

Good jobs, bad jobs, and the upward mobility potential of self-employment*

GEOFFREY BORCHHARDT[†]

University of Oregon

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Most of the self-employed eventually return to paid employment. Prior research documents a penalty for former entrepreneurs re-entering paid employment but assumes a background of a standard job. Could entrepreneurship facilitate upward mobility for workers from casual employment backgrounds instead? For these workers, entrepreneurship can demonstrate previously hidden ability through observable business performance and provide them with a more stable base from which to seek standard employment. I investigate the earnings and employment trajectories of former entrepreneurs in India. Among those who previously worked in non-salaried employment, those with a short spell in self-employment experience a small penalty. Those with a longer, successful spell reap large earnings premiums and transition to salaried employment at higher rates than matched controls. In contrast, most former entrepreneurs from salaried backgrounds face persistent earnings penalties. These results reveal how the upward mobility potential of entrepreneurship depends on alternative employment options. By relaxing assumptions of stable employment backgrounds, this study extends the careers lens to the modal entrepreneur globally.

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[†]Lillis Business Complex, 955 E 13th Ave, Eugene, OR 97403; gebo@uoregon.edu

Introduction

When does entrepreneurship provide a path to upward social mobility? Though only recently framed in these terms (Rider, Choe, Myung, and McCullers, 2023), prior work points to upward mobility arising, if at all, within entrepreneurship itself. Entrepreneurs, on average, earn less than employees, but a select few reap large, positive returns (e.g. Hamilton, 2000; Moskowitz and Vissing-Jørgensen, 2002). These returns accrue to those building successful organizations: they incorporate, hire employees, are highly educated, and draw on strong nonroutine cognitive abilities (Levine and Rubinstein, 2017; Sorgner, Fritsch, and Kritikos, 2017; Van Praag, van Witteloostuijn, and van der Sluis, 2013). A more recent careers lens finds that those who succeed and stay reap the earnings premium, while most return to paid employment after brief experimentation (Burton, Sørensen, and Dobrev, 2016; Manso, 2016). If entrepreneurship is to provide a path to upward mobility for more than the select few, the spell would have to help these returnees reach a better job than the one they left. Instead, a related body of work finds a penalty: former entrepreneurs receive fewer callbacks and lower wages. To employers, the spell signals weaker commitment and fit and leaves ability harder to judge (Botelho and Chang, 2023; Kacperczyk and Younkin, 2022; Mahieu, Melillo, Reichstein, and Thompson, 2021).¹ Even entrepreneurial success does not appear to ease this re-entry.

This evidence, however, comes almost entirely from wealthy economies, prompting recent calls to extend theories to entrepreneurs from non-Western contexts and marginalized backgrounds (Botelho, Gulati, and Sorenson, 2024; Hwang and Phillips, 2024).² Workers in these settings rarely hold the standard, salaried jobs that make up the common alternative

¹For exceptions focused on subgroups, see Campbell (2013); Kaiser and Malchow-Møller (2011); Merida and Rocha (2021).

²Scholars distinguish between different types of entrepreneurship—sometimes setting it apart from self-employment—and disagree on what to call them. Outside high-income economies, the job-creating type is rare. The vast majority of the self-employed run solo- or micro-enterprises with limited growth potential, often referred to as subsistence or sustenance entrepreneurship (Banerjee and Duflo, 2007; ILO, 2021). This is the form of self-employment on which this paper focuses. In keeping with the careers literature on entrepreneurship, I refer to these workers as both entrepreneurs and self-employed (Sørensen and Sharkey, 2014).

to self-employment in prior work. Instead, they engage in casual work that is short-term and precarious—arrangements that remain comparatively rare in the West, where the literature labels them nonstandard employment (Hewison and Kalleberg, 2013; ILO, 2016; Pedulla, 2016). This difference reshapes both the demand- and supply-side mechanisms that govern the long-term consequences of self-employment. The penalty documented so far rests on a demand-side signaling explanation. Yet, commitment concerns and evaluation uncertainty matter less when the jobs these workers return to are temporary and productivity is directly observable (Galperin, Hahl, Sterling, and Guo, 2020; Leung, 2014). Most critically, casual work generates few of the signals of ability that employers rely on when hiring for stable, salaried jobs, the positions that represent the clearest form of upward mobility. If entering self-employment costs these workers little of what it takes to reach such jobs, could it serve as a pathway to upward social mobility for entrepreneurs from casual employment backgrounds?

At least two complementary forces could turn a self-employment spell into upward mobility in contexts with high casual employment. First, on the demand-side, even the solo self-employment and micro-entrepreneurship common in these settings might send a stronger signal of ability than casual work, which rarely provides observable performance indicators over time. Hegde and Tumlinson (2021) demonstrate that workers lacking traditional credentials can perform well in entrepreneurship because markets directly reward ability. This success becomes visible, and over time generates its own signal. And even though prior work shows ability signals from entrepreneurship to be noisy, thereby adding to the penalty (Mahieu et al., 2021), even a noisy ability signal can exceed that of casual work where short tenures and replaceability reveal little about a worker’s quality beyond the task at hand. The same ability-performance relationship that lets these workers succeed in entrepreneurship may thus also let them signal ability to standard employers.

Second, on the supply-side, self-employment may open a path to better employment by helping entrepreneurs avoid the precarity of casual work and thereby enabling selective exits. Compared to casual work, self-employment can be a higher and more stable source of income

in these settings (Borchhardt and Sorenson, 2022). This should raise entrepreneurs’ reservation wages and let them hold out for longer, only selectively returning to paid employment for better work (Gimeno, Folta, Cooper, and Woo, 1997).

These two forces converge on the same observable pattern: successful entrepreneurs with longer spells move into better-paying, standard jobs. On the demand-side, a longer and more successful spell allows workers to more credibly demonstrate ability, and the resulting signal should be valued most by employers hiring for better (e.g., salaried) work. On the supply-side, the advantage of a stable base should grow over time, as unsuccessful entrepreneurs exit early (Manso, 2016), enabling successful ones to hold out for better positions. A short spell, by contrast, develops neither a signal nor a stable base. Instead, it disrupts the ongoing search and network ties on which casual work depends.

I examine the long-term income and career trajectories of the formerly self-employed in India. The majority of paid workers in India engage in casual, often daily-wage, work (Maiti, 2013). At the other end of the spectrum exist standard, salaried jobs with formal protections (ILO, 2018). The context is thus well-suited to study both entrepreneurs from casual employment arrangements whose prior employment generates few signals facilitating upward mobility and those from standard employment backgrounds similar to those in prior work.

I create a matched sample using large-scale survey data from 2016-2023 with triannual waves. Data availability has previously limited the study of (longitudinal) entrepreneurial returns to wealthier countries. The frequency of these survey data allows me to identify i) the persistence of earnings and employment trends, ii) control cases with similar pre-entrepreneurship employment and earnings histories, and iii) when an entrepreneurial spell begins and ends in a setting in which people switch between short spells of paid- and self-employment (Donovan, Lu, and Schoellman, 2023).

I examine the post-exit earnings and likelihood of salaried employment of two groups of former entrepreneurs: those from non-salaried and those from standard salaried employment

backgrounds. Former entrepreneurs from non-salaried backgrounds exhibit earnings trajectories comparable to those of control cases who remained in paid employment. However, this average hides important heterogeneity: those with brief entrepreneurial spells earn slightly less than controls, while those with longer spells earn substantially more. The earnings premiums are largest among entrepreneurs whose ventures performed well, most pronounced in urban labor markets, and coincide with higher rates of transition into salaried employment.

Former entrepreneurs who previously held salaried jobs experience significant earnings penalties upon re-entering the workforce, similar to patterns documented in Western economies.³ This discount persists for at least four years, and these individuals face difficulties securing new salaried positions. However, exploratory analyses indicate positive returns for the most successful and hint at dynamics that differ from prior work. I reconcile these contrasting findings by drawing on supply-side mechanisms—which have largely been held constant in prior work—that should extend across entrepreneurial backgrounds.

These findings contribute to the literature in several ways. First, they identify self-employment as a path to upward mobility for workers from casual employment backgrounds who sustain a successful spell. This presents a boundary condition of prior work, indicating that demand-side mechanisms operate differently when the (implicit) assumption of stable employment alternatives is not met. Instead, self-employment provides a positive signal, a possibility theorized in prior work but rarely observed empirically (Botelho and Chang, 2023). Supply-side dynamics further amplify the upward mobility trajectory. The supply-side mechanisms also explain why some formerly salaried entrepreneurs come out ahead and help reconcile conflicting findings. Increased reservation wages and a more stable base apply regardless of employment background. For such selective exits to pay off, however, entrepreneurial earnings must beat the worker’s alternative, a far higher bar when that alternative is a salaried rather than casual job. Prior work observed penalties for success in

³A larger share of these ventures may resemble the organization-building entrepreneurship of prior work, offering a closer comparison to Western studies. Yet, their earnings patterns do not vary by the venture type distinction, as I discuss below.

research designs that isolate the demand side (Botelho and Chang, 2023). When the design also captures selective exits, the penalty weakens or reverses (Luzzi and Sasson, 2016; Mahieu et al., 2021). More broadly, what enables mobility is entrepreneurial success relative to the worker’s own baseline.

Second, this study responds to calls for more inclusive entrepreneurship theories that center marginalized founders and entrepreneurs outside of Western contexts (Botelho et al., 2024; Hwang and Phillips, 2024). I study these dynamics in India, but the patterns among workers from casual backgrounds speak to what the West labels nonstandard employment, a segment that keeps expanding in high-income economies (Pedulla, 2016). Concurrent research on formerly incarcerated entrepreneurs in the US documents similar upward mobility (Hwang, 2021), while work in other contexts finds that ex-offenders fare best when they secure regular employment (Rocha and Sunao, 2026). My findings emphasize the role of employment alternatives. Where standard employment is within reach, it may well remain the better path; where it is unlikely and casual work more realistic, successful self-employment offers more direct access to upward mobility. The mobility potential of entrepreneurship thus extends beyond stigmatized groups to those excluded from standard employment more broadly. Lastly, this study empirically extends the careers perspective on entrepreneurship to emerging markets. In these economies, research on entrepreneurs has focused on entry into, and performance during, entrepreneurship (e.g. Bandiera, Burgess, Das, Gulesci, Rasul, and Sulaiman, 2017; Banerjee, Duflo, and Sharma, 2021; Carlson, 2023). By going beyond the entrepreneurial spell itself, my findings demonstrate when self-employment generates long-term advantages, countering characterizations of such self-employment as disguised unemployment and reconceptualizing its role for the vast majority of entrepreneurs globally.

The entrepreneurial penalty

Following early studies comparing the earnings of entrepreneurs to those of paid employees, recent work has taken a careers lens, recognizing that most entrepreneurs eventually return to paid employment, and started studying their transition back to the workforce (Botelho and Chang, 2023; Burton et al., 2016; Kacperczyk and Younkin, 2022). While social mobility is multi-dimensional, spanning income, employment, and social status (Rider et al., 2023), I focus on the dimensions the careers literature speaks to most directly: earnings and employment outcomes.

Most studies in this literature find that former entrepreneurs earn less in their first job back than those who remained in paid employment (Baptista, Lima, and Preto, 2012; Kaiser and Malchow-Møller, 2011; Mahieu et al., 2021).⁴ In Belgium, this earnings penalty persists in the long run (Mahieu, Melillo, and Thompson, 2022). Entrepreneurs are evaluated more negatively by potential employers. They receive fewer callbacks after job applications (Botelho and Chang, 2023; Kacperczyk and Younkin, 2022). However, exceptions exist both in the short run in Norway (Luzzi and Sasson, 2016) and in the long run for those in Denmark with entrepreneurial experience early in life (Merida and Rocha, 2021). Prior work primarily emphasizes two related classes of mechanisms explaining this entrepreneurial penalty.⁵

Fit and commitment. Hiring managers evaluate a spell in entrepreneurship as a signal that the applicant values autonomy and wide decision-making latitude. That inference raises doubts about the candidate’s willingness to accept the formal controls, hierarchy, and team-

⁴A second stream of research views self-employment as a form of experimentation (Manso, 2016). Entrepreneurs can learn about their entrepreneurial ability and test the potential of their ideas. If they do not succeed, they can return to paid employment, minimizing downside risk. In contrast to studies focusing on how *former* entrepreneurs fare, these papers estimate the returns to attempting entrepreneurship, including future earnings *both* in entrepreneurship and in paid employment. These studies consistently report positive long-term returns to experimenting with entrepreneurship in the US (e.g. Dillon and Stanton, 2017; Manso, 2016). This earnings premium is driven by the potential earnings in entrepreneurship outweighing the temporary earnings penalty experienced by unsuccessful entrepreneurs.

⁵It also emphasizes a preference for part-time work as part of the explanation of lower earnings for former entrepreneurs (Mahieu et al., 2022). The focus of this study is on people with poor employment options. For this group part-time work rarely represents a choice, but is due to an inability to find consistent work.

based routines that prevail in most established firms. The same autonomy cue also heightens expectations that a founder will soon leave to launch another venture. Because employers value hires who will both fit the organization and stay long enough to justify recruiting and training costs, they penalize former entrepreneurs on the grounds of cultural misfit and low commitment (Botelho and Chang, 2023; Kacperczyk and Younkin, 2022).

Evaluation of ability. A second class of mechanisms proposes that prospective employers discount former entrepreneurs’ wages because a spell in self-employment provides an ambiguous—or even adverse—signal of ability. First, entrepreneurship might hold little information value (Mahieu et al., 2021). Start-up outcomes hinge on many factors beyond the founder’s control, leaving outsiders unable to verify the entrepreneur’s true contribution. Facing this informational uncertainty, evaluators offer lower wages. Second, an exit from entrepreneurship can itself be interpreted as failure. Prior paid-employment histories create a positive signal of a candidate’s ability; an entrepreneurial exit contradicts that signal, leading employers to downgrade perceived productivity and discount wages (Mahieu et al., 2022).

Casual employment and signals of ability

This paper centers on workers from casual employment backgrounds. Following the careers literature, I refer to them as entrepreneurs when self-employed (Sørensen and Sharkey, 2014), though the vast majority of their ventures are solo- or micro-enterprises rather than the job-creating ventures often used to distinguish entrepreneurship from self-employment (Banerjee and Duflo, 2007; ILO, 2021). It is this form of self-employment the reader should keep in mind throughout.⁶

⁶In India 99% of businesses employ 10 or fewer employees, 95% employ 5 or fewer (Hsieh and Olken, 2014). I later examine a smaller group of entrepreneurs from salaried backgrounds, more comparable to those in prior studies and more likely to include organization-building ventures. Solo- and employer-entrepreneurship may signal different abilities, but my data record at best coarse proxies for incorporation or hiring, and I therefore do not center this distinction in the theory or empirics. Instead of venture types, my argument compares an entrepreneurial spell with each worker’s own counterfactual. Even a solo venture can reveal ability that casual work leaves hidden.

The penalty mechanisms above, however, assume a) counterfactual workers from *standard employment* arrangements—salaried, stable positions, often in high-skill roles—who can signal ability through affiliation to an employer, and b) entrepreneurs who aim to return to such standard employment. Most careers-lens research studies founders who held such positions before entering entrepreneurship (e.g. Burton, Sørensen, and Beckman, 2002; Sørensen and Sharkey, 2014). Under these assumptions, entering entrepreneurship is costly because it both forgoes the accumulation of signals from paid employment and is itself read negatively. But alternative employment options vary considerably, and the value proposition of entrepreneurship is a function of workers’ outside options (Borchhardt and Sorenson, 2022; Gimeno et al., 1997; Rider et al., 2023). Understanding the upward mobility potential of self-employment thus requires asking which signals a worker’s alternative employment would have generated and for which jobs those signals hold value.

Workers in standard employment relationships generate signals about their ability, reliability, and cultural fit as byproducts of their affiliation to an employer (Althauser and Kalleberg, 1981; Galperin et al., 2020; Wilk and Cappelli, 2003). An organization’s continued willingness to employ someone serves as an implicit endorsement, one documented through performance reviews, promotions, and professional references. Such signals prove particularly valuable in high-skill positions, and at high-status organizations, where ability is difficult to observe directly (Bidwell, Won, Barbulescu, and Mollick, 2015; Sørensen and Kalleberg, 1981). Entering entrepreneurship disrupts this accumulation, and the disruption is most consequential for those attempting to return to standard jobs.

Casual employment functions differently. Workers cycle through engagements lasting days, weeks, or months at most, accumulating neither formal performance documentation nor institutional references (Pedulla, 2016; Valenzuela, 2003). The literature also labels such arrangements *nonstandard employment*, a term coined against the benchmark of standard, salaried work in high-income countries, where these arrangements remain less common despite growing in prevalence (Bidwell, Briscoe, Fernandez-Mateo, and Sterling, 2013; Kalleberg,

2009; Pedulla, 2016). While I draw on these theories, the nonstandard label fits emerging economies poorly: in India, Peru, Ecuador, and Vietnam, 50-65% of workers engage in temporary, casual work, making it the most common form of paid employment rather than a deviation from a *standard* (ILO, 2016, 2021; Maiti, 2013). I therefore refer to the work itself as casual employment. Its fragmented nature becomes visible at construction sites and street corners where workers gather each morning, competing for positions that offer no guarantee of tomorrow’s employment: “[w]orkers often wait several hours before securing work for the day, and [...] experience bouts of unemployment that last several days, and periods of unemployment lasting several weeks” (Valenzuela, 2003, p.319). Sustained employment relationships are rare.

The prevalence of casual work hints at a critical boundary condition for existing theory. Its fragmented structure cannot provide the basic signal generated by continuous standard employment, which implicitly endorses a worker as possessing the competence their employer requires to retain them. Within casual labor markets themselves, this deficit matters little. Employers hiring for casual work prioritize immediate productivity over these long-term signals. They need workers who can perform short, observable tasks with minimal training and can be quickly replaced if performance lags (Valenzuela, 2003). The mechanisms behind the entrepreneurial penalty are therefore less likely to dominate here: its underlying theories assume “that the employment in question is continuous and is expected [...] to be long term” (Galperin et al., 2020, p.283), an assumption casual positions violate. Instead, hiring for short-term employment rewards demonstrable, job-specific experience. Workers may point to sites at which they recently worked, tools they own, or former coworkers who can vouch for their experience (Portes and Haller, 2005; Valenzuela, 2003).⁷ Beyond such signals of experience, remaining in casual work also keeps workers plugged into a continuous chain of engagements through which employers and coworkers pass along information about the next

⁷The absence of the right experience still gets penalized. But the evaluative logic differs from the uncertainty penalty in salaried hiring (Mahieu et al., 2021): workers are penalized for lacking proof of the skill at hand.

job (Portes and Haller, 2005). These signals and connections hold value for securing the next casual engagement. For transitions into standard employment, the absence of affiliation-based signals may count against these workers: employers interpret histories of casual work as an inability to secure or maintain standard positions, raising concerns about competence and commitment (Pedulla, 2016).

Taken together, the entrepreneurial penalty rests on comparing a spell in self-employment to experience in a salaried job, with the entrepreneur aiming to return to another salaried position. For entrepreneurs from casual employment backgrounds, both sides of this comparison change. Their alternative generates few of the signals that grant access to salaried jobs, and the casual employers they return to value immediate productivity over signals of commitment and fit. For these workers, other costs of entry should exist: a spell in self-employment interrupts the recent, job-specific experience and the chain of engagements and contacts through which casual workers find the next job—a cost to which I return below. But entrepreneurship does not imply forgoing the signals that aid transitions into standard employment. This raises the question of whether self-employment might offer a path to upward mobility instead.

Entrepreneurship and social mobility

Two complementary forces suggest it might: on the demand-side, a spell may generate the signals of ability that casual work cannot; on the supply-side, it may mean entrepreneurs can afford to wait to exit for better work. Prior research has focused primarily on the former class of mechanisms.

Entrepreneurship as an ability signal

Entrepreneurship offers something casual employment does not: observable, attributable performance outcomes. Unlike casual employment, where fragmented labor leaves little doc-

umented trail of affiliation or individual contribution, entrepreneurship generates evidence of a founder’s ability (e.g., revenues earned, customers served, or duration of survival) that others can observe and evaluate through the business’s operations themselves.

These entrepreneurial outcomes serve as the basis for meaningful ability signals because, as Hegde and Tumlinson (2021) demonstrate, entrepreneurship rewards ability more directly than wage employment. In standard employment, hiring processes rely on credentials and employer affiliations as proxies for ability. This creates frictions for high-ability workers who do not possess commensurate credentials or affiliations. In contrast, entrepreneurship offers a pathway by which workers can become residual claimants of their own productivity rather than accepting jobs limited by their observable signals. Market competition, in turn, ties performance to ability: higher-ability entrepreneurs are more likely to survive and grow their ventures while lower-ability entrepreneurs face exit.

