and respond through reported earnings rather than employment exits, is consistent with information about the rule as the content of transmission: acting on that information through earnings adjustments requires some understanding of how the rule operates, while exits from formal employment do not. Taken together with the BSS and distance-to-threshold results, this evidence points to information about the income rule as the content being transmitted through workplace interaction.

6.3 Alternative Channels: Social Influence and Firm Responses

We close this section by ruling out two alternative explanations for the spillover patterns: social influence among workers regardless of program participation, and firm-level adjustments

to a peer’s suspension.

The first alternative is social influence: workers adjusting their behavior to match coworkers’ regardless of their own program participation (Montgomery and Casterline, 1996). In our setting, social influence would predict that coworkers reduce reported earnings or leave formal employment because their suspended peer did, independently of whether the rule applies to them. Because the homophily evidence in Section 6.1 is consistent with both information transmission and social influence (similarity matters for both), we test for social influence directly by examining whether the same workplace shock affects non-AFAM-PE coworkers, for whom information about the income rule has no value.

As in Section 6.1, we estimate the RD specification for non-AFAM-PE coworkers who match AFAM-PE peers on predetermined socioeconomic characteristics. Finding significant spillover effects in this group would support the social influence hypothesis, since information about income-eligibility rules and sanctions should have little value for workers who are not AFAM-PE beneficiaries. Appendix Figure G.3 summarizes the results. We find no significant effects on the labor market outcomes of non-AFAM-PE coworkers; if anything, the estimates are slightly positive. This finding, together with the evidence of stronger spillovers among similar AFAM-PE coworkers and peers, suggests that social influence is unlikely to be the main mechanism driving the peer effects.

The second alternative is that peers’ direct responses to enforcement prompt firms to adjust employment or compensation policies, indirectly affecting other AFAM-PE coworkers (Jäger and Heining, 2022). For example, peers who receive a benefit suspension may choose to exit formal employment to conceal earnings from AFAM authorities and regain eligibility. Such shocks could affect firms’ hiring and separation decisions, as well as the wages and retention rates of incumbent workers. Using administrative firm-level data, we compare outcomes six months after firms are exposed to income-ineligible *vs.* income-eligible peers close to the cutoff. Appendix Table G.12 reports the corresponding fuzzy RD estimates for four firm-level outcomes: total employment excluding AFAM-PE coworkers in the analysis sample, new hires, worker separations, and average labor income. We find no statistically significant effects on any of these outcomes. These null results suggest that firm-level adjustments are unlikely to explain the spillover patterns documented above, even though peers

themselves respond to income-eligibility enforcement.

7 Concluding remarks

Standard assessments of means-tested transfer programs focus on directly enforced households. The evidence in this paper shows that these assessments are incomplete. In Uruguay’s AFAM-PE, eligibility enforcement transmits information through workplace networks: coworkers of suspended beneficiaries reduce registered labor earnings by roughly one-third of baseline, with effects emerging within two months and persisting for at least a year. The response operates almost entirely on the extensive margin of formal employment, and the evidence suggests that most of the decline reflects transitions to informal work rather than exit from the labor force. These responses feed back into program outcomes, lowering coworkers’ likelihood of future suspension by 14 percentage points.

Several pieces of evidence point to information transmission through workplace networks as the channel. In a context where the income rule is imperfectly understood by most beneficiaries, a peer’s suspension acts as a salient information shock that updates other beneficiaries’ beliefs about the cost of formal earnings. Spillovers are concentrated in small firms and among similar coworkers, where workplace interactions are more frequent and substantive. They are nearly three times larger when the suspended peer actively contacted the program’s support service to inquire about the suspension. Non-beneficiary coworkers matched on observable characteristics show no significant responses, a pattern more consistent with information transmission than with social influence or common firm shocks.

These indirect responses are large enough to alter the welfare evaluation of eligibility-enforcement events. Analyses focused on households that directly experience eligibility enforcement substantially understate the short-run efficiency cost: ignoring network-mediated responses is a quantitatively important source of bias in the welfare evaluation of means-tested transfers. Incorporating spillover-induced fiscal externalities into welfare assessment lowers the MVPF from 0.53 to 0.36, placing it at the lower end of estimates for cash transfer programs ([Hendren and Sprung-Keyser, 2020](#)). On the design side, the finding that enforcement events are the primary channel through which beneficiaries learn about the income rule

points to communication of eligibility conditions as part of the policy toolkit. The net effect of clearer communication is ambiguous: clearer rules could reduce the information asymmetry that drives the observed spillovers and mitigate labor market distortions, but they could also make the threshold more salient and increase avoidance. Communication should therefore be treated as a policy instrument whose effect on the joint distribution of compliance and avoidance needs to be evaluated rather than assumed.

Our estimates are local to the income-eligibility cutoff, the limited-information environment during the period of analysis, and our definition of workplace networks as coworkers employed in the same firm. Accordingly, the spillover-augmented MVPF should be interpreted as applying to AFAM-PE as implemented under the enforcement practices observed during the study period, rather than to a full-information counterfactual in which the income rule is well known. In addition, spillovers through networks that we do not observe, such as neighborhoods, extended family, or informal-firm networks, could imply a larger total indirect effect of enforcement. Finally, our efficiency calculations capture short-run behavioral responses and do not account for potential long-run gains from the program, such as improvements in children’s education and health or in beneficiaries’ labor market trajectories over time (Aizer et al., 2016; Barr et al., 2022; Giacobasso, 2026; Aizer et al., 2022). Incorporating these longer-term effects is essential for a complete welfare assessment of whether the benefits of income-tested transfers outweigh their costs. More broadly, we expect the forces we document to generalize to settings in which formal earnings are observable by the state, sanctions are salient, workplace interaction is repeated, and informality provides a realistic margin of adjustment, which characterize many means-tested transfer programs in middle- and low-income countries.

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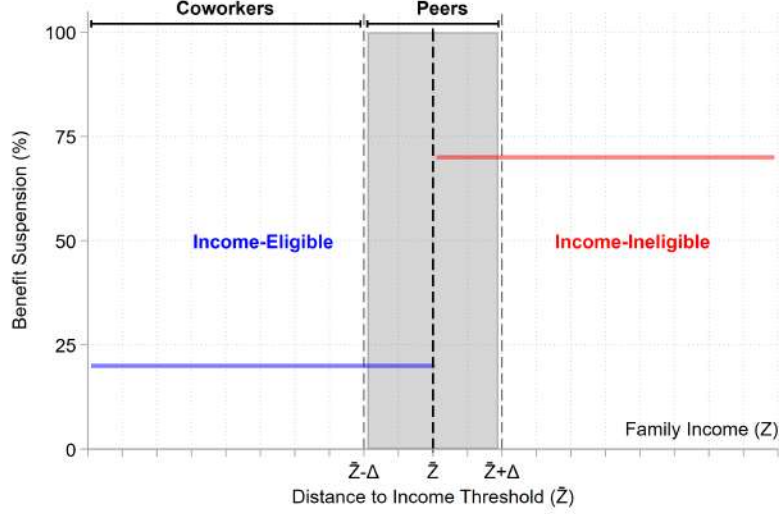
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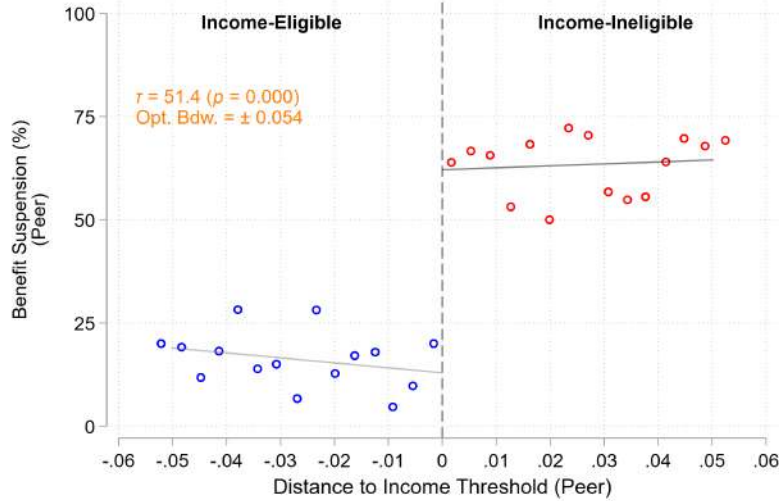
Figures

Figure 1: Quasi-random Eligibility and First Stage

(a) *Quasi-random Eligibility based on Income-testing Rule*



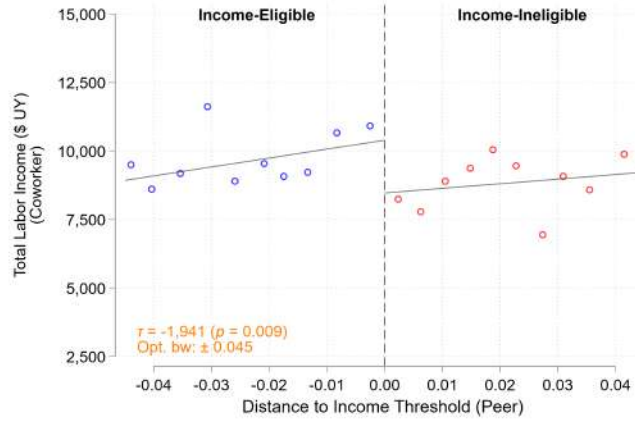
(b) *Effect of Eligibility Rule on Suspension of AFAM Benefit*



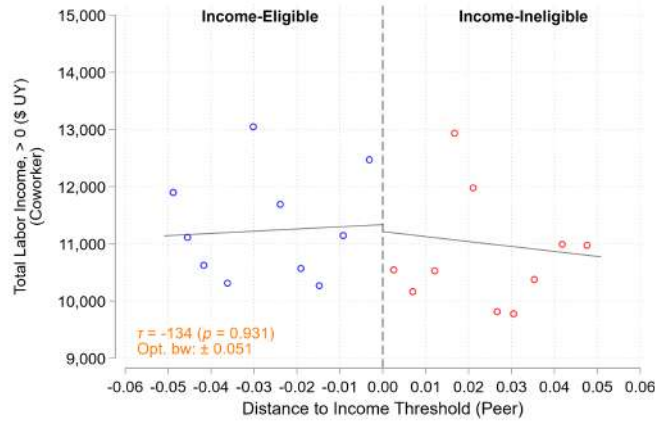
Notes: This figure illustrates the identification strategy and the first-stage relationship of the RD design. Panel (a) provides a stylized representation of the income-testing rule. The running variable is family verifiable income Z , and eligibility changes discontinuously at the threshold within a narrow window $[\bar{Z} - \Delta, \bar{Z} + \Delta]$. Beneficiary peers to the left of the cutoff are income-eligible, while those to the right are income-ineligible. Panel (b) plots the share of peers whose AFAM benefit is suspended as a function of family income relative to the threshold in 2009–2013. The sample corresponds to the peer sample described in Section 3.2, in which each peer appears once. Estimates are obtained using local linear regressions with triangular kernel weights and MSE-optimal bandwidths following [Calonico et al. \(2019\)](#). The reported discontinuity of 51 p.p. reflects the jump in the probability of benefit suspension at the income-eligibility threshold estimated in the peer sample. Dots denote mean outcomes within 15 equally spaced bins and solid lines denote fitted values. Robust standard errors are clustered at the firm level.

Figure 2: Main Effects on Coworkers' Labor Outcomes

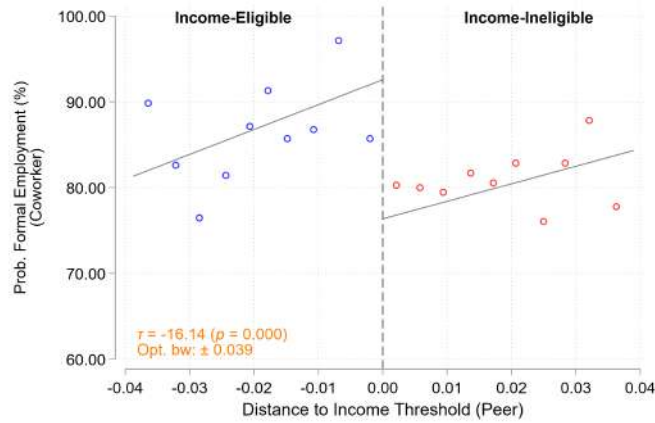
(a) *Total Labor Income*



(b) *Positive Labor Income*

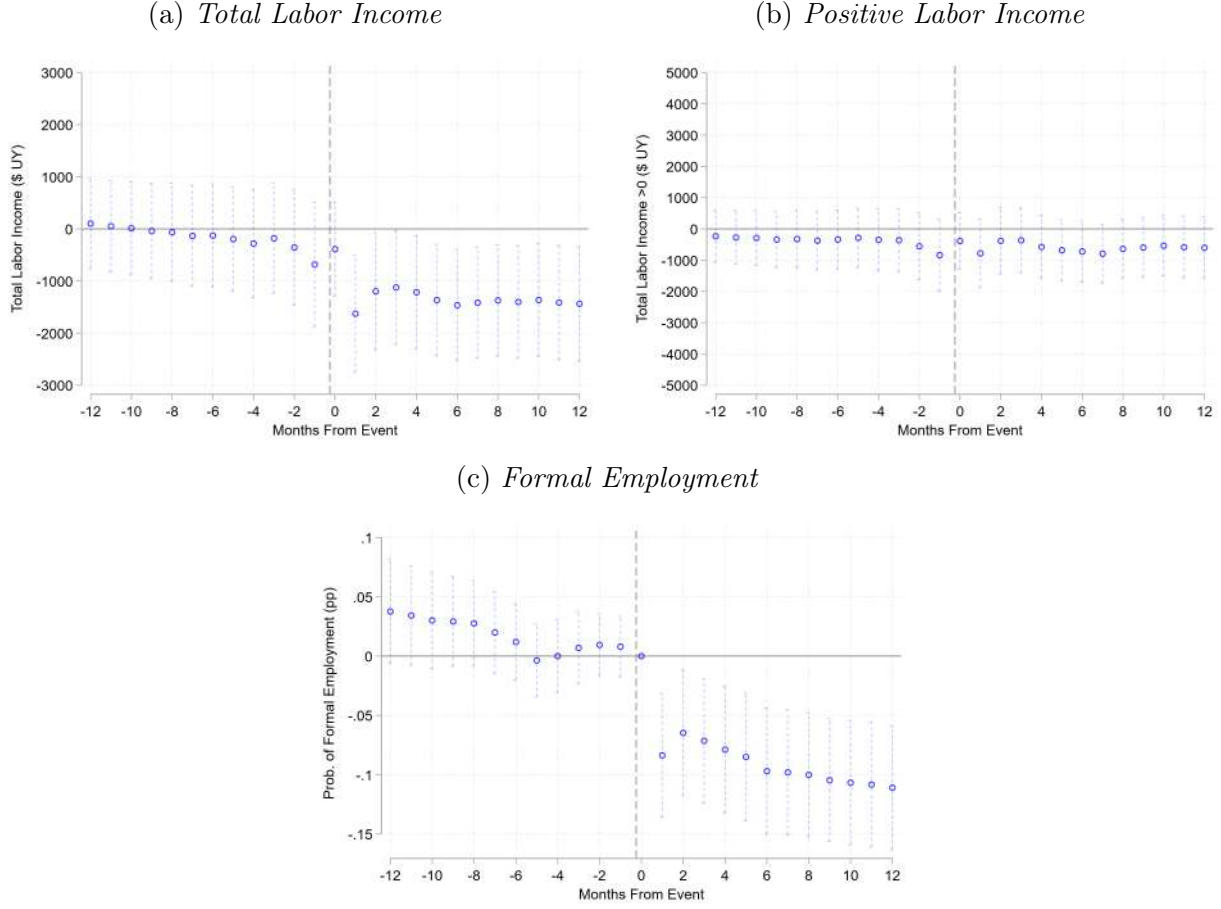


(c) *Formal Employment*



Notes: This figure presents reduced-form effects of peers' income eligibility on coworkers' labor market outcomes: total labor earnings in panel (a), labor earnings conditional on working in panel (b), and formal employment in panel (c). Outcomes are measured six months after the peer's income-testing event. The running variable is peers' family verifiable income relative to the eligibility threshold. Estimates are obtained using local linear regressions with triangular kernel weights and MSE-optimal bandwidths following [Calonic et al. \(2019\)](#). Dots denote mean outcomes within 10 equally spaced bins and solid lines denote fitted values on each side of the cutoff. The reported discontinuity corresponds to the local reduced-form effect of peer eligibility. Robust standard errors are clustered at the firm level. The sample corresponds to the analysis sample described in Section 3.2.

Figure 3: Dynamic (cumulative) Effects on Coworkers' Labor Outcomes: Average Outcomes Since the Event

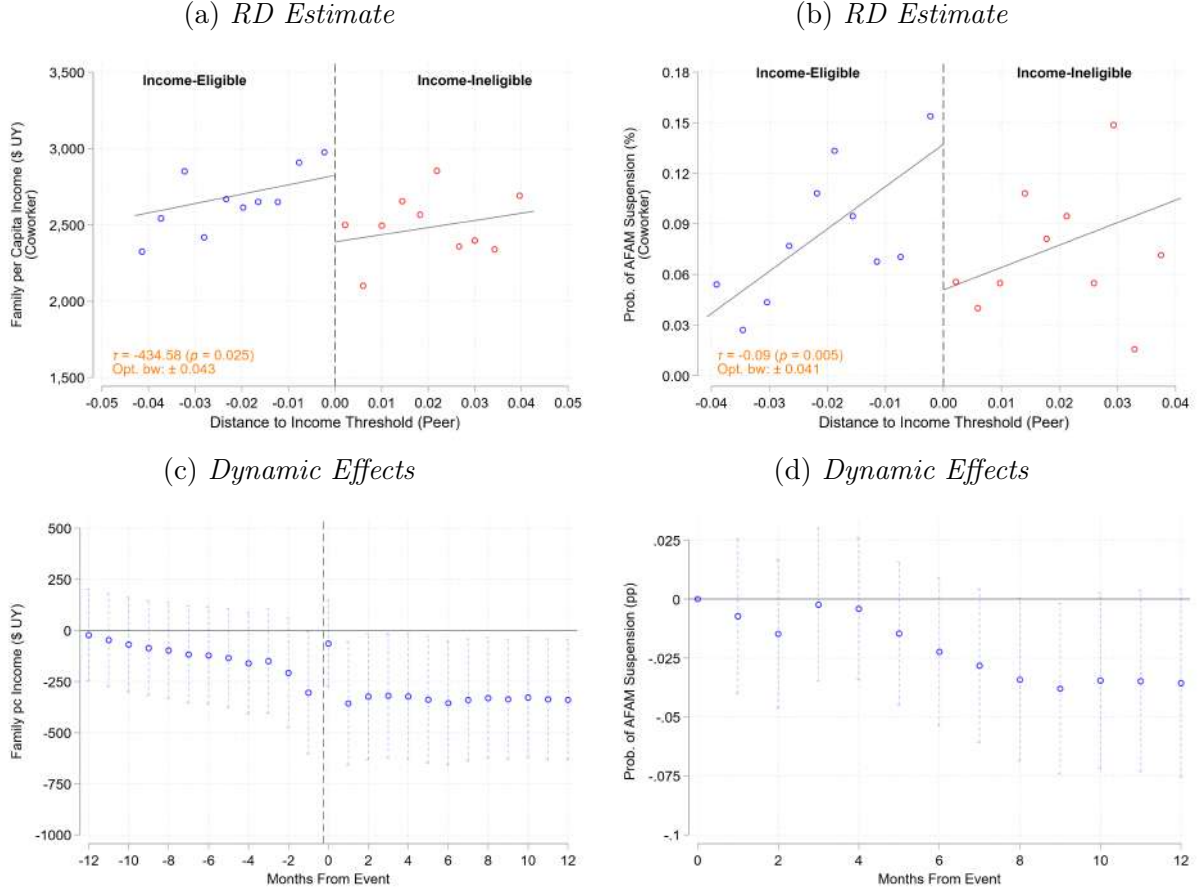


Notes: This figure presents dynamic reduced-form effects of peers' income eligibility on coworkers' labor market outcomes: total labor earnings in panel (a), labor earnings conditional on working in panel (b), and formal employment in panel (c). Each point reports the estimate from Equation 3 for the corresponding event-time horizon, where outcomes are defined as the average value from the event month up to month t , with $t = 0$ denoting the event month. Because outcomes are defined cumulatively, each coefficient should be interpreted as the cumulative reduced-form effect up to that horizon. Estimation uses local linear regressions with triangular kernel weights and MSE-optimal bandwidths following Calonico et al. (2019). Vertical bars denote 95 percent confidence intervals based on robust standard errors clustered at the firm level. The sample corresponds to the analysis sample described in Section 3.2.

Figure 4: Effect of Peers' Eligibility on Coworkers' Future Compliance

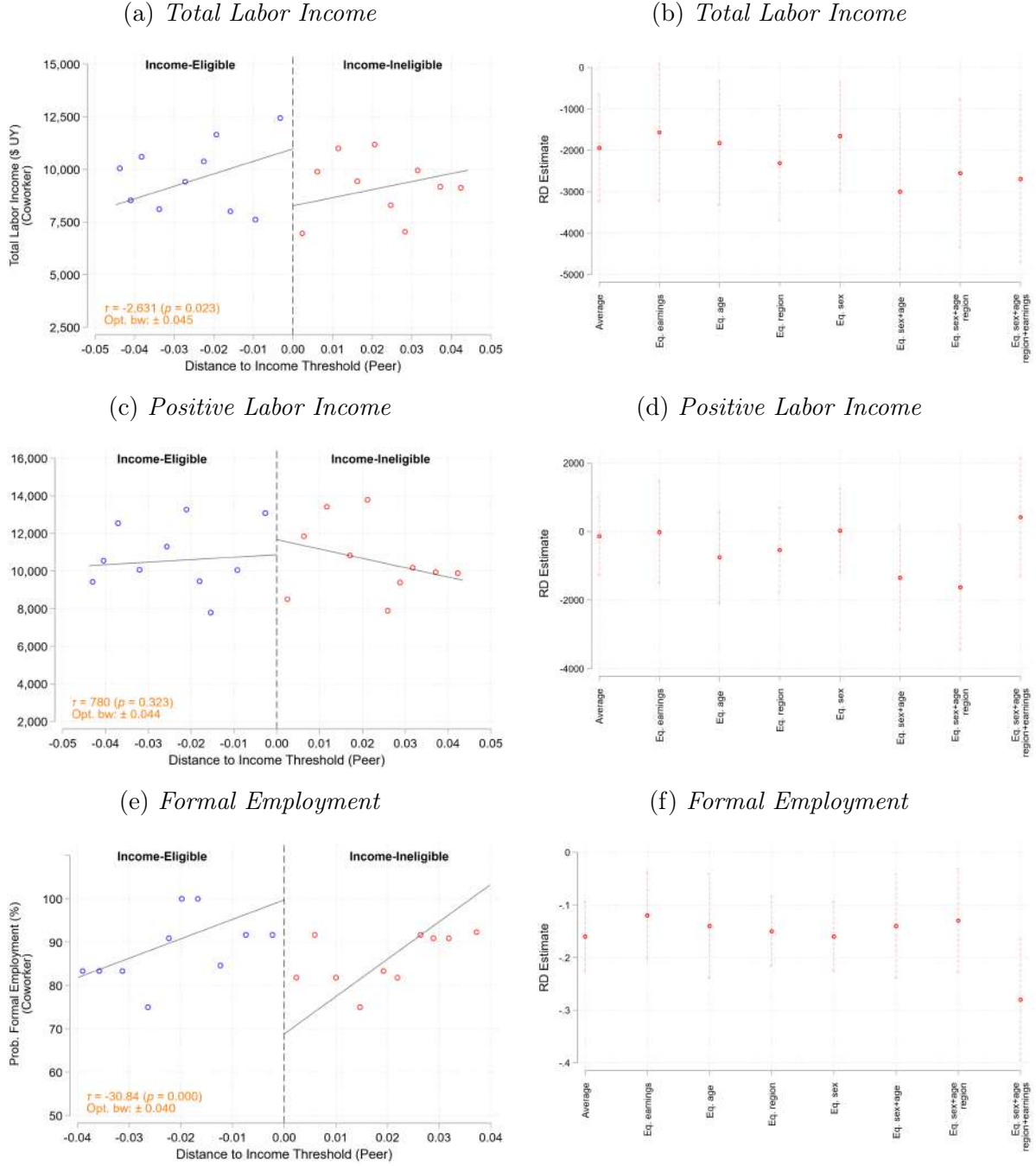
Panel A. Household Verifiable per Capita Income

Panel B. Benefit Suspension



Notes: This figure presents reduced-form effects of peers' income eligibility on coworkers' future compliance with the AFAM income rule. Panels (a) and (c) use coworkers' household verifiable per capita income as the outcome, while panels (b) and (d) use an indicator for subsequent benefit suspension due to income noncompliance. Panels (a) and (b) report RD estimates for outcomes measured six months after the peer's income-testing event. Panels (c) and (d) report dynamic (cumulative) estimates at different event-time horizons. Each estimate corresponds to outcomes averaged from the event month up to the corresponding horizon, with month 0 denoting the event month. Because outcomes are defined cumulatively, each coefficient should be interpreted as the cumulative reduced-form effect up to that horizon. Estimates are obtained from Equation 3 using local linear regressions with triangular kernel weights and MSE-optimal bandwidths following Calónico et al. (2019). Vertical bars denote 95 percent confidence intervals based on robust standard errors clustered at the firm level. The sample corresponds to the analysis sample described in Section 3.2.