This ability-performance relationship transforms entrepreneurship into a signaling opportunity for workers from casual backgrounds. When a street vendor builds a thriving business or a shop owner expands, they demonstrate signs of ability that would remain hidden in casual work. As long as employers believe entrepreneurial performance predicts employment performance, whether through innate ability, developed skills, or demonstrated work ethic, successful entrepreneurship should enable transitions to higher-quality employment.⁸

Importantly, this signaling mechanism strengthens over time through both selection and accumulation of observable performance outcomes. Initially, entrepreneurial spells provide limited information to prospective employers and are, at best, a noisy signal (Mahieu et al., 2021). Many individuals attempt self-employment, and early survival may reflect factors like initial capital or temporary market conditions rather than ability. However, as ventures

⁸What exactly constitutes the “ability” that drives entrepreneurial success remains theoretically ambiguous. It might reflect innate productivity differences that manifest across contexts (Hegde and Tumlinson, 2021), skills either possessed beforehand or developed through running a venture (such as execution capabilities, customer management, and other generalist competencies [Campbell, 2013; Lindbjerg and Vladasel, 2024]), or simply work ethic and perseverance that maintaining a venture demonstrates. For my theoretical argument, however, these distinctions are secondary. It only requires entrepreneurship to produce a clearer quality signal than casual work on any one of these dimensions.

mature, two dynamics reduce signal noise. First, the selection process intensifies: low-ability entrepreneurs exit when they cannot generate sustainable revenue, so extended survival becomes an increasingly informative signal of ability over time (Manso, 2016). Second, observable markers of success increase over time (Mahieu et al., 2021). Long lines of customers, expansion to prominent market locations, or hiring an employee all provide tangible evidence of entrepreneurial success (e.g. Doering and Liu, 2019). These accumulated markers allow employers to distinguish between varying degrees of entrepreneurial success—and by extension, between different ability levels among workers from casual employment backgrounds.⁹

Selective exits out of entrepreneurship

Whereas the signaling mechanism operates through employers’ evaluations, a second, supply-side pathway is driven by entrepreneurs’ own exit decisions. This pathway, too, begins with selection: low performers exit self-employment quickly (Manso, 2016). Those who remain tend to out-earn what casual work would have paid them. This realized performance raises their reservation wage, i.e., the lowest wage an offer must clear before they would return to paid employment (Gimeno et al., 1997). Since most casual jobs match neither their new pay nor stability, few entrepreneurs find reason to return.

Beyond raising the bar for acceptable offers, a viable venture changes how entrepreneurs can search. In casual work, finding the next job is a continuous process and financial precarity induces pressure to take the first offer (Sundar, 2011; Valenzuela, 2003). In contrast, entrepreneurship can provide more stable earnings and reduce the time spent searching for daily employment. This allows entrepreneurs to hold out for longer and apply selectively to higher-quality jobs while remaining self-employed.

⁹The signaling value of the counterfactual is key: in prior work, a noisy signal induced an earnings discount for former entrepreneurs (Mahieu et al., 2021). This discount appears most pronounced for those with higher earnings before entrepreneurship, i.e., those who are most likely to have held standard, salaried employment. In contrast, casual employment generates little portable evidence of individual ability. As such, any informative signal represents a gain over that baseline. The entrepreneurial signal remains noisy, since ventures succeed or fail for reasons beyond founder ability, but relative to the casual baseline its value is positive.

Notably, these dynamics presume voluntary exits, timed by entrepreneurs. Those whose businesses fail are unlikely to develop higher reservation wages, and selective exits are unlikely to materialize. Those who stay self-employed for longer, however, are more likely to run stable businesses outperforming most options in casual employment—and it is they who should reap an earnings premium upon returning.

Implications for social mobility following entrepreneurship

Both mechanisms outlined above—ability signals and selective exits—suggest upward mobility trajectories for entrepreneurs from casual employment backgrounds with largely converging predictions. Both presume a minimal level of entrepreneurial success, which is why the resulting premiums should accrue to the select subset of entrepreneurs whose ventures do well. The two mechanisms need not be read as competing explanations, and the predictions that follow do not depend on which one dominates. This section builds on that logic to derive what kinds of jobs entrepreneurs exit into, which labor markets make such trajectories possible, and what it implies for those with short spells.

As outlined above, both mechanisms predict earnings premiums to be most pronounced for former entrepreneurs with long, successful spells. A longer spell increases the visibility of the signal and the likelihood of the business having survived due to the owner’s ability. It also provides the entrepreneur with more time to engage in selective, on-the-job search for alternative employment options. Similarly, a high-performing business is more likely to create indicators of success visible to potential future employers and should imply higher profits for the entrepreneur, thereby raising their reservation wage.

For entrepreneurs with long, successful spells, both lenses, but especially the signaling perspective, predict earnings premiums realized through transitions into better employment, the clearest instance of which is standard, salaried work. Successful self-employment spells should provide a signal of ability. Even if the signal remains noisy (Mahieu et al., 2021), it should exceed casual work in its capacity to convey worker quality on dimensions valued by

employers offering standard work. In effect, a successful venture substitutes for the affiliation-based signals these workers lack. Casual employers, who value direct evidence of specific skills, have little use for more abstract evidence of underlying ability, but standard employers rely on it. The track record thus enables what was previously out of reach: transitions from casual work into standard, salaried positions.

The selective exit explanation leads to a similar prediction of transitions into better employment. In a fragmented, dual labor market for paid employment in which standard jobs offer high wages and job security (Althauser and Kalleberg, 1981), there is little reason for entrepreneurs to exit self-employment unless they can move into better-paying work, with stable, salaried jobs the most appealing such destination.

Urban labor markets. If the clearest path to upward mobility is based on transitions into stable, salaried employment, trajectory differences should be most pronounced in labor markets in which employers offer standard work. In urban centers, a successful street vendor or small shop owner can leverage their entrepreneurial experience to transition into, e.g., a formal sales position. In rural markets dominated by agricultural labor, the same entrepreneurial success may yield few mobility benefits as few employers exist who value such signals or offer the salaried positions to which entrepreneurs might exit.

Short spells. If long spells in entrepreneurship develop signals valued by employers offering standard work and selective exits require continued survival, what happens when a spell remains short? These entrepreneurs are forced to return to the casual labor market and might face a penalty there too, as anticipated above, albeit a different one compared to the commitment or fit mismatch identified in prior research. Compared to their peers who remained continuously in casual employment, returning entrepreneurs gave up i) the specialized skill signals required for many casual jobs and ii) the continuous chain of casual jobs through which workers stay connected to future employment opportunities.

Both losses matter and short-spell entrepreneurs might thus experience a (small) earn-

ings discount upon their return to paid employment. Casual hiring rewards demonstrable, job-specific experience. Missing evidence of such experience sends a negative signal. A construction crew wants someone who has, e.g., handled a concrete mixer before, and the typical self-employment options—agricultural work, street vending, or petty services—instead develop skills which casual employers do not seek. Returning entrepreneurs must likewise rebuild the relationships through which employers and coworkers vouch for abilities and pass along information about upcoming employment opportunities (Portes and Haller, 2005; Valenzuela, 2003). Similar to the consequences of job losses among Western low-wage workers (Rose and Shem-Tov, 2023), the fast return from self-employment thus resembles a career disruption, slowing job search and creating employment instability that results in decreased earnings.

Context: India

I focus on India to study the career trajectories of former entrepreneurs from standard versus casual employment backgrounds. India’s large economy features substantial populations of both types of workers: those engaging in stable, salaried work and those whose temporary employment generates few affiliation-based signals (ILO, 2018, 2021). This heterogeneity makes India ideal for examining how entrepreneurial experience affects workers from both backgrounds within the same institutional context.

India has been a market economy since 1991 and although it quickly grew to become one of the world’s largest economies, it remains lower-middle-income (Bhagwati and Panagariya, 2013). Its per capita income is similar to that of sub-Saharan African countries like Angola, Kenya, or Côte d’Ivoire. Around 10% of its population lives on less than \$2 per day.¹⁰

Opportunities for stable employment are scarce. More than 60% of those in paid employment engage in nonstandard casual work that does not create a strong affiliation with

¹⁰https://databankfiles.worldbank.org/public/ddpext_download/poverty/33EF03BB-9722-4AE2-ABC7-AA2972D68AFE/Global_POVEQ_IND.pdf

an employer (ILO, 2018). This casual work manifests as a daily scramble for employment at *labour chowks*, or *nakas*—designated corners at which workers gather from early morning, often waiting for hours in the heat or rain with few amenities. When contractors arrive, crowds cluster around the vehicles. Some are taken while many go home empty-handed that day. Workers may undercut each other’s wages to avoid losing the day’s work. Getting selected depends on immediate physical assessment rather than credentials, with workers carrying their own tools. A construction laborer hired today for laying bricks has no assurance of tomorrow’s work, pivoting between construction sites or loading docks based on daily demand. Agricultural laborers similarly may be hired based on seasonal cycles and immediate farm needs. Domestic workers move between multiple households daily, negotiating wages with each employer for cleaning, cooking, or childcare, their continued employment contingent on the daily satisfaction of families who often view them as easily replaceable (ILO, 2016, 2018; Maiti, 2013; Sundar, 2011).

In contrast, only around 20% are salaried. These positions, concentrated in urban centers and requiring higher education, have become intensely competitive as education levels rise. Employers increasingly use short-term contracts to circumvent labor protections that historically distinguished permanent from temporary workers (Hewison and Kalleberg, 2013; Maiti, 2013). The gulf between salaried and casual employment can be stark, separating formal contracts and career progression from a daily scramble for work (ILO, 2018).

Around 50% of working Indians engage in self-employment (ILO, 2018). Their businesses tend to be small, employ few outside of the immediate family, and often lack formal registration (Banerjee and Duflo, 2007). Entrepreneurs often sell food, small goods, or clothes at local markets. At the other end of the spectrum exists a smaller, yet thriving, entrepreneurial ecosystem populated by high-growth startups and venture capital (e.g. Hans and Vissa, 2023).

Data and Methods

I use data from the Consumer Pyramids Household Survey (CPHS) of the Centre for Monitoring the Indian Economy (CMIE) to estimate trajectories after self-employment. The survey has been used to explore issues around policy (Chodorow-Reich, Gopinath, Mishra, and Narayanan, 2019) and employment (Jayaraman and Khan, 2023). On questions related to employment, the CPHS resembles the US Current Population Survey (Chodorow-Reich et al., 2019).

CMIE surveys households three times each year, with four months passing between each wave. The survey covers roughly 160,000 households each wave and uses a regionally-stratified sampling approach similar to the Indian Census. The data contain earnings and employment information on each individual household member, whom I track over time. I use data on people surveyed in the CPHS between January 2016 and December 2023. Although the CPHS data reach back to 2014, employment status was not recorded until 2016. Eight years of data do not capture the same time horizons as Manso (2016) or Mahieu et al. (2022). But high rates of switching jobs in the Indian context imply that even shorter time horizons contain several post-entrepreneurship employment spells and capture long-term effects.¹¹

The main advantage of the data lies in their large sample size and high frequency. Prior surveys in emerging economies, such as the India Human Development Survey or RAND Family Life Surveys, had smaller samples and gaps of up to 7 years between survey waves, precluding the estimation of long-term returns. Particularly for casual jobs, employment spells are short-lived. The short intervals between waves of the CMIE data, thus, enable me to identify frequent moves between employment arrangements. They also allow me to capture the *persistence* of income trajectories for former entrepreneurs. This matters because prior work finds penalties to be permanent in some settings and temporary in others (cf. Mahieu et al., 2022; Manso, 2016).

¹¹Manso (2016) models effect sizes up to 12 years after *entry* into entrepreneurship; Mahieu et al. (2022) up to 6 years after *exiting* entrepreneurship. I show results for up to 15 waves, or 5 years, after the entrepreneurial spell ends.

However, the data are not without flaw. CMIE defines employment as solely pertaining to income-generating activities, which makes capturing all forms of women’s work, as well as comparing gender patterns to other data sources, difficult (Jayaraman and Khan, 2023, however, argue that this is measurement error and use the data to study women’s employment). The sample may also miss the extreme end of India’s poorest households (Somanchi, 2021). But casual work extends far beyond the ultra-poor, and my matching design does not require representativeness on these dimensions. My approach estimates earnings trajectories among people with specific attributes, rather than population-average effects.

Ideally, the data would also contain information on job spells and hybrid entrepreneurship. Similar studies compare those entering entrepreneurship to others switching jobs (Rider, Thompson, Kacperczyk, and Tåg, 2019; Sorenson, Dahl, Canales, and Burton, 2021). This is less critical in this context as most of the non-salaried can be assumed to frequently switch jobs. While mechanisms might differ for hybrid entrepreneurs (Folta, Delmar, and Wennberg, 2010), CMIE records the primary form of employment only. My estimand, thus, identifies the long-term effects of switching to self-employment as a primary source of employment—the more meaningful quantity in this setting.

Measuring entrepreneurship and employment backgrounds. I operationalize entrepreneurship as self-employment, based on the survey question asking whether the respondent’s primary form of income-generating activity is paid- or self-employment.

I divide the sample based on whether workers entered entrepreneurship from standard versus casual employment. I use CMIE’s employment arrangement variable to classify workers in permanent salaried positions as *salaried*, indicating a job with affiliation to a traditional employer offering standard work. Those from any temporary- or daily wage work—the *non-salaried*—lack such affiliations due to the informal, short-term nature of their employment and represent the casual workers (Althausen and Kalleberg, 1981; ILO, 2016; Maiti, 2013). I remove respondents who were unemployed right before entering entrepreneurship but do not

condition on entrepreneurs returning to any specific type of paid employment.¹²

Sample Construction and Matching

I employ a matching strategy to compare the long-term trajectories of former entrepreneurs to control cases with similar employment and earnings histories before self-employment. I restrict the survey to adults between the ages of 18 and 70 who record at least one employment spell and are surveyed at least ten times. I also remove entrepreneurs who became self-employed during 2020 to assuage concerns that findings are driven by the COVID-19 pandemic.

The ideal study estimates the effect of entering entrepreneurship for the first time to avoid capturing effects of repeat entrepreneurship (Eesley and Roberts, 2012). I remove all individuals who experience a self-employment spell in the first four waves in which they are observed. Due to left-censoring and the comparatively short time horizon of the data, this approach may still miss entrepreneurship spells pre-2016. Effects in this paper might, thus, better be characterized as entering entrepreneurship for the *first time in a while*. However, considering the frequency of self-employment spells in this setting, this might be a more realistic estimand and the findings of this study are robust to using more stringent restrictions.

I combine (coarsened) exact matching with nearest neighbor matching to identify a control group of similar others who did not enter self-employment (Iacus, King, and Porro, 2012; Sorenson et al., 2021).¹³ For each person who enters entrepreneurship, I identify potential matches based on age bracket (25 or younger, 26-35, 36-45, 46-55, 56+), education level (4th grade or less, 5-9th grade, some high school, high school degree, university degree), household size (3 or fewer, 4, 5 or more), gender, whether they live in an urban or rural region, and whether they work in permanent salaried employment or not. For those who previously

¹²A formerly non-salaried entrepreneur might thus return to the same type of temporary work, be unemployed, or even find permanent salaried work immediately upon their exit.

¹³Some prior work (e.g. Mahieu et al., 2022; Manso, 2016) uses propensity score matching instead. Iacus et al. (2012) point out the advantages of CEM over propensity score methods. The former ensures balance on the (coarsened) matching variables. Given the size of the data, CEM could be used without losing power to identify effects. It also allows increased emphasis on on pre-treatment income.

engaged in non-salaried employment, I also match on their state of residence.^{14,15}

Next, I follow prior work estimating income trajectories by matching on pre-treatment earnings (Campbell, 2013; Mahieu et al., 2022; Sorenson et al., 2021). Among potential matches identified via CEM, I select up to four control cases using Mahalanobis matching. Specifically, I identify matches using the two pre-entrepreneurship years’ a) average income and b) earnings slope (% change from wave t-6 to t-1).

Overall, the matching approach tries to identify similar others who could have, but did not, enter entrepreneurship. Matching on the income and employment history leading up to entry ensures that entrepreneurs and control cases have similar earnings trajectories and employment (in-)stability. It aims to guard against financial needs or resources driving entry. While matching on pre-treatment outcomes can induce bias under exogenous assignment, it substantially reduces bias when outcomes are correlated with unobserved confounders as is expected in the case of entrepreneurial entry (Ham and Miratrix, 2022).¹⁶ For example, if ability or a willingness to work long hours predicts earnings, which is especially likely in casual work, matching on several waves of pre-treatment earnings minimizes such differences.

I also condition on the control case being employed during the wave when the “treated” person enters self-employment. This aims to ensure that effects are not driven by control cases exiting the labor market.¹⁷ However, the rest of the time period during which the

¹⁴I identify matches with replacement, and at the time of entry into the first entrepreneurial spell observed in the data. Each “treated” person, thus, only appears once in the final sample. Control cases, however, can appear repeatedly. For regressions, standard errors are clustered at the matched group and household level to reflect this potential correlation across matched groups.

¹⁵I do not match on the state of residence for previously salaried entrepreneurs to ensure that there are enough candidates among which to match on pre-treatment income. Combining CEM with income matching involves a tradeoff: stricter CEM criteria leave fewer candidates from which to match on income histories. As there are fewer salaried employees than non-salaried ones, I can be slightly more stringent and still ensure good quality matches among the latter. I ensure robustness to this matching strategy in the Appendix.

¹⁶Ham and Miratrix (2022) and Roth, Sant’Anna, Bilinski, and Poe (2023) note that matching on pre-treatment outcomes means that the main identifying assumption becomes that of unconfoundedness, rather than parallel trends. Like most other papers estimating the returns to entrepreneurial experience (e.g. Campbell, 2013; Mahieu et al., 2022), this study does not observe random entry into self-employment and relies, at its core, on the selection-on-observables assumption. Therefore, reducing bias is the first-order concern.

¹⁷To the extent that entrepreneurship affects career trajectories by helping avoid unemployment, this approach might underestimate effects. However, not conditioning on the employment of control cases might artificially inflate estimates if control cases are not seeking work at the time of entrepreneurial entry for other

“treated” person is self-employed remains a black box and the control case could fall into unemployment or transition into better, salaried employment during the spell.

Table 1 shows summary statistics of the data before and after matching to illustrate balance and representativeness of the matched sample. Before matching, those who eventually become entrepreneurs report higher pre-entry earnings and more positive earnings trajectories, skew male, are more likely to come from the middle of the educational distribution, and are less likely to be unemployed than their counterparts who do not become entrepreneurs. The final sample contains matches for 21,708 entrepreneurs (just above 70%) using 76,222 control cases. The self-employed in the matched sample closely resemble the self-employed in the full pre-matching sample. Their earnings are slightly lower, as no good matches exist for a few high earners. Distributions of variables not matched on (religion, caste, household savings, assets, expenditures) are very similar.

The matched control cases look much more similar to entrepreneurs than the full population before matching. Control cases earn slightly more but are on slightly less positive earnings slopes before entry. They are also less likely to report having other family members engaging in self-employment. The rest of the variables, including those not used in the matching procedure, look comparable across entrepreneurs and control cases. In robustness checks, I employ more restrictive matching conditions.

Entrepreneurial spells tend to be short in this setting, similar to the US (Manso, 2016). Figure 1a shows that over 60% last a year or less. Less than 10% last more than three years. Figure 1b shows that more than 30% of entrepreneurs re-enter. To ensure a direct comparison to prior work, I remove those entrepreneurs who eventually re-enter self-employment from my main analysis. For context-relevant transparency, Appendix Figure A.1 shows earnings trajectories for this group separately.

reasons. Estimates thus capture the long-term effect of having gained entrepreneurial experience rather than remained in paid employment.

Empirical Estimation

I estimate earnings before- and after entrepreneurship using a matched group fixed effect event study, combining approaches from Sorenson et al. (2021) and Mahieu et al. (2022). I estimate the following equation:

$$y_{it} = \alpha_{g,t} + \sum_{h=-Q}^H \beta_h D_{i,t-h} + \epsilon_{it} \quad (1)$$

in which $\alpha_{g,t}$ indicates a set of interacted wave-specific time t and matched group g fixed effects. The matched group fixed effects act as flexible controls of all variables included in the matching procedure and do not presume any functional form. By interacting them with time fixed effects, I flexibly account for different groups of people experiencing dissimilar time shocks. The coefficients $\sum_{h=-Q}^H \beta_h$ capture earnings trends of the entrepreneurs with $D_{i,t-h}$ indicating the dummy variables that jointly identify the time (survey waves) before entering and after exiting self-employment. The spell in entrepreneurship itself—defined by consecutive waves in self-employment and ending with the first wave in paid- or unemployment—is excluded for both the entrepreneur and matched controls. ϵ_{it} denotes the error term.

Dependent variables: Income and salaried employment. y_{it} is the dependent variable, primarily measured for each individual as a survey-wave’s most recent month’s *income* in Indian Rupees (Rs).¹⁸ This measure includes all income attributable to the individual including wages, dividends, and earned interest. For those who re-enter entrepreneurship (included only in the appendix), it also includes income attributable to a business. The estimate thus collectively captures long-term differences in earnings driven by the wage rate, hours worked, stability of earnings, earnings slopes, and employment types.

The secondary dependent variable is *salaried employment*. This is constructed in the same manner as the variable dividing the sample into employment backgrounds: those in

¹⁸Over the course of the sample \$1 equals between Rs 65-85. Across all waves, the average monthly earnings of previously non-salaried entrepreneurs, and their matched counterparts, are Rs 8.5k. For those salaried before entrepreneurship, the average monthly income across all waves is Rs 24k.

permanent, salaried work are classified as *salaried*.

Salaried versus non-salaried backgrounds. My empirical strategy estimates separate models via OLS for workers who entered entrepreneurship from salaried versus non-salaried employment, comparing former entrepreneurs to matched non-entrepreneurs within each group. This approach follows directly from the theoretical framework: entrepreneurship functions through fundamentally different mechanisms for workers with versus without employer affiliation-based signals.

This split-sample approach differs from a pooled model with interactions between employment background and entrepreneurship. An interaction model addresses how the effect of entrepreneurship varies for otherwise similar individuals who differ in their employment arrangement. But workers in salaried and non-salaried employment are not “otherwise similar”. Salaried workers are typically from backgrounds more privileged in terms of education, ability, and socioeconomic status. Attempting to compare workers across backgrounds would thereby isolate extreme, atypical cases: e.g., comparing high-earning non-salaried workers to unusually low-performing salaried workers. My approach instead asks how entrepreneurship affects workers within each employment segment, comparing entrepreneurs to non-entrepreneurs who face similar (signaling) constraints.¹⁹

Earnings and career trajectories after self-employment

Figure 2 plots estimated coefficients with 95% confidence intervals from seven waves (just above two years) before entrepreneurship to 5 years after entrepreneurship. The two vertical,

¹⁹The split-sample approach implies that the two groups differ not only in employment background but also along correlated dimensions including education, caste, and socioeconomic status. Within each group, matching on education and pre-entry earnings controls for these differences. At the same time, these correlated factors reflect access to signals of ability beyond just those stemming from employment. This raises the question whether my theory can extend to workers who lack other, non-employment-based, ability signals. Appendix Figure A.14 shows that sample divisions based on education or caste produce similar though attenuated patterns compared to those based on employment background. This suggests that employment background most directly captures access to relevant signals of ability, but others might also play a role. I return to this larger question in the discussion section.

dashed lines demarcate the pre- and post-entrepreneurship time periods. The entrepreneurial spell itself is excluded. Each coefficient estimates the monthly earnings difference between entrepreneurs and matched controls t waves before or after entrepreneurship. The green diamonds show the earnings difference between previously salaried entrepreneurs and their counterparts; blue triangles indicate entrepreneurs from non-salaried backgrounds. Figure 2 excludes entrepreneurs who experience a second spell in self-employment.²⁰

Trajectories from non-salaried backgrounds

I begin with the earnings trajectories of the self-employed from non-salaried backgrounds. Figure 2 shows similar patterns between former entrepreneurs and matched controls: a mostly insignificant premium peaking at around Rs 370, or 4%, 16 months after entrepreneurship, which turns slightly negative three years post-exit (Table A.1 in the Appendix shows summary statistics by group). Overall, effect sizes stay below 5% and rarely cross conventional significance thresholds.

At first glance, then, the self-employed from casual backgrounds appear unable to translate their experience into upward mobility. Their time in self-employment rather resembles engaging in just another form of casual employment. However, the proposed advantage of self-employment rests on the assumption that entrepreneurs improve their signals and position over time, and with success. Figure 1 showed that the vast majority of spells are short. For entrepreneurs to distinguish themselves, they need continued success. I, therefore, investigate whether entrepreneurs with longer stints differ from the rest.

Figure 3 graphs point estimates after re-estimating equation 1 for formerly non-salaried entrepreneurs but distinguishes between those with a short (<1 year) and longer (1+ years)

²⁰For transparency, Figure A.1, in the appendix, includes these entrepreneurs who re-enter entrepreneurship at some point after their first spell. They (orange squares) reap an earnings premium that increases over time. Immediately upon exit, while everyone works in paid employment, former entrepreneurs only report a small earnings premium of Rs 250. But the premium grows to around Rs 2,500 (or 30%) after two years, as re-entry increases. These large effects are consistent with prior work highlighting a sizable earnings premium for entrepreneurs (during entrepreneurship) at the bottom of the income distribution in countries such as India or Bangladesh (Balboni, Bandiera, Burgess, Ghatak, and Heil, 2021; Banerjee et al., 2021; Borchhardt and Sorenson, 2022).

entrepreneurial spell. I estimate results for a shorter time horizon post-exit since there are fewer post-entrepreneurship observations for those with a longer entrepreneurial spell.²¹

Figure 3 indicates that treating all of the self-employed from casual employment backgrounds as one group hides variation important for understanding when entrepreneurship can facilitate upward mobility. In contrast to Figure 2, former entrepreneurs with only a short spell in entrepreneurship reap a small, but persistent, earnings penalty upon returning to (presumably casual) paid work. Point estimates for this group (light blue triangles) show them to earn about Rs 300, or 3.5%, less per month than those who did not become entrepreneurs. Not every estimate is significant, but the consistent effect sizes (most at least marginally significant) support reading this as a small penalty. Considering that almost 40% of entrepreneurs report only a single, short spell, there is a sizable portion of the non-salaried for whom entrepreneurship comes with a cost. The penalty is small, however, next to that for entrepreneurs from standard employment backgrounds.

Conversely, those who remain self-employed for longer (green circles) reap a premium in future paid work. In the first wave after entrepreneurship they earn Rs 1,700 more than matched controls, around 20% more than the monthly earnings of the non-salaried. After two years, they earn almost Rs 3,000 more, though point estimates start to decline toward three years. Unfortunately, the data cannot distinguish whether this decline begins a trend or reflects a single cohort under right-censoring. Over the first eight waves, these findings appear consistent with a more generous reading of entrepreneurial experience for those from casual employment. Rather than just engaging in another form of casual work, those with longer, more successful spells improve their position and find better-paid work.

²¹Due to the right-censored nature of the data, estimating earnings past three years post-entrepreneurship for entrepreneurs with longer spells thus derives far-out estimates based on single cohorts: those who entered in 2018 and whose spells just barely lasted over a year. For most matched groups (later entrants and longer spells), the data end before wave 10 post-exit.

Transitions into salaried employment

Beyond higher incomes, both the signaling and selective exit explanations predict transitions out of casual employment into better jobs. I estimate linear probability models of the same structure as equation 1 but use an indicator for salaried employment as the dependent variable, distinguishing between those with a short and long spell in self-employment.

Figure 4 shows that long-spell entrepreneurs from casual backgrounds are substantially more likely to transition to salaried employment than matched controls. They are 5-12 percentage points more likely to engage in permanent, standard work. Pre-trends indicate small differences between entrepreneurs and controls in the likelihood of working in salaried employment pre-entry. I therefore corroborate results with a caliper, restricting the sample to better matched groups with control cases within a Mahalanobis distance $D_M \leq 1$ (in terms of pre-treatment earnings). This improves pre-trends without meaningfully changing results for those with a long spell (see Appendix Figure A.2). However, for short-spell entrepreneurs it adjusts post-exit point estimates downward such that they approach zero. A short spell thus does not appear to provide the upward mobility that success does.^{22,23}

To provide more direct evidence that transitions into better employment drive the earnings premium—as predicted by both theoretical mechanisms, though especially signaling—I estimate earnings trajectories for long-spell entrepreneurs, distinguishing by employment arrangement during the first wave post-exit. Figure 5 shows that those who transition

²²In a related test, I estimate whether entrepreneurs move into more desirable occupations or industries. I rank order occupations and industries by their average earnings and re-estimate the regression behind Figure 3 using these ranks as the dependent variables. Figure A.3, in the appendix, shows that entrepreneurs from non-salaried backgrounds with a long spell in entrepreneurship transition to higher-paying occupations and industries.

²³In robustness checks I ensure that these patterns are not driven by founders re-classifying their employment status as “salaried” by paying themselves a salary. While not a major concern considering the nature of the survey and the rarity of formal incorporation, this should guard against the most successful entrepreneurs gaining their advantage during entrepreneurship. Beyond the employment arrangement variable, I draw on the occupation category “businessman”—those who own or manage business premises and capital, and thus the group most likely to have formalized a venture—and reclassify everyone in this category as self-employed, regardless of their employment arrangement (less than 0.5% of the observations in the businessman category are coded as salaried). I re-run the matching procedure following this change and find the earnings premium and the higher rate of transition into salaried employment for long-spell entrepreneurs unchanged (Appendix Figure A.18).

to salaried employment experience a large earnings premium compared to control cases. Those who return to non-salaried employment after a long spell reap a much smaller earnings premium.²⁴ Overall, these patterns are consistent with successful entrepreneurial spells translating into upward mobility along a gradient of better jobs—most clearly into salaried employment, but also into higher-paying non-salaried work.²⁵

Who experiences the earnings premium?

Beyond predicting earnings premiums for those transitioning into better employment, the theoretical argument implies that the largest advantages should accrue to those who performed best during self-employment and are situated in urban labor markets. As theorized, a better-performing business should both raise the reservation wage and generate more credible, visible signals. Without such success, both advantages weaken: a merely surviving business generates few signals worth leveraging, though it may still allow entrepreneurs to time their re-entry and avoid part of the disruption cost of short spells. Their returns should thus fall somewhere between the premium of their successful counterparts and the penalty of short spells. To explore this, I divide the long-spell entrepreneurs into those reporting below (vs above) median earnings (including take-home pay from the business) during the spell.²⁶ In the CPHS, most income from entrepreneurship is captured at the household level. I divide that number evenly across self-employed household members, adding it to the main income measure. This take-home component reflects reinvestment and household allocation decisions. I still treat it as a rough proxy for business performance: among microenterprises in emerging economies, reinvestment rates are low and business and household finances barely separate (Banerjee and Duflo, 2007; de Mel, McKenzie, and Woodruff, 2009). The mea-

²⁴For transparency, Appendix Figure A.20 re-graphs Figure 5, including those whose first observation post-exit is in unemployment.

²⁵These destination differences also hold relative to the entrepreneurs' own spells: nearly three-quarters of those transitioning into salaried work out-earn their spell earnings, while only around a third of those returning to non-salaried work do. Still, a majority of even this group out-earns their matched controls (Appendix Table A.7).

²⁶See Appendix B for more information on how CMIE captures business earnings.

sure should therefore capture both increased reservation wages (higher take-home pay) and improved signals (from a well-run business); I do not adjudicate between the two here.

Entrepreneurial earnings play a key role in career advancement. Panel 6a shows that those with a long spell and above median entrepreneurial earnings are driving the earnings premium, earning between Rs 2,000-4,000 (or 23%-48%) more than controls. They also show higher rates of transitioning into salaried employment (Panel 6b), though point estimates are noisier and differences smaller than for the income estimations. Again, pre-trends indicate slightly higher earnings and rates of salaried employment for high-performers before entrepreneurship, but results change little under the $D_M \leq 1$ caliper (Appendix Figure A.7).

Notably, these findings also provide some insight into what is (not) signaled by a successful stint in entrepreneurship. Considering the precarious, temporary nature of non-salaried employment, continuous employment in entrepreneurship might serve as a signal of grit or persistence, rather than innate ability. But if the earnings premium of long-spell entrepreneurs were driven by persistence, we would expect outcomes to only weakly depend on entrepreneurial performance. If entrepreneurship signals ability, instead, we would expect those with well-run businesses to accumulate more visible pieces of evidence of their ability and thus report higher earnings. While consistent with both selective exit and ability signaling, the findings do not support survival alone signaling work ethic or persistence.

A related question is whether the returns reflect the development of (industry-)specific capabilities. If the premium stemmed from industry-specific human capital, we would expect it to accrue to those returning to the same sector as their venture, as documented for entrepreneurs in higher-income settings (Kaiser and Malchow-Møller, 2011). The data show, if anything, the opposite. Column 1 in Table 2 uses solely the matched sample of long-spell entrepreneurs from casual employment backgrounds. The main coefficient indicates the average effect of having entrepreneurial experience. That is, it averages out time trends from earlier graphs to provide a single point estimate, but maintains the same matching- and fixed effects structure from equation 1. I interact the post-entrepreneurship variable with an indicator

for remaining in the same industry.²⁷ Column 1 shows that entrepreneurs switching to new industries earn an average premium of nearly Rs 3,500 per month, while those remaining in the same industry see this premium reduced by more than half. This is in line with high-paying salaried jobs existing in different sectors from those in which businesses commonly get started. These patterns thus support selective exits or an ability signal explanation over sector-specific human capital as primary drivers of the earnings premium.²⁸

These patterns also do not support an (industry-specific) networks-based explanation of career advancement. If self-employment expands networks, one would expect this to largely stem from within the industry of the entrepreneur’s business. If new contacts facilitate transitions into better paid employment, they do not appear to be doing so in their core industry.

Lastly, I probe whether the premium depends on the venture resembling the more capitalized businesses behind the entrepreneurial earnings premium in wealthier economies (Levine and Rubinstein, 2017). The CPHS notes the occupation status “businessman” for those who own or manage fixed business premises with invested capital—the closest proxy to the more capitalized, organization-building ventures of prior work. The premium persists even for those outside this category (Appendix Figure A.19a). It is thus not solely driven by growth-oriented ventures but extends to the businesses typically available to casual workers. That extension matters, since fixed premises and invested capital are beyond the reach of most casual workers and a route running only through such ventures would limit the upward mobility potential for most entrepreneurs. Still, those in the businessman category appear to reap larger returns. This is consistent with both mechanisms developed above: such ventures tend to be larger and higher-earning, which should increase their visibility and signal as well as the entrepreneur’s reservation wage. The differences are noisy, however, and best read

²⁷The main effect of industry switching is absorbed by the fixed effects as it varies only across entrepreneurs.

²⁸Exploratory analyses in Appendix Table A.9 probe this further by adding destination-industry fixed effects. The main effect of entrepreneurial experience decreases substantially but remains positive and significant, while the same-industry interaction term loses significance. These patterns indicate that the earnings premium is, in large part, driven by former entrepreneurs entering new industries that remain inaccessible to those without entrepreneurial experience and further refute an industry-specific skills story.

as suggestive, as the businessman classification also correlates with prior access to capital, selecting on a better-off subset of the previously casually employed.

Boundary condition: Local labor markets. The upward mobility argument is contingent on transitions into better forms of employment and should weaken when such opportunities are scarce. I test this boundary condition by examining heterogeneity across the urban-rural divide. Standard salaried employment concentrates heavily in urban areas. In my data, 85% of salaried workers are located in urban settings, leaving rural entrepreneurs with limited opportunities for advancement into stable employment. We should observe stronger effects in urban areas where such positions are more abundant.

Figure 7 presents earnings trajectories for previously non-salaried entrepreneurs by spell length and urban-rural location. The results strongly support a boundary condition. For those with short spells, the urban-rural divide appears to matter little. Long-spell entrepreneurs, however, exhibit stark differences: former urban entrepreneurs earn premiums exceeding Rs 3,000 after two years, while their rural counterparts show near-zero effects.

Is the premium more than selective exits?

The patterns so far are consistent with both selective exits and signaling, raising the possibility that solely the former drives my findings thus far. While the data make it difficult to distinguish between supply- and demand-side mechanisms, I provide suggestive evidence that both explanations are operative here.

I first document selective exits by exploring the relationship between earnings in entrepreneurship and the likelihood of exit. I show Kaplan-Meier survival rates for above (vs below) median earners during the entrepreneurial spell in Appendix Figure A.15 and estimate a discrete-time, complementary log-log hazard model predicting exit based on last wave's entrepreneurial earnings in Appendix Table A.5. Findings indicate that higher earnings decrease the likelihood of exit, but increase the hazard of transitioning into salaried employment. These patterns are in line with selective exits: successful entrepreneurs are less

likely to return to paid employment unless a better opportunity arises.

That selective exits operate need not mean they account for the full story. If the premium were entirely driven by entrepreneurs' raised reservation wages, we might expect one of the following two patterns.

First, if entrepreneurs only return to paid work when they can earn more in paid employment, we would expect their post-exit earnings to exceed earnings during the entrepreneurial spell. Only around 35% of those with a long spell in self-employment earn more after entrepreneurship than during it (see Appendix Table A.6). However, more than half of them report higher earnings than matched controls. In fact, almost a quarter report lower earnings than during the entrepreneurial spell but still outearn control cases. If the earnings premium were mechanically driven by entrepreneurs only exiting when post-exit wages exceed entrepreneurial returns, entrepreneurs taking a pay cut should not outearn controls. Of course, these patterns do not rule out a selective exit story, since utility from employment goes beyond wages: a "better" job might offer more stability, as salaried work does. Another look at the data (Appendix A.7) shows that it is those exiting into the best, salaried, jobs who outearn both controls and their entrepreneurial spell.²⁹

Second, prior work often treats reservation wages as elevated for an extended period of time (e.g. Krueger and Mueller, 2016). Taken together with selective search taking time, one version of reservation wages might predict that entrepreneurs would search longer *after* the entrepreneurial spell. In that case, one would expect the earnings premium to be driven by long-spell entrepreneurs who take longer to transition into paid work. Appendix Figure A.16 contradicts this account. It splits the long-spell entrepreneurs into those who exited directly into paid employment versus those who first report unemployment following self-employment. The former group drives the earnings premium.³⁰ The earnings premium thus appears to be

²⁹Those who exit into non-salaried work often earn less than during their entrepreneurial spell, raising the question of why they leave at all. Conditioning on a return to non-salaried work likely selects on more involuntary, push-type exits—a faltering business, seasonal work, or costs I do not observe—yet even this group largely outearns matched controls.

³⁰A related pattern is consistent with reservation wages not accounting for the entirety of the story. When I regress post-exit earnings on both the average level of earnings during the spell and the direction of the

driven by the self-employed identifying strong opportunities in paid employment during their spell rather than engaging in a drawn-out job search process after self-employment.

What would a pure selective exit explanation predict? Documenting that selective exits operate is easier than *ruling in* a signaling explanation with these kinds of data. Consider, then, what would have to be true for selective exits alone to explain the patterns. Because transitions into salaried employment are the clearest form of upward mobility I observe, I use them to outline the logic. The elevated rate of transitions into salaried work must come from either more applications being sent to these kinds of jobs or from a higher success rate per application. Selective exits and reservation wages should operate on the first of these two. A more stable base may afford more time to send applications, while higher reservation wages imply that a smaller share is sent to low-wage (non-salaried) jobs, leaving more time for salaried applications. That the earnings premium is driven by immediate transitions into salaried employment implies that these changes must operate through on-the-job search behavior during self-employment, rather than more savings enabling post-entrepreneurship search from unemployment. A signaling story would imply changes to the success rate instead. Thus, if we hold the signaling side (i.e., success rates) constant, we can isolate the logic that would imply purely supply-side mechanisms being operative. Success rates being equal across entrepreneurs and those in paid employment reduces the question to whether either can send more applications to salaried jobs.

If the premium were driven solely by supply-side exit dynamics, the self-employed would thus have to send more applications to salaried jobs. This assumption is not directly testable with my data. But consider that running a business, especially a successful one, takes time and effort. And the premium is driven by precisely the entrepreneurs of these successful

earnings slope (increasing or decreasing over time), the latter does not significantly predict post-exit income. If reservation wages were dynamically updating, we would expect declining earnings in self-employment to predict a lowered reservation wage and therefore lower post-exit earnings. In contrast, signals should be slower to update while the business remains operational. I show these results in Appendix Table A.8, but treat them cautiously as entrepreneurial earnings can be volatile and strong assumptions are required to view these as discriminating between reservation wage versus signaling explanations.

businesses. While success also raises the reservation wage, it can only increase the number of applications to salaried jobs until all applications are targeting salaried jobs. After a certain level of financial success, it thus levels off. But better-performing spells continue to demand more time to run the business and do not level off at the same threshold at which entrepreneurial earnings exceed those from casual work. In other words, a pure selective-exit account thus requires the most demanding, highest-performing spells to also be the ones freeing the most time for salaried applications. As such, the evidence appears more suggestive of both mechanisms operating together: the successfully self-employed can focus their applications on higher-paying, salaried jobs and are, simultaneously, evaluated more positively by prospective employers than applicants from casual-employment backgrounds.

Evidence consistent with signaling. Moreover, salaried jobs are filled through hiring processes that rely heavily on signals (Wilk and Cappelli, 2003). So if a supply-side explanation alone is unlikely to account for the entirety of the increased transition rate, the remainder of the observed differences likely stems from how applicants are evaluated. Still, three more patterns provide suggestive evidence consistent with the signaling mechanism.

First, consider whether other family members were self-employed alongside the focal entrepreneur. Family involvement creates competing interpretations. Family members could function as employees, which should help entrepreneurs develop management skills valued by future employers (Banerjee and Duflo, 2007; Campbell, 2013). In Western contexts, running employer-businesses is associated with higher earnings (Sorgner et al., 2017). Alternatively, family involvement could dilute the ability signal. Did the business succeed because of the focal entrepreneur or were others the driving force? The data support the latter interpretation. Column 2 of Table 2 shows that entrepreneurs without family members in self-employment earn a substantial premium of Rs 2,700, but this effect disappears almost entirely when other household members are also self-employed. This pattern is consistent with ability signaling: when success cannot be clearly attributed to the individual entrepreneur, the signaling value of entrepreneurship is diminished.