Figure 5: Heterogeneous Effects by Coworker-Peer Similarity

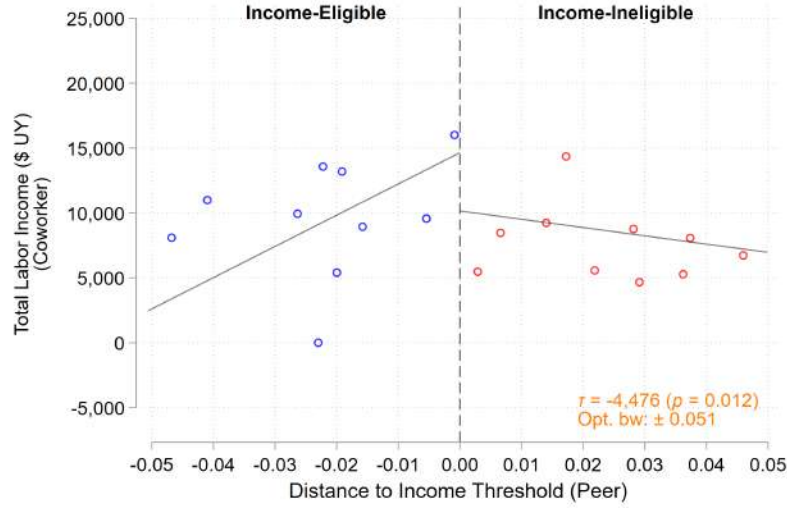


Notes: This figure presents heterogeneous reduced-form effects of peers' income eligibility on coworkers' labor market outcomes by coworker-peer similarity. Panels (a), (c), and (e) report baseline RD plots for total labor earnings, labor earnings conditional on working, and formal employment, respectively, measured six months after the peer's income-testing event. Panels (b), (d), and (f) report RD point estimates for subsamples of coworkers matched to peers on selected observable characteristics, as indicated on the horizontal axis. Estimates are obtained from Equation (3) using local linear regressions with triangular kernel weights and MSE-optimal bandwidths following Calónico et al. (2019). Vertical bars denote 95 percent confidence intervals. Robust standard errors are clustered at the firm level. The sample corresponds to the analysis sample described in Section 3.2.

Figure 6: Heterogeneous Effects by Peer Contact with the BSS

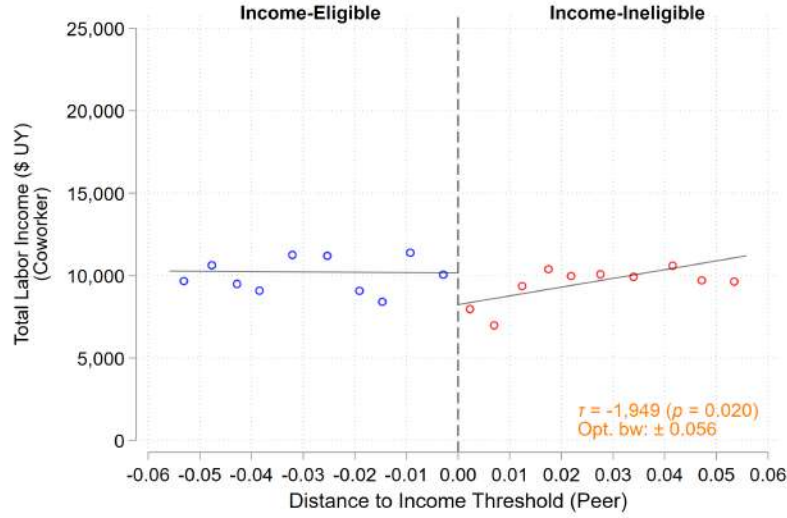
Panel I. Peer Contacted the BSS

(a) *Total Labor Income*



Panel II. Peer Did Not Contact the BSS

(b) *Total Labor Income*



Notes: This figure presents heterogeneous reduced-form effects of peers' income eligibility on coworkers' labor market outcomes by whether the peer contacted the Beneficiary Support Service (BSS) after benefit suspension. Panel I reports estimates for coworkers exposed to peers who contacted the BSS, while Panel II reports estimates for coworkers exposed to peers who did not. Outcomes are measured six months after the peer's income-testing event. Estimates are obtained from Equation (3) using local linear regressions with triangular kernel weights and MSE-optimal bandwidths following [Calonico et al. \(2019\)](#). Dots denote binned means and solid lines denote fitted values. Robust standard errors are clustered at the firm level. The sample corresponds to the analysis sample described in Section 3.2.

Tables

Table 1: RD Estimates of the Effect of Peers' Suspension on Coworkers' Labor Market Outcomes, 6 Months After Event

	Total Labor Income (1)	Labor Income > 0 (2)	Prob. Formal Employment (3)
<i>Panel A. Fuzzy RDD Estimate</i>			
Peers' Suspension (HH Income > Thresh.)	-3307.69** (1477.96)	-75.14 (1236.41)	-0.29*** (0.09)
Robust p-value	0.023	0.929	0.001
<i>Panel B. Reduced Form</i>			
Income-Eligibility Enforcement	-1941.38*** (785.01)	-134.44 (691.92)	-0.16*** (0.04)
Robust p-value	0.009	0.931	0.000
Mean baseline	9741.86	11315.66	0.86
Effect size (%)	-19.93	-1.19	-18.67
<i>Panel C. First Stage</i>			
Income-Eligibility Enforcement	0.56*** (0.07)	0.56*** (0.07)	0.57*** (0.07)
Robust p-value	0.000	0.000	0.000
Opt. Bandwidth	.045	.05	.04
Observations	1505	1541	1372

Notes: This table reports RD estimates of the effect of peers' income-eligibility enforcement on coworkers' labor market outcomes measured six months after the peer's income-testing event. Panel A reports the fuzzy RD estimates (IV) based on the 2SLS estimation of Equation 1. Panel B reports the reduced-form estimates based on Equation 3. Panel C reports the first-stage estimates based on Equation 2. The sample corresponds to the coworker sample described in Section 3.2, in which each peer-coworker link constitutes one observation. All specifications use local linear regressions with triangular kernel weights and MSE-optimal bandwidths following Calonico et al. (2019). Robust standard errors clustered at the firm level are reported in parentheses. Mean baseline reports the average outcome among coworkers of peers below the income cutoff (i.e., coworkers of peers who qualify for AFAM-PE). Effect size reports the estimated coefficient as a percentage of the baseline mean. Statistical significance based on robust p-values: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 2: RD Estimates of the Effect of Peers' Suspension on Coworkers' Future Compliance

	HH Verifiable per capita Income (1)	Prob. AFAM Suspension (2)	Total HH Verifiable Income (3)	Household Size (4)
<i>Panel A. Fuzzy RDD Estimate</i>				
Peers' Suspension (HH Income > Thresh.)	-642.91* (378.77)	-0.14** (0.06)	-1586.39 (1733.62)	0.00 (0.00)
Robust p-value	0.090	0.018	0.407	0.598
<i>Panel B. Reduced Form</i>				
Income-Eligibility Enforcement	-434.58** (202.07)	-0.09*** (0.03)	-1206.42 (957.07)	-0.00 (0.00)
Robust p-value	0.025	0.005	0.205	0.832
Mean baseline	2665.70	0.08	11930.94	4.51
Effect size (%)	-16.30	-103.06	-10.11	0.00
<i>Panel C. First Stage</i>				
Income-Eligibility Enforcement	0.56*** (0.07)	0.56*** (0.07)	0.56*** (0.07)	0.56*** (0.07)
Robust p-value	0.000	0.000	0.000	0.000
Opt. Bandwidth	.043	.041	.0455	.0405
Observations	1510	1442	1624	1421

Notes: This table reports RD estimates of the effect of peers' income-eligibility enforcement on coworkers' future compliance outcomes and household characteristics measured six months after the peer's income-testing event. Panel A reports the fuzzy RD estimates (IV) based on the 2SLS estimation of Equation 1. Panel B reports the reduced-form estimates based on Equation 3. Panel C reports the first-stage estimates based on Equation 2. The sample corresponds to the coworker sample described in Section 3.2, in which each peer-coworker link constitutes one observation. All specifications use local linear regressions with triangular kernel weights and MSE-optimal bandwidths following Calonico et al. (2019). Robust standard errors clustered at the firm level are reported in parentheses. Mean baseline reports the average outcome among coworkers of peers below the income cutoff (i.e., coworkers of peers who qualify for AFAM-PE). Effect size reports the estimated coefficient as a percentage of the baseline mean. Statistical significance based on robust p-values: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 3: Heterogeneous Reduced-Form Effects of Peers' Suspension on Coworkers' Labor Market and Compliance Outcomes

	Individual Outcomes			Household Outcomes	
	Labor Income (1)	Labor Income > 0 (2)	Prob. Formal Income (3)	Prob. AFAM-PE Suspension (4)	Per Capita HH Income (5)
<i>Panel A. Reduced-form estimate by firm establishment structure</i>					
<i>Single-establishment firms</i>	-2381.69***	-616.36	-0.17***	-0.06	-435.39**
Robust p-value	0.004	0.496	0.001	0.131	0.049
Observations	1053	941	733	915	940
<i>Multi-establishment firms</i>	-373.72	-37.85	-0.02	-0.13***	-349.77
Robust p-value	0.718	0.915	0.915	0.001	0.158
Observations	729	439	507	629	730
<i>Panel B. Reduced-form estimate by firm size</i>					
<i>Firm Size ≤ 30 workers</i>	-3089.78***	-602.42	-0.20***	0.01	-593.90**
Robust p-value	0.004	0.645	0.000	0.693	0.045
Observations	515	393	479	522	522
<i>Firm Size > 30 workers</i>	-1812.34**	390.57	-0.13***	-0.13***	-371.16*
Robust p-value	0.040	0.553	0.009	0.000	0.091
Observations	870	997	873	830	900
<i>Panel C. Reduced-form estimate by share of AFAM</i>					
<i>Prop. AFAM ≤ median</i>	-1205.18	-108.37	-0.12**	-0.05	-430.25*
Robust p-value	0.231	0.770	0.037	0.261	0.088
Observations	702	583	838	757	640
<i>Prop. AFAM > median</i>	-2056.09**	-255.67	-0.18***	-0.08**	-524.00**
Robust p-value	0.034	0.919	0.001	0.013	0.041
Observations	946	696	781	944	837
<i>Panel D. Reduced-form estimate by stability of the job</i>					
<i>Low-stability jobs</i>	-2865.00***	-471.24	-0.20***	-0.11***	-600.96**
Robust p-value	0.001	0.692	0.001	0.002	0.037
Observations	677	561	509	509	565
<i>High-stability jobs</i>	-125.50	1035.87	-0.09**	-0.04	-401.61
Robust p-value	0.899	0.283	0.019	0.309	0.115
Observations	520	459	535	505	537
<i>Panel E. Reduced-form estimate by tenure</i>					
<i>Tenure ≤ 1yr</i>	-3085.56***	-45.86	-0.25***	-0.15***	-496.52*
Robust p-value	0.003	0.971	0.000	0.001	0.100
Observations	529	390	632	479	541
<i>Tenure > 1yr</i>	-1412.77	-609.86	-0.06	-0.06	-0.04
Robust p-value	0.138	0.402	0.222	0.222	0.405
Observations	877	1008	813	813	876
<i>Panel F. Reduced-form estimate by educational level</i>					
<i>Low education</i>	-1445.75	322.72	-0.14***	-0.07**	-230.07
Robust p-value	0.118	0.544	0.003	0.022	0.412
Observations	1241	1061	1189	1143	1313
<i>High education</i>	-2647.96***	-2919.56***	-0.10**	-0.14**	-1649.55***
Robust p-value	0.006	0.002	0.040	0.022	0.000
Observations	567	258	469	376	294

Notes: This table reports heterogeneous reduced-form estimates of the effect of peers' income-eligibility enforcement on coworkers' labor market and compliance outcomes across alternative coworker subgroups. Each row reports the reduced-form estimate from Equation 3 for the indicated subgroup, together with robust p-values and the number of observations. Subgroups are defined by establishment structure, firm size, share of AFAM beneficiaries in the firm, job stability, tenure and educational attainment as indicated in the table. The sample corresponds to the coworker sample described in Section 3.2, in which each peer-coworker link constitutes one observation. All specifications use local linear regressions with triangular kernel weights and MSE-optimal bandwidths following Calonico et al. (2019). Robust standard errors are clustered at the firm level. Statistical significance based on robust p-values: *** p<0.01, ** p<0.05, * p<0.10.

Table 4: MVPF-Based Welfare Calculations for AFAM-PE Enforcement Events

	Amount	Notes
<i>Panel A: Per-coworker and per-event components</i>		
1. Transfer amount (mechanical cost / baseline WTP)	1358	Average transfer amount
2. Direct effect: Change in peers' labor income	3757	Appendix Table H.1
3. Spillover effect: Change in coworkers' labor income (per coworker)	3307	Table 1
4. Direct fiscal externality	1202	$(2) \times 0.32$
5. Spillover fiscal externality (per event)	1765	$(3) \times 0.32 \times \overline{N}^{cow}$
6. Fiscal cost of preserved eligibility (per event)	317	$(1) \times 0.14 \times \overline{N}^{cow}$
7. WTP from preserved eligibility (per event)	317	Same amount as row (6)
8. Total fiscal externalities	3284	$(4) + (5) + (6)$
<i>Panel B: MVPF under alternative information environments</i>		
Direct MVPF (direct effects only)	0.530	95% CI: [0.327, 0.977]
MVPF including spillovers (current information environment)	0.360	95% CI: [0.225, 0.600]
Conservative MVPF excluding preserved-transfer WTP	0.290	95% CI: [0.191, 0.489]
MVPF with one exposed coworker	0.407	Assume $N^{cow} = 1$
MVPF with two exposed coworkers	0.344	Assume $N^{cow} = 2$

Notes: This table reports an MVPF-based welfare exercise for AFAM eligibility-enforcement events under alternative assumptions regarding spillover effects. All fiscal components are normalized per enforcement event (i.e., one peer suspension). The transfer amount in row 1 corresponds to the average AFAM transfer and represents both the mechanical cost of the transfer and the baseline willingness-to-pay (WTP), under the assumption that beneficiaries value 1 UYU received at 1 UYU. Row 3 reports the estimated labor-income response for a single exposed coworker, whereas rows 5–7 aggregate these responses to the event level by multiplying by the average number of exposed beneficiary coworkers per peer in the estimation sample, $\overline{N}^{cow} = 1.668$. This captures the network scale through which the enforcement shock propagates across coworkers. The direct fiscal externality in row 4 equals the estimated reduction in peers' labor income (row 2) multiplied by the payroll tax rate (0.32). The spillover fiscal externality in row 5 equals the estimated per-coworker reduction in labor income (row 3) multiplied by the payroll tax rate and by $\overline{N}^{cow}$. The fiscal cost of preserved eligibility in row 6 equals the transfer amount (row 1) multiplied by the estimated reduction in suspension probability among coworkers (0.14) and by $\overline{N}^{cow}$. Row 7 values these preserved transfers as additional willingness-to-pay under the same WTP assumption. Direct labor responses are computed using the estimated responses of treated peers (Appendix Table H.1), while spillover responses are computed using coworkers' labor market responses reported in Table 1. Panel B reports alternative welfare calculations under different information environments and spillover effects. The direct MVPF corresponds to a standard evaluation that accounts only for the responses of directly sanctioned peers, whereas the spillover-adjusted MVPF also incorporates coworkers' responses in the limited-information environment described in the text. Confidence intervals are computed using 1,000 cluster-bootstrap replications at the peer-firm level. Monetary amounts are expressed in monthly Uruguayan pesos.

Online Appendix
(For Online Publication Only)

Vilá, Bergolo, Cruces

Enforcement Spillovers and the Efficiency Costs of Means-Tested

Transfers: Evidence from Workplace Networks

July 2, 2026

A Institutional Background and Enforcement

This appendix provides additional institutional background on AFAM-PE and documents the enforcement of its income-eligibility requirement during the period of analysis. The main text summarizes the features of the program that are central to the research design; here we provide further detail on program eligibility, benefits, sanctions, beneficiaries’ information about program rules, and the evolution of benefit suspensions over time.

A.1 Program Context

Uruguay is an upper-middle-income country with a long-established social protection system. Most social insurance programs are linked to formal employment and are funded through payroll contributions made by employers and employees. Registered, or formal, employment means that workers are declared by their employers to the Social Security Administration (SSA), with the corresponding taxes and contributions paid. Formal workers are therefore entitled to benefits such as health coverage, unemployment insurance, and pensions.

During the period analyzed in this paper, however, around 20% of Uruguay’s workforce was informally employed. Informality may arise because workers are employed by unregistered firms or because formal firms hire workers “off the books” (Ulyssea, 2018). In addition, some formally employed workers may receive part of their compensation informally, through “payments under the table” (Feinmann et al., 2025). This coexistence of formal and informal work is central to our setting, because AFAM-PE eligibility is monitored using only verifiable formal income.

A.2 The AFAM-PE Program

The AFAM-PE program (*Asignaciones Familiares del Plan de Equidad*, Law 18.227) is a means-tested cash transfer program targeted to low-income families with children, implemented in Uruguay in 2008.¹⁷ As a conditional cash transfer, AFAM-PE includes health and

¹⁷During the period analyzed, Uruguay’s social assistance system was shaped by reforms initiated in the mid-2000s. Following the severe economic crisis of 2002–2003, low-paid registered workers and informal workers with children were particularly vulnerable. In response, the government introduced several measures aimed at broadening social protection coverage, including AFAM-PE.

school attendance requirements for children in beneficiary households. The program became one of the main sources of income support for households with children. In 2018, it covered almost 380,000 children under age 18, representing 44% of all children in that age group, and its budget represented about 0.3% of GDP.

A.3 Eligibility and Targeting

AFAM-PE uses two mechanisms to determine eligibility, both at the moment of application and during program participation.

Proxy means test. The first mechanism is a proxy means test, based on a poverty score computed by program officials using socioeconomic characteristics reported by the household on the application form.¹⁸ Households with a poverty score above a predetermined level are eligible to participate in the program. This score is typically computed only when households apply to AFAM-PE and is not subject to continuous verification during participation.

Income test. The second mechanism is an income test. Only households with income below a given eligibility threshold qualify for AFAM-PE. The income definition used for this eligibility check includes all sources of registered income: earnings from formal employment, as reported by employers to tax and social security authorities, and pension income observed in administrative records.¹⁹ We refer to this measure as *verifiable income*, since it corresponds to the income sources that program authorities can observe in administrative records and automatically compare with the eligibility threshold.

The income threshold is defined in per capita terms and varies with family size. Specifically, the family’s verifiable per capita income is calculated by aggregating registered labor earnings and other income sources included in the official eligibility protocol, and dividing this total by the number of household members. Appendix I describes the construction of this

¹⁸The algorithm is based on the coefficients of a probit model in which the dependent variable equals one if the household belongs to the lowest quintile of per capita income and zero for households above the first quintile but below the median of the per capita income distribution. The original model was estimated using a fully saturated function of household variables drawn from Uruguay’s national household survey. The resulting coefficients were used to predict the poverty score for each applicant household based on information from the AFAM-PE application form. For details, see [Amarante and Vigorito \(2011\)](#) and [Bergolo and Cruces \(2021\)](#).

¹⁹Informal earnings are reported at the time of application, but they are not part of the continuous monitoring of income eligibility.

variable in detail. Families with verifiable per capita income below the eligibility threshold qualify for AFAM-PE and remain eligible while complying with the income requirement. At the end of 2008, the per capita income threshold was approximately US\$136, representing about half of the poverty line for a single-person household. The threshold was adjusted periodically according to changes in the official Consumer Price Index.

While AFAM-PE eligibility depends on both the proxy means test and the income test, our analysis focuses on income testing for three reasons. First, income testing is the criterion used by the administration to monitor eligibility during program participation. Second, income eligibility is automatically monitored on a monthly basis using updated administrative records on registered income. Third, the income threshold creates a notch in the household budget set: benefits are not gradually withdrawn as earnings rise, but may be suspended once verifiable income exceeds the threshold. This generates incentives to adjust formal labor supply or earnings reporting.

A.4 Monetary Benefits and the Income Notch

After applying to the program and being deemed eligible, beneficiary families receive a monthly cash transfer that depends on the number of children under age 18 in the household, their school level, and disability status.²⁰ The resulting transfers were relatively generous. During 2008–2012, the transfer represented, on average, about 68% of the minimum wage for an average AFAM household and about 48% of the poverty line (Bergolo and Cruces, 2021).

An important feature of AFAM-PE is that the benefit amount does not depend on household verifiable income, as long as the household remains below the eligibility threshold. Thus, families can increase formal income without any reduction in benefits up to the cutoff, but crossing the threshold may trigger a discrete loss of benefits. This notch is the central institutional feature behind the incentives analyzed in the paper.

²⁰Specifically, the amount of the grant is calculated as

$$grant = \alpha(ben_{017})^{0.6} + \beta(ben_{liceal})^{0.6} + (\alpha + \beta)ben_{disc},$$

where ben_{017} is the number of child beneficiaries, ben_{liceal} is the number of children in high school, ben_{disc} is the number of beneficiaries with disabilities, and α and β correspond to transfer amounts equivalent to approximately US\$33 and US\$14 in 2008.

A.5 Eligibility Monitoring and Sanctions

The AFAM-PE administration conducts automatic monthly checks of income eligibility. When a beneficiary household’s verifiable monthly per capita income exceeds the eligibility threshold, the household receives a sanction. There are two levels of sanctions.

Suspension. When the household does not meet the income requirement, the benefit is suspended for a maximum of three months. During this period, income verification continues. If the household’s updated verifiable income falls below the eligibility threshold, the household automatically re-enters the program and receives the benefit again.

Termination. If the household fails to comply with the income-eligibility requirement for more than three consecutive months, its participation is terminated and no further cash transfers are paid. Terminated households can only return to the program by reapplying and going through the standard eligibility procedures again.

Households can also be sanctioned for reasons other than income noncompliance. For example, if a household fails to comply with health checks or school attendance requirements for one or more children, the payment associated with those children can be blocked until compliance is recertified. For our empirical analysis, however, the relevant sanction is suspension due to income noncompliance.

Finally, beneficiary households receive no warning before benefits are suspended. Moreover, official communications do not clearly specify the reason for noncompliance or how to remedy the situation. This feature is relevant for interpreting enforcement events as potentially informative shocks for beneficiaries and their coworkers.

A.6 Information About Eligibility Requirements

Information about AFAM-PE entry and continuity requirements is limited, especially regarding the income test. Official websites provide information about health and school conditionalities, but little detail on how household vulnerability and income eligibility are assessed or enforced. Interviews with program officers are consistent with the view that information about the income rule was opaque for applicants and beneficiary households.

Survey evidence is consistent with this opacity: in the AFAM-PE Follow-Up Survey,

61% of respondents knew about school or health conditionalities, but only 9% reported being aware that eligibility depended on additional requirements, a category that includes the income rule. While this measure does not isolate knowledge of the income requirement itself, it suggests limited awareness of eligibility criteria beyond the program’s most salient conditions. In addition, 29% of respondents believed that AFAM-PE eligibility did not entail any requirements.²¹ This limited knowledge creates scope for enforcement events to transmit information about the existence and consequences of the income rule.

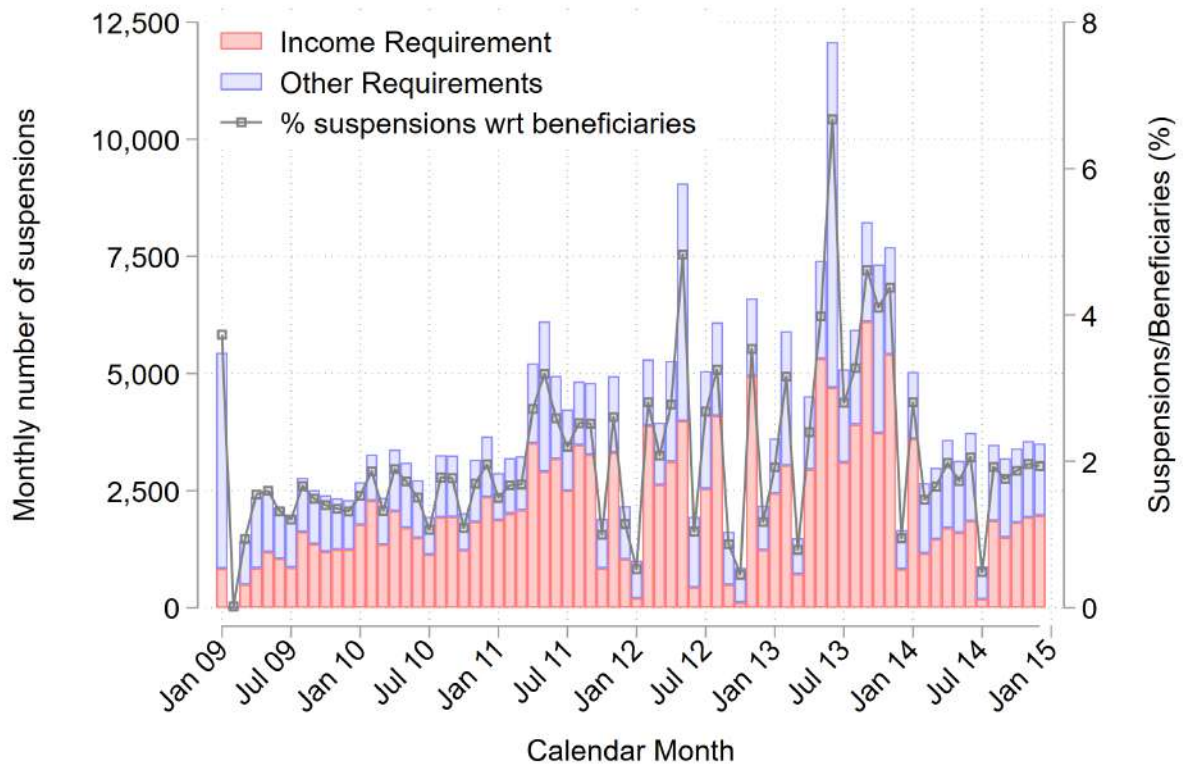
A.7 Benefit Suspensions Over Time

A natural question is whether the income requirement was actively enforced by AFAM-PE authorities during the period of analysis. To examine this issue, we use AFAM-PE administrative records, which contain the history of sanctions for each beneficiary household and the reason for each sanction.

Figure A.1 reports the monthly number of benefit suspensions from 2009 to 2015. Two patterns stand out. First, suspensions occur in most months during this period, with the proportion of suspended households ranging from about 0.5% to 6% of active beneficiaries. Second, suspensions due to income noncompliance account for most of the total number of suspended families, although suspensions due to other requirements are also substantial. These patterns provide direct evidence that the income-eligibility requirement was continuously monitored and that sanctions for noncompliance were enforced.

²¹The AFAM-PE Follow-Up Survey (2011–2013) was designed and implemented by researchers at the Instituto de Economía (IECON) of the Universidad de la República (UDELAR), in collaboration with MIDES and researchers at the Institute of Statistics and the Department of Sociology at UDELAR (Amarante and Vigorito, 2011). Data from this survey were used in previous research by some of the authors of this article; see, for example, Bergolo and Cruces (2021).

Figure A.1: Distribution of Benefit Suspensions Over Time



Notes: This figure shows the monthly number of AFAM benefit suspensions during 2009–2015. Red bars correspond to suspensions due to failure to satisfy the income requirement, while blue bars correspond to suspensions due to other program requirements. The connected line reports the monthly ratio of total suspensions to active beneficiaries (right axis). Data come from AFAM administrative records.

B Sample Construction and Descriptives

This appendix describes the construction of the peer and coworker samples used in the empirical analysis. It details the selection criteria, the definition of the event, and the restrictions imposed to ensure predetermined workplace networks and a well-defined regression discontinuity design.

Peers. We begin with adult individuals (aged 18 or older) belonging to households that received AFAM-PE benefits during 2008–2013. Our empirical strategy exploits quasi-random variation in income eligibility within a narrow window around the AFAM-PE income threshold. We therefore select adult beneficiaries whose household verifiable monthly per capita income first falls within a symmetric window of size 2Δ around the threshold at some point between January 2009 and December 2013. We define this first occurrence as the event month. If a household enters the window more than once during the sample period, we retain only the first event.

In the baseline specification, we set $\Delta = 20\%$, so the peer sample includes adult beneficiaries from households with verifiable monthly per capita income between 20% below and 20% above the eligibility threshold. Although AFAM-PE began operating in 2008, enforcement of the income-eligibility rule was fully implemented only in 2009; accordingly, January 2009 is the first admissible event month. We impose two additional restrictions. First, we exclude beneficiaries whose households had experienced a suspension or termination before the event month. Second, we retain only peers continuously employed at the same firm for at least six months prior to the event. This condition ensures that workplace links are predetermined with respect to the enforcement shock.

Coworkers. Firms in our administrative records are identified using the legal employer identifier used by the social security administration. This definition corresponds to the legal employing unit and may include one or multiple physical establishments. Consequently, coworkers are initially defined at the employer level rather than at the establishment level. To assess whether this distinction affects the interpretation of workplace interactions, the empirical analysis later examines heterogeneity by establishment structure, comparing single-establishment and multi-establishment firms.

Each adult peer is linked to the firm where he or she was employed in the event month. Within those firms, we define coworkers as adult AFAM-PE beneficiaries who had worked at the same firm as the peer for at least six months during the year preceding the event. We exclude coworkers whose households had experienced a benefit suspension before the event month. We further require coworkers' household verifiable monthly per capita income to lie below the lower bound of the peer income window, $\bar{Z} - \Delta$, in the event month. Therefore, coworkers remain income-eligible for AFAM-PE and are not directly exposed to the quasi-random variation induced by the threshold.

A technical complication arises because some coworkers may be linked to multiple peers within the same firm. In such cases, the relevant running variable and treatment assignment become ambiguous, particularly when peers lie on opposite sides of the cutoff. To avoid this issue, the baseline analysis restricts attention to firms with exactly one adult peer in the income window at the relevant event month. During the period of analysis, 32,684 firms employed at least one AFAM-PE beneficiary, of which 12,191 contained at least one peer whose household income entered the eligibility window around the cutoff. Applying the additional restrictions of one peer per firm yields the final estimation sample of 4,154 firms. These restrictions are imposed to ensure a well-defined RD design and predetermined workplace links rather than to select firms based on outcome realizations. Consequently, the estimated effects should be interpreted as local responses for workplace networks with a uniquely identified exposed peer rather than as effects averaged across all possible network configurations within the program.

Panel Construction. We merge the peer and coworker samples with monthly labor market records and retain individuals observed over the 12 months before and after the event month. Firms may contribute more than one event to the analysis provided that the corresponding event windows do not overlap.

Analysis Sample. The final sample contains 5,136 peers and 8,568 coworkers distributed across 4,154 firms. The number of peers exceeds the number of firms because some firms contribute multiple non-overlapping events. Appendix Table [B.1](#) reports descriptive statistics for both samples. On average, selected firms employ more than 44 workers and have 9

AFAM-PE beneficiaries, although the median number of beneficiaries is 5, indicating that some larger firms drive the mean. In the Uruguayan context, these correspond primarily to medium and large employers, which is consistent with the requirement that at least two AFAM-PE beneficiaries work in the same establishment. In terms of worker characteristics, the network is composed mainly of relatively young workers, many of them male and with secondary education. Households of both peers and coworkers contain roughly four members, including at least two adults, of whom only one is formally employed on average. Household and individual labor income are higher for peers than for coworkers, reflecting the sample design: peers are selected near the eligibility cutoff, whereas coworkers are selected from households below the lower bound of the income window.

Table B.1: Descriptive Statistics

	Peers		Coworkers	
	Mean (1)	SD (2)	Mean (1)	SD (2)
<i>Panel A. Individual/Households (HH) characteristics</i>				
Female (%)	40.03	(49.00)	40.85	(49.16)
Age	31.08	(9.84)	31.87	(9.59)
Complete Primary or less (%)	47.88	(49.96)	43.06	(49.52)
Montevideo (capital city) (%)	45.56	(49.81)	43.31	(49.55)
Married/in couple (%)	61.68	(48.62)	61.39	(48.69)
HH size	3.93	(1.32)	4.56	(1.53)
Number of adults	2.16	(0.83)	2.19	(0.87)
Number of children	1.80	(0.94)	2.38	(1.24)
Proxy Means Test	0.18	(0.12)	0.20	(0.12)
Income > threshold (%)	30.49	(46.04)	N/A	(.)
<i>Panel B. Income and employment outcomes</i>				
HH Verifiable per capita Income	5113.82	(689.31)	2522.89	(1054.30)
Total HH Income	19954.05	(7641.13)	11360.37	(5729.61)
HH Labor Income	19699.03	(7593.13)	11288.28	(5684.57)
N formal employed	1.58	(0.63)	1.19	(0.43)
Individual Labor Income	13275.14	(6009.73)	9917.69	(4595.09)
<i>Panel C. Firms</i>				
AFAM beneficiaries	9.24	(13.65)	9.24	(13.65)
Firm Size	44.24	(57.48)	44.24	(57.48)
Number Workers excl. peers and coworkers	38.62	(57.39)	38.62	(57.39)
Average Firm Wage	11068.28	(5866.34)	11068.28	(5866.34)
Worker separations (%)	17.82	(24.48)	17.82	(24.48)
Worker hires (%)	16.41	(19.68)	16.41	(19.68)
Observations	5136		8568	
Number of firms	4154		4154	

Notes: This table reports descriptive statistics for the peer and coworker samples, as defined in Section 3.2. Columns (1) and (3) report sample means, while columns (2) and (4) report standard deviations. Panel A presents individual and household characteristics. Panel B presents income and employment outcomes. Panel C presents firm-level characteristics for firms employing peers and coworkers. All variables are measured six months before the peer's family income falls within the eligibility window, unless otherwise indicated. Data are drawn from administrative social security, tax, and AFAM program records.

C Validity Tests for the RD Design

This appendix presents a series of empirical tests to assess the validity of our RD design.

The first set of tests evaluates whether beneficiaries precisely manipulate the running variable around the income-eligibility cutoff. First, Figure C.1 plots the distribution of household verifiable per capita income around the threshold. The figure shows no noticeable excess mass just below the cutoff. Formal continuity tests for the running variable, based on Cattaneo et al. (2018) and McCrary (2008), also fail to reject the null hypothesis of continuity around the income cutoff (p -value = 0.14). Second, we test whether predetermined characteristics change discontinuously at the threshold. If manipulation were present, we would expect observable characteristics to differ systematically between households just below and just above the cutoff. Table C.1 reports separate RD estimates using peer baseline characteristics as dependent variables and the income-eligibility indicator, $1[z_{k\tau} > 0]$, as the explanatory variable. None of the 13 individual-level variables shows a statistically significant discontinuity at the threshold; the only significant difference appears in worker separations at the firm level. Table C.2 reports analogous tests for coworkers and shows no systematic discontinuities in individual or firm-level characteristics at the cutoff. Overall, these results provide no evidence of precise manipulation and support the validity of the RD design.

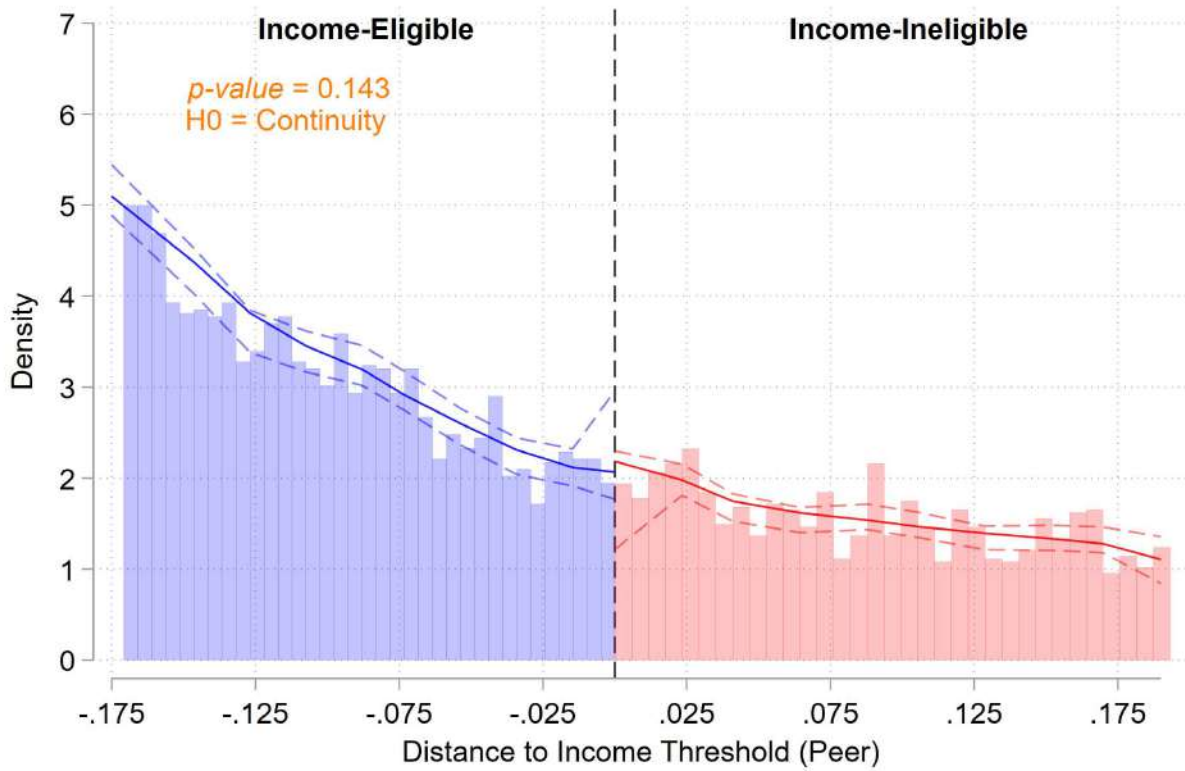
Next, we assess whether correlated workplace shocks drive our results. We implement a placebo test using non-AFAM-PE coworkers employed in the same firms as the peers. If our estimates were driven by a mean-reverting firm-level shock, similar effects should appear among all workers in the firm, regardless of whether they participate in AFAM-PE.

We apply the main empirical strategy to non-beneficiary coworkers with labor income levels comparable to those in our baseline sample. Specifically, we define non-beneficiary coworkers as workers employed in the same firm as the peer for at least six months in the year before the event, and restrict the sample to workers with income below the eligibility threshold and outside the peer income window.²² Appendix Table C.3, columns (1) through

²²Since the income used for verification is measured at the household level, and we do not observe household structure for workers who are not AFAM-PE beneficiaries, we assume that they belong to households of average size (5 individuals) and that their income is the household's only source of formal income. These assumptions are used only to determine eligibility for inclusion in the placebo sample and do not directly enter the estimation procedure. While alternative assumptions could affect sample composition, the purpose of this exercise is to construct a comparison group with comparable economic conditions but without direct exposure to AFAM-PE incentives.

(3), reports placebo estimates for total labor income, labor income conditional on working, and formal employment six months after the peer's event. Panel B reports the reduced-form estimates, while Panel A reports the corresponding 2SLS estimates. The estimated effects are small and statistically insignificant across all margins. In the reduced form, the decline represents less than 1% of the mean of the outcome variable. These results suggest that the main estimates are unlikely to be driven by firm-level shocks affecting all workers in the firm.

Figure C.1: Manipulation Test for Peers' Household Verifiable Income



Notes: This figure presents a density continuity test for the running variable, defined as peers' household verifiable per capita income relative to the eligibility threshold. The test follows [Cattaneo et al. \(2018\)](#). Bars show the empirical distribution on each side of the cutoff, while solid lines denote local polynomial density estimates. The reported p-value corresponds to the null hypothesis of continuity at the threshold. The sample corresponds to the peer sample described in [Section 3.2](#).

Table C.1: RD Differences in Baseline Characteristics (Peers)

	Ineligible Intercept	Eligible Intercept	Difference (2) - (1)	p-value Robust	Opt. bw
	(1)	(2)	(3)	(4)	(5)
<i>Panel A. Individual/Households (HH) characteristics</i>					
Female	35.69	40.98	5.29	.376	.06
Age	32.6	33.92	1.32	.244	.075
Complete Primary or less (%)	64.26	58.36	-5.9	.337	.068
Montevideo (capital city, %)	62.06	67.2	5.14	.337	.049
HH size	3.31	3.43	.12	.513	.067
Number of adults in HH	2.03	2.08	.04	.579	.075
Number of children in HH	1.33	1.37	.04	.871	.064
Proxy Means Test	.08	.07	0	.663	.045
<i>Panel B. Income and employment outcomes</i>					
HH Verifiable per capita Income	5197.99	5195.28	-2.71	.98	.059
Total HH Income	15326.57	16140.56	813.99	.412	.066
HH Labor Income	14953.72	15515.53	561.81	.515	.078
N formal employed	1.14	1.12	-.01	.935	.06
Individual Labor Income	14991.5	14880.2	-111.3	.798	.052
<i>Panel C. Firms</i>					
Firm Size	56.38	58.95	2.57	.648	.065
AFAM beneficiaries	10.31	11.99	1.68	.301	.048
Number Workers excl	48.67	49.9	1.22	.869	.067
Average Firm Wage	10311.18	9500.34	-810.83	.332	.054
Worker separations (%)	17.04	25	7.95	.039	.037
Worker hires (%)	13.82	11.29	-2.53	.312	.048
Observations	5136	5136	5136	5136	5136

Notes: This table reports RD balance tests for the peer sample, as defined in Section 3.2. Each row reports a separate local linear RD regression using the indicated baseline characteristic as the outcome. Column (1) reports the estimated intercept on the income-ineligible side of the cutoff, and column (2) reports the estimated intercept on the income-eligible side. Column (3) reports the difference between columns (2) and (1). Column (4) reports robust p-values, and column (5) reports the MSE-optimal bandwidth selected following Calonico et al. (2019). Estimates use triangular kernel weights. Baseline characteristics are measured six months before the peer's family income falls within the eligibility window. Robust standard errors are clustered at the firm level.

Table C.2: RD Differences in Baseline Characteristics (Coworkers)

	Ineligible Intercept	Eligible Intercept	Difference (2) - (1)	p-value Robust	Opt. bw
	(1)	(2)	(3)	(4)	(5)
<i>Panel A. Individual/Households (HH) characteristics</i>					
Female	38.73	46.27	7.54	.235	.08
Age	31.45	31.53	.07	.969	.06
Complete Primary or less (%)	38.76	40.19	1.43	.72	.07
Montevideo (capital city, %)	54.45	51.89	-2.56	.764	.05
HH size	4.77	4.91	.14	.38	.05
Number of adults in HH	2.18	2.25	.07	.515	.05
Number of children in HH	2.58	2.6	.02	.774	.06
Proxy Means Test	.26	.25	-.01	.635	.06
<i>Panel B. Income and employment outcomes</i>					
HH Verifiable per capita Income	1741.13	1675.81	-65.32	.493	.06
Total HH Income	7378	7400.74	22.75	.903	.06
HH Labor Income	7506.48	7442.16	-64.32	.954	.06
N formal employed	.98	1.05	.06	.118	.06
Individual Labor Income	7544.01	7206.93	-337.07	.614	.05
<i>Panel C. Firms</i>					
Firm Size	57.52	61.17	3.64	.559	.05
AFAM beneficiaries	12.6	14.09	1.48	.33	.04
Number Workers excl	50.37	53.04	2.66	.722	.06
Average Firm Wage	9399.72	9249.04	-150.69	.873	.08
Worker separations (%)	25.9	31.39	5.49	.104	.04
Worker hires (%)	12.12	10.44	-1.68	.508	.06
Observations	8568	8568	8568	8568	8568

Notes: This table reports RD balance tests for the coworker sample, as defined in Section 3.2. Each row reports a separate local linear RD regression using the indicated baseline characteristic as the outcome. Column (1) reports the estimated intercept on the income-ineligible side of the cutoff, and column (2) reports the estimated intercept on the income-eligible side. Column (3) reports the difference between columns (2) and (1). Column (4) reports robust p-values, and column (5) reports the MSE-optimal bandwidth selected following Calonico et al. (2019). Estimates use triangular kernel weights. Baseline characteristics are measured six months before the peer's family income falls within the eligibility window. Robust standard errors are clustered at the firm level.