Second, prior work demonstrates that age moderates how employers perceive entrepreneurial experience, with younger entrepreneurs typically faring better than older counterparts (Mahieu et al., 2022; Merida and Rocha, 2021). For workers from standard employment, this pattern reflects evaluation uncertainty: employers have limited information about young workers, making entrepreneurial failure less consequential than it would be for someone with an established track record. However, this logic reverses for casual workers. When employers observe older workers in precarious employment, they infer these workers have not demonstrated capabilities for higher-level positions, setting correspondingly low expectations (Pedulla, 2016). By succeeding in entrepreneurship, they exceed these expectations and generate a stronger positive signal than younger workers for whom uncertainty holds potential. Column 3 of Table 2 supports this mechanism: I interact the post-entrepreneurship variable with an indicator for workers under 35 and find that older workers derive greater benefits from entrepreneurship, consistent with the signaling explanation.

Lastly, the signaling mechanism should be particularly valuable for workers who lack alternative pathways to credible signals. While employer affiliation-based signals are one such pathway, in-group membership provides another. Members of dominant social groups enjoy in-group trust and have access to network connections who might vouch for their ability, potentially substituting for other ability signals. I construct a variable indicating whether an individual belongs to their district’s majority caste (from over 400 different castes). Column 4 of Table 2 reveals that the entrepreneurship premium essentially disappears for members of their district’s numerical majority caste. This pattern suggests that the entrepreneurial signal of ability is most valuable precisely for those who lack both employer affiliation and socio-cultural endorsements.³¹

³¹While network expansion could provide an alternative explanation for these results, earlier evidence of transitions into salaried employment in new industries, specifically among high-performing entrepreneurs, makes this less likely. The transitions into different forms of employment and new industries indicate that they are not driven by proximate professional contacts a new entrepreneur might make. Instead, these findings hint at a more general theory in which success in entrepreneurship can alleviate disadvantage faced on multiple dimensions: whether from employment background or a socio-cultural position.

What about short spells?

While the career advancement of successful entrepreneurs from non-salaried employment backgrounds represents the more novel piece of this paper, the earnings penalty for entrepreneurs with short spells differs theoretically from the penalty observed in prior work. I briefly explore its drivers in Appendix C.1. In short, the findings are consistent with short entrepreneurial spells resembling a disruption of a chain of casual employment spells. By stepping away, returning workers forgo the recent task experience and workplace relationships that let them demonstrate to the next casual employer they can do a specific job, and they disrupt the continuous search process that short-term employment requires. The penalty disappears when comparing the entrepreneurs to control cases who switched industries or occupations at the time of the entrepreneurial spell. Paid workers who remained in their industry (or occupation) did substantially better than short-spell entrepreneurs (see Appendix Table A.3). Further, I divide the short-spell entrepreneurs into those who directly return to paid employment and those who experience at least one wave of unemployment (Appendix Figure A.17). The latter group consistently reports lower earnings, while those who return directly initially outearn controls before settling on par with them. In other words, the earnings discount is concentrated among those whose return to the chain of casual employment was most delayed, consistent with a disruption account.

Trajectories from salaried backgrounds

Lastly, I explore the patterns for previously *salaried* entrepreneurs to assess how findings in the Indian context compare to prior work. Before entering entrepreneurship, they report similar earnings to matched controls. Point estimates in Figure 2 are occasionally noisy but remain statistically indistinguishable from zero. Upon exiting entrepreneurship, formerly salaried entrepreneurs earn substantially less than those who remained in paid employment. They earn around Rs 3,400 less during the first wave after entrepreneurship (a 16% discount relative to previously salaried non-entrepreneurs) and just over Rs 2,000 less after three

years. After four years, 95% confidence intervals consistently include zero, though this, in part, appears to be driven by imprecisely estimated coefficients.

One more piece of evidence echoes prior Western findings: the likelihood of returning to salaried employment. Figure 8 plots the likelihood of salaried employment for those who entered entrepreneurship from salaried employment. When using the same sample as before (blue triangles), pre-trends indicate a slightly lower likelihood of salaried work for eventual entrepreneurs. Therefore, I also show results using a $D_M \leq 1$ caliper. This shifts point estimates upward, such that the confidence intervals of the pre-trends include zero, but does not meaningfully change the interpretation of results. Reminiscent of prior work (Botelho and Chang, 2023; Kacperczyk and Younkin, 2022), I find that formerly salaried entrepreneurs in India are substantially less likely to return to salaried employment. The probability of salaried work is 12–14 percentage points lower compared to control cases immediately upon exit. However, they recover to the levels of matched cases after around four years, mirroring the slope of earnings trajectories in Figure 2.

As with non-salaried origins, treating salaried-origin entrepreneurs as a single group obscures important variation. When I split by spell length and by performance during the spell, the average penalty resolves into two very different trajectories. Figure A.12 replicates Figures 3 and 4 for previously salaried entrepreneurs. For those with a long spell, the earnings discount appears temporary and turns into an earnings premium after a little less than two years, though point estimates are noisy (Panel A.12a). Panel A.12b, however, appears to show that this earnings premium is not driven by a higher likelihood of working in salaried employment. While that appears puzzling at first, Figure A.13 provides more clarity. Panel A.13a shows that the premium is driven by long-spell entrepreneurs with higher entrepreneurial earnings, while Panel A.13b shows this group to be as likely to hold salaried work as controls. Successful previously salaried entrepreneurs, in other words, return to salaried employment at the rate of controls while earning more. What separates them appears to be how the venture performed rather than what kind of venture it was: splitting

these entrepreneurs by the “businessman” category introduced above shows little difference in trajectories (Appendix Figure A.19b).

While the average earnings penalty and lower likelihood of returning to salaried employment support findings from Western contexts, the increased earnings for successful entrepreneurs appear, at first, to contradict the hiring penalty successful founders face in Botelho and Chang (2023). However, audit study designs in Botelho and Chang (2023) and Kacperczyk and Younkin (2022) isolate the demand side by construction and cannot observe selective exits. But the selective exit logic underlying the premium for formerly non-salaried entrepreneurs should extend to those from salaried origins.³² In fact, consistent with selective exits, prior observational studies find the earnings penalty to be most pronounced among quick returnees (Mahieu et al., 2021) and find that firm performance positively predicts post-entrepreneurship earnings (Luzzi and Sasson, 2016). One read of these findings is that the ability to exit selectively can outweigh the penalty signal for successful entrepreneurs.

Overall, entrepreneurship offers a route to upward mobility, but one that reaches only a select few. The majority report only short spells and return to paid work worse off than matched controls. Gains accrue to those who remain self-employed for longer and perform well, but the degree of these gains differs across employment backgrounds. Those from non-salaried backgrounds experience clear upward mobility. They move from casual into better work, even salaried positions. Those from salaried backgrounds can, at best, hope to return to salaried employment at the rate of controls, though the most successful reap an earnings premium. The downside—the loss of salaried employment and a more persistent earnings discount after short spells—also looms larger for those from salaried backgrounds. The potential for upward mobility is therefore particularly salient for workers who begin without access to salaried employment. For them, signaling and selective exit point in the same direction; for those from salaried backgrounds, a successful spell must first overcome

³²Though the bar is higher: for selective exit to favor waiting, a venture must outperform a stable, salaried position rather than casual work, a threshold entrepreneurial returns rarely clear.

the entrepreneurial penalty before any premium emerges.

Robustness checks

The results are robust to three sets of checks, detailed in Appendix D. First, to address biases that can affect two-way fixed-effects event studies, I re-estimate the main specifications with the imputation estimator of Borusyak, Jaravel, and Spiess (2024), which shows trajectories closely tracking the main results (Appendix Figure A.6). Second, I use progressively more stringent matching specifications—adding extended employment-arrangement history, religion and caste, household assets, and industry and occupation histories—and find comparable patterns, though estimates grow noisier as stringency rises (Appendix Figure A.8). Third, I replace matching with entropy balancing (Hainmueller, 2012), which produces qualitatively similar patterns despite larger differences in pre-treatment earnings (Appendix Figure A.9).

Discussion

Recent research documents that former entrepreneurs face a penalty when returning to paid work (Botelho and Chang, 2023; Kacperczyk and Younkin, 2022; Mahieu et al., 2022), but it assumes a specific counterfactual: workers in standard, salaried employment. Such standard employment signals ability to employers of other standard jobs. Entrepreneurs forfeit these signals by leaving paid work. I asked whether self-employment can offer a pathway to upward mobility for the many casual workers around the globe, whose employment generates few such signals for standard employment.

I examine earnings and employment trajectories of former entrepreneurs in India to address this question. Entrepreneurs from casual employment backgrounds with short spells face modest but persistent penalties. In contrast, those with longer spells earn substantial premiums, at times exceeding 25%. The premium concentrates among those with above-average earnings during entrepreneurship. Crucially, successful entrepreneurs transition to

salaried employment, indicative of upward mobility. Still, it bears emphasizing that only some entrepreneurs reap these advantages, as over 60% of entrepreneurial spells in my setting are short and associated with a small earnings penalty.

Previously salaried entrepreneurs experience significant earnings penalties relative to matched controls. These penalties mirror findings from Western contexts, with discounts persisting for at least four years and coinciding with a reduced likelihood of returning to salaried jobs. However, here too, those entrepreneurs with higher-earning spells do better post-exit: they experience an earnings premium and are just as likely to return to salaried employment as controls.

The patterns are consistent with two complementary mechanisms. On the demand side, successful entrepreneurship generates the observable, attributable evidence of ability that casual work cannot. This extends Hegde and Tumlinson (2021). The same ability-performance relationship that drives higher returns for entrepreneurs lacking traditional signals, in turn, generates its own signals. On the supply side, higher earnings during self-employment raise reservation wages and provide a stable base from which to search, letting entrepreneurs exit only for better opportunities.

The results for successful previously salaried entrepreneurs build on this last point, reconciling my findings with prior evidence of a penalty even for successful entrepreneurs. Much prior work on the entrepreneurial penalty isolates demand-side effects (Botelho and Chang, 2023; Kacperczyk and Younkin, 2022), while studies allowing for supply-side dynamics find reduced penalties for long spells (Mahieu et al., 2021) or premiums for successful ones (Luzzi and Sasson, 2016). Selective exits might thus, over time, make up for a signaling-based penalty. But compared to (the income from) a salaried outside option, the bar entrepreneurial success needs to clear is high and rarely met.

Taken together, these findings demonstrate how the specific populations of entrepreneurs we study, and their counterfactual employment options, shape theories of entrepreneurial careers. The careers lens builds on the implicit scope condition that the entrepreneur’s coun-

terfactual is stable, standard employment when deriving the prediction that it raises concerns about commitment and fit. When we relax this implicit assumption, entrepreneurship can generate what casual employment does not—signals of ability and a stable base from which to search. This insight extends the careers lens to the vast population of entrepreneurs globally who lack stable employment alternatives (Hewison and Kalleberg, 2013; ILO, 2021). More broadly, this emphasizes the need for research on entrepreneurial careers to clearly specify the choice sets available to potential entrepreneurs.

By examining entrepreneurs whose choice sets lack standard employment, this paper also responds to calls to build more inclusive entrepreneurship theories drawing on founders from non-WEIRD and marginalized backgrounds (Botelho et al., 2024; Hwang and Phillips, 2024). These papers argue that shifting focus from elite founders reveals not just findings for new populations but fundamentally different theoretical mechanisms that allow for more generalizable theories. Putting my paper in conversation with concurrent research on other groups excluded from standard employment suggests such theoretical generalizability: formerly incarcerated entrepreneurs in the US earn more both during self-employment (Hwang and Phillips, 2023) and, similar to my findings, after returning to paid work (Hwang, 2021).³³ The same logic should extend beyond employment histories to other markers that prevent workers from signaling ability or bar them from standard employment. Indeed, I find similar patterns when dividing my sample by education or caste instead of employment background (Appendix Figure A.14), and the premium for entrepreneurs from non-salaried backgrounds is strongest among those in their district’s numerical minority castes, a position that likely carries stigma of its own. Collectively, these findings suggest that (particularly successful) entrepreneurship may provide upward mobility to a broad variety of disadvantaged groups, whether their disadvantage stems from stigma or from lacking the signals that grant access

³³In contrast, Rocha and Sunao (2026) study ex-offenders in Denmark who face far weaker stigma, having served their convictions through community service rather than incarceration. In that setting, entrepreneurship carries a penalty compared to regular employment. In other words, when standard employment remains within reach—potentially due to the stigma from their convictions here being light enough—the entrepreneurial advantage disappears. This pattern is consistent with my paper’s broader argument that what entrepreneurship offers depends on the employment alternatives workers actually hold.

to standard employment. In line with Rider et al. (2023), both forms of disadvantage constitute labor market frictions, which they propose render entrepreneurship more likely to produce upward mobility. My findings are consistent with that proposition and extend it to post-entrepreneurship outcomes.

The findings also distinguish entrepreneurship from the precarious work it is often grouped with. Prior work documents similarities between self-employment and various forms of non-standard work, including gig work, casual employment, and even unemployment (Burtch, Carnahan, and Greenwood, 2018; Donovan et al., 2023; Maiti, 2013). Especially in low-income economies, entrepreneurship is sometimes characterized as merely “disguised unemployment,” and research on the entrepreneurial penalty in the Global North draws on theories of nonstandard work, which also identify a penalty in comparison to standard work (Pedulla, 2016). My findings suggest self-employment is not interchangeable with these arrangements. Unlike casual work, a successful spell generates observable, attributable evidence of ability, and a base from which to search, giving self-employment a potential for upward mobility that other precarious work lacks. This prompts the question of how far characterizations of disguised unemployment should reach.

Empirically, this study extends the careers perspective on entrepreneurship to emerging markets, where the majority of global entrepreneurs operate. Research taking this perspective has so far remained confined to high-income economies. The entrepreneurs of emerging economies have instead been studied by a broader literature spanning entrepreneurship research and development economics. Because entry into self-employment in these settings is often driven by limited alternatives, this research has focused on entry into, and performance during, entrepreneurship (e.g. Bandiera et al., 2017; Carlson, 2023; Donovan et al., 2023). This study extends this work to what happens after entrepreneurship ends.

This extension also complements experimental work in development economics. While prior research has examined the long-term consequences of livestock-based asset-transfer programs among the ultra-poor (Balboni et al., 2021; Banerjee et al., 2021), my study extends

beyond both rural, ultra-poor populations and livestock-based ventures. The asset-transfer literature shows increased earnings, consumption, and even employment for households randomly assigned livestock. These gains are driven by profits relaxing financial constraints in rural settings. In contrast, I show that entrepreneurial success can itself enable transitions into salaried employment for those in urban areas, whether by generating signals of ability valued by prospective employers or by providing a more stable base from which to search. These advantages arise without grants or policy programs and emerge even when entrepreneurs bear the costs of their ventures themselves. By demonstrating when self-employment generates long-term advantages regardless of entry motivations, these findings shed new light on its role for the vast majority of entrepreneurs globally.

While India merits study as the world’s most populous country, several factors suggest these patterns may extend to other contexts where workers face barriers to standard employment. Most directly, India’s labor market dynamics parallel those of many emerging economies where formal employment remains limited (ILO, 2016). But these dynamics are not confined to the Global South. Similar casual arrangements exist in high-income economies, where so-called nonstandard employment remains less common, but is growing (Kalleberg, 2009; Pedulla, 2016). While casual employment in India represents a more precarious case of short-term work, the dynamics documented here should extend (if in attenuated form) to nonstandard workers in these economies. Indeed, the prominence of necessity entrepreneurship research in Western contexts demonstrates that poor employment alternatives drive many into self-employment across all economies (Nikiforou, Dencker, and Gruber, 2019; O’Donnell, Leger, O’Gorman, and Clinton, 2024). Nevertheless, future research should examine economies and workers at varying levels of precarity to establish the breadth of these mechanisms and understand when, and for whom, entrepreneurship offers a pathway to upward mobility across the globe.

Important questions remain. First, this paper purposely theorizes about ability abstractly to distinguish it from other classes of mechanisms, such as commitment and fit, and to provide

evidence concordant with the level of detail in the data. However, at a more granular level, it remains unclear what exactly the ability signal contains and whether employers read it as signaling specific skills. This closely relates to the question of whether entrepreneurship makes visible an underlying, innate form of ability or helps develop specific skills. Both of these questions are left for future research to tease apart. Second, while supply-side dynamics can reconcile the premiums for successful entrepreneurs with the penalties in prior work, my design cannot separate what a spell signals to employers from what its success lets entrepreneurs hold out for. Nor can I rule out that the signal itself has turned positive for the previously salaried in my setting. Third, my data only offer limited evidence on the question of which casual workers are likely to sustain spells long enough for mobility in the first place. Future research could aim to collect more fine-grained data that help distinguish which spells are closer to disguised unemployment or seasonal self-employment never meant to scale to a full business. These could help identify individual or structural characteristics that predict entry into the successful spells able to generate upward mobility.

Overall, the value of entrepreneurship depends fundamentally on an individual's alternatives. Both realities coexist: entrepreneurship may represent the most promising path forward for someone engaged in precarious, casual work while simultaneously being inferior to stable salaried employment. The contribution of this paper lies in recognizing that entrepreneurship research has long examined only the latter comparison while overlooking the former. For the millions of workers globally whose alternatives consist of casual work, whether in emerging economies or increasingly in the polarized labor markets of the Global North, entrepreneurship may function not as a risky departure from stability but as a pathway to creating it. Successful entrepreneurs from casual backgrounds demonstrate ability through business performance, generating signals valued in standard employment—and gain a base from which to hold out for exactly such work. This reconceptualization positions entrepreneurship as a mobility mechanism for the substantial share of workers who lack access to standard employment.

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Tables and Figures

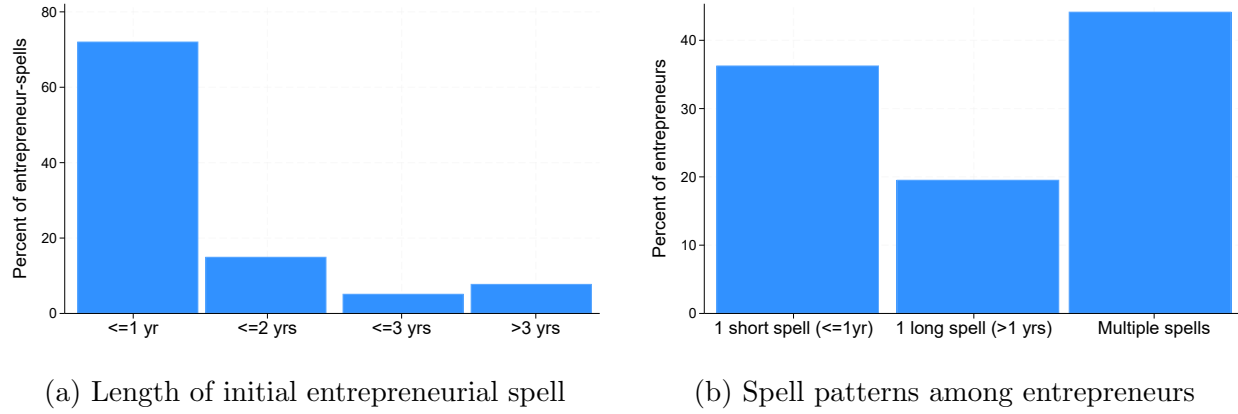


Figure 1: Length and re-entry patterns

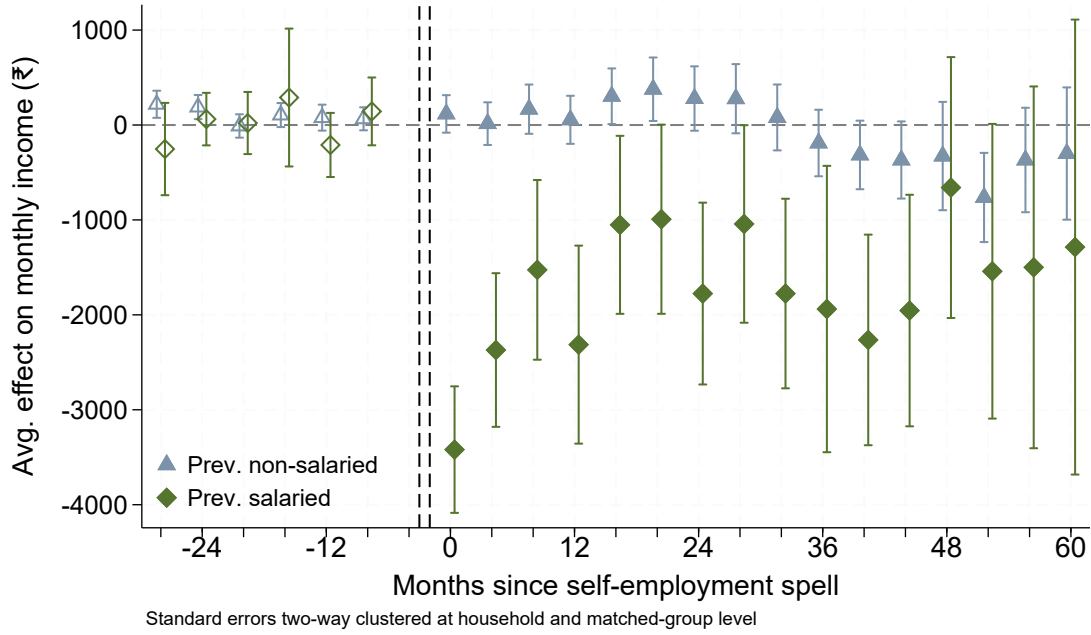


Figure 2: Earnings trajectories of entrepreneurs returning to paid employment by pre-treatment employment arrangement. The left of the vertical, dashed lines indicates time periods before entrepreneurship. The right indicates time periods after entrepreneurship. Time in entrepreneurship itself is removed from the estimation.