Table C.3: Placebo Test Using Non-Beneficiary Coworkers in the Same Firm as Peers

	Total Labor Income (1)	Labor Income > 0 (2)	Prob. Formal Employment (3)
<i>Panel A. Fuzzy RDD Estimate</i>			
Peers' Suspension (HH Income > Thresh.)	-311.18 (2662.68)	883.73 (2236.37)	0.00 (0.15)
Robust p-value	0.787	0.427	0.683
<i>Panel B. Reduced Form</i>			
Income-Eligibility Enforcement	-64.11 (537.64)	154.73 (422.57)	0.00 (0.03)
Robust p-value	0.782	0.435	0.686
Mean baseline	10358.25	14900.98	0.71
Effect size (%)	-0.62	1.04	0.16
<i>Panel C. First Stage</i>			
Income-Eligibility Enforcement	0.20*** (0.08)	0.20*** (0.08)	0.18** (0.07)
Robust p-value	0.006	0.007	0.016
Opt. Bandwidth	.045	.04	.055
Observations	24922	17440	31480

Notes: This table reports placebo RD estimates using non-beneficiary coworkers employed in the same firm as peers. Outcomes are measured six months after the peer's income-testing event. Panel A reports the fuzzy RD estimates (IV) based on the 2SLS estimation of Equation 1. Panel B reports the reduced-form estimates based on Equation 3. Panel C reports the first-stage estimates based on Equation 2. All specifications use local linear regressions with triangular kernel weights and MSE-optimal bandwidths following Calonico et al. (2019). Robust standard errors clustered at the firm level are reported in parentheses. Mean baseline reports the average outcome among coworkers of peers below the income cutoff (i.e., coworkers of peers who qualify for AFAM-PE). Effect size reports the estimated coefficient as a percentage of the baseline mean. Statistical significance based on robust p-values: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The absence of significant reduced-form effects is consistent with the interpretation that main results operate through beneficiary-specific information or incentive channels. The sample corresponds to the placebo coworker sample described in Section 3.2.

D Additional Results on Labor Supply, Informal Employment, and Compliance

This appendix presents additional results that complement the main analysis in two ways. First, we examine whether the decline in coworkers’ formal employment reflects changes in working time, non-employment, or transitions into informal employment. Second, we assess whether the decline in household verifiable per capita income documented in the main text is driven by changes in total household verifiable income or by changes in reported household size.

We first examine whether peers’ income-eligibility enforcement affects coworkers’ working time in their formal jobs, measured by days and hours worked in the SSA records. Table D.1 reports the corresponding fuzzy RD estimates. Columns (1) and (2) focus on days worked for all coworkers and for those who remain formally employed, respectively. Columns (3) and (4) repeat the exercise using hours worked. We find no statistically significant effects in any specification, consistent with the null effects on the intensive margin of formal employment documented in the main text.

We then assess whether the decline in formal employment reflects an increase in non-employment or a shift toward informal employment. Since SSA records only capture registered employment, an exit from these records may correspond either to non-employment or to a transition into informal work. To distinguish between these margins, we use information from the AFAM-PE Follow-Up Survey, conducted between 2011 and 2013. The survey covers the post-event period for a subset of our sample and includes self-reported measures of registered employment, informal employment, unemployment, and labor market non-participation for each adult household member. We implement an exact matching procedure to select one worker from the follow-up survey for each coworker in our main sample. The matching variables include the household poverty score, measured in bins of 0.5 percentage points, sex, age, region, and survey month.²³

Table D.2 reports the results for the survey-linked subsample. Columns (1) through (3)

²³We also linked the coworker sample to the follow-up survey using unique individual identifiers rather than statistical matching procedures. However, only 77 observations were successfully merged.

replicate the main labor market results from Table 1 using administrative records, restricted to this subsample. The 2SLS estimates in Panel A are qualitatively similar to those in the full sample, although somewhat smaller in magnitude: the decline in total registered labor income is driven primarily by a statistically significant 19 p.p. reduction in formal employment.

Columns (4) through (8) report the corresponding fuzzy RD estimates using self-reported outcomes from the AFAM-PE Follow-Up Survey, including measures of formal employment, informal employment, and non-employment. Two results stand out. First, the survey-based estimates for formal labor income and formal employment are broadly consistent with those based on administrative records (Columns (4) to (6)). Second, the survey evidence is consistent with the decline in formal employment induced by peers' income-eligibility enforcement being associated with a shift toward informal employment rather than exit from the labor market. The probability of informal employment increases by 16 p.p. (p -value = 0.043), while the effect in non-employment is 3 p.p. and statistically insignificant (p -value = 0.526). These estimates indicate that the decline in formal employment is largely driven by transitions into informal employment.

Finally, we examine whether the decline in household verifiable per capita income documented in the main text reflects changes in total household verifiable income or changes in reported household size. Figure D.1, Panels (a) and (b), presents the reduced-form RD effects of peers' income eligibility on total household verifiable income and household size, respectively. Columns (3) and (4) of Table 2 report the corresponding fuzzy RD estimates.

Panel (a) shows a negative discontinuity in coworkers' total household verifiable income at the eligibility cutoff, although the effect is not statistically significant at conventional levels (p -value = 0.205). Panel (b) shows that household size remains stable around the cutoff. This indicates that the observed decline in the key enforcement variable—household verifiable per capita income—is not driven by changes in the reported number of household members.

Table D.1: RD Estimates of the Effect of Peers' Suspension on Coworkers' Labor Supply Outcomes

	Total Days Worked (1)	Days Worked > 0 (2)	Total Hours Worked (3)	Hours Worked (4)
<i>Panel A. Fuzzy RDD Estimate</i>				
Peers' Suspension (HH Income > Thresh.)	-2.36 (3.11)	1.62 (2.23)	-5.73 (7.24)	2.23 (6.37)
Robust p-value	0.477	0.423	0.469	0.641
<i>Panel B. Reduced Form</i>				
Income-Eligibility Enforcement	-1.49 (1.74)	0.62 (1.31)	-2.61 (4.11)	1.85 (3.97)
Robust p-value	0.394	0.591	0.585	0.521
Mean baseline	14.21	20.66	33.92	49.35
Effect size (%)	-10.47	2.99	-7.69	3.74
<i>Panel C. First Stage</i>				
Income-Eligibility Enforcement	0.58*** (0.07)	0.57*** (0.07)	0.58*** (0.07)	0.58*** (0.08)
Robust p-value	0.000	0.000	0.000	0.000
Opt. Bandwidth	.043	.0525	.0435	.036
Observations	1509	1277	1527	884

Notes: This table reports RD estimates of the effect of peers' income-eligibility enforcement on coworkers' labor market outcomes measured six months after the peer's income-testing event. Panel A reports the fuzzy RD estimates (IV) based on the 2SLS estimation of Equation 1. Panel B reports the reduced-form estimates based on Equation 3. Panel C reports the first-stage estimates based on Equation 2. The sample corresponds to the coworker sample described in Section 3.2, in which each peer-coworker link constitutes one observation. All specifications use local linear regressions with triangular kernel weights and MSE-optimal bandwidths following Calonico et al. (2019). Robust standard errors clustered at the firm level are reported in parentheses. Mean baseline reports the average outcome among coworkers of peers below the income cutoff (i.e., coworkers of peers who qualify for AFAM-PE). Effect size reports the estimated coefficient as a percentage of the baseline mean. Statistical significance based on robust p-values: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

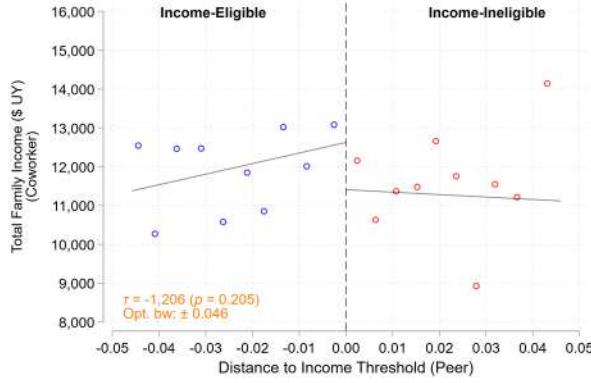
Table D.2: RD Estimates of the Effect of Peers' Suspension on Coworkers' Formal and Informal Labor Market Outcomes

	Adm. Records			Follow-up Survey				
	Formal Labor Income (1)	Formal Labor Income > 0 (2)	Prob. Formal (3)	Formal Labor Income (4)	Formal Labor Income > 0 (5)	Prob. Formal (6)	Prob. Non Employed (7)	Prob. Informal (8)
<i>Panel A. Fuzzy RDD Estimate</i>								
Peers' Suspension (HH Income > Thresh.)	-2688.17 (2286.62)	-731.72 (1973.02)	-0.19 (0.13)	-1209.13 (2110.22)	434.21 (2714.48)	-0.16* (0.11)	-0.03 (0.06)	0.16** (0.10)
Robust p-value	0.194	0.837	0.102	0.594	0.745	0.088	0.526	0.043
<i>Panel B. Reduced Form</i>								
Income-Eligibility Enforcement	-1318.15 (1498.57)	96.30 (1270.49)	-0.12 (0.09)	-634.94 (1455.89)	440.89 (1711.10)	-0.09 (0.08)	-0.02 (0.04)	0.09* (0.07)
Robust p-value	0.301	0.783	0.115	0.691	0.660	0.153	0.535	0.082
Mean baseline	8808.38	11214.22	0.78	5646.65	6574.39	0.88	0.03	0.09
Effect size (%)	-14.96	0.86	-15.12	-11.24	6.71	-10.71	-58.33	105.98
<i>Panel C. First Stage</i>								
Income-Eligibility Enforcement	0.69*** (0.11)	0.69*** (0.11)	0.69*** (0.11)	0.69*** (0.12)	0.69*** (0.12)	0.69*** (0.12)	0.69*** (0.12)	0.70*** (0.12)
Robust p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Opt. Bandwidth	0.060	0.060	0.060	0.055	0.055	0.050	0.050	0.055
Observations	407	403	403	375	368	342	342	366

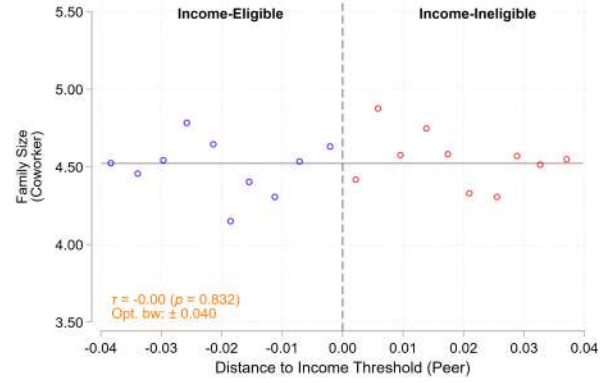
Notes: This table reports RD estimates of the effect of peers' income-eligibility enforcement on coworkers' formal and informal labor market outcomes measured six months after the peer's income-testing event. Panel A reports the fuzzy RD estimates (IV) based on the 2SLS estimation of Equation 1. Panel B reports the reduced-form estimates based on Equation 3. Panel C reports the first-stage estimates based on Equation 2. The sample corresponds to the coworker sample described in Section 3.2, in which each peer-coworker link constitutes one observation. Columns (1)–(3) report formal labor market outcomes constructed from administrative social security records. Columns (4)–(6) report the corresponding formal labor market outcomes using self-reported information from the Follow-up Survey, while columns (7)–(8) report survey-based measures of informal employment and non-employment. All specifications use local linear regressions with triangular kernel weights and MSE-optimal bandwidths following Calonico et al. (2019). Robust standard errors clustered at the firm level are reported in parentheses. Mean baseline reports the average outcome among coworkers of peers below the income cutoff (i.e., coworkers of peers who qualify for AFAM-PE). Effect size reports the estimated coefficient as a percentage of the baseline mean. Statistical significance based on robust p-values: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Sample sizes vary across columns according to data availability.

Figure D.1: Effect of Peers' Eligibility on Coworkers' Family Outcomes

(a) *Household Verifiable Income*



(b) *Household Size*



Notes: This figure presents reduced-form effects of peers' income eligibility on coworkers' family outcomes: household verifiable income in panel (a) and household size in panel (b). Outcomes are measured six months after the peer's income-testing event. Estimates are obtained from Equation 3 using local linear regressions with triangular kernel weights and MSE-optimal bandwidths following [Calonico et al. \(2019\)](#). Dots denote mean outcomes within 10 equally spaced bins and solid lines denote fitted values on each side of the cutoff. The reported discontinuity corresponds to the local reduced-form effect at the threshold. Robust standard errors are clustered at the firm level. The sample corresponds to the coworker sample described in Section 3.2.

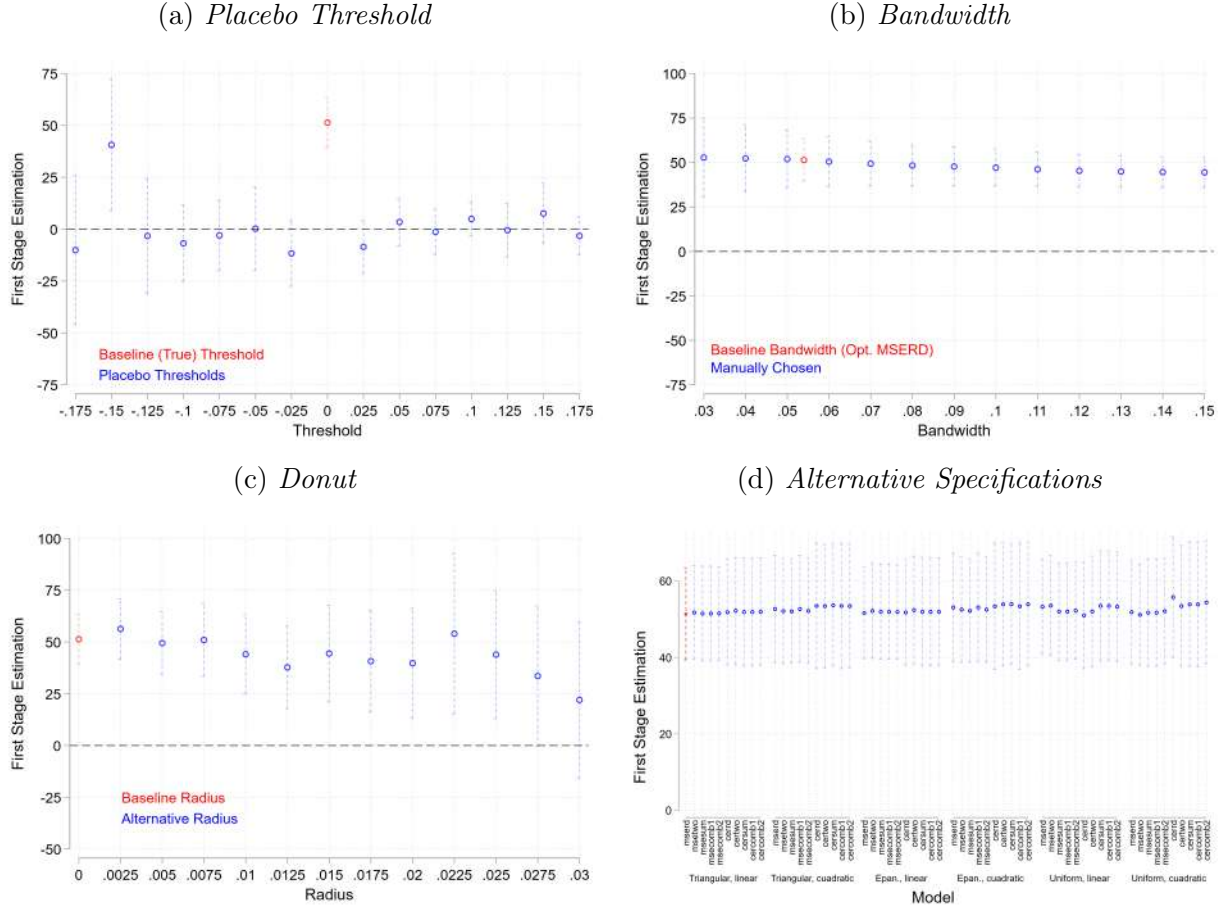
E Robustness Tests

We assess the sensitivity of the baseline spillover estimates to alternative specifications and standard RD implementation choices.

Figures [E.1](#) through [E.7](#) report reduced-form RD estimates for alternative RD implementation choices. First, we conduct falsification tests using placebo eligibility thresholds. Panel (a) of each figure displays the distribution of placebo reduced-form estimates, showing that the baseline estimates are unlikely to be driven by chance. Second, we examine sensitivity to alternative bandwidth choices. Panel (b) of each figure shows that the estimates for the outcomes that are statistically significant in the baseline specification remain statistically significant across bandwidths. Estimates based on smaller windows are somewhat larger but less precise, while estimates based on larger windows are smaller but more precise. The intensive-margin estimates remain close to zero across bandwidth choices, consistent with the baseline results. Third, Panels (c) report donut RD estimates that exclude observations close to the cutoff. The estimates remain statistically significant and, if anything, become somewhat larger as the donut radius increases. Finally, Panel (d) combines alternative RD implementation choices, including kernel functions, polynomial orders, and bandwidth-selection procedures. The results are essentially unchanged when using alternative weights or polynomial specifications, and vary only slightly under alternative optimization algorithms.

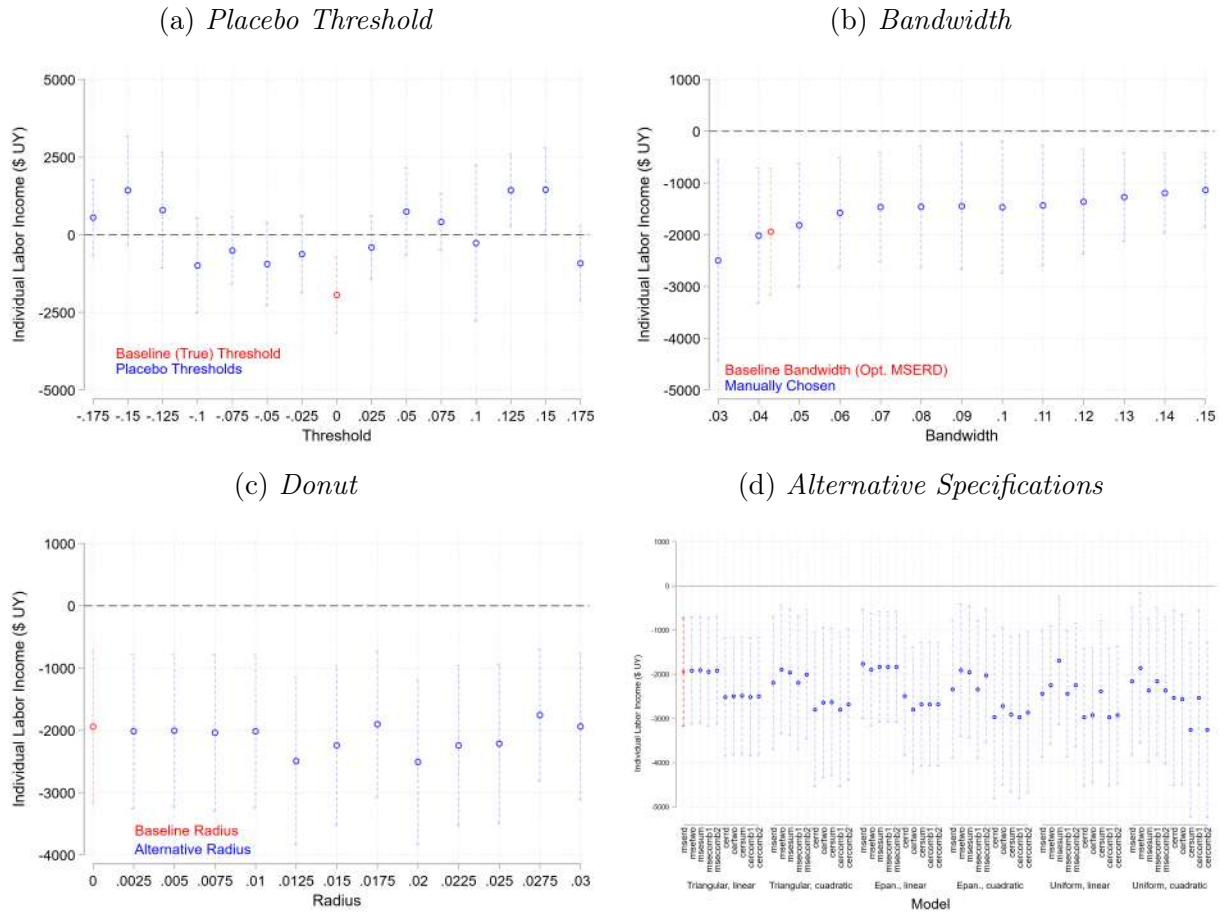
We also assess the robustness of our main labor market and household-level results using two alternative regression specifications. Tables [E.1](#) and [E.2](#) report estimates that exclude all predetermined control variables. The results are qualitatively similar to the baseline estimates. Tables [E.3](#) and [E.4](#) display estimates clustering standard errors at the household rather than firm level. The estimated standard errors and statistical significance are very similar to those in the baseline specification. Overall, the estimates are robust to the main implementation choices in the RD design.

Figure E.1: Robustness of the First-Stage Effect of Eligibility on AFAM Benefit Suspension



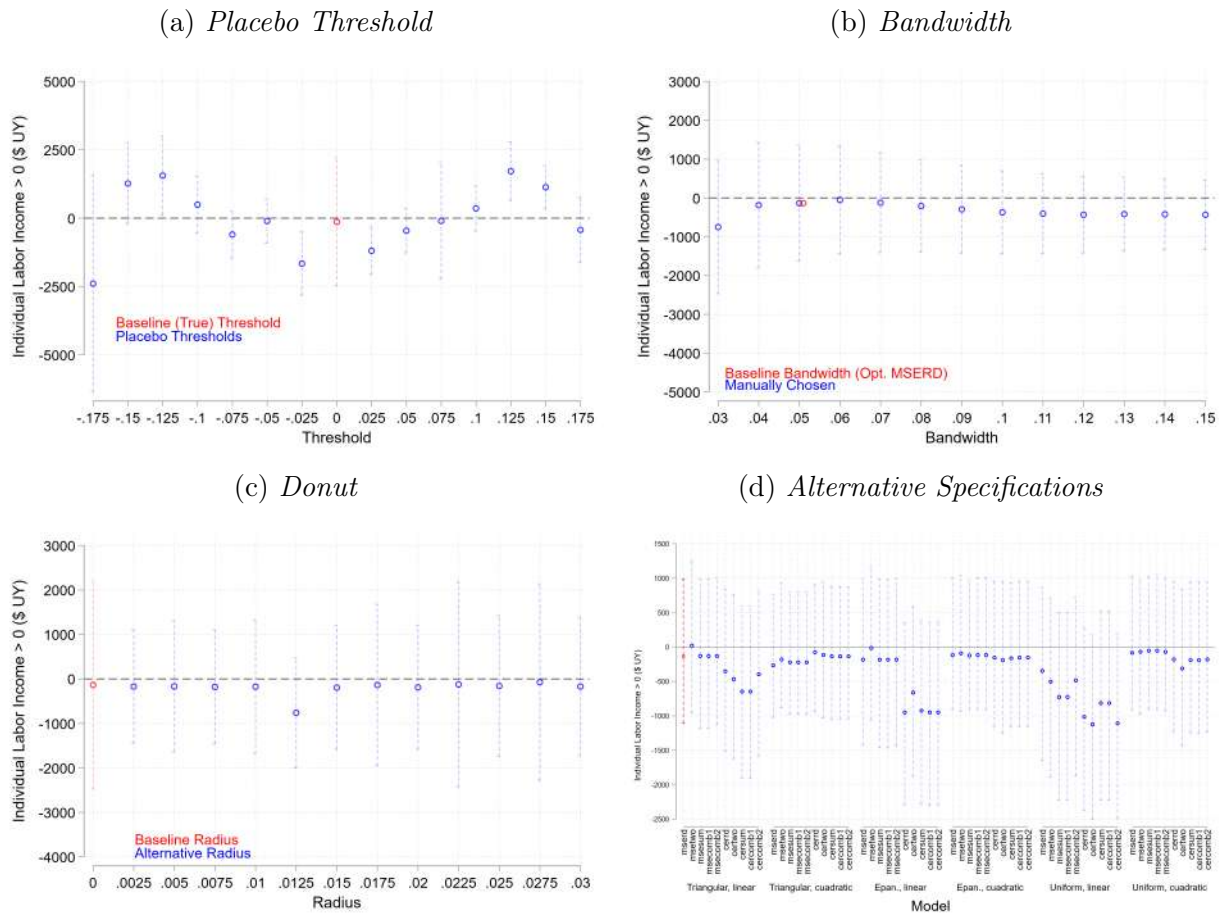
Notes: Each panel reports a robustness exercise for the baseline RD specification. Panel (a) presents placebo threshold estimates using alternative false eligibility cutoffs. Panel (b) reports estimates using alternative bandwidth choices. Panel (c) reports donut RD estimates excluding observations close to the threshold under alternative exclusion radii. Panel (d) reports estimates from alternative specifications varying the polynomial order, kernel function, and bandwidth selection procedure. Red markers denote the baseline estimate, while blue markers denote alternative estimates. Vertical bars denote 90 percent confidence intervals based on robust standard errors. Baseline specifications use local linear regressions with triangular kernel weights and MSE-optimal bandwidths following [Calonico et al. \(2019\)](#). The sample corresponds to the peer sample described in Section 3.2.

Figure E.2: Robustness of Reduced-Form Effects on Coworkers' Total Labor Income



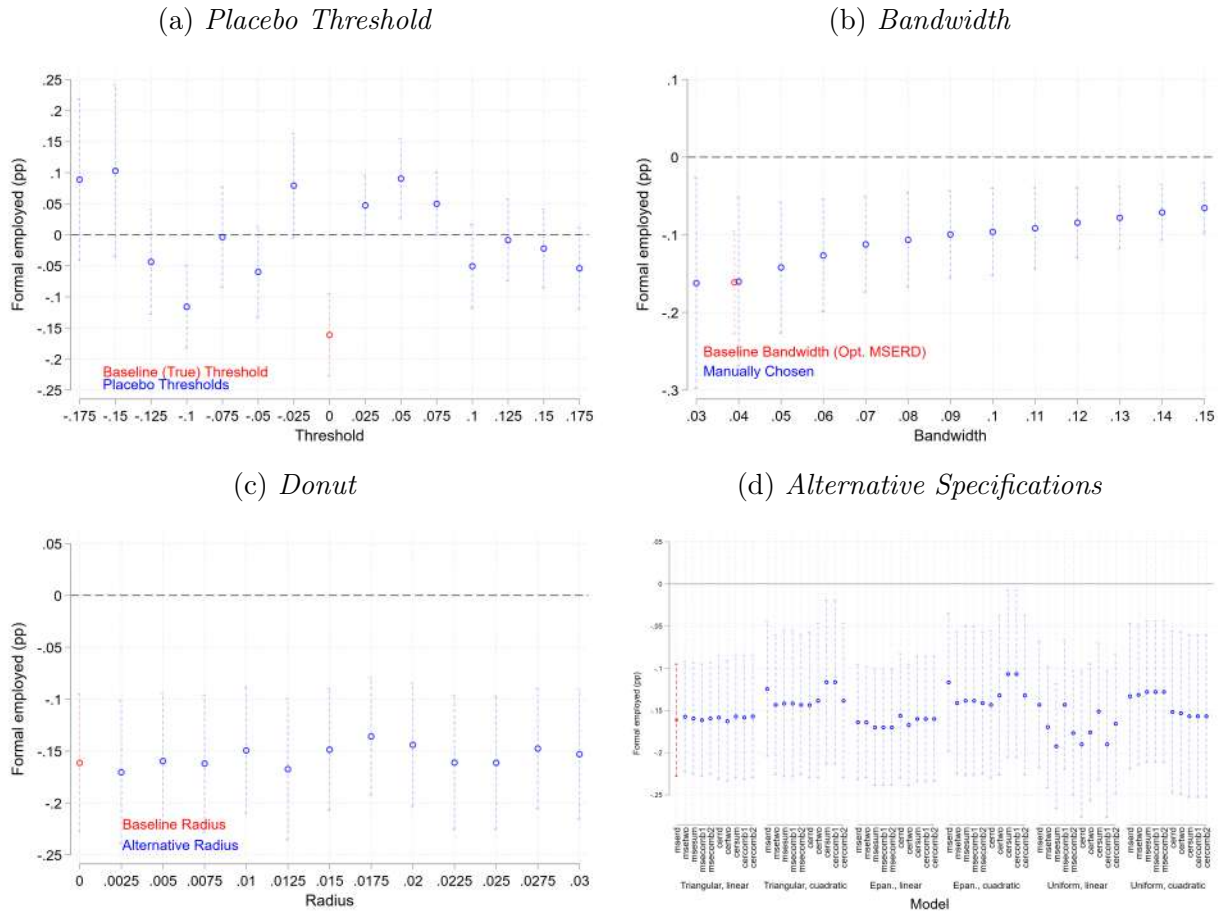
Notes: See notes to Figure E.1.

Figure E.3: Robustness of Reduced-Form Effects on Coworkers' Positive Labor Income



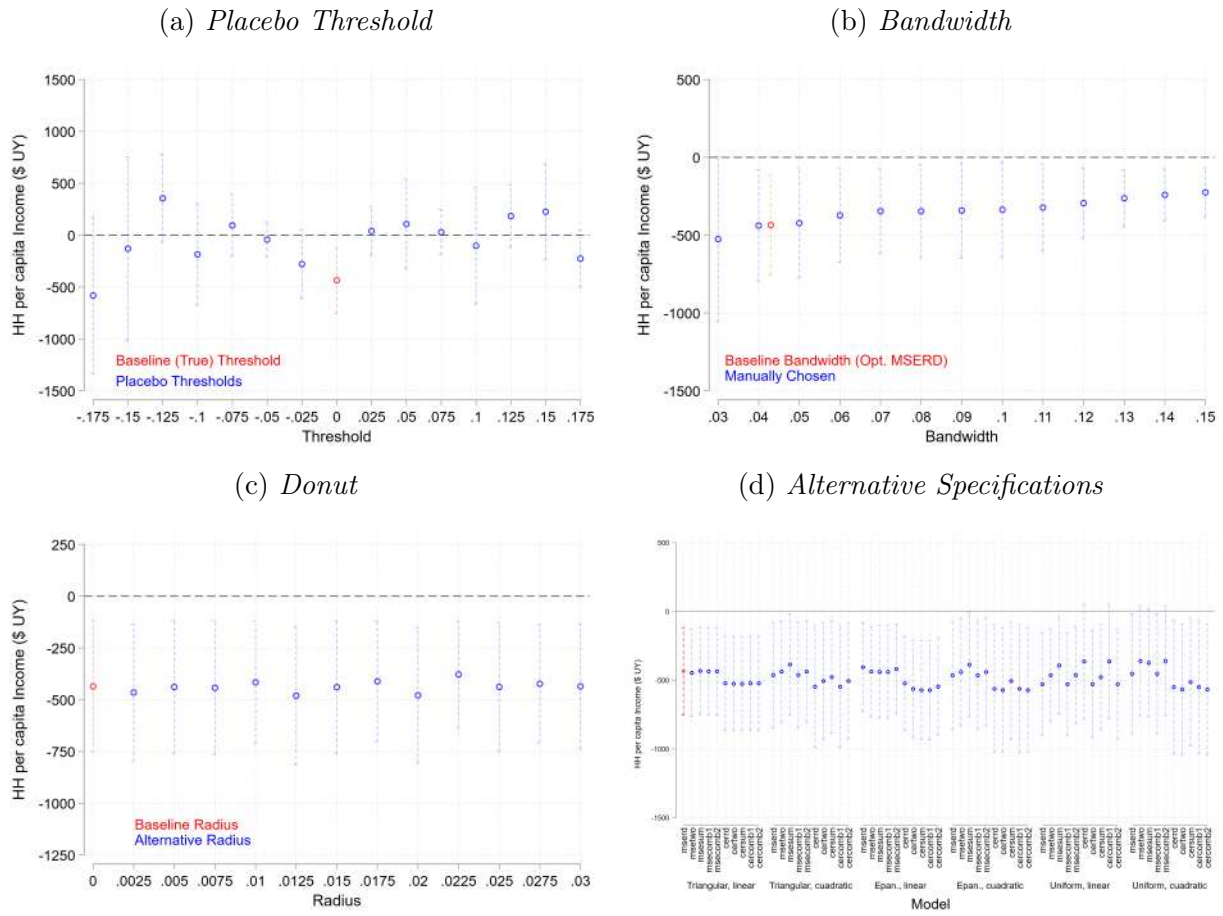
Notes: See notes to Figure E.1.

Figure E.4: Robustness of Reduced-Form Effects on Coworkers' Formal Employment



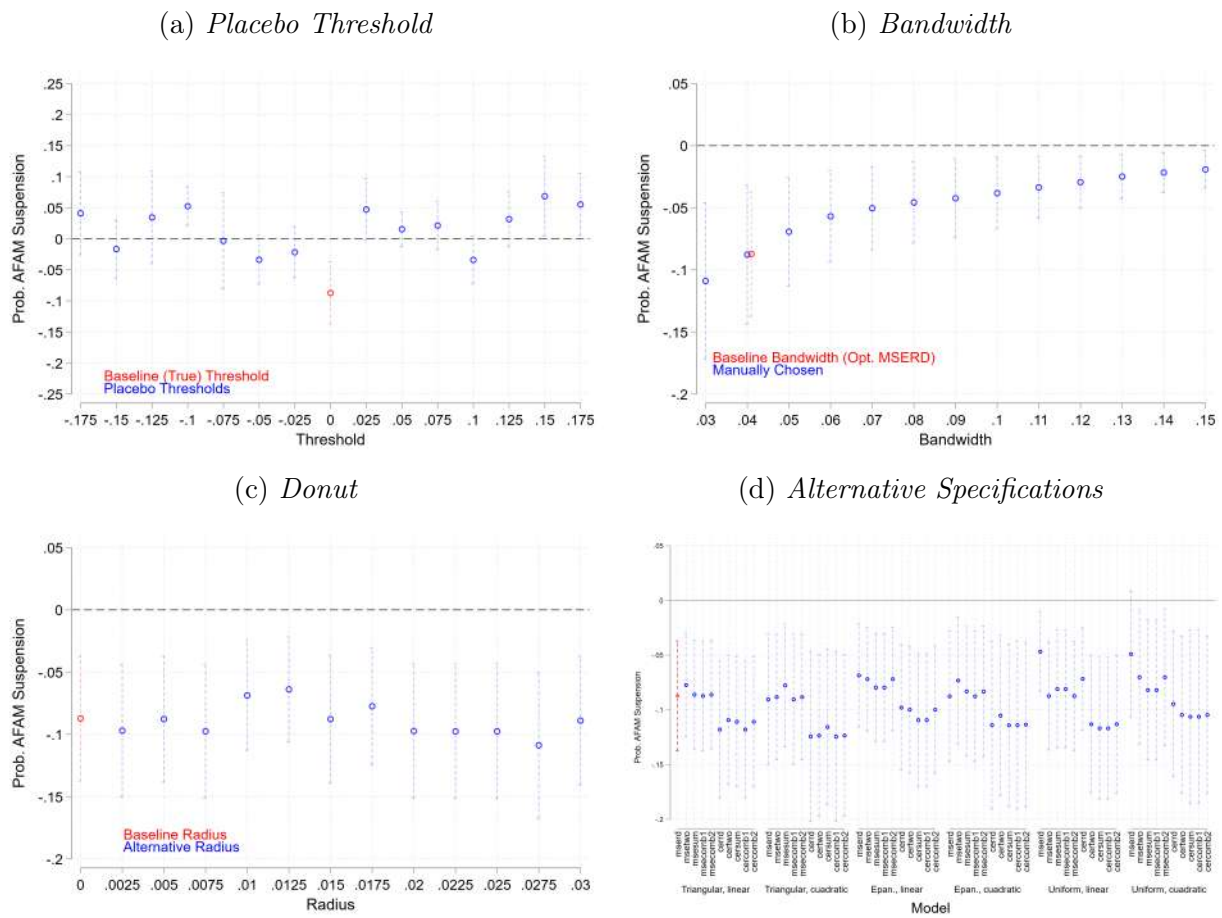
Notes: See notes to Figure E.1.

Figure E.5: Robustness of Reduced-Form Effects on Coworkers' Household Verifiable per Capita Income



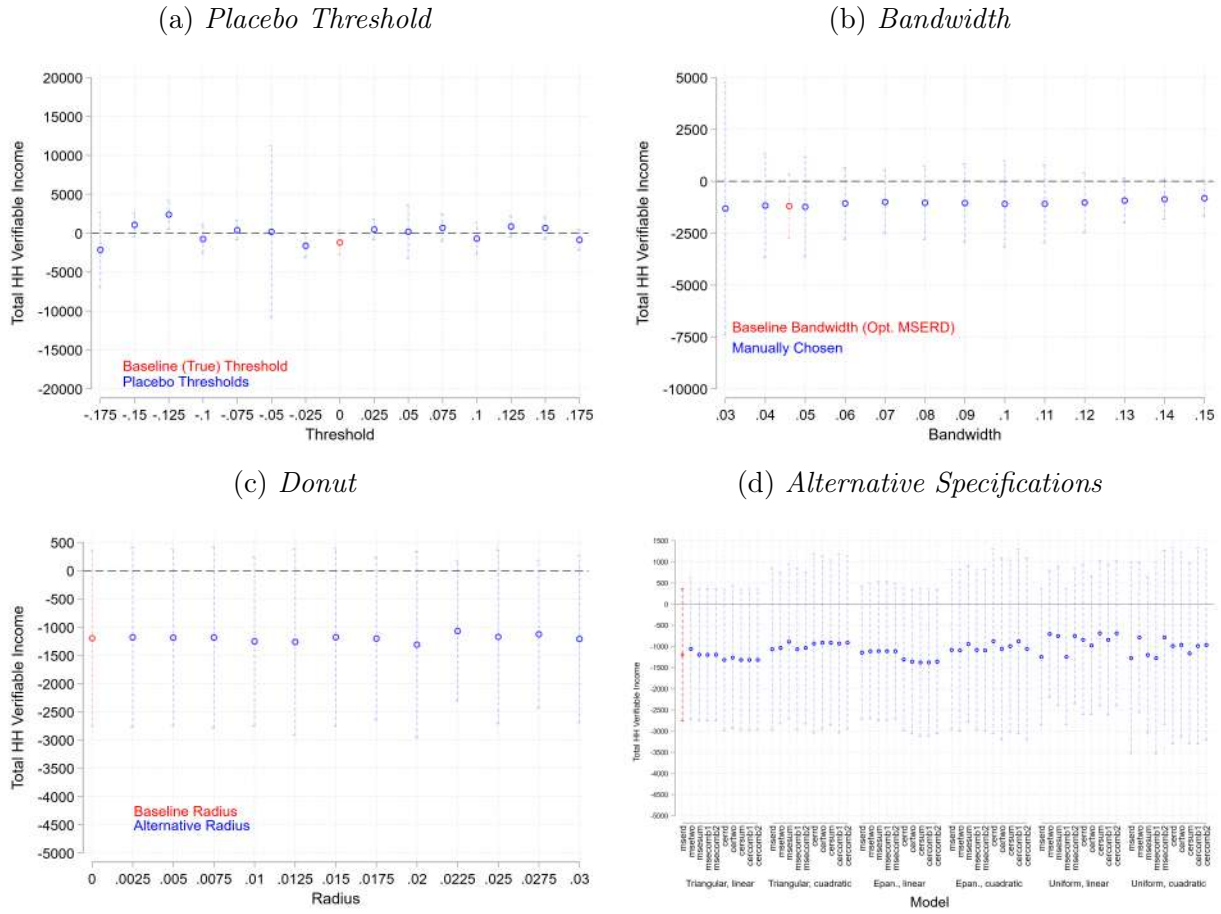
Notes: See notes to Figure E.1.

Figure E.6: Robustness of Reduced-Form Effects on Coworkers' Benefit Suspension



Notes: See notes to Figure E.1.

Figure E.7: Robustness of Reduced-Form Effects on Coworkers' Household Verifiable Total Income



Notes: See notes to Figure E.1.

Table E.1: RD Estimates of the Effect of Peers' Suspension on Coworkers' Labor Market Outcomes, Without Controls

	Total Labor Income (1)	Labor Income > 0 (2)	Prob. Formal Employment (3)
<i>Panel A. Fuzzy RDD Estimate</i>			
Peers' Suspension (HH Income > Thresh.)	-2812.90** (1350.70)	-208.26 (1380.34)	-0.29*** (0.09)
Robust p-value	0.048	0.971	0.001
<i>Panel B. Reduced Form</i>			
Income-Eligibility Enforcement	-1715.75** (731.87)	-207.95 (789.96)	-0.16*** (0.04)
Robust p-value	0.018	0.852	0.000
Mean baseline	9815.38	11256.51	0.87
Effect size (%)	-17.48	-1.85	-18.71
<i>Panel C. First Stage</i>			
Income-Eligibility Enforcement	0.56*** (0.07)	0.57*** (0.07)	0.57*** (0.08)
Robust p-value	0.000	0.000	0.000
Opt. Bandwidth	.05	.05	.035
Observations	1835	1462	1324

Notes: This table reports robustness RD estimates of the effect of peers' income-eligibility enforcement on coworkers' labor market outcomes measured six months after the peer's income-testing event. Specifications exclude baseline controls. Panel A reports the fuzzy RD estimates (IV) based on the 2SLS estimation of Equation 1. Panel B reports the reduced-form estimates based on Equation 3. Panel C reports the first-stage estimates based on Equation 2. The sample corresponds to the coworker sample described in Section 3.2, in which each peer-coworker link constitutes one observation. All specifications use local linear regressions with triangular kernel weights and MSE-optimal bandwidths following Calonico et al. (2019). Robust standard errors clustered at the firm level are reported in parentheses. Mean baseline reports the average outcome among coworkers of peers below the income cutoff (i.e., coworkers of peers who qualify for AFAM-PE). Effect size reports the estimated coefficient as a percentage of the baseline mean. Statistical significance based on robust p-values: *** p<0.01, ** p<0.05, * p<0.10.

Table E.2: RD Estimates of the Effect of Peers' Suspension on Coworkers' Household Outcomes, Without Controls

	HH Verifiable per capita Income (1)	Prob. AFAM-PE Suspension (2)	Total HH Verifiable Income (3)	Household Size (4)
<i>Panel A. Fuzzy RDD Estimate</i>				
Peers' Suspension (HH Income > Thresh.)	-790.14* (454.59)	-0.11* (0.07)	-1585.59 (2149.84)	0.23 (0.31)
Robust p-value	0.078	0.081	0.509	0.414
<i>Panel B. Reduced Form</i>				
Income-Eligibility Enforcement	-481.00** (219.75)	-0.07** (0.03)	-1118.65 (1091.11)	0.11 (0.16)
Robust p-value	0.020	0.032	0.305	0.395
Mean baseline	2701.41	0.08	11929.23	4.53
Effect size (%)	-17.81	-78.65	-9.38	2.52
<i>Panel C. First Stage</i>				
Income-Eligibility Enforcement	0.53*** (0.08)	0.53*** (0.09)	0.52*** (0.08)	0.50*** (0.07)
Robust p-value	0.000	0.000	0.000	0.000
Opt. Bandwidth	.0535	.0485	.055	.0695
Observations	1878	1738	1937	2422

Notes: This table reports robustness RD estimates of the effect of peers' income-eligibility enforcement on coworkers' household outcomes measured six months after the peer's income-testing event. Specifications exclude baseline controls. Panel A reports the fuzzy RD estimates (IV) based on the 2SLS estimation of Equation 1. Panel B reports the reduced-form estimates based on Equation 3. Panel C reports the first-stage estimates based on Equation 2. The sample corresponds to the coworker sample described in Section 3.2, in which each peer-coworker link constitutes one observation. All specifications use local linear regressions with triangular kernel weights and MSE-optimal bandwidths following Calonico et al. (2019). Robust standard errors clustered at the firm level are reported in parentheses. Mean baseline reports the average outcome among coworkers of peers below the income cutoff (i.e., coworkers of peers who qualify for AFAM-PE). Effect size reports the estimated coefficient as a percentage of the baseline mean. Statistical significance based on robust p-values: *** p<0.01, ** p<0.05, * p<0.10.

Table E.3: RD Estimates of the Effect of Peers' Suspension on Coworkers' Labor Market Outcomes, Household-Level Clustering

	Total Labor Income (1)	Labor Income > 0 (2)	Prob. Formal Employment (3)
<i>Panel A. Fuzzy RDD Estimate</i>			
Peers' Suspension (HH Income > Thresh.)	-3582.07** (1496.90)	-117.67 (1179.49)	-0.29*** (0.09)
Robust p-value	0.013	0.980	0.001
<i>Panel B. Reduced Form</i>			
Income-Eligibility Enforcement	-2173.18*** (830.04)	-132.48 (662.88)	-0.16*** (0.05)
Robust p-value	0.005	0.868	0.000
Mean baseline	9847.25	11238.13	0.87
Effect size (%)	-22.07	-1.18	-18.51
<i>Panel C. First Stage</i>			
Income-Eligibility Enforcement	0.57*** (0.05)	0.56*** (0.05)	0.58*** (0.05)
Robust p-value	0.000	0.000	0.000
Opt. Bandwidth	.035	.05	.03
Observations	1315	1471	1140

Notes: This table reports robustness RD estimates of the effect of peers' income-eligibility enforcement on coworkers' labor market outcomes measured six months after the peer's income-testing event. Robust standard errors are clustered at the household level. Panel A reports the fuzzy RD estimates (IV) based on the 2SLS estimation of Equation 1. Panel B reports the reduced-form estimates based on Equation 3. Panel C reports the first-stage estimates based on Equation 2. The sample corresponds to the coworker sample described in Section 3.2, in which each peer-coworker link constitutes one observation. All specifications use local linear regressions with triangular kernel weights and MSE-optimal bandwidths following Calonico et al. (2019). Mean baseline reports the average outcome among coworkers of peers below the income cutoff (i.e., coworkers of peers who qualify for AFAM-PE). Statistical significance based on robust p-values: *** p<0.01, ** p<0.05, * p<0.10.