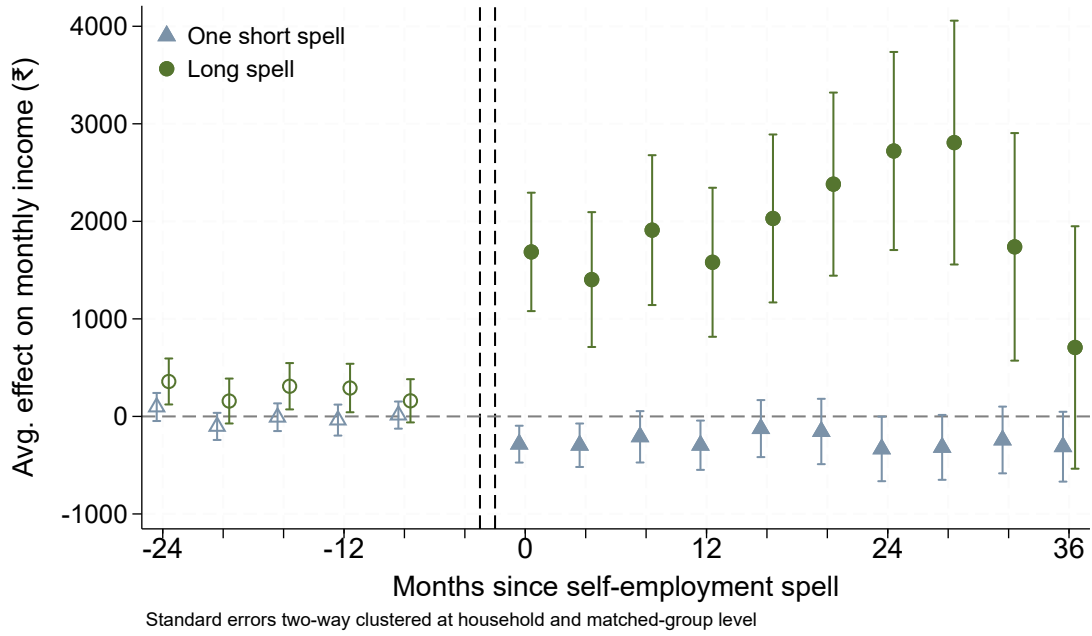


Figure 3: Earnings trajectories of formerly non-salaried entrepreneurs by spell length.

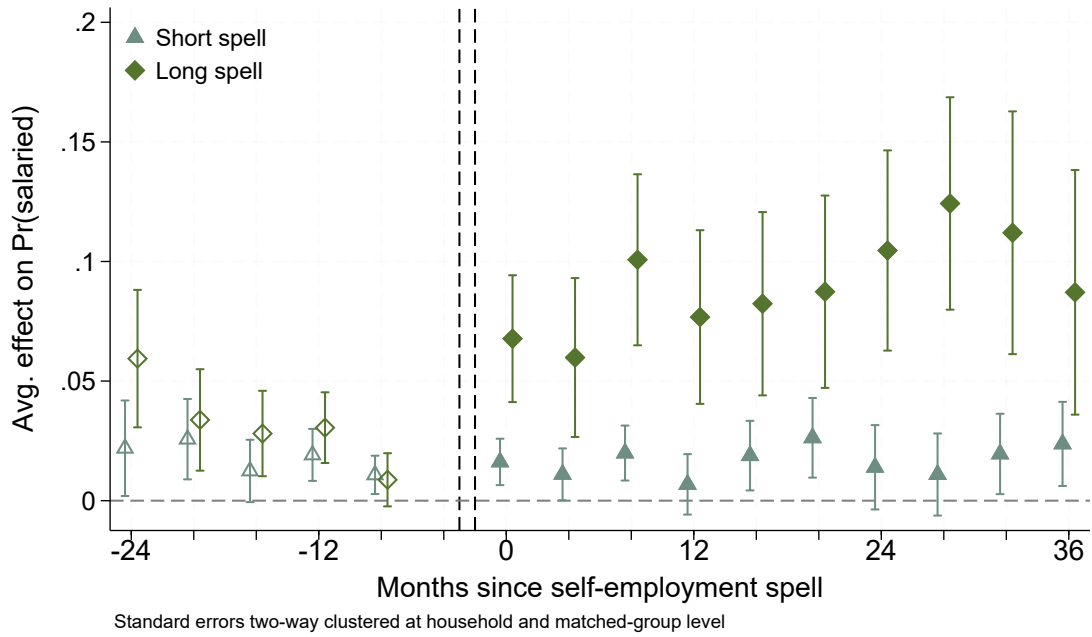


Figure 4: Likelihood of working in salaried employment for formerly non-salaried entrepreneurs

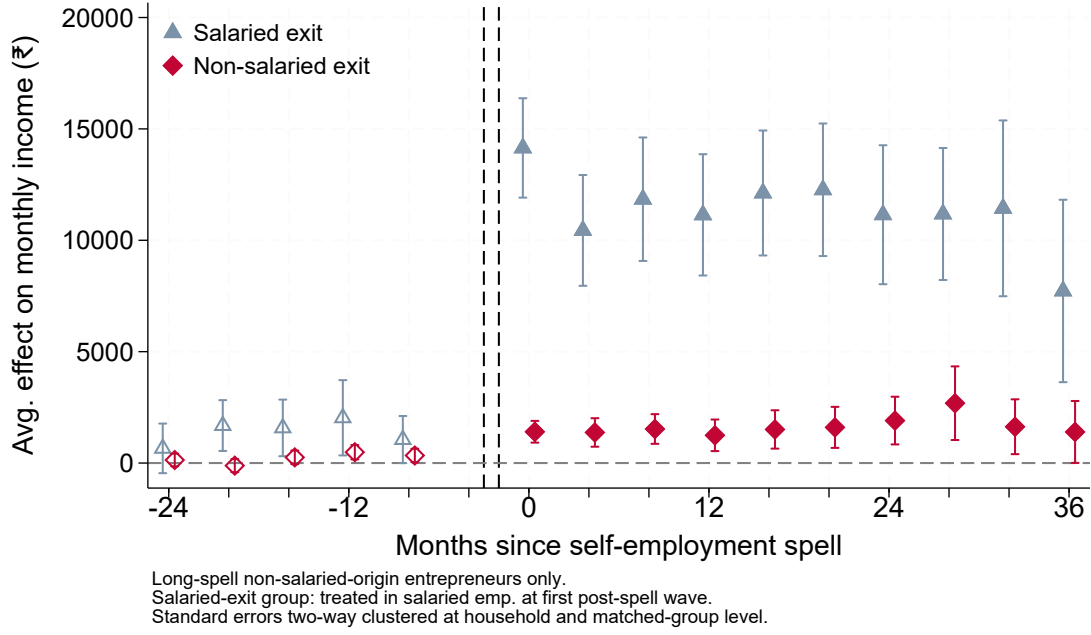


Figure 5: Earnings trajectories of formerly non-salaried entrepreneurs with a long spell by post-entrepreneurship employment arrangement (conditional on being employed; the non-employed are shown separately in Appendix Figure A.20).

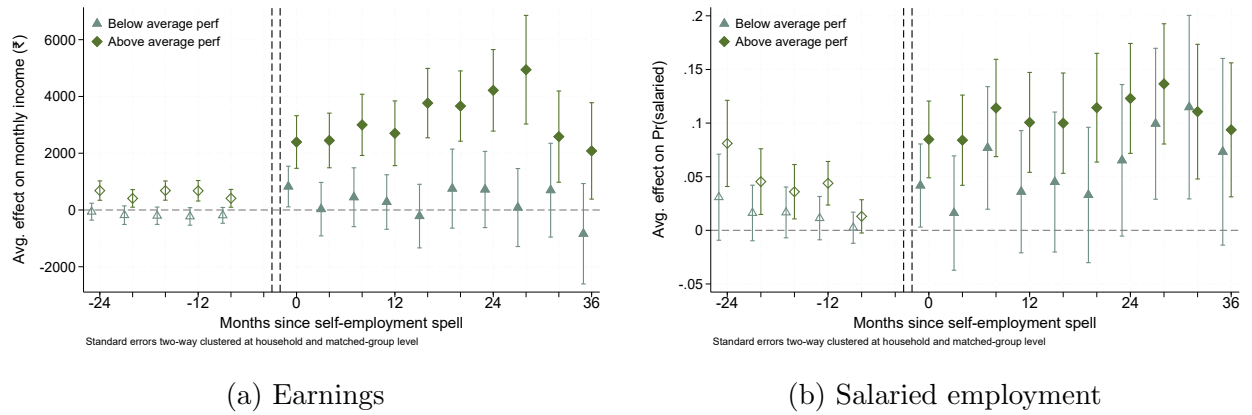
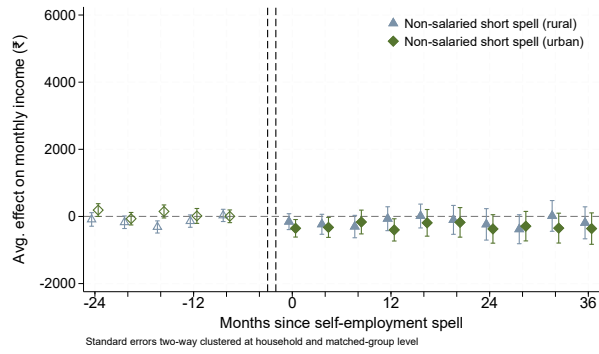
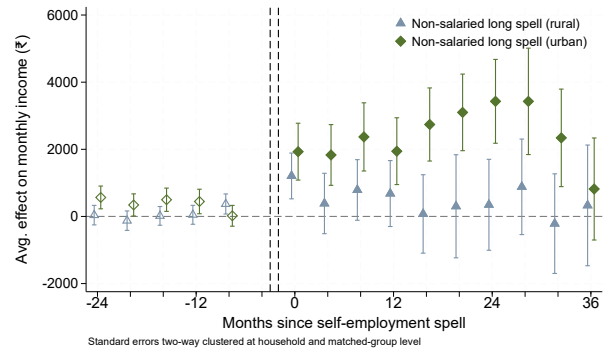


Figure 6: Career trajectories of formerly non-salaried entrepreneurs *with a long spell* by performance during self-employment.



(a) Short spell



(b) Long spell

Figure 7: Earnings trajectories for previously non-salaried entrepreneurs by spell length and urban vs rural status.

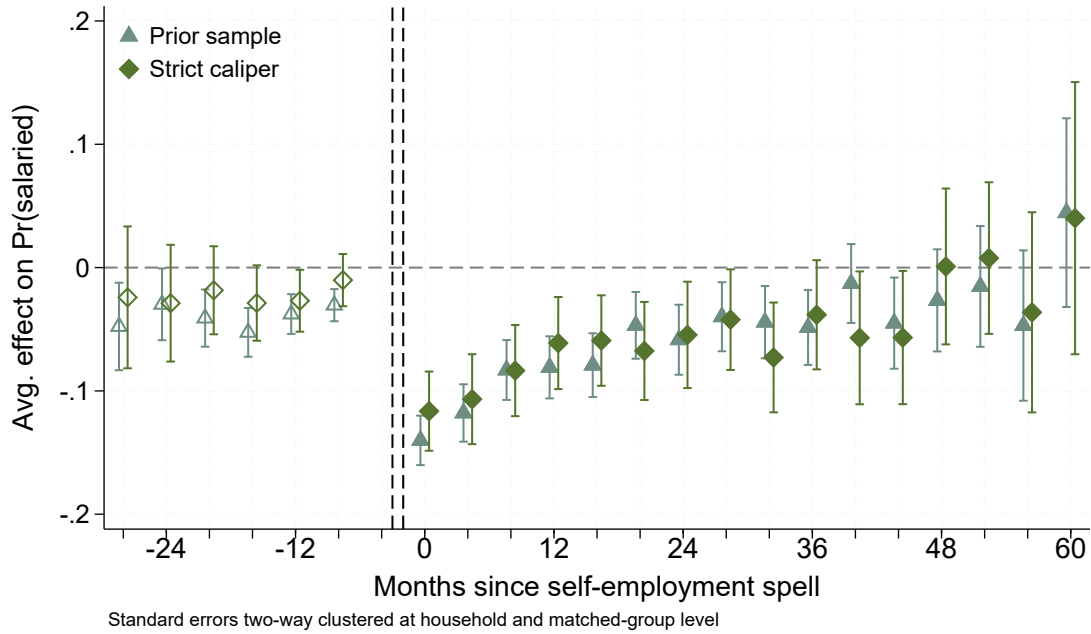


Figure 8: Likelihood of working in salaried employment for formerly salaried entrepreneurs. Strict caliper estimates are based on a sample using control cases whose earnings histories are within a Mahalanobis distance $D_M \leq 1$ of treated cases.

Table 1: Matching summary statistics

	Pre-matching				Post-matching			
	Never ENT		Entrepreneurs		Control cases		Entrepreneurs	
	mean	sd	mean	sd	mean	sd	mean	sd
6 wave avg income (1k Rs)	7.93	11.47	12.46	11.24	11.96	9.98	11.56	10.07
3 wave avg income (1k Rs)	8.29	12.51	13.61	11.94	13.21	11.02	12.86	11.09
6 wave inc. slope (pct)	2.42	159.54	13.40	27.01	11.36	20.83	14.08	26.44
3 wave inc. slope (pct)	6.66	50.12	24.10	48.46	24.13	41.98	26.68	47.01
Woman	0.48	0.50	0.09	0.28	0.08	0.27	0.08	0.27
Age	37.86	12.72	38.34	11.00	37.44	11.03	37.83	10.98
<i>Education</i>								
<5th grade	0.19	0.40	0.14	0.34	0.13	0.34	0.14	0.35
5th-9th grade	0.30	0.46	0.36	0.48	0.40	0.49	0.39	0.49
10th-11th grade	0.14	0.35	0.20	0.40	0.19	0.39	0.20	0.40
High school	0.14	0.35	0.13	0.34	0.12	0.33	0.13	0.33
College	0.22	0.42	0.17	0.37	0.15	0.36	0.15	0.35
Hindu	0.88	0.33	0.85	0.36	0.88	0.32	0.85	0.36
Upper/middle caste	0.27	0.44	0.26	0.44	0.21	0.41	0.26	0.44
Household size	2.73	1.72	2.86	1.75	2.95	1.77	2.92	1.76
Employed	0.53	0.50	0.92	0.27	0.95	0.21	0.92	0.27
Salaried	0.18	0.38	0.25	0.43	0.27	0.45	0.25	0.43
Other SE in household	0.24	0.43	0.15	0.36	0.10	0.30	0.16	0.36
<i>Household savings types</i>								
in fixed deposits	0.66	0.47	0.66	0.47	0.67	0.47	0.68	0.47
in life insurance	0.53	0.50	0.52	0.50	0.47	0.50	0.49	0.50
in business	0.02	0.15	0.01	0.11	0.01	0.09	0.01	0.10
in gold	0.98	0.14	0.97	0.18	0.96	0.19	0.97	0.18
in real estate	0.98	0.14	0.99	0.10	0.98	0.14	0.99	0.11
in other medium	0.43	0.50	0.39	0.49	0.38	0.49	0.36	0.48
<i>Household assets</i>								
Owns cattle	0.13	0.33	0.14	0.35	0.12	0.32	0.14	0.35
Owns air conditioner	0.10	0.30	0.07	0.26	0.06	0.23	0.06	0.24
Owns fridge	0.63	0.48	0.60	0.49	0.57	0.50	0.59	0.49
Owns two-wheeler	0.68	0.47	0.67	0.47	0.62	0.48	0.66	0.47
Owns tractor	0.01	0.08	0.01	0.08	0.00	0.05	0.01	0.07
Owns computer	0.13	0.33	0.08	0.28	0.08	0.28	0.07	0.26
Owns car	0.10	0.30	0.07	0.25	0.06	0.23	0.06	0.23
<i>Household expenditures</i>								
Total (1k Rs)	12.68	7.99	11.93	6.89	11.77	6.92	11.64	6.83
Total, adjusted (1k Rs)	13.39	9.41	12.57	6.84	12.43	7.05	12.25	6.71
EMIs (1k Rs)	0.42	1.69	0.32	1.58	0.33	1.35	0.28	1.57
non-vehicle/house EMIs (1k Rs)	0.13	0.64	0.10	0.53	0.10	0.49	0.09	0.49
N	130,620		29,593		76,267		21,701	

Variables measured two waves before entry. Income average and slope based on waves leading up to entry.

The sample of never-entrepreneurs pre-matching is a random sample, with the distribution across waves mirroring that of entrepreneurial entry (i.e. there is more entrepreneurial entry in early 2018, so there are proportionally more observations from 2018 for the never-treated as well).

Table 2: OLS estimations predicting monthly earnings for formerly non-salaried entrepreneurs with a longer spell – by return industry, other family entrepreneurs, age, and majority caste membership.

	(1)	(2)	(3)	(4)
Post-entrepreneurship	3,472*** (353)	2,723*** (389)	2,484*** (418)	1,685*** (368)
Post-ENT X stay in industry	-2,050** (677)			
Post-ENT X family SE		-2,238*** (646)		
Post-ENT X age <35			-1,513* (626)	
Reg. majority caste member				56 (176)
Post-ENT X majority caste				-1,532* (664)
Constant	10,236*** (50)	9,788*** (49)	9,788*** (49)	10,402*** (60)
Obs	58,085	67,375	67,375	64,984
N entrepreneurs	1,289	1,513	1,513	1,432
R2	0.55	0.56	0.56	0.61
Match group X surv. wave FEs	Yes	Yes	Yes	Yes

Standard errors clustered by matched group and household. * $p < 0.05$, ** $p < 0.01$; *** $p < 0.001$.

Main effects of the moderating variables are subsumed by the fixed effect.

Models are estimated following the main matching procedure described above.

Appendices

A Appendix Tables and Figures

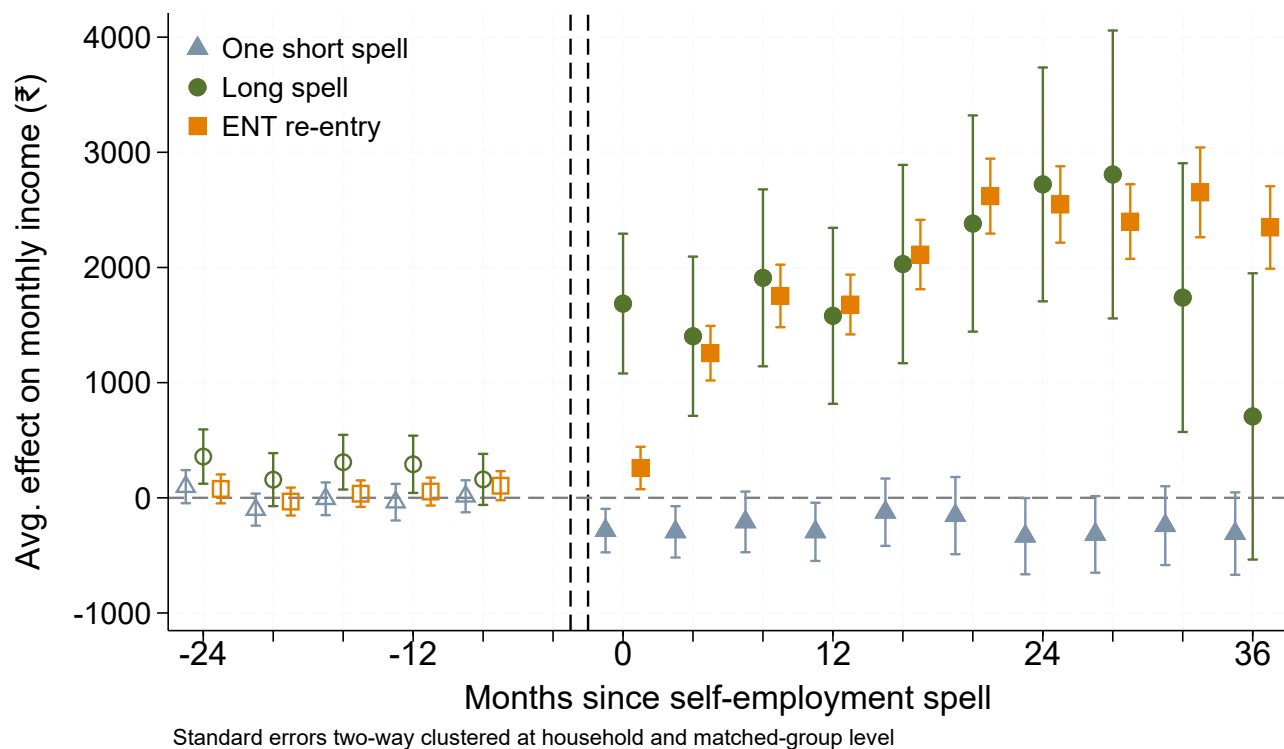


Figure A.1: Earnings trajectories of formerly non-salaried entrepreneurs by spell length and repeated entry into entrepreneurship.