Table E.4: RD Estimates of the Effect of Peers' Suspension on Coworkers' Household Outcomes, Household-Level Clustering

	HH Verifiable per capita Income (1)	Prob. AFAM-PE Suspension (2)	Total HH Verifiable Income (3)	Household Size (4)
<i>Panel A. Fuzzy RDD Estimate</i>				
Peers' Suspension (HH Income > Thresh.)	-685.39* (415.71)	-0.15** (0.07)	-1534.77 (1833.74)	0.00 (0.00)
Robust p-value	0.092	0.022	0.430	0.542
<i>Panel B. Reduced Form</i>				
Income-Eligibility Enforcement	-466.96** (229.29)	-0.09*** (0.04)	-1194.67 (1026.10)	-0.00 (0.00)
Robust p-value	0.031	0.008	0.228	0.707
Mean baseline	2712.27	0.09	11819.06	4.50
Effect size (%)	-17.22	-102.42	-10.11	0.00
<i>Panel C. First Stage</i>				
Income-Eligibility Enforcement	0.57*** (0.05)	0.57*** (0.05)	0.56*** (0.05)	0.57*** (0.05)
Robust p-value	0.000	0.000	0.000	0.000
Opt. Bandwidth	.036	.0385	.044	.036
Observations	1288	1368	1547	1293

Notes: This table reports robustness RD estimates of the effect of peers' income-eligibility enforcement on coworkers' household outcomes measured six months after the peer's income-testing event. Robust standard errors are clustered at the household level. Panel A reports the fuzzy RD estimates (IV) based on the 2SLS estimation of Equation 1. Panel B reports the reduced-form estimates based on Equation 3. Panel C reports the first-stage estimates based on Equation 2. The sample corresponds to the coworker sample described in Section 3.2, in which each peer-coworker link constitutes one observation. All specifications use local linear regressions with triangular kernel weights and MSE-optimal bandwidths following Calonico et al. (2019). Mean baseline reports the average outcome among coworkers of peers below the income cutoff (i.e., coworkers of peers who qualify for AFAM-PE). Effect size reports the estimated coefficient as a percentage of the baseline mean. Statistical significance based on robust p-values: *** p<0.01, ** p<0.05, * p<0.10.

F Heterogeneity Analysis

This appendix reports heterogeneous effects of peers' income-eligibility enforcement across worker and job characteristics: age, gender, education, job stability, tenure, and firm attributes.

Table F.1: Heterogeneous Reduced-Form Effects of Peers' Suspension on Coworkers' Labor Market and Compliance Outcomes

	Individual Outcomes			Household Outcomes	
	Labor Income (1)	Labor Income > 0 (2)	Prob. Formal Income (3)	Prob. AFAM-PE Suspension (4)	Per Capita HH Income (5)
<i>Panel A. Reduced-form estimate by age</i>					
<i>Age ≤ 25</i>	-1567.75	243.25	-0.17***	-0.07*	-455.03*
Robust p-value	0.143	0.681	0.009	0.093	0.096
Observations	624	495	531	526	510
<i>Age > 25</i>	-1924.16**	-728.79	-0.12**	-0.07**	-413.58*
Robust p-value	0.032	0.387	0.011	0.024	0.065
Observations	967	893	921	1077	998
<i>Panel B. Reduced-form estimate by gender</i>					
<i>Male</i>	-2313.08**	-107.88	-0.20***	-0.01	-489.05*
Robust p-value	0.026	0.909	0.000	0.786	0.066
Observations	973	790	861	1119	985
<i>Female</i>	-1326.91*	121.13	-0.09	-0.20***	-339.34
Robust p-value	0.089	0.871	0.109	0.000	0.136
Observations	603	498	718	509	571

Notes: This table reports heterogeneous reduced-form estimates of the effect of peers' income-eligibility enforcement on coworkers' labor market and compliance outcomes across alternative coworker subgroups. Each row reports the reduced-form estimate from Equation 3 for the indicated subgroup, together with robust p-values and the number of observations. Subgroups are defined by age and gender as indicated in the table. The sample corresponds to the coworker sample described in Section 3.2, in which each peer-coworker link constitutes one observation. All specifications use local linear regressions with triangular kernel weights and MSE-optimal bandwidths following Calonico et al. (2019). Robust standard errors are clustered at the firm level. Statistical significance based on robust p-values: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table F.2: RD Estimates for the Effect of Peers' Suspension on their Coworkers' Labor Market and Compliance Outcomes. Age less than 25. RD Estimates 6 Months After Peers' Suspension.

	Individual Outcomes			Household Outcomes	
	Labor Income (1)	Labor Income > 0 (2)	Prob. Formal Income (3)	Prob. AFAM Suspension (4)	Per Capita HH Income (5)
<i>Panel A. Fuzzy RDD Estimate</i>					
Peers' Suspension (HH Income > Thresh.)	-2934.71 (2357.64)	826.50 (1678.51)	-0.33** (0.16)	-0.11 (0.10)	-674.35 (635.10)
Robust p-value	0.202	0.548	0.015	0.178	0.177
<i>Panel B. Reduced Form</i>					
Income-Eligibility Enforcement	-1567.75 (1138.89)	243.25 (868.08)	-0.17*** (0.08)	-0.07* (0.05)	-455.03* (344.20)
Robust p-value	0.143	0.681	0.009	0.093	0.096
Mean baseline	8961.46	10921.30	0.82	0.07	2645.62
Effect size (%)	-17.49	2.23	-20.54	-97.12	-17.20
<i>Panel C. First Stage</i>					
Income-Eligibility Enforcement	0.51*** (0.09)	0.52*** (0.08)	0.54*** (0.09)	0.56*** (0.09)	0.56*** (0.09)
Robust p-value	0.000	0.000	0.000	0.000	0.000
Opt. Bandwidth	.065	.06	.055	.05	.05
Observations	624	495	531	526	510

Notes: Tables F.2–F.11 report RD estimates for alternative coworker subsamples. Panel A reports the fuzzy RD estimates (IV) based on the 2SLS estimation of Equation 1. Panel B reports the reduced-form estimates based on Equation 3. Panel C reports the first-stage estimates based on Equation 2. The sample corresponds to the coworker sample described in Section 3.2, in which each peer–coworker link constitutes one observation. All specifications use local linear regressions with triangular kernel weights and MSE-optimal bandwidths following Calonico et al. (2019). Robust standard errors clustered at the firm level are reported in parentheses. Mean baseline reports the average outcome among coworkers of peers below the income cutoff (i.e., coworkers of peers who qualify for AFAM-PE). Effect size reports the estimated coefficient as a percentage of the baseline mean. Statistical significance based on robust p-values: *** p<0.01, ** p<0.05, * p<0.10. The sample restriction for each table is indicated in the title. The underlying population corresponds to the coworker sample described in Section 3.2.

Table F.3: RD Estimates for the Effect of Peers' Suspension on their Coworkers' Labor Market and Compliance Outcomes. Age more than 25. RD Estimates 6 Months After Peers' Suspension.

	Individual Outcomes			Household Outcomes	
	Labor Income (1)	Labor Income > 0 (2)	Prob. Formal Income (3)	Prob. AFAM Suspension (4)	Per Capita HH Income (5)
<i>Panel A. Fuzzy RDD Estimate</i>					
Peers' Suspension (HH Income > Thresh.)	-2862.37* (1570.53)	-811.75 (1514.45)	-0.21** (0.08)	-0.10* (0.06)	-513.50 (411.19)
Robust p-value	0.072	0.607	0.013	0.080	0.232
<i>Panel B. Reduced Form</i>					
Income-Eligibility Enforcement	-1924.16** (921.23)	-728.79 (876.68)	-0.12** (0.05)	-0.07** (0.04)	-413.58* (230.59)
Robust p-value	0.032	0.387	0.011	0.024	0.065
Mean baseline	10202.91	11398.87	0.89	0.08	2740.54
Effect size (%)	-18.86	-6.39	-13.15	-86.15	-15.09
<i>Panel C. First Stage</i>					
Income-Eligibility Enforcement	0.59*** (0.08)	0.58*** (0.08)	0.59*** (0.08)	0.58*** (0.08)	0.58*** (0.08)
Robust p-value	0.000	0.000	0.000	0.000	0.000
Opt. Bandwidth	.035	.04	.035	.04	.04
Observations	967	893	921	1077	998

Notes: See notes to Table F.2.

Table F.4: RD Estimates for the Effect of Peers' Suspension on their Coworkers' Labor Market and Compliance Outcomes. Male. RD Estimates 6 Months After Peers' Suspension.

	Individual Outcomes			Household Outcomes	
	Labor Income (1)	Labor Income > 0 (2)	Prob. Formal Income (3)	Prob. AFAM Suspension (4)	Per Capita HH Income (5)
<i>Panel A. Fuzzy RDD Estimate</i>					
Peers' Suspension (HH Income > Thresh.)	-3473.73* (1753.97)	95.80 (1658.01)	-0.33*** (0.10)	-0.01 (0.06)	-699.23 (437.24)
Robust p-value	0.053	0.763	0.000	0.921	0.124
<i>Panel B. Reduced Form</i>					
Income-Eligibility Enforcement	-2313.08** (1064.67)	-107.88 (1034.13)	-0.20*** (0.05)	-0.01 (0.03)	-489.05* (271.28)
Robust p-value	0.026	0.909	0.000	0.786	0.066
Mean baseline	10994.92	12695.39	0.87	0.06	2781.45
Effect size (%)	-21.04	-0.85	-22.84	-19.66	-17.58
<i>Panel C. First Stage</i>					
Income-Eligibility Enforcement	0.62*** (0.08)	0.62*** (0.08)	0.60*** (0.08)	0.62*** (0.08)	0.62*** (0.08)
Robust p-value	0.000	0.000	0.000	0.000	0.000
Opt. Bandwidth	.045	.045	.04	.055	.045
Observations	973	790	861	1119	985

Notes: See notes to Table F.2.

Table F.5: RD Estimates for the Effect of Peers' Suspension on their Coworkers' Labor Market and Compliance Outcomes. Female. RD Estimates 6 Months After Peers' Suspension.

	Individual Outcomes			Household Outcomes	
	Labor Income (1)	Labor Income > 0 (2)	Prob. Formal Income (3)	Prob. AFAM Suspension (4)	Per Capita HH Income (5)
<i>Panel A. Fuzzy RDD Estimate</i>					
Peers' Suspension (HH Income > Thresh.)	-2597.92* (1417.46)	-344.87 (1226.26)	-0.18* (0.11)	-0.26*** (0.09)	-422.03 (388.49)
Robust p-value	0.053	0.738	0.098	0.002	0.258
<i>Panel B. Reduced Form</i>					
Income-Eligibility Enforcement	-1326.91* (846.52)	121.13 (747.09)	-0.09 (0.06)	-0.20*** (0.06)	-339.34 (249.31)
Robust p-value	0.089	0.871	0.109	0.000	0.136
Mean baseline	7721.41	9148.90	0.86	0.12	2528.51
Effect size (%)	-17.18	1.32	-10.80	-159.99	-13.42
<i>Panel C. First Stage</i>					
Income-Eligibility Enforcement	0.66*** (0.08)	0.66*** (0.08)	0.58*** (0.08)	0.70*** (0.08)	0.67*** (0.08)
Robust p-value	0.000	0.000	0.000	0.000	0.000
Opt. Bandwidth	.04	.04	.05	.035	.04
Observations	603	498	718	509	571

Notes: See notes to Table F.2.

Table F.6: RD Estimates for the Effect of Peers' Suspension on their Coworkers' Labor Market and Compliance Outcomes. Low Education. RD Estimates 6 Months After Peers' Suspension.

	Individual Outcomes			Household Outcomes	
	Labor Income (1)	Labor Income > 0 (2)	Prob. Formal Income (3)	Prob. AFAM Suspension (4)	Per Capita HH Income (5)
<i>Panel A. Fuzzy RDD Estimate</i>					
Peers' Suspension (HH Income > Thresh.)	-2162.31 (1592.89)	909.08 (1405.17)	-0.25*** (0.10)	-0.12* (0.07)	-228.48 (420.34)
Robust p-value	0.208	0.376	0.009	0.052	0.750
<i>Panel B. Reduced Form</i>					
Income-Eligibility Enforcement	-1445.75 (905.41)	322.72 (836.66)	-0.14*** (0.05)	-0.07** (0.04)	-230.07 (239.08)
Robust p-value	0.118	0.544	0.003	0.022	0.412
Mean baseline	9837.94	11442.94	0.86	0.08	2668.93
Effect size (%)	-14.70	2.82	-16.36	-94.25	-8.62
<i>Panel C. First Stage</i>					
Income-Eligibility Enforcement	0.57*** (0.08)	0.58*** (0.08)	0.57*** (0.08)	0.57*** (0.08)	0.58*** (0.08)
Robust p-value	0.000	0.000	0.000	0.000	0.000
Opt. Bandwidth	.045	.05	.045	.045	.05
Observations	1241	1061	1189	1143	1313

Notes: See notes to Table F.2.

Table F.7: RD Estimates for the Effect of Peers' Suspension on their Coworkers' Labor Market and Compliance Outcomes. High Education. RD Estimates 6 Months After Peers' Suspension.

	Individual Outcomes			Household Outcomes	
	Labor Income (1)	Labor Income > 0 (2)	Prob. Formal Income (3)	Prob. AFAM Suspension (4)	Per Capita HH Income (5)
<i>Panel A. Fuzzy RDD Estimate</i>					
Peers' Suspension (HH Income > Thresh.)	-5664.82** (2506.46)	-5985.43*** (2285.87)	-0.19* (0.11)	-0.25** (0.13)	-2646.23*** (738.05)
Robust p-value	0.013	0.006	0.065	0.045	0.000
<i>Panel B. Reduced Form</i>					
Income-Eligibility Enforcement	-2647.96*** (1125.45)	-2919.56*** (1033.97)	-0.10** (0.06)	-0.14** (0.07)	-1649.55*** (366.51)
Robust p-value	0.006	0.002	0.040	0.022	0.000
Mean baseline	9475.84	11386.28	0.87	0.09	3003.02
Effect size (%)	-27.94	-25.64	-11.69	-156.67	-54.93
<i>Panel C. First Stage</i>					
Income-Eligibility Enforcement	0.51*** (0.09)	0.61*** (0.08)	0.51*** (0.09)	0.55*** (0.08)	0.61*** (0.08)
Robust p-value	0.000	0.000	0.000	0.000	0.000
Opt. Bandwidth	.06	.03	.05	.04	.03
Observations	567	258	469	376	294

Notes: See notes to Table F.2.

Table F.8: RD Estimates for the Effect of Peers' Suspension on their Coworkers' Labor Market and Compliance Outcomes. Low-stability Jobs. RD Estimates 6 Months After Peers' Suspension.

	Individual Outcomes			Household Outcomes	
	Labor Income (1)	Labor Income > 0 (2)	Prob. Formal Income (3)	Prob. AFAM Suspension (4)	Per Capita HH Income (5)
<i>Panel A. Fuzzy RDD Estimate</i>					
Peers' Suspension (HH Income > Thresh.)	-3646.57*** (1324.34)	-278.59 (1228.07)	-0.31*** (0.10)	-0.11** (0.06)	-555.60 (394.85)
Robust p-value	0.007	0.966	0.002	0.026	0.186
<i>Panel B. Reduced Form</i>					
Income-Eligibility Enforcement	-2865.00*** (926.24)	-471.24 (937.21)	-0.20*** (0.06)	-0.11*** (0.04)	-600.96** (291.39)
Robust p-value	0.001	0.692	0.001	0.002	0.037
Mean baseline	10680.16	12308.81	0.87	0.07	2740.02
Effect size (%)	-26.83	-3.83	-22.79	-162.73	-21.93
<i>Panel C. First Stage</i>					
Income-Eligibility Enforcement	0.76*** (0.08)	0.76*** (0.09)	0.77*** (0.09)	0.77*** (0.10)	0.77*** (0.09)
Robust p-value	0.000	0.000	0.000	0.000	0.000
Opt. Bandwidth	.05	.05	.035	.035	.04
Observations	677	561	509	509	565

Notes: See notes to Table F.2.

Table F.9: RD Estimates for the Effect of Peers' Suspension on their Coworkers' Labor Market and Compliance Outcomes. High-stability Jobs. RD Estimates 6 Months After Peers' Suspension.

	Individual Outcomes			Household Outcomes	
	Labor Income (1)	Labor Income > 0 (2)	Prob. Formal Income (3)	Prob. AFAM Suspension (4)	Per Capita HH Income (5)
<i>Panel A. Fuzzy RDD Estimate</i>					
Peers' Suspension (HH Income > Thresh.)	-204.21 (3579.51)	3067.40 (3705.29)	-0.30* (0.21)	-0.16 (0.17)	-1256.97 (1010.48)
Robust p-value	0.988	0.243	0.059	0.260	0.130
<i>Panel B. Reduced Form</i>					
Income-Eligibility Enforcement	-125.50 (1092.17)	1035.87 (1211.29)	-0.09** (0.05)	-0.04 (0.05)	-401.61 (288.89)
Robust p-value	0.899	0.283	0.019	0.309	0.115
Mean baseline	9752.17	11039.65	0.87	0.09	2850.10
Effect size (%)	-1.29	9.38	-10.88	-51.14	-14.09
<i>Panel C. First Stage</i>					
Income-Eligibility Enforcement	0.30** (0.12)	0.30** (0.13)	0.31** (0.12)	0.30** (0.13)	0.31** (0.12)
Robust p-value	0.029	0.050	0.020	0.040	0.015
Opt. Bandwidth	.045	.045	.05	.045	.05
Observations	520	459	535	505	537

Notes: See notes to Table F.2.

Table F.10: RD Estimates for the Effect of Peers' Suspension on their Coworkers' Labor Market and Compliance Outcomes. Tenure less than 1 year. RD Estimates 6 Months After Peers' Suspension.

	Individual Outcomes			Household Outcomes	
	Labor Income (1)	Labor Income > 0 (2)	Prob. Formal Income (3)	Prob. AFAM Suspension (4)	Per Capita HH Income (5)
<i>Panel A. Fuzzy RDD Estimate</i>					
Peers' Suspension (HH Income > Thresh.)	-6263.34*** (2374.64)	227.62 (1675.13)	-0.50*** (0.15)	-0.27*** (0.10)	-932.11 (581.97)
Robust p-value	0.006	0.873	0.000	0.003	0.124
<i>Panel B. Reduced Form</i>					
Income-Eligibility Enforcement	-3085.56*** (1107.54)	-45.86 (1075.56)	-0.25*** (0.06)	-0.15*** (0.05)	-496.52* (295.12)
Robust p-value	0.003	0.971	0.000	0.001	0.100
Mean baseline	9018.21	11033.28	0.81	0.07	2669.53
Effect size (%)	-34.21	-0.42	-31.42	-201.36	-18.60
<i>Panel C. First Stage</i>					
Income-Eligibility Enforcement	0.52*** (0.08)	0.53*** (0.08)	0.53*** (0.08)	0.54*** (0.08)	0.52*** (0.08)
Robust p-value	0.000	0.000	0.000	0.000	0.000
Opt. Bandwidth	.04	.035	.045	.035	.04
Observations	529	390	632	479	541

Notes: See notes to Table F.2.

Table F.11: RD Estimates for the Effect of Peers' Suspension on their Coworkers' Labor Market and Compliance Outcomes. Tenure of more than 1 year. RD Estimates 6 Months After Peers' Suspension.

	Individual Outcomes			Household Outcomes	
	Labor Income (1)	Labor Income > 0 (2)	Prob. Formal Income (3)	Prob. AFAM Suspension (4)	Per Capita HH Income (5)
<i>Panel A. Fuzzy RDD Estimate</i>					
Peers' Suspension (HH Income > Thresh.)	-1932.33 (1479.98)	-949.47 (1372.63)	-0.11 (0.08)	-0.04 (0.07)	-223.20 (412.64)
Robust p-value	0.208	0.495	0.169	0.605	0.554
<i>Panel B. Reduced Form</i>					
Income-Eligibility Enforcement	-1412.77 (963.45)	-609.86 (779.31)	-0.06 (0.05)	-0.04 (0.05)	-258.28 (265.40)
Robust p-value	0.138	0.402	0.222	0.405	0.267
Mean baseline	10335.13	11561.92	0.88	0.09	2770.41
Effect size (%)	-13.67	-5.27	-7.33	-40.38	-9.32
<i>Panel C. First Stage</i>					
Income-Eligibility Enforcement	0.65*** (0.08)	0.62*** (0.07)	0.65*** (0.08)	0.65*** (0.08)	0.65*** (0.08)
Robust p-value	0.000	0.000	0.000	0.000	0.000
Opt. Bandwidth	.04	.055	.04	.04	.04
Observations	877	1008	813	876	830

Notes: See notes to Table F.2.

Table F.12: Descriptive Statistics by Gender for Peer and Coworker Samples.

	Male		Female	
	Mean	SD	Mean	SD
<i>Panel A. Peers</i>				
Individual Labor Income (\$UY)	15559.77	(6106.62)	10296.99	(4612.87)
HH Verifiable per capita Income (\$UY)	5189.77	(641.71)	5020.46	(747.12)
HH Income (30%-20% of thr.)	41.82	(49.33)	45.26	(49.79)
HH Income (50%-20% of thr.)	64.09	(47.98)	61.33	(48.71)
Prop. of female workers (%)	0.08	(0.85)	0.35	(2.26)
Prop. of AFAM female workers (%)	0.05	(0.49)	0.26	(1.12)
Observations	3197		2172	
<i>Panel B. Coworkers</i>				
Individual Labor Income (\$UY)	11058.89	(4776.68)	8265.24	(3746.83)
HH Verifiable per capita Income (\$UY)	2639.05	(1037.89)	2354.70	(1055.35)
HH Income (30%-20% of thr.)	13.89	(34.59)	9.63	(29.50)
HH Income (50%-20% of thr.)	48.54	(49.98)	38.69	(48.71)
Prop. of female workers (%)	0.11	(1.02)	0.38	(1.70)
Prop. of AFAM female workers (%)	0.07	(0.53)	0.32	(1.02)
Observations	5068		3500	

Notes: This table reports descriptive statistics for the peer and coworker samples by gender. Columns (1)-(2) report means and standard deviations for men, and columns (3)-(4) for women. Panel A corresponds to peers and Panel B to coworkers. Household income refers to monthly verifiable per capita income, and income brackets are defined relative to the AFAM-PE eligibility threshold. “Prop. of female workers” denotes the share of female workers in the firm, and “Prop. of AFAM female workers” the share of female AFAM beneficiaries among workers in the firm. All variables are measured in the month prior to the event. The sample corresponds to Section 3.2.

G Mechanisms

This appendix provides additional heterogeneity and mechanism analyses. It examines variation in effects by firm size, workforce composition, and coworkers’ distance from the eligibility threshold, and includes placebo tests for non-beneficiary workers.

G.1 Heterogeneity by Coworker Age and Gender

Panel A of Table F.1 reports heterogeneity by age. We split coworkers into younger adults, aged 18–25, and older adults, aged 26 and above. The extensive-margin response is somewhat larger among younger coworkers. Column (3) shows a 17 p.p. decline in formal employment among young coworkers exposed to income-ineligible *vs.* income-eligible peers, compared with a 12 p.p. decline among older coworkers. However, this larger formal-employment response among younger coworkers does not translate into a larger decline in total registered labor income.

We next examine heterogeneity by gender. Panel B shows that responses to peers’ income-eligibility enforcement are stronger among male coworkers, particularly along the extensive margin. In Column (3), the estimated reduction in formal employment among coworkers exposed to income-ineligible peers is more than twice as large for men as for women. This pattern is somewhat counterintuitive given that women often exhibit higher transitions out of formal employment. One possible explanation is that male workers play a more pivotal role in determining household eligibility. As shown in Table F.12, male coworkers earn substantially more than female coworkers (UY\$ 11,059 *vs.* UY\$ 8,265), and their households also have higher average income. Consistent with this, a larger share of male coworkers’ households is concentrated near the eligibility threshold: 48.5% lie between 20% and 50% below the threshold, compared to 38.7% for female coworkers, and 13.9% versus 9.6% fall in the narrower band between 20% and 30% below. This would make adjustments in male formal employment more effective in avoiding benefit suspension. A second mechanism relates to workplace networks. Coworker networks are disproportionately male, with lower shares of female workers (11% *vs.* 38%) and female AFAM beneficiaries (7% *vs.* 32%) across male and female samples, respectively. Under homophily in workplace interactions, this increases the

likelihood that male coworkers are exposed to and respond to peers' eligibility shocks.