The ENT re-entry category refers to entrepreneurs who eventually return to self-employment. These observations have *not* been included in the estimation for Figure 2.

Table A.1: Full sample summary statistics

	Prev. non-salaried				Prev. salaried			
	Control cases		Entrepreneurs		Control cases		Entrepreneurs	
	mean	sd	mean	sd	mean	sd	mean	sd
Income (Rs 1k)	8.34	6.82	9.52	9.79	20.83	17.40	18.61	19.21
Woman	0.09	0.29	0.09	0.29	0.06	0.23	0.07	0.25
Age	37.44	11.62	38.16	11.67	39.96	11.14	40.51	11.12
<i>Education</i>								
<5th grade	0.18	0.39	0.17	0.38	0.03	0.17	0.05	0.22
5th-9th grade	0.48	0.50	0.42	0.49	0.16	0.37	0.21	0.41
10th-11th grade	0.18	0.39	0.21	0.41	0.21	0.41	0.26	0.44
High school	0.09	0.29	0.12	0.32	0.20	0.40	0.22	0.41
College	0.06	0.24	0.08	0.27	0.39	0.49	0.27	0.44
Hindu	0.88	0.33	0.84	0.36	0.89	0.31	0.85	0.35
Upper/middle caste	0.14	0.34	0.21	0.40	0.38	0.49	0.37	0.48
Household size	2.91	1.77	2.82	1.75	2.55	1.67	2.60	1.68
Employed	0.88	0.32	0.88	0.33	0.90	0.30	0.90	0.30
Salaried	0.08	0.27	0.06	0.24	0.83	0.38	0.45	0.50
Other SE in household	0.10	0.30	0.19	0.39	0.10	0.30	0.16	0.36
<i>Household savings types</i>								
in fixed deposits	0.50	0.50	0.53	0.50	0.72	0.45	0.67	0.47
in life insurance	0.35	0.48	0.41	0.49	0.77	0.42	0.69	0.46
in business	0.01	0.10	0.04	0.19	0.02	0.14	0.05	0.22
in gold	0.94	0.24	0.95	0.22	0.97	0.17	0.97	0.17
in real estate	0.99	0.10	0.99	0.11	0.98	0.12	0.99	0.10
in other medium	0.28	0.45	0.30	0.46	0.70	0.46	0.56	0.50
<i>Household assets</i>								
Owns house	0.99	0.09	0.99	0.11	0.98	0.14	0.99	0.11
Owns cattle	0.13	0.34	0.17	0.38	0.06	0.24	0.11	0.31
Owns air conditioner	0.02	0.14	0.02	0.15	0.19	0.39	0.14	0.34
Owns fridge	0.49	0.50	0.54	0.50	0.86	0.35	0.82	0.39
Owns two-wheeler	0.58	0.49	0.65	0.48	0.80	0.40	0.82	0.39
Owns tractor	0.00	0.05	0.01	0.09	0.01	0.08	0.01	0.12
Owns computer	0.02	0.14	0.03	0.16	0.21	0.40	0.15	0.36
Owns car	0.01	0.10	0.02	0.13	0.17	0.38	0.14	0.35
<i>Household expenditures</i>								
Total (1k Rs)	10.32	15.16	10.52	4.96	15.45	17.23	14.23	8.63
Total, adjusted (1k Rs)	11.07	15.27	11.33	5.27	16.17	17.34	15.01	8.58
EMIs (1k Rs)	0.24	1.01	0.24	1.00	0.70	2.39	0.46	1.85
non-vehicle/house EMIs (1k Rs)	0.08	0.41	0.09	0.46	0.12	0.72	0.12	0.72
N	1,451,516		398,715		734,898		117,182	

This table presents pooled results of the different groups across all waves, i.e. including both pre- and post-entrepreneurship observations.

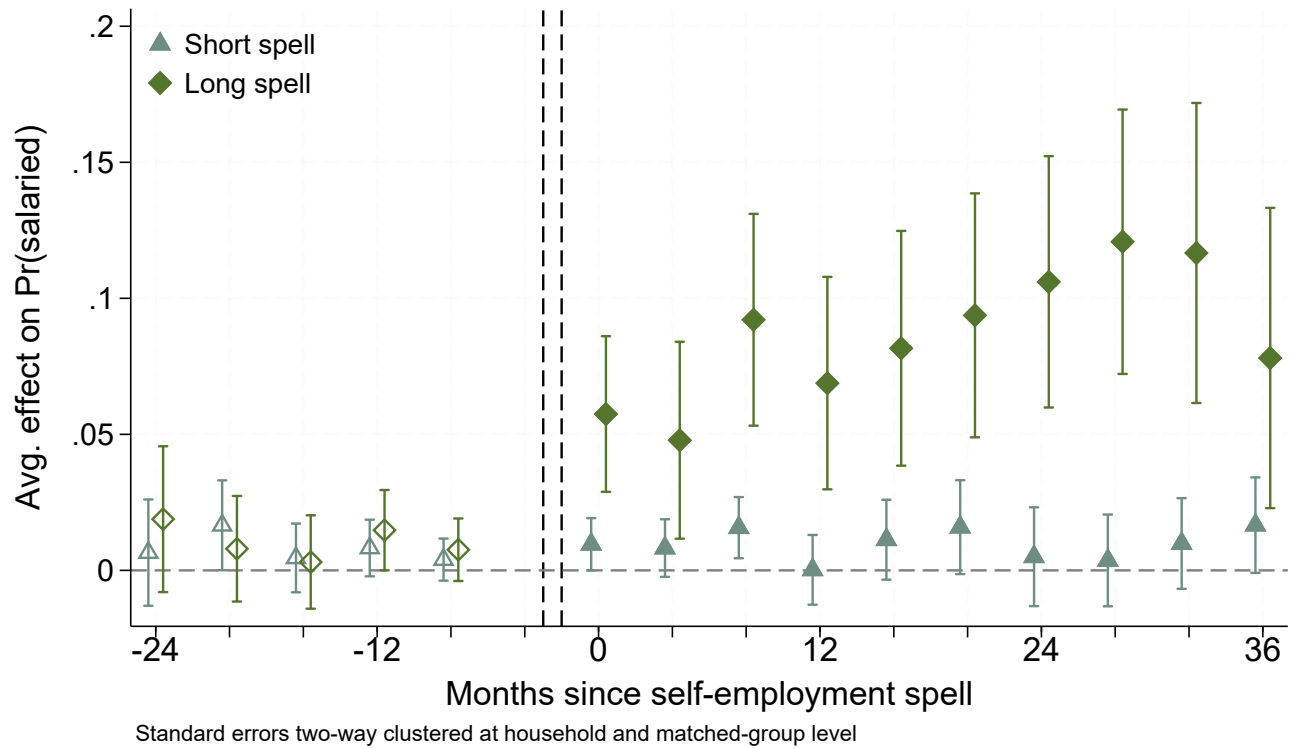
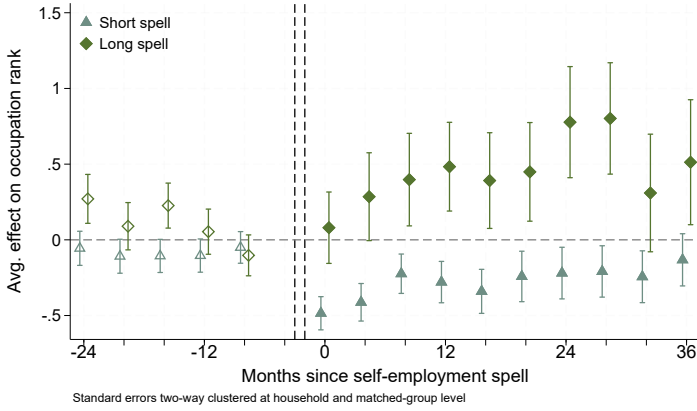
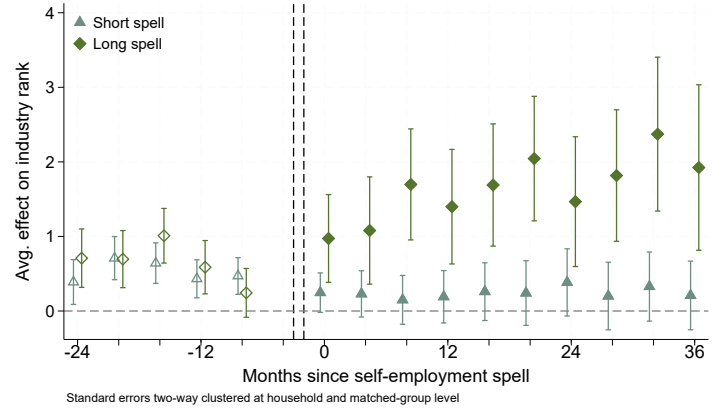


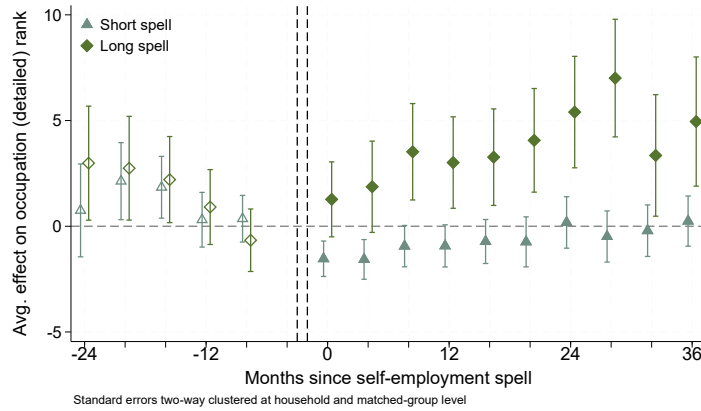
Figure A.2: Likelihood of working in salaried employment for formerly non-salaried entrepreneurs. Estimated using control cases whose earnings histories are within a Mahalanobis distance $D_M \leq 1$ of treated cases. Compare to Figure 4.



(a) Occupation – 21 levels



(b) Industry – 39 levels



(c) Occupation (detailed) – 187 levels, available starting 2018

Figure A.3: Estimating occupation and industry earning ranks for previously non-salaried entrepreneurs by spell-length.

The dependent variables are continuous variables that indicate the income rank of the respective occupation/industry. For example, panel (a) is based on the primary occupation variable reported by CMIE, *nature of occupation*, which is based on ISCO standards and includes 21 different occupations. I calculate the average monthly earnings of people who report each occupation as their primary one, and rank order them such that 1 indicates the occupation with the lowest average earnings and 21 the highest. The reported industry variable contains 39 levels. The second occupation variable is more detailed, with 187 separate occupations, but was only recorded starting in 2018, thus increasing noise.

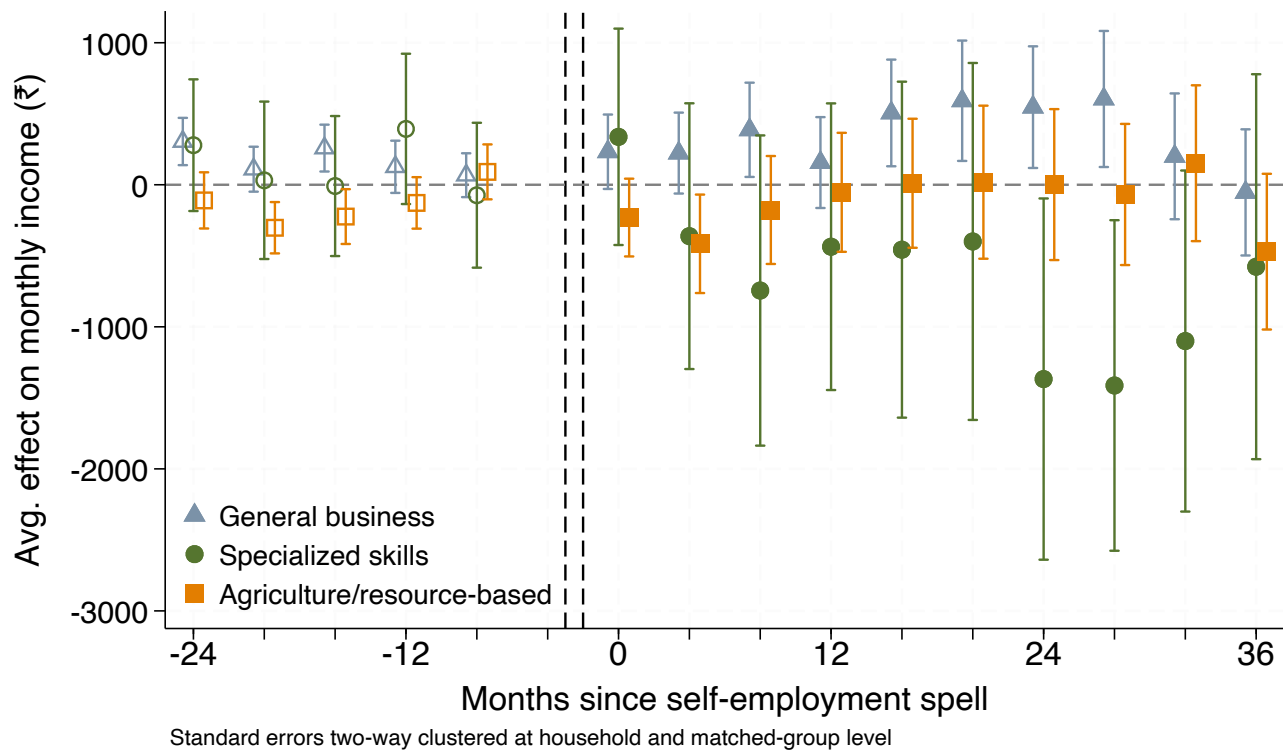


Figure A.4: Earnings trajectories for previously non-salaried entrepreneurs by venture industry classification.

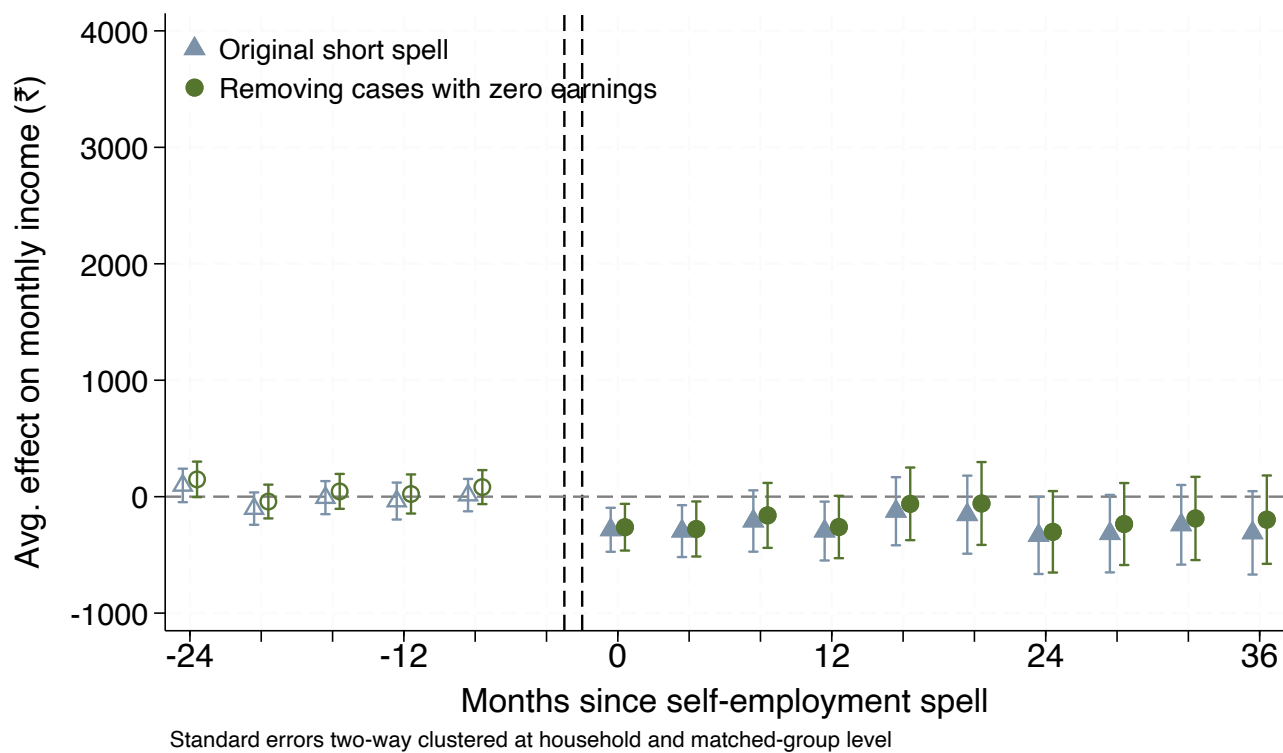


Figure A.5: Earnings trajectories for previously non-salaried entrepreneurs with a short spell – removing cases without any entrepreneurial earnings.

Table A.2: OLS estimations predicting monthly earnings for formerly non-salaried entrepreneurs with a short spell – by majority caste membership, other household members being employed, and entering entrepreneurship in a different industry from prior employment.

	(1)	(2)	(3)
Post-entrepreneurship	-288** (106)	-694*** (120)	-441** (147)
Reg. majority caste member	120 (69)		
Post-ENT X majority caste	-176 (214)		
Household member employed		-1,207*** (69)	
Post-ENT X HH employed		126 (207)	
Post-ENT X ENT in diff. industry vs pre-ENT			248 (182)
Constant	8,666*** (25)	8,775*** (42)	8,100*** (18)
Obs	285,795	307,587	263,186
N entrepreneurs	4,883	4,884	
R2	0.62	0.57	0.58
Match group X surv. wave FEs	Yes	Yes	Yes
Control cases			

Standard errors clustered by matched group and household.

* $p < 0.05$, ** $p < 0.01$; *** $p < 0.001$.

Models are estimated following the main matching procedure described above.

The main effect of Entrepreneurship in a different industry is colinear with the fixed effects and drops out.

Table A.3: OLS estimations predicting monthly earnings for formerly non-salaried entrepreneurs with a short spell – comparing entrepreneurs to control cases who switch vs stay in their industry or occupation of employment

	(1)	(2)	(3)	(4)
Post-entrepreneurship	-112 (107)	-684*** (106)	-22 (131)	-615*** (99)
Constant	7,986*** (23)	8,287*** (23)	7,493*** (32)	8,328*** (20)
Obs	138,275	134,101	79,178	164,756
N entrepreneurs	4,022	3,797	2,805	4,186
R2	0.63	0.63	0.67	0.62
Match group X surv. wave FEs	Yes	Yes	Yes	Yes
Control cases	New ind.	Same ind.	New occ.	Same occ.

Standard errors clustered by matched group and household. * $p < 0.05$, ** $p < 0.01$; *** $p < 0.001$.
Models are estimated following the main matching procedure described above.

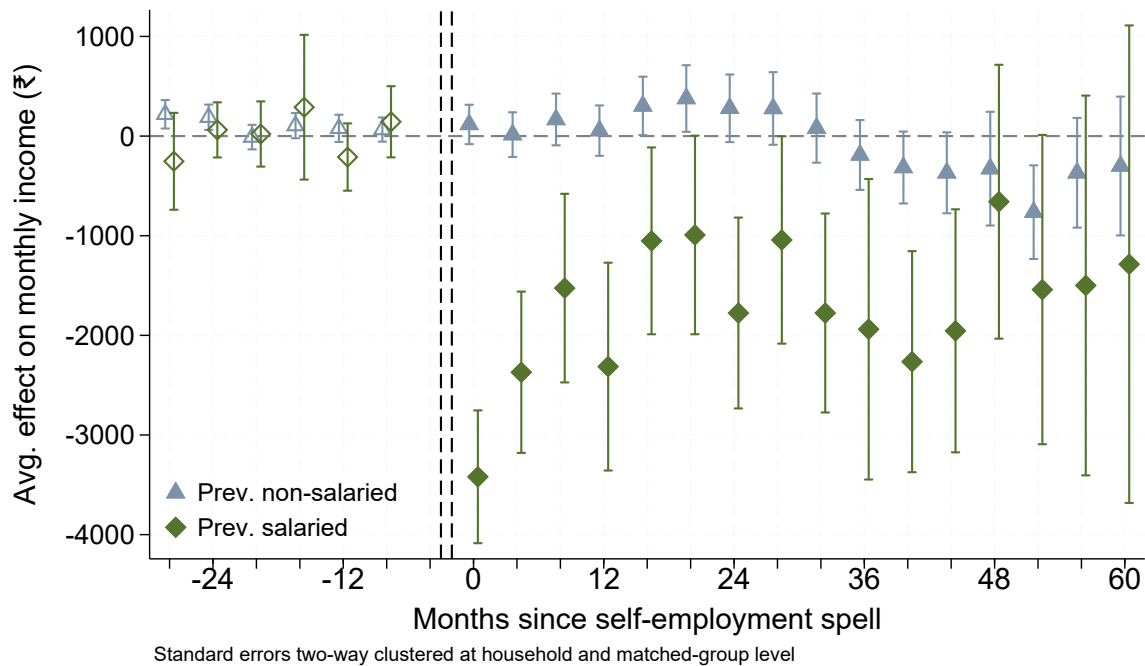
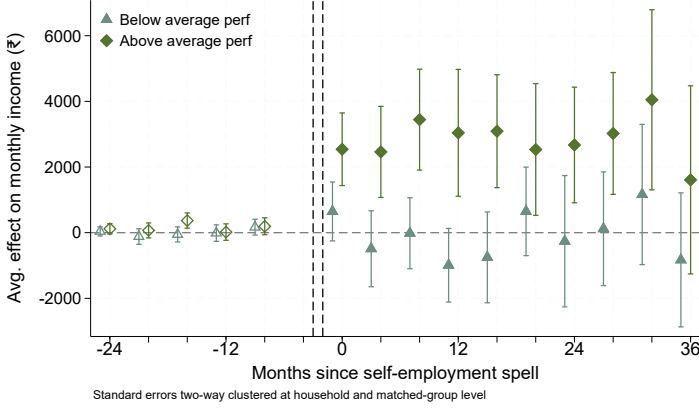
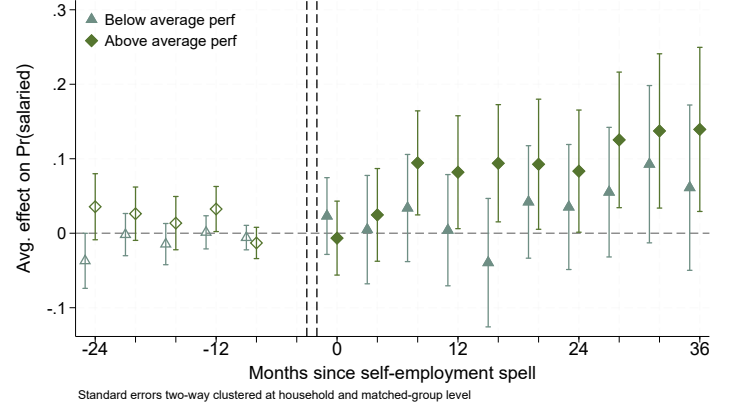


Figure A.6: Earnings trajectories of entrepreneurs returning to paid employment by pre-treatment employment arrangement. Estimated following Borusyak et al. (2024). Compare to Figure 2



(a) Earnings



(b) Salaried employment

Figure A.7: Career trajectories of formerly non-salaried entrepreneurs *with a long spell* by performance during self-employment. Estimated using control cases whose earnings histories are within a Mahalanobis distance $D_M \leq 1$ of treated cases. Compare to Figure 6.