G.2 Additional Results

Table G.1: RD Estimates for the Effect of Peers' Suspension on their Coworkers' Labor Market and Compliance Outcomes. Firms with fewer than 30 workers. RD Estimates 6 Months After Peers' Suspension.

	Individual Outcomes			Household Outcomes	
	Labor Income (1)	Labor Income > 0 (2)	Prob. Formal Income (3)	Prob. AFAM Suspension (4)	Per Capita HH Income (5)
<i>Panel A. Fuzzy RDD Estimate</i>					
Peers' Suspension (HH Income > Thresh.)	-6340.34** (2540.04)	-1779.29 (1883.27)	-0.42*** (0.14)	0.05 (0.11)	-1119.41* (623.93)
Robust p-value	0.016	0.465	0.003	0.559	0.096
<i>Panel B. Reduced Form</i>					
Income-Eligibility Enforcement	-3089.78*** (1061.34)	-602.42 (887.93)	-0.20*** (0.06)	0.01 (0.05)	-593.90** (286.62)
Robust p-value	0.004	0.645	0.000	0.693	0.045
Mean baseline	9853.39	11521.56	0.87	0.08	2867.41
Effect size (%)	-31.36	-5.23	-22.98	15.42	-20.71
<i>Panel C. First Stage</i>					
Income-Eligibility Enforcement	0.50*** (0.10)	0.50*** (0.10)	0.50*** (0.10)	0.50*** (0.09)	0.50*** (0.09)
Robust p-value	0.000	0.000	0.000	0.000	0.000
Opt. Bandwidth	.045	.04	.04	.045	.045
Observations	515	393	479	522	522

Notes: Tables G.1–G.11 report RD estimates for alternative coworker subsamples. Panel A reports the fuzzy RD estimates (IV) based on the 2SLS estimation of Equation 1. Panel B reports the reduced-form estimates based on Equation 3. Panel C reports the first-stage estimates based on Equation 2. The sample corresponds to the coworker sample described in Section 3.2, in which each peer–coworker link constitutes one observation. All specifications use local linear regressions with triangular kernel weights and MSE-optimal bandwidths following Calonico et al. (2019). Robust standard errors clustered at the firm level are reported in parentheses. Mean baseline reports the average outcome among coworkers of peers below the income cutoff (i.e., coworkers of peers who qualify for AFAM-PE). Effect size reports the estimated coefficient as a percentage of the baseline mean. Statistical significance based on robust p-values: *** p<0.01, ** p<0.05, * p<0.10. The sample restriction for each table is indicated in the title. The underlying population corresponds to the coworker sample described in Section 3.2.

Table G.2: RD Estimates for the Effect of Peers' Suspension on their Coworkers' Labor Market and Compliance Outcomes. Firms with 30 Workers or More. RD Estimates 6 Months After Peers' Suspension.

	Individual Outcomes			Household Outcomes	
	Labor Income (1)	Labor Income > 0 (2)	Prob. Formal Income (3)	Prob. AFAM Suspension (4)	Per Capita HH Income (5)
<i>Panel A. Fuzzy RDD Estimate</i>					
Peers' Suspension (HH Income > Thresh.)	-2293.22 (1585.49)	1007.54 (1422.58)	-0.22** (0.10)	-0.18*** (0.07)	-383.18 (387.07)
Robust p-value	0.102	0.374	0.015	0.003	0.287
<i>Panel B. Reduced Form</i>					
Income-Eligibility Enforcement	-1812.34** (1024.01)	390.57 (896.68)	-0.13*** (0.05)	-0.13*** (0.04)	-371.16* (248.18)
Robust p-value	0.040	0.553	0.009	0.000	0.091
Mean baseline	9821.93	11324.65	0.86	0.09	2631.69
Effect size (%)	-18.45	3.45	-15.41	-141.65	-14.10
<i>Panel C. First Stage</i>					
Income-Eligibility Enforcement	0.66*** (0.10)	0.63*** (0.09)	0.66*** (0.10)	0.66*** (0.10)	0.66*** (0.10)
Robust p-value	0.000	0.000	0.000	0.000	0.000
Opt. Bandwidth	.035	.05	.035	.035	.035
Observations	870	997	873	830	900

Notes: See notes to Table G.1.

Table G.3: RD Estimates for the Effect of Peers' Suspension on their Coworkers' Labor Market and Compliance Outcomes. Proportion of AFAM workers less than the median. RD Estimates 6 Months After Peers' Suspension.

	Individual Outcomes			Household Outcomes	
	Labor Income (1)	Labor Income > 0 (2)	Prob. Formal Income (3)	Prob. AFAM Suspension (4)	Per Capita HH Income (5)
<i>Panel A. Fuzzy RDD Estimate</i>					
Peers' Suspension (HH Income > Thresh.)	-1578.13 (1611.65)	-312.15 (1276.36)	-0.16* (0.09)	-0.06 (0.07)	-446.58 (403.84)
Robust p-value	0.302	0.681	0.090	0.301	0.272
<i>Panel B. Reduced Form</i>					
Income-Eligibility Enforcement	-1205.18 (1088.59)	-108.37 (921.62)	-0.12** (0.05)	-0.05 (0.05)	-430.25* (279.34)
Robust p-value	0.231	0.770	0.037	0.261	0.088
Mean baseline	10537.78	11687.71	0.89	0.10	2921.25
Effect size (%)	-11.44	-0.93	-12.96	-46.98	-14.73
<i>Panel C. First Stage</i>					
Income-Eligibility Enforcement	0.72*** (0.09)	0.72*** (0.08)	0.68*** (0.08)	0.70*** (0.08)	0.72*** (0.09)
Robust p-value	0.000	0.000	0.000	0.000	0.000
Opt. Bandwidth	.04	.04	.05	.045	.04
Observations	702	583	838	757	640

Notes: See notes to Table G.1.

Table G.4: RD Estimates for the Effect of Peers' Suspension on their Coworkers' Labor Market and Compliance Outcomes. Proportion of AFAM workers more than the median. RD Estimates 6 Months After Peers' Suspension.

	Individual Outcomes			Household Outcomes	
	Labor Income (1)	Labor Income > 0 (2)	Prob. Formal Income (3)	Prob. AFAM Suspension (4)	Per Capita HH Income (5)
<i>Panel A. Fuzzy RDD Estimate</i>					
Peers' Suspension (HH Income > Thresh.)	-3931.67** (1746.15)	-575.10 (1782.54)	-0.36*** (0.12)	-0.14** (0.07)	-880.13* (501.96)
Robust p-value	0.038	0.949	0.002	0.046	0.077
<i>Panel B. Reduced Form</i>					
Income-Eligibility Enforcement	-2056.09** (932.02)	-255.67 (963.55)	-0.18*** (0.06)	-0.08** (0.04)	-524.00** (271.29)
Robust p-value	0.034	0.919	0.001	0.013	0.041
Mean baseline	9393.19	11059.97	0.83	0.07	2548.43
Effect size (%)	-21.89	-2.31	-22.10	-118.05	-20.56
<i>Panel C. First Stage</i>					
Income-Eligibility Enforcement	0.55*** (0.08)	0.55*** (0.08)	0.55*** (0.08)	0.55*** (0.08)	0.55*** (0.08)
Robust p-value	0.000	0.000	0.000	0.000	0.000
Opt. Bandwidth	.05	.045	.045	.05	.045
Observations	946	696	781	944	837

Notes: See notes to Table G.1.

Table G.5: RD Estimates for the Effect of Peers' Suspension on their Coworkers' Labor Market and Compliance Outcomes. Single-establishment firms. RD Estimates 6 Months After Peers' Suspension.

	Individual Outcomes			Household Outcomes	
	Labor Income (1)	Labor Income > 0 (2)	Prob. Formal Income (3)	Prob. AFAM Suspension (4)	Per Capita HH Income (5)
<i>Panel A. Fuzzy RDD Estimate</i>					
Peers' Suspension (HH Income > Thresh.)	-3992.95*** (1482.31)	-1288.76 (1382.17)	-0.27*** (0.09)	-0.08 (0.08)	-634.18 (400.42)
Robust p-value	0.007	0.404	0.001	0.190	0.103
<i>Panel B. Reduced Form</i>					
Income-Eligibility Enforcement	-2381.69*** (851.01)	-616.36 (802.19)	-0.17*** (0.05)	-0.06 (0.05)	-435.39** (237.28)
Robust p-value	0.004	0.496	0.001	0.131	0.049
Mean baseline	9698.23	11534.19	0.85	0.08	2665.27
Effect size (%)	-24.56	-5.34	-19.75	-73.44	-16.34
<i>Panel C. First Stage</i>					
Income-Eligibility Enforcement	0.62*** (0.07)	0.61*** (0.07)	0.63*** (0.07)	0.63*** (0.07)	0.63*** (0.07)
Robust p-value	0.000	0.000	0.000	0.000	0.000
Opt. Bandwidth	.045	.05	.03	.04	.04
Observations	1053	941	733	915	940

Notes: See notes to Table G.1.

Table G.6: RD Estimates for the Effect of Peers' Suspension on their Coworkers' Labor Market and Compliance Outcomes. Multi-establishment firms. RD Estimates 6 Months After Peers' Suspension.

	Individual Outcomes			Household Outcomes	
	Labor Income (1)	Labor Income > 0 (2)	Prob. Formal Income (3)	Prob. AFAM Suspension (4)	Per Capita HH Income (5)
<i>Panel A. Fuzzy RDD Estimate</i>					
Peers' Suspension (HH Income > Thresh.)	-124.68 (1951.76)	822.67 (1849.19)	-0.08 (0.11)	-0.18** (0.09)	-370.56 (503.51)
Robust p-value	0.974	0.697	0.573	0.022	0.481
<i>Panel B. Reduced Form</i>					
Income-Eligibility Enforcement	-373.72 (1091.18)	-37.85 (1266.12)	-0.02 (0.06)	-0.13*** (0.05)	-349.77 (263.95)
Robust p-value	0.718	0.915	0.915	0.001	0.158
Mean baseline	9698.03	10871.64	0.89	0.10	2740.58
Effect size (%)	-3.85	-0.35	-2.28	-127.14	-12.76
<i>Panel C. First Stage</i>					
Income-Eligibility Enforcement	0.54*** (0.12)	0.58*** (0.15)	0.58*** (0.15)	0.60*** (0.14)	0.54*** (0.12)
Robust p-value	0.000	0.000	0.000	0.000	0.000
Opt. Bandwidth	.06	.04	.04	.05	.06
Observations	729	439	507	629	730

Notes: See notes to Table G.1.

Table G.7: RD Estimates for the Effect of Peers' Suspension on their Coworkers' Labor Market Outcomes. RD Estimates 6 Months After Peers' Suspension. Equal Sex, Age, Region and Income.

	Individual Outcomes		
	Labor Income (1)	Labor Income > 0 (2)	Prob. Formal Income (3)
<i>Panel A. Fuzzy RDD Estimate</i>			
Peers' Suspension (HH Income > Thresh.)	-3541.85** (1622.41)	1351.25 (1699.36)	-0.39*** (0.10)
Robust p-value	0.040	0.347	0.000
<i>Panel B. Reduced Form</i>			
Income-Eligibility Enforcement	-2630.64** (1187.57)	780.21 (1000.82)	-0.31*** (0.07)
Robust p-value	0.023	0.323	0.000
Mean baseline	9666.55	10788.01	0.88
Effect size (%)	-27.21	7.23	-34.95
<i>Panel C. First Stage</i>			
Income-Eligibility Enforcement	0.74*** (0.08)	0.75*** (0.08)	0.79*** (0.08)
Robust p-value	0.000	0.000	0.000
Opt. Bandwidth	.045	.045	.04
Observations	275	238	235

Notes: See notes to Table G.1.

Table G.8: RD Estimates for the Effect of Peers' Suspension on their Coworkers' Labor Market and Compliance Outcomes. 6 Months After Peers' Suspension. Peers Contacted Beneficiary Support Service (BSS).

	Labor Income (1)	Prob. AFAM Suspension (2)	Per Capita HH Income (3)
<i>Panel A. Fuzzy RDD Estimate</i>			
Peers' Suspension (HH Income > Thresh.)	-11059.66** (5581.18)	-0.20 (0.19)	-3005.47** (1204.43)
Robust p-value	0.048	0.512	0.011
<i>Panel B. Reduced Form</i>			
Income-Eligibility Enforcement	-4476.20** (1786.10)	-0.09 (0.06)	-1201.91*** (383.32)
Robust p-value	0.012	0.368	0.002
Mean baseline	10682.94	0.15	2780.10
Effect size (%)	-41.90	-59.69	-43.23
<i>Panel C. First Stage</i>			
Income-Eligibility Enforcement	0.40*** (0.12)	0.42*** (0.12)	0.41*** (0.13)
Robust p-value	0.001	0.000	0.002
Opt. Bandwidth	.05	.04	.05
Observations	98	83	95

Notes: See notes to Table G.1.

Table G.9: RD Estimates for the Effect of Peers' Suspension on their Coworkers' Labor Market and Compliance Outcomes. 6 Months After Peers' Suspension. Peers Did Not Contact the Beneficiary Support Service (BSS).

	Labor Income (1)	Prob. AFAM Suspension (2)	Per Capita HH Income (3)
<i>Panel A. Fuzzy RDD Estimate</i>			
Peers' Suspension (HH Income > Thresh.)	-3416.22* (1755.90)	-0.05 (0.07)	-530.85 (476.48)
Robust p-value	0.051	0.412	0.251
<i>Panel B. Reduced Form</i>			
Income-Eligibility Enforcement	-1949.37** (879.50)	-0.04 (0.04)	-408.13* (248.03)
Robust p-value	0.020	0.214	0.069
Mean baseline	10033.28	0.08	2792.57
Effect size (%)	-19.43	-46.05	-14.61
<i>Panel C. First Stage</i>			
Income-Eligibility Enforcement	0.53*** (0.09)	0.51*** (0.08)	0.54*** (0.09)
Robust p-value	0.000	0.000	0.000
Opt. Bandwidth	.055	.06	.055
Observations	1215	1291	1149

Notes: See notes to Table G.1.

Table G.10: RD Estimates for the Effect of Peers' Suspension on their Coworkers' Labor Market and Compliance Outcomes. 6 Months After Peers' Suspension. Household Income between 20% and 30% below the Income Threshold.

	Individual Outcomes			Household Outcomes	
	Labor Income (1)	Labor Income > 0 (2)	Prob. Formal Income (3)	Prob. AFAM Suspension (4)	Per Capita HH Income (5)
<i>Panel A. Fuzzy RDD Estimate</i>					
Peers' Suspension (HH Income > Thresh.)	-9722.52*** (3925.04)	-6502.71* (4340.59)	-0.53*** (0.20)	-0.24 (0.19)	-3160.57*** (1304.27)
Robust p-value	0.010	0.085	0.003	0.122	0.008
<i>Panel B. Reduced Form</i>					
Income-Eligibility Enforcement	-8461.08*** (2015.96)	-4642.21*** (1959.33)	-0.33*** (0.11)	-0.14* (0.10)	-2065.01*** (607.13)
Robust p-value	0.000	0.005	0.000	0.073	0.000
Mean baseline	13651.52	14192.65	0.92	0.12	4016.07
Effect size (%)	-61.98	-32.71	-35.64	-119.96	-51.42
<i>Panel C. First Stage</i>					
Income-Eligibility Enforcement	0.56*** (0.12)	0.56*** (0.11)	0.57*** (0.12)	0.57*** (0.11)	0.57*** (0.11)
Robust p-value	0.000	0.000	0.000	0.000	0.000
Opt. Bandwidth	.04	.06	.04	.055	.055
Observations	183	228	183	241	242

Notes: See notes to Table [G.1](#).

Table G.11: RD Estimates for the Effect of Peers' Suspension on their Coworkers' Labor Market and Compliance Outcomes. 6 Months After Peers' Suspension. Household Income more than 30% below the Income Threshold.

	Individual Outcomes			Household Outcomes	
	Labor Income (1)	Labor Income > 0 (2)	Prob. Formal Income (3)	Prob. AFAM Suspension (4)	Per Capita HH Income (5)
<i>Panel A. Fuzzy RDD Estimate</i>					
Peers' Suspension (HH Income > Thresh.)	-2402.05* (1430.70)	772.05 (1190.36)	-0.27*** (0.09)	-0.06 (0.06)	-167.13 (347.42)
Robust p-value	0.093	0.439	0.003	0.219	0.675
<i>Panel B. Reduced Form</i>					
Income-Eligibility Enforcement	-1315.42 (836.84)	512.92 (724.93)	-0.15*** (0.05)	-0.04 (0.03)	-144.82 (201.25)
Robust p-value	0.109	0.381	0.001	0.129	0.488
Mean baseline	9220.17	10749.92	0.85	0.08	2479.08
Effect size (%)	-14.27	4.77	-18.16	-57.28	-5.84
<i>Panel C. First Stage</i>					
Income-Eligibility Enforcement	0.60*** (0.08)	0.59*** (0.07)	0.61*** (0.08)	0.58*** (0.07)	0.59*** (0.08)
Robust p-value	0.000	0.000	0.000	0.000	0.000
Opt. Bandwidth	.035	.045	.035	.05	.04
Observations	1163	1135	1058	1509	1237

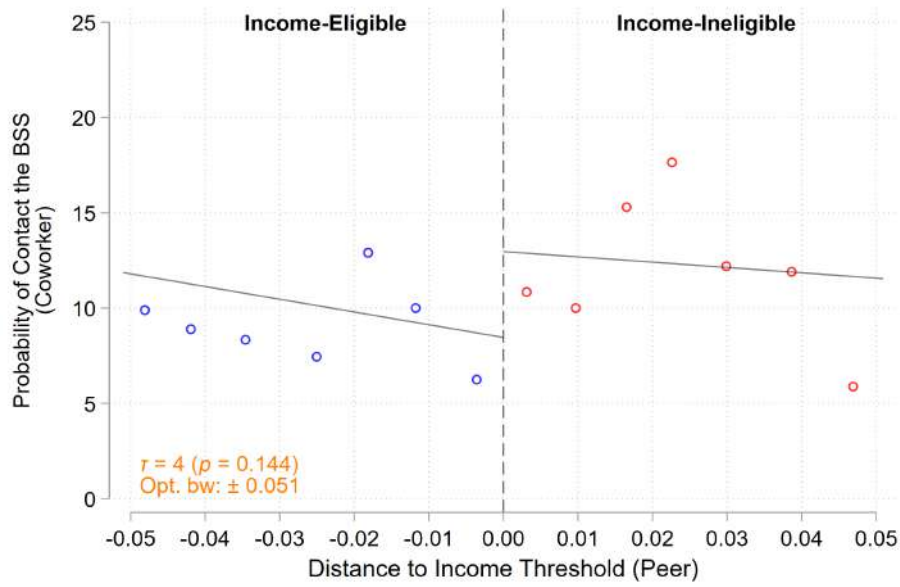
Notes: See notes to Table G.1.

Table G.12: RD Estimates of the Effect of Peers' Suspension on Firm Outcomes

	Number of Employees (1)	New Hires (2)	Exits (3)	Employees' Earnings (4)
<i>Panel A. Fuzzy RDD Estimate</i>				
Peers' Suspension (HH Income > Thresh.)	7.33 (10.39)	1.81 (2.09)	1.20 (2.07)	1164.17 (1772.42)
Robust p-value	0.536	0.390	0.749	0.497
<i>Panel B. Reduced Form</i>				
Income-Eligibility Enforcement	3.82 (5.99)	0.99 (1.18)	0.82 (1.18)	545.55 (1020.48)
Robust p-value	0.537	0.371	0.657	0.599
Mean baseline	32.72	4.89	5.62	10958.35
Effect size (%)	11.69	20.26	14.63	4.98
<i>Panel C. First Stage</i>				
Income-Eligibility Enforcement	0.59*** (0.07)	0.58*** (0.07)	0.59*** (0.07)	0.57*** (0.07)
Robust p-value	0.000	0.000	0.000	0.000
Opt. Bandwidth	.04	.05	.045	.055
Observations	674	777	708	852

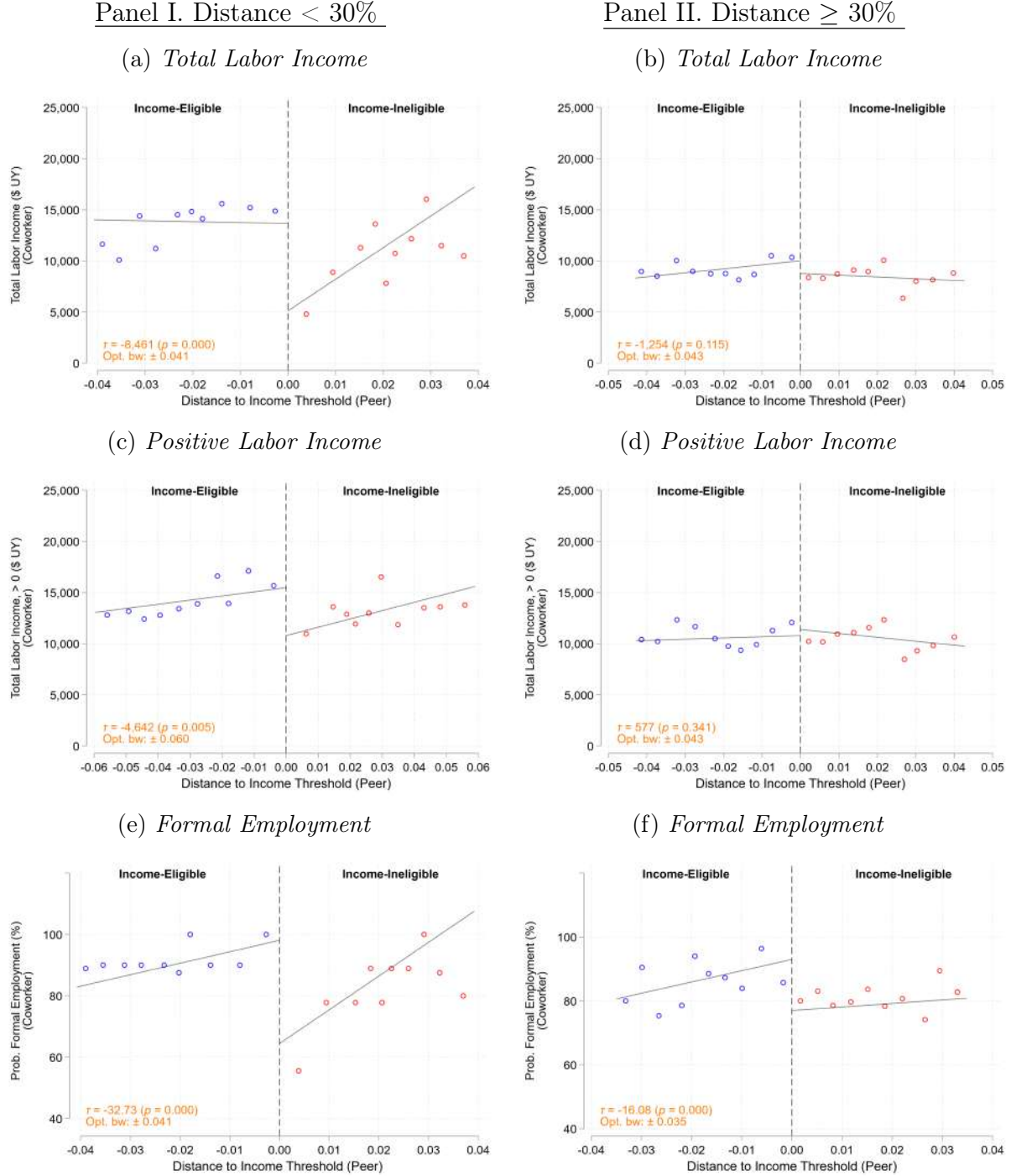
Notes: This table reports RD estimates of the effect of peers' income-eligibility enforcement on firm outcomes measured six months after the peer's income-testing event. Panel A reports the fuzzy RD estimates (IV) based on the 2SLS estimation of Equation 1. Panel B reports the reduced-form estimates based on Equation 3. Panel C reports the first-stage estimates based on Equation 2. Outcomes include employment, hires, separations, and employees' earnings, as indicated by the column headings. All specifications use local linear regressions with triangular kernel weights and MSE-optimal bandwidths following [Calonico et al. \(2019\)](#). Robust standard errors clustered at the firm level are reported in parentheses. Mean baseline reports the average outcome among coworkers of peers below the income cutoff (i.e., coworkers of peers who qualify for AFAM-PE). Effect size reports the estimated coefficient as a percentage of the baseline mean. Statistical significance based on robust p-values: *** p<0.01, ** p<0.05, * p<0.10. The sample corresponds to the firm-level sample described in Section 3.2.