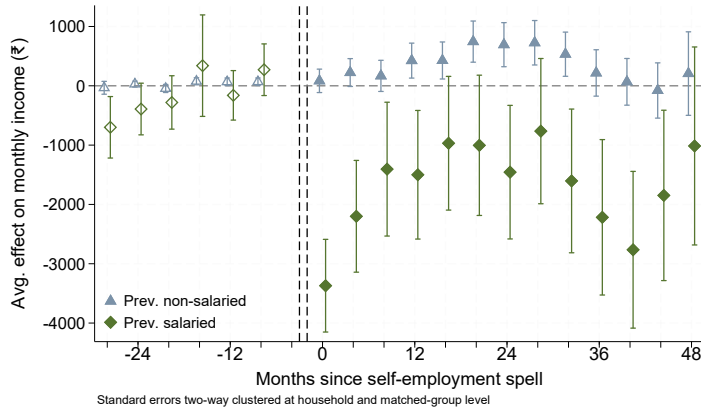
Table A.4: OLS estimations predicting monthly earnings for formerly non-salaried entrepreneurs with a long spell — “treated”/former entrepreneurs only. Predicting post-exit income based on earnings characteristics of the entrepreneurial spell.

	(1)
Rising trajectory	208 (1,109)
Avg. spell earnings (1k Rs)	144** (56)
Constant	10,827*** (1,102)
Obs	8,057
Sample	All long-spell
R ²	0.67
CEM strata + survey wave FEs	Yes

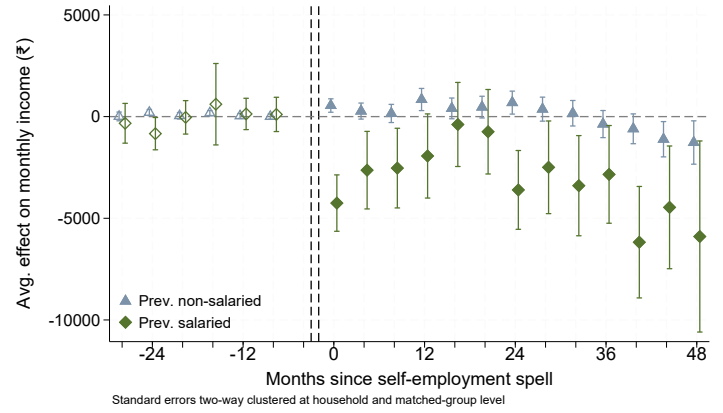
Standard errors two-way clustered by household and individual. * $p < 0.05$, ** $p < 0.01$; *** $p < 0.001$.

Sample restricted to treated (entrepreneurial) units, post-exit observations only.

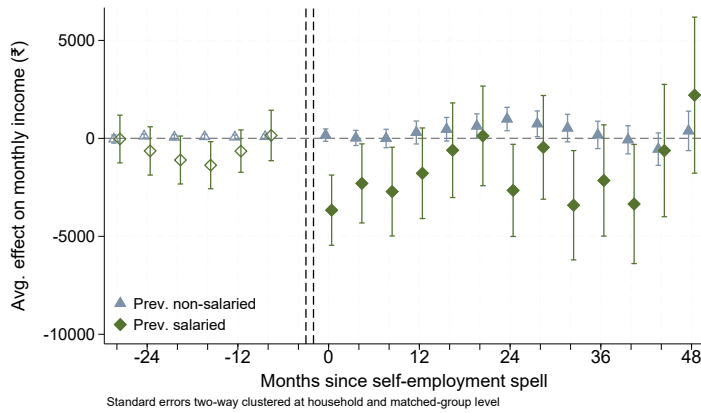
CEM strata absorb all matching covariates.



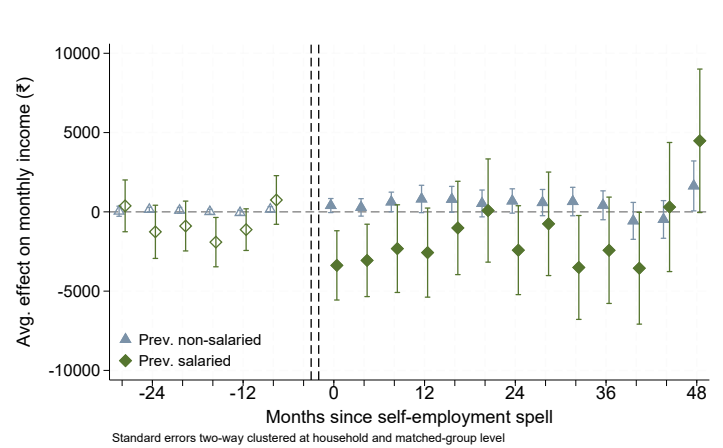
(a) Matching on longer history of employment arrangement



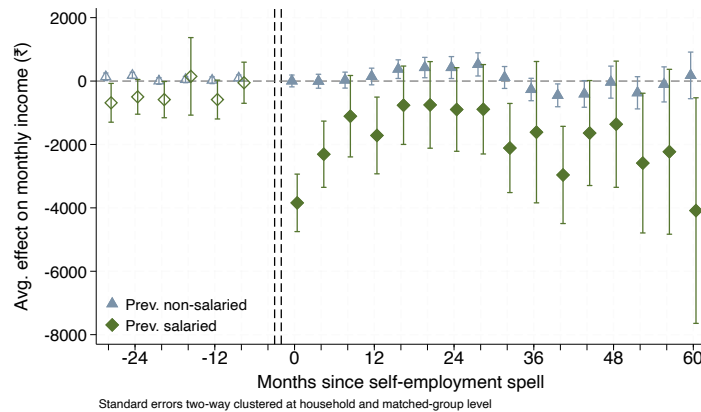
(b) Additionally conditioning on longer, consistent employment in (non-)salaried work



(c) Additionally matching on religion, caste, other household self-employment, three waves of employment type, and three waves of occupation

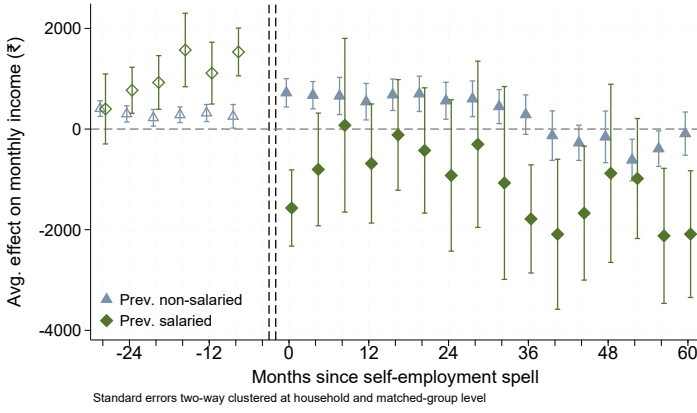


(d) Additionally matching on household savings, home ownership, and cattle ownership

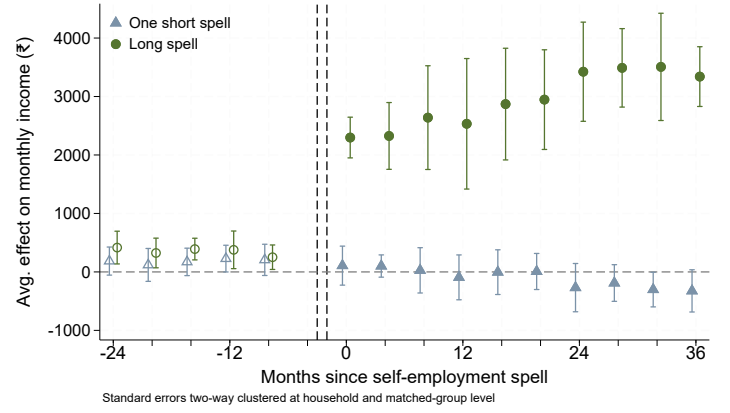


(e) Matching on industry and occupation

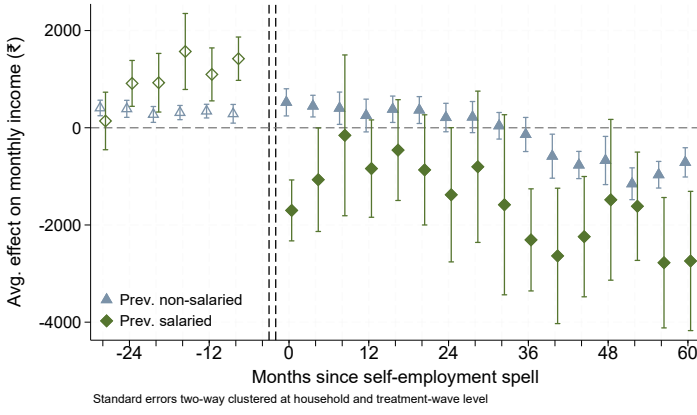
Figure A.8: Alternative matching criteria. Compare to Figure 2.



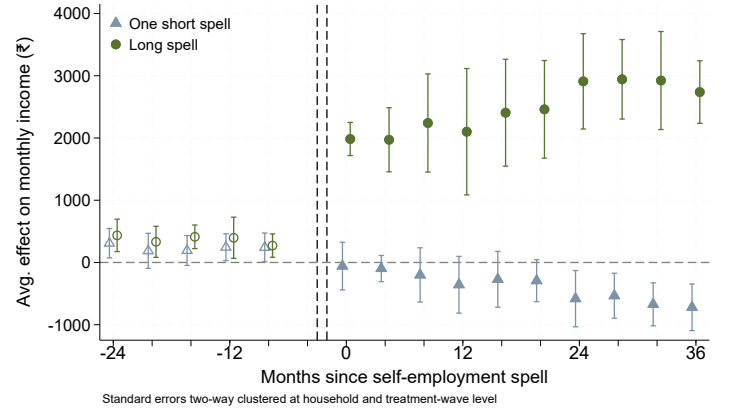
(a) By pre-treatment employment arrangement, balancing on first moment.



(b) Previously non-salaried by spell length, balancing on first moment.

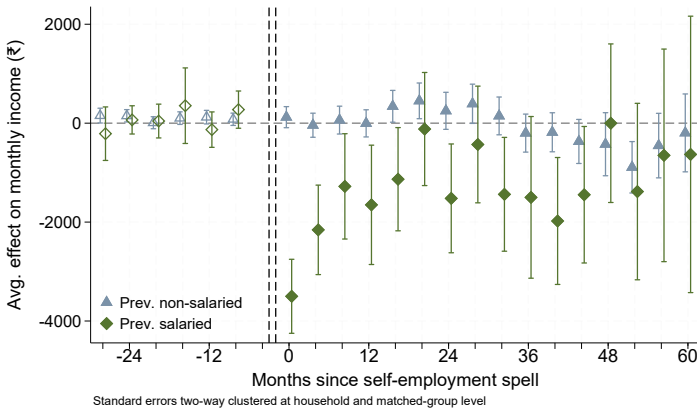


(c) By pre-treatment employment arrangement, balancing on second moment.

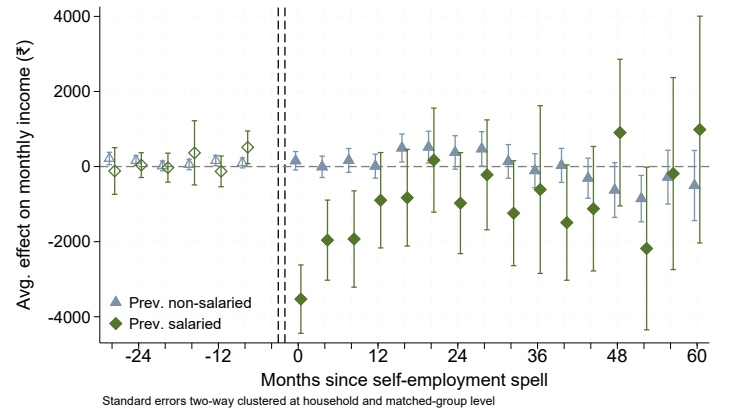


(d) Previously non-salaried by spell length, balancing on second moment.

Figure A.9: Earnings trajectories following an entropy balancing approach instead of matching. Compare to Figures 2 and 3. Balancing variables include same set as in main matching specification.



(a) 2 control cases



(b) 1 control case

Figure A.10: Fewer control cases. Compare to Figure 2.

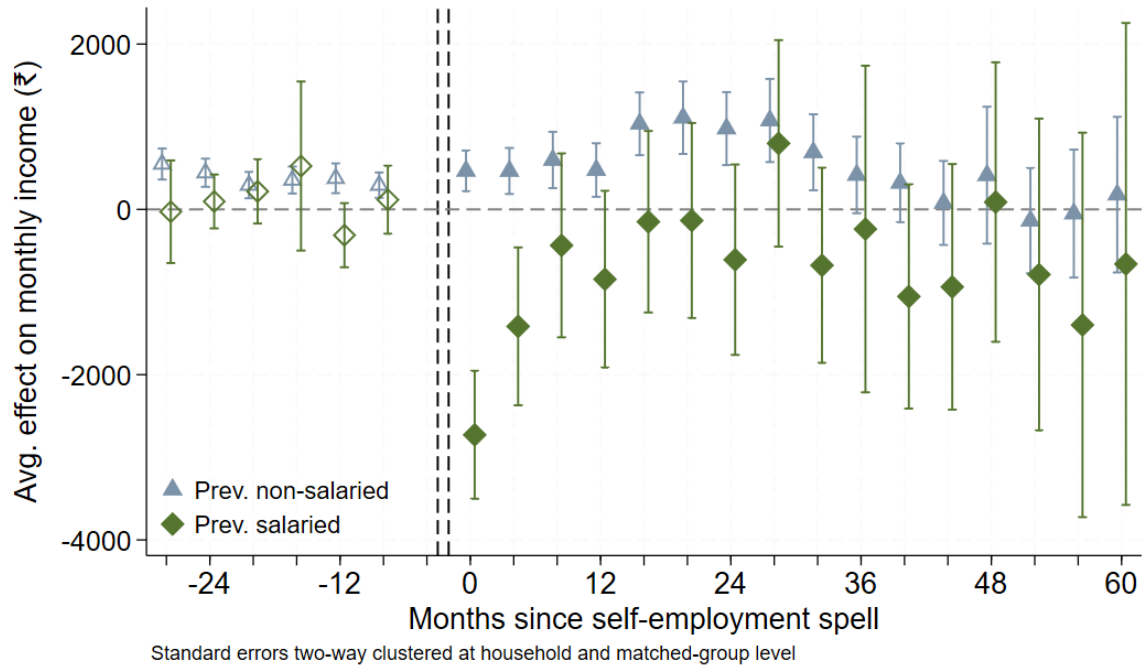
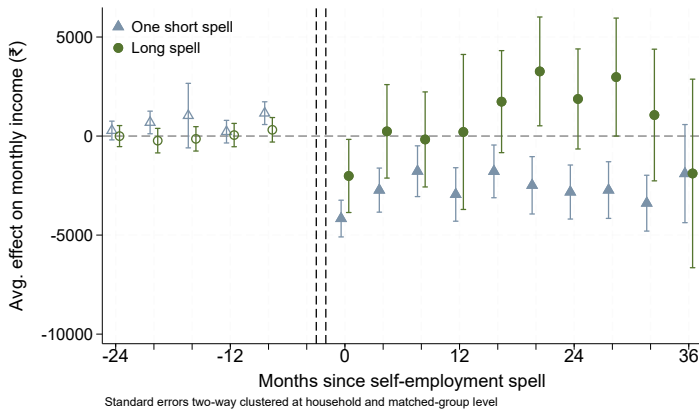
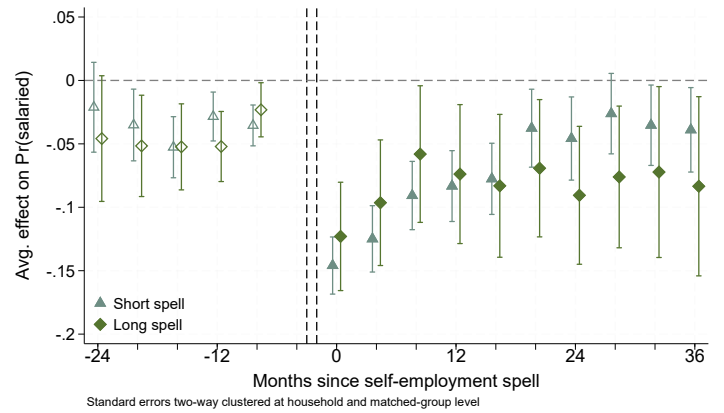


Figure A.11: Earnings trajectories of entrepreneurs returning to paid employment by pre-treatment employment arrangement removing all cases who report a second household member entering self-employment. Compare to Figure 2

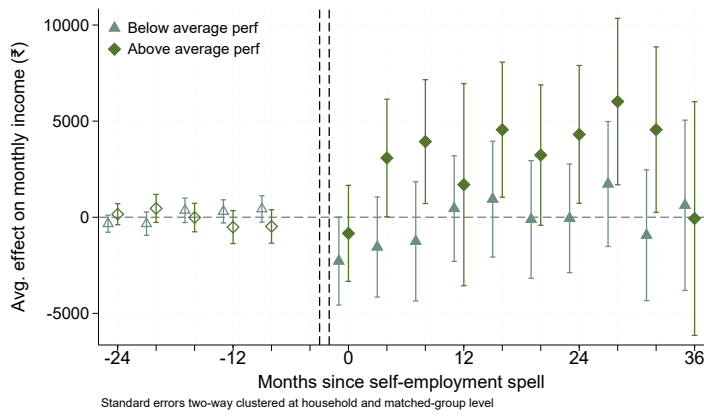


(a) Earnings trajectories

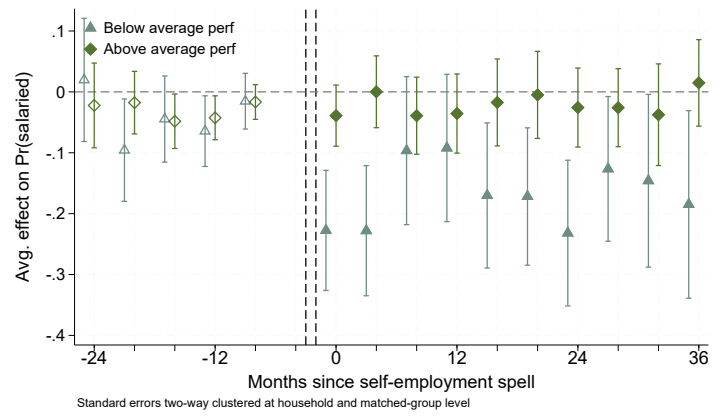


(b) Likelihood of working in salaried employment

Figure A.12: Earnings trajectories and likelihood of working in salaried employment for previously salaried entrepreneurs by spell length