Figure G.1: Effect of Peers' Eligibility on Coworkers' Probability of Contacting the BSS



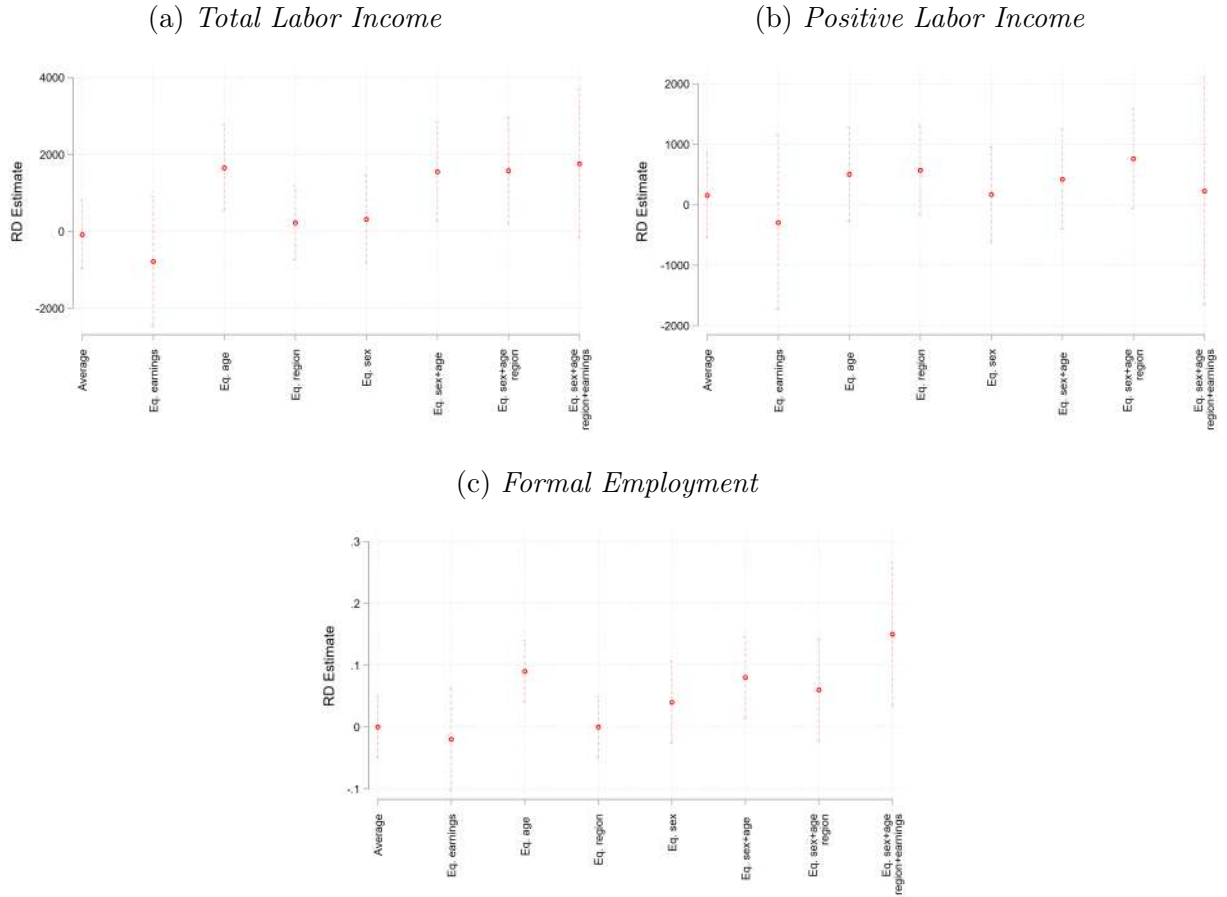
Notes: This figure presents the reduced-form effect of peers' income eligibility on the probability that coworkers contact the Beneficiary Support Service (BSS). The outcome is measured six months after the peer's income-testing event. Estimates are obtained from Equation 3 using local linear regressions with triangular kernel weights and MSE-optimal bandwidths following Calonico et al. (2019). Dots denote mean outcomes within 10 equally spaced bins and solid lines denote fitted values on each side of the cutoff. The reported discontinuity corresponds to the local reduced-form effect at the threshold. Robust standard errors are clustered at the firm level. The sample corresponds to the coworker sample described in Section 3.2.

Figure G.2: Heterogeneous Effects by Coworkers' Distance from the Income Cutoff



Notes: This figure presents heterogeneous reduced-form effects of peers' income eligibility on coworkers' labor market outcomes by coworkers' distance from the AFAM household income cutoff. Panel I reports estimates for coworkers whose household verifiable per capita income lies between 20% and 30% below the cutoff, while Panel II reports estimates for coworkers more than 30% below the cutoff. Outcomes are measured six months after the peer's income-testing event. Estimates are obtained from Equation 3 using local linear regressions with triangular kernel weights and MSE-optimal bandwidths following Calonico et al. (2019). Dots denote mean outcomes within 10 equally spaced bins and solid lines denote fitted values on each side of the cutoff. Robust standard errors are clustered at the firm level. The sample corresponds to the coworker sample described in Section 3.2.

Figure G.3: Heterogeneous Effects on Non-Beneficiary Coworkers by Coworker–Peer Similarity



Notes: This figure reports placebo reduced-form estimates for non-beneficiary coworkers employed in the same firm as peers. Panels (a)–(c) show estimates for total labor income, labor income conditional on working, and formal employment, respectively. Each point reports the reduced-form estimate from Equation 3 for the indicated subgroup defined by coworker–peer similarity in observable characteristics. Vertical bars denote 95 percent confidence intervals based on robust standard errors clustered at the firm level. The absence of systematic effects is consistent with the interpretation that the main results operate through beneficiary-specific information or incentive channels. The sample corresponds to the placebo coworker sample described in Section 3.2.

H Direct Effect

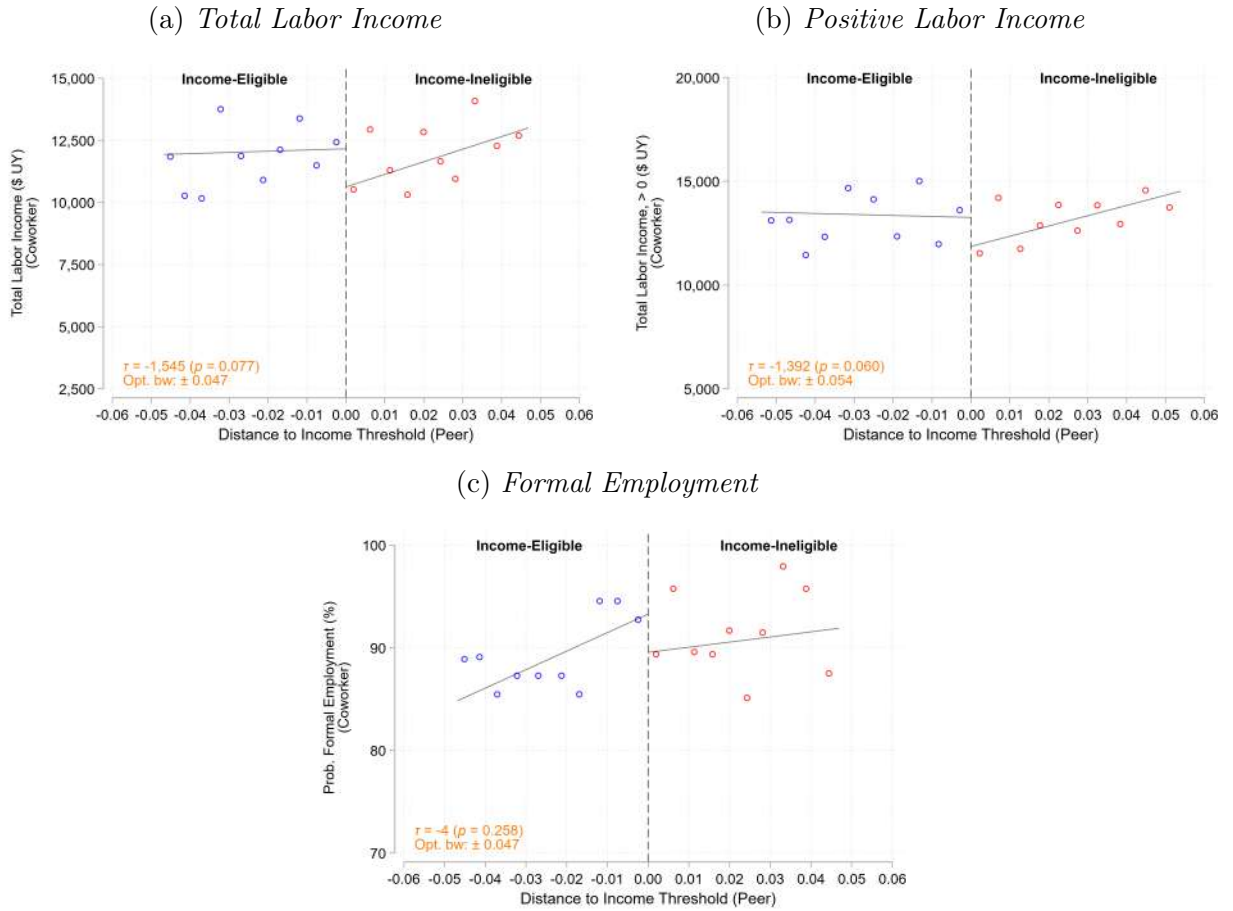
This appendix reports the direct effects of income-eligibility enforcement on peers' own labor market outcomes. It complements the main analysis by documenting how treated individuals adjust their labor supply in response to benefit suspension.

Table H.1: RD Estimates of the Effect of Suspension on Peers' Labor Market Outcomes, Three Months After Suspension

	Total Labor Income (1)	Labor Income > 0 (2)	Prob. Formal Employment (3)
<i>Panel A. Fuzzy RDD Estimate</i>			
Peers' Suspension (HH Income > Thresh.)	-3757.44 (2548.00)	-3617.06* (2382.72)	-0.09 (0.11)
Robust p-value	0.115	0.099	0.279
<i>Panel B. Reduced Form</i>			
Income-Eligibility Enforcement	-1544.58* (961.52)	-1392.16* (838.87)	-0.04 (0.04)
Robust p-value	0.077	0.060	0.258
Mean baseline	11836.39	13158.82	0.89
Effect size (%)	-13.05	-10.58	-4.14
<i>Panel C. First Stage</i>			
Income-Eligibility Enforcement	0.40*** (0.07)	0.38*** (0.06)	0.40*** (0.07)
Robust p-value	0.000	0.000	0.000
Opt. Bandwidth	.045	.055	.045
Observations	1033	1064	1018

Notes: This table reports RD estimates of the effect of income-eligibility enforcement on peers' own labor market outcomes measured three months after suspension. Panel A reports the fuzzy RD estimates (IV) based on the 2SLS estimation of Equation 1. Panel B reports the reduced-form estimates based on Equation 3. Panel C reports the first-stage estimates based on Equation 2. The sample corresponds to the peer sample described in Section 3.2, in which each peer appears once. All specifications use local linear regressions with triangular kernel weights and MSE-optimal bandwidths following Calonico et al. (2019). Robust standard errors clustered at the firm level are reported in parentheses. Mean baseline reports the average outcome among peers below the income cutoff (i.e., peers who qualify for AFAM-PE). Effect size reports the estimated coefficient as a percentage of the baseline mean. Statistical significance based on robust p-values: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Figure H.1: Effect of Eligibility on Peers' Labor Market Outcomes, Three Months After the Event



Notes: This figure presents reduced-form effects of income eligibility on peers' own labor market outcomes: total labor income in panel (a), labor income conditional on working in panel (b), and formal employment in panel (c). Outcomes are measured three months after the income-testing event. Estimates are obtained from Equation 3 using local linear regressions with triangular kernel weights and MSE-optimal bandwidths following Calónico et al. (2019). Dots denote mean outcomes within 10 equally spaced bins and solid lines denote fitted values on each side of the cutoff. The reported discontinuity corresponds to the local reduced-form effect at the threshold. Robust standard errors are clustered at the firm level. The sample corresponds to the peer sample described in Section 3.2.

I Constructing the Family’s Verifiable Income Variable

This appendix documents the construction of household verifiable income used in the analysis. It details the reconstruction procedure based on administrative records and presents validation exercises and robustness checks related to measurement error.

The AFAM-PE income-eligibility rule requires a monthly verification of household per capita income against the corresponding threshold. However, the household verifiable income measure is only observed in AFAM administrative records while households remain active beneficiaries. Once a household exceeds the threshold and benefits are suspended, this variable is no longer available. To recover a comparable measure for all households, we reconstruct the income variable using the official protocol employed by AFAM-PE administrators and the same underlying administrative sources available to program authorities. The protocol, provided by the AFAM-PE authorities for research purposes, combines the procedures used both at the application stage and during ongoing eligibility verification. The first step identifies all household members and establishes family links between them and the potential beneficiary (a child younger than 18 years old).

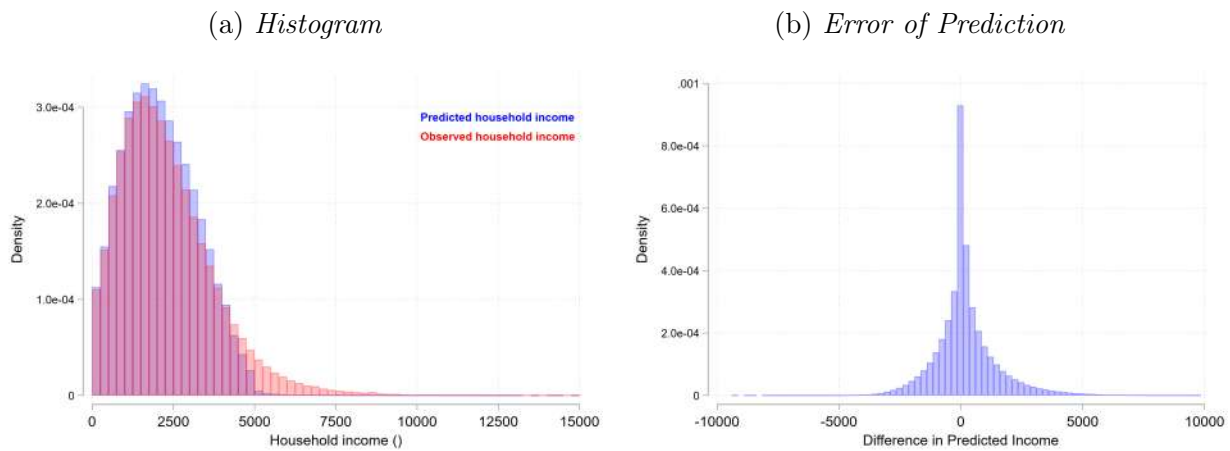
Labor income. The eligibility calculation includes labor income from all household members and all reported sources. We first aggregate formal labor earnings from salaried and self-employment activities, regardless of contract type (monthly, seasonal, or daily). These records come from monthly social security filings submitted by employers to the *Banco de Prevision Social* (BPS), the same source used by AFAM-PE administrators. A 15% deduction is applied to account for social security contributions. When current records are unavailable, administrators may rely on information from the previous two months. Our reconstructed measure instead uses ex-post administrative data with updated monthly records in all cases. At the application stage, administrative labor income is compared with self-reported household income (typically informal earnings), and the higher value is retained. We replicate this rule, although in most cases applicants report zero informal income.

Other sources of income. We also incorporate formal non-labor income, including pensions, sick leave payments, maternity benefits, and disability transfers. These amounts are obtained from complementary social security and tax records. Total income from these

sources is included, except for disability benefits, from which 1 BPC is deducted.²⁴ As with labor income, when current records are missing, administrators may rely on lagged information. For pensions, the program compares administrative records with self-reported amounts and retains the larger value. Household income excludes transfer payments, whether contributory or non-contributory.

Household verifiable income. The final variable used for eligibility determination equals total household income, as defined above, divided by the number of household members. Figure I.1 provides a first validation exercise comparing this reconstructed measure with the household per capita income observed in AFAM administrative records for households whose official income remains observable (i.e., those below the threshold). Panel (a) compares the distributions of observed and predicted income, while Panel (b) reports the distribution of prediction errors. Figure I.2 complements this exercise with scatter plots of observed versus predicted income in levels and logs for the universe of AFAM-PE households in 2009. Across specifications, the reconstructed measure closely tracks the official variable and shows no evidence of systematic bias.

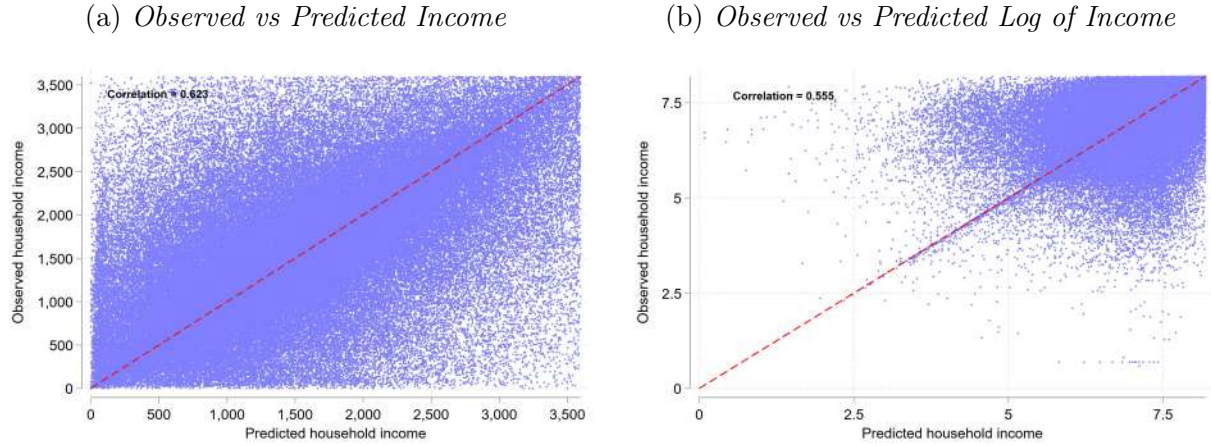
Figure I.1: Distribution of Predicted Household Income and Prediction Errors



Notes: This figure validates the reconstructed household verifiable income measure. Panel (a) compares the distributions of observed and predicted household income, where the predicted value is the measure reconstructed following the official protocol described in the text. Panel (b) shows the distribution of prediction errors, defined as observed minus predicted household income. The sample corresponds to households whose official income remains observable in AFAM administrative records.

²⁴The Benefit and Contribution Base (BPC) is an indexed monetary unit used in Uruguay to determine taxes and transfers, updated annually for inflation. In 2014, 1 BPC was UY\$ 2,819 (approximately US\$ 120).

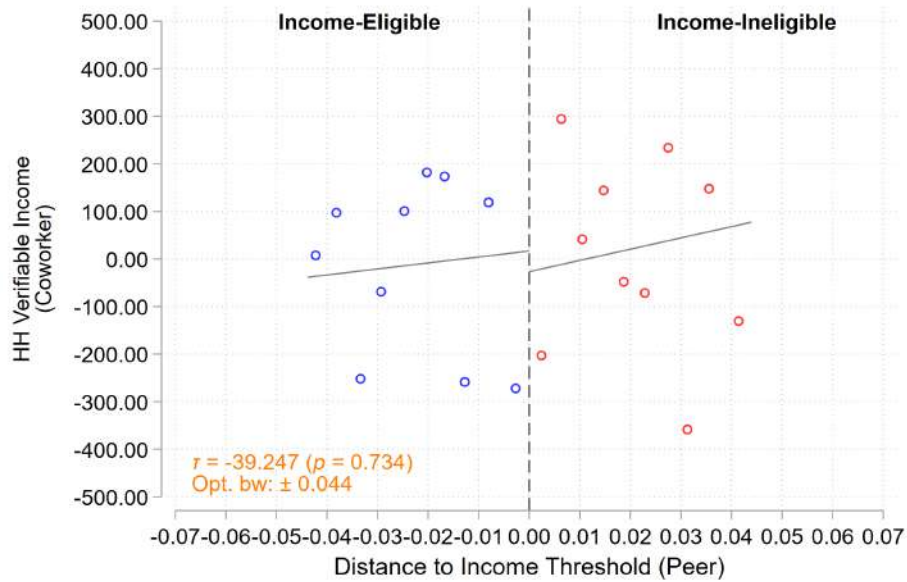
Figure I.2: Observed and Predicted Household Income



Notes: This figure compares observed and predicted household income, where the predicted value is the measure reconstructed following the official protocol described in the text. Panel (a) plots levels of observed versus predicted income. Panel (b) plots log observed versus log predicted income. The dashed 45-degree line indicates perfect agreement. Reported correlations summarize the fit of the reconstruction. The sample corresponds to the universe of AFAM-PE households in 2009 whose official income is observable in administrative records.

Finally, we implement an additional validation exercise using the difference between predicted and observed household verifiable income as the outcome variable. Using the coworker sample, we test whether prediction errors vary discontinuously at the peer income-eligibility threshold. Figure I.3 reports the corresponding reduced-form estimates. The estimated discontinuity is close to zero (approximately UY\$ 65) and statistically insignificant (p -value = 0.422), indicating no evidence of systematic differences in prediction errors around the cut-off. While this exercise does not directly validate measurement error in peers' reconstructed income—the relevant running variable in the RD design—it provides indirect evidence suggesting that the imputation procedure does not generate systematic discontinuities around the threshold.

Figure I.3: RD Estimates for Prediction Errors in Family Verifiable Income, Coworker Sample



Notes: This figure reports an RD test using the prediction error in family verifiable income as the outcome for the coworker sample. Prediction error is defined as observed minus predicted income. Estimates are obtained using local linear regressions with triangular kernel weights and MSE-optimal bandwidths following [Calonico et al. \(2019\)](#). Dots denote mean outcomes within 10 equally spaced bins and solid lines denote fitted values on each side of the cutoff. The reported discontinuity corresponds to the local RD estimate at the threshold. Robust standard errors are clustered at the firm level. The absence of a discontinuity supports the validity of the income imputation procedure around the cutoff.