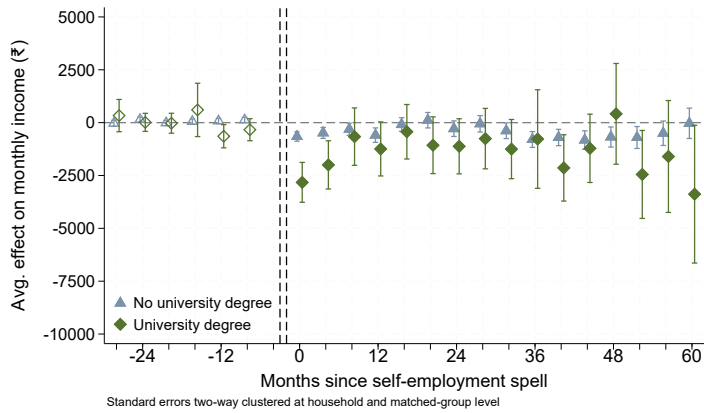


(a) Earnings trajectories

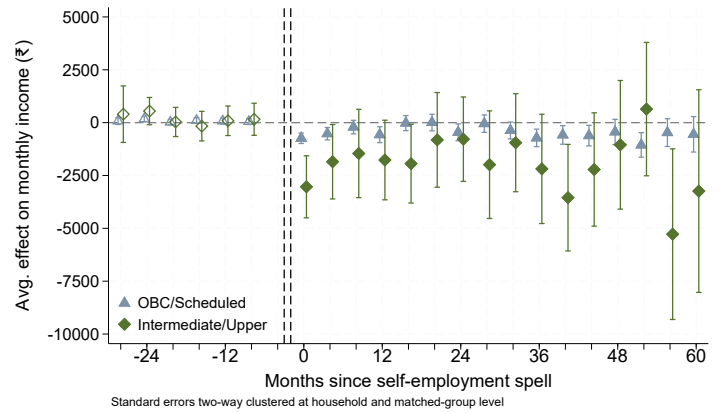


(b) Likelihood of working in salaried employment

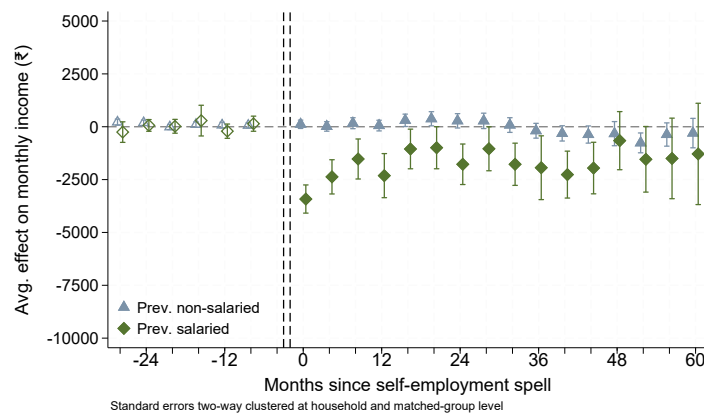
Figure A.13: Earnings trajectories and likelihood of working in salaried employment for previously salaried entrepreneurs with a long spell by performance in entrepreneurship



(a) By education



(b) By caste



(c) By salaried (same as Figure 2, Y-axis re-scaled to ease comparison to panels (a) and (b))

Figure A.14: Earnings trajectories post-entrepreneurship by education and caste – compare to Figure 2

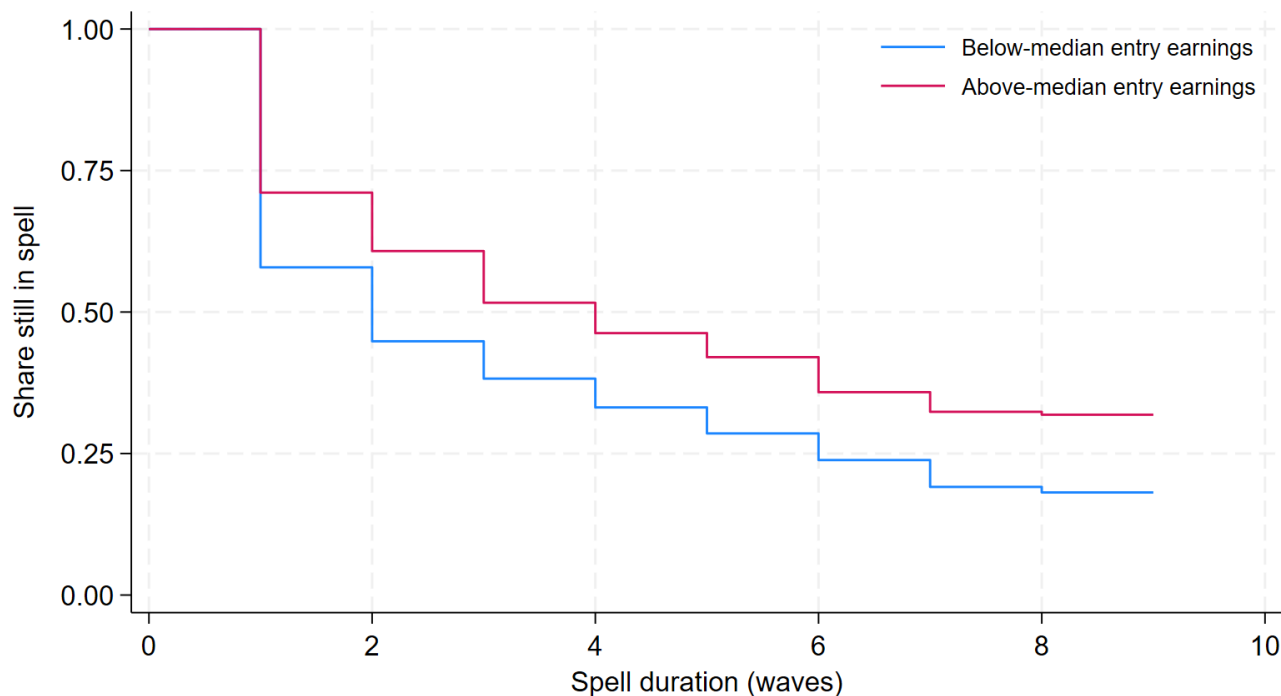
Table A.5: Discrete-time hazard estimates of exiting self-employment as a function of lagged entrepreneurial earnings, for formerly non-salaried entrepreneurs – all-cause exit and competing risks by destination (into salaried vs non-salaried employment).

	All exits		Exit → salaried		Exit → non-salaried	
	(1)	(2)	(3)	(4)	(5)	(6)
HH.ID						
Lagged earnings (1k Rs)	0.973*** (0.003)	0.987*** (0.003)	1.010*** (0.002)	1.009*** (0.002)	0.972*** (0.003)	0.983*** (0.004)
Observations	20660	20660	20573	20455	20660	20657
Demographic controls	No	Yes	No	Yes	No	Yes

Discrete-time complementary log-log; one row per at-risk spell-wave.

Reported: $\exp(\text{coef})$, the per-wave hazard ratio; a value below 1 means higher (lagged) earnings LOWER the exit hazard. All columns include i.t duration dummies (suppressed). Within each destination, the second column adds demographic controls (gender, caste, region type, state, age) as covariates. Sample: treated single-spell, non-salaried origin. Competing risks censor other-cause exits at their exit time. SE clustered by indiv.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.



Discrete-time Kaplan-Meier survival; includes only entrepreneurs from non-salaried employment backgrounds, split on above/below-median entry-wave earnings (fixed).

Figure A.15: Kaplan-Meier survival rates in self-employment by entry-wave earnings. Formerly non-salaried entrepreneurs. Each curve traces the share of entrepreneurs still in their self-employment spell by spell duration (survey waves), split by whether their entry-wave earnings are above or below the sample median.

Table A.6: Post-exit earnings relative to own spell and to matched controls, long-spell entrepreneurs from non-salaried backgrounds

Post-exit earnings vs. own spell	Post-exit earnings vs. matched control		Total
	Above	Below	
Above spell earnings	25.8	8.5	34.3
Pay cut vs. spell	27.4	38.3	65.7
Total	53.2	46.8	100.0

Cells are the share (%) of long-spell entrepreneurs from non-salaried backgrounds ($N = 1,452$); rows and columns sum to the margins. Earnings during self-employment are measured at the last spell wave, right before exit. The highlighted cell showcases those who took a pay cut relative to their own spell yet still out-earned their matched controls.

Table A.7: Share of long-spell entrepreneurs out-earning their matched controls and their own pre-exit earnings, by exit destination

Exit destination	N	Beat control	Beat own spell
Salaried exit	234	85.0	69.7
Non-salaried exit	902	60.5	35.6

Cells are the share (%) of each exit group of long-spell entrepreneurs from non-salaried backgrounds. “Beat control” is the share earning more post-exit than their matched controls; “Beat own spell” the share earning more than at their last spell wave, right before exit. The sample is restricted to units with post-exit, pre-exit, and matched-control earnings all observed and does not include exits into unemployment (which make up the difference in observations to Table A.6).

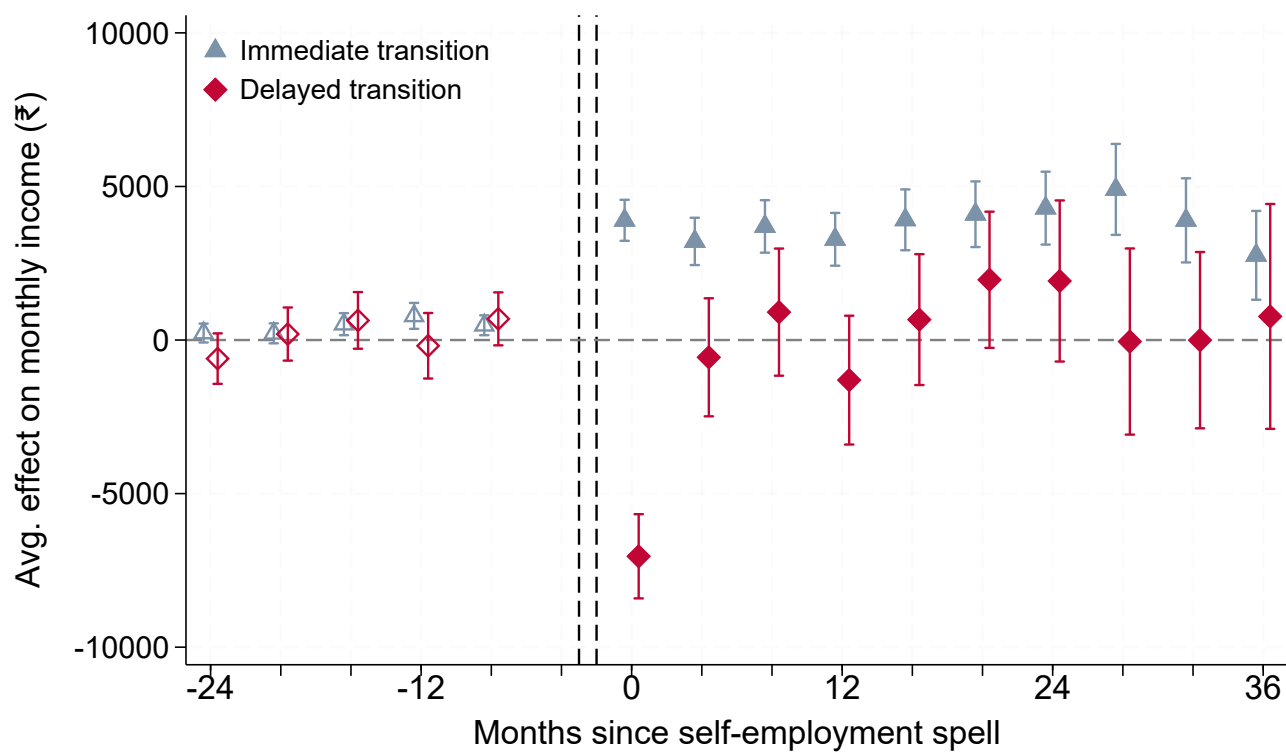


Figure A.16: Post-exit earnings of long-spell entrepreneurs from non-salaried backgrounds, by timing of transition into paid work

Notes. The sample is long-spell entrepreneurs from non-salaried backgrounds. Immediate transitions are those in any paid work at the first post-spell wave. Delayed transitions are those not employed at the first post-spell wave but employed later. Standard errors are two-way clustered at the household and matched group level.

Table A.8: Post-exit income: trajectory versus level of entrepreneurial-spell earnings

	(1)	(2)
Rising trajectory (last > first)	208 (1,109)	
Rising trajectory (OLS slope)		-473 (974)
Avg. spell earnings (1,000 Rs)	144** (56)	146** (56)
Constant	10,827*** (1,102)	11,191*** (1,032)
Obs	8,057	8,057
Sample	All long-spell	All long-spell
Matched-stratum + wave FEs	Yes	Yes

Sample: long-spell entrepreneurs, post-exit observations only. The dependent variable is post-exit monthly individual income (Rs).

Rising trajectory indicates that earnings rose over the entrepreneurial spell: defined as the last spell month exceeding the first, or as a positive within-spell slope of earnings on spell duration.

Avg. spell earnings is the average monthly income earned during the spell (in thousands of Rs). Entering trajectory and level jointly isolates whether the post-exit premium reflects how much was earned during the spell (level) or whether earnings were rising (direction).

All models include matched-stratum and survey-wave fixed effects; standard errors are two-way clustered by household and individual.

* p< 0.05, ** p< 0.01, *** p< 0.001.

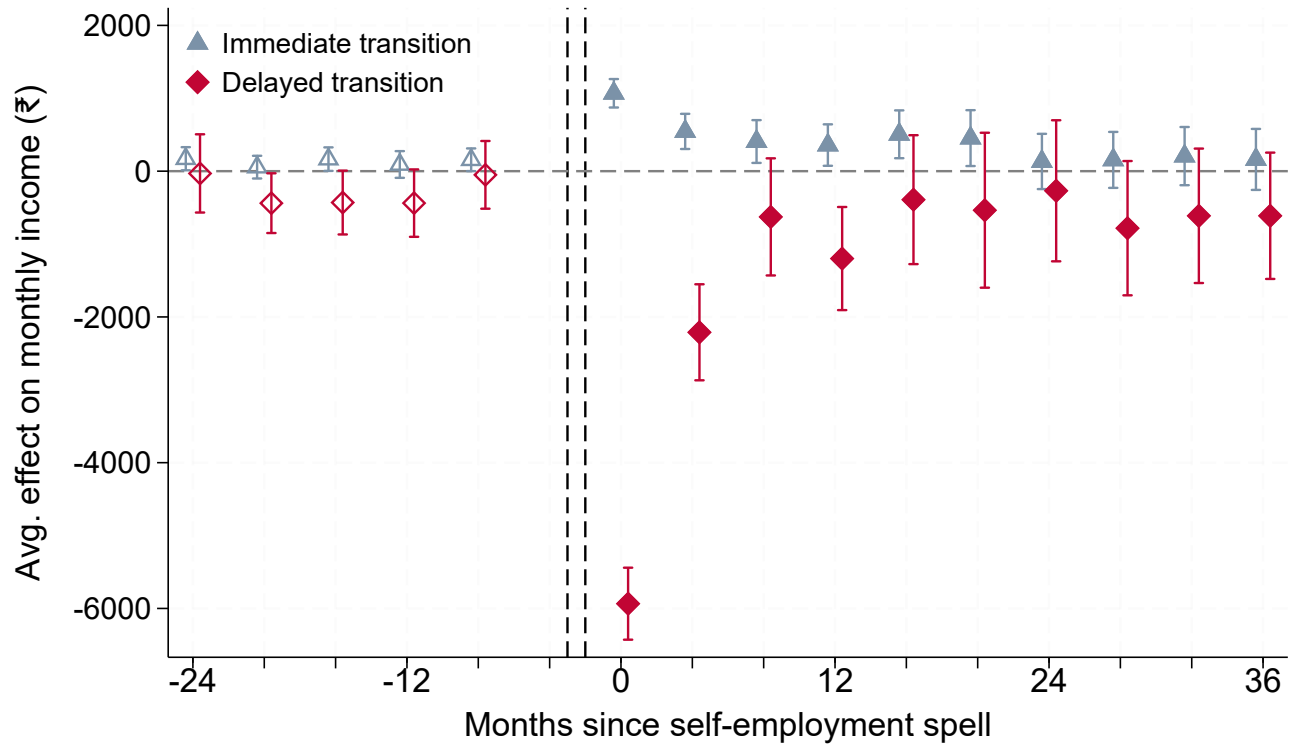


Figure A.17: Post-exit earnings of short-spell entrepreneurs from non-salaried backgrounds, by timing of transition into paid work

Notes. The sample is short-spell entrepreneurs from non-salaried backgrounds. Immediate transitions are those in any paid or employed work at the first post-spell wave. Delayed transitions are those not employed at the first post-spell wave but employed later. Standard errors are two-way clustered at the household and matched group level.

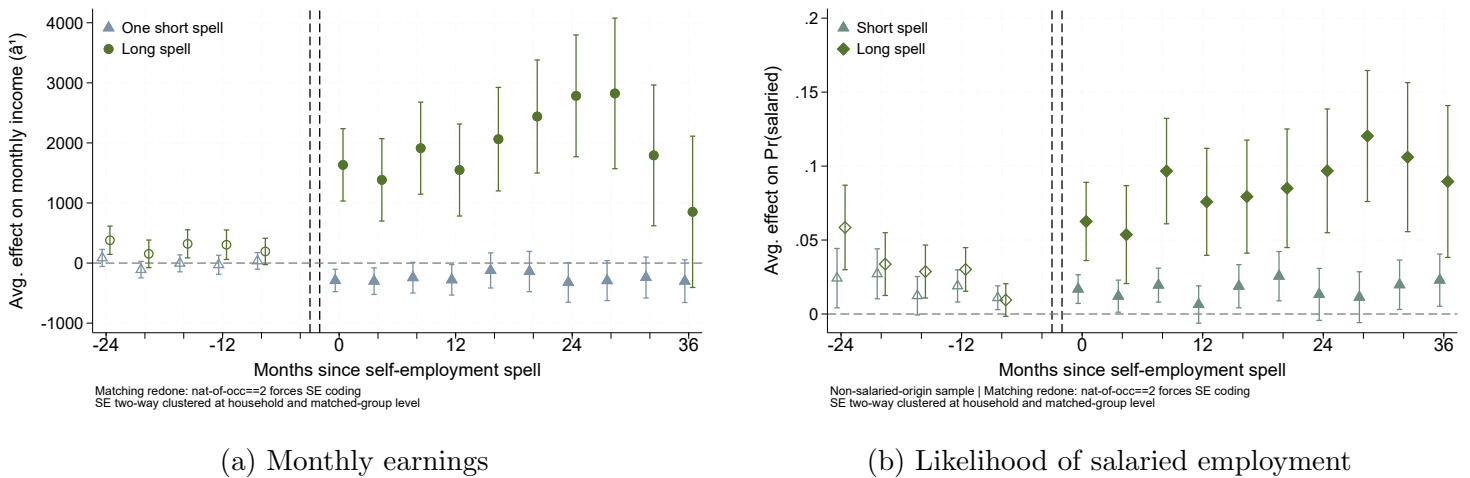
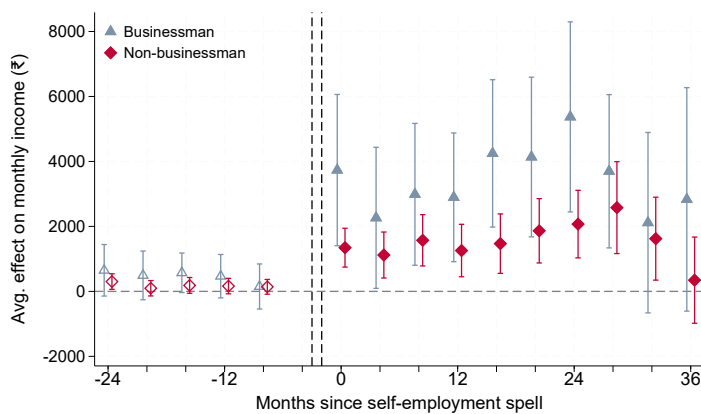
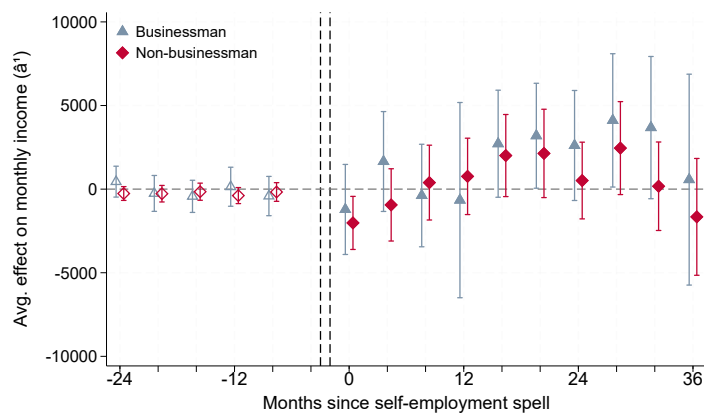


Figure A.18: Earnings trajectories (panel a) and likelihood of salaried employment (panel b) for formerly non-salaried entrepreneurs by spell length, after reclassifying as self-employed any respondent coded in the “businessman” occupation who reported a salaried arrangement, and re-running the matching. Compare to Figures 3 and 4.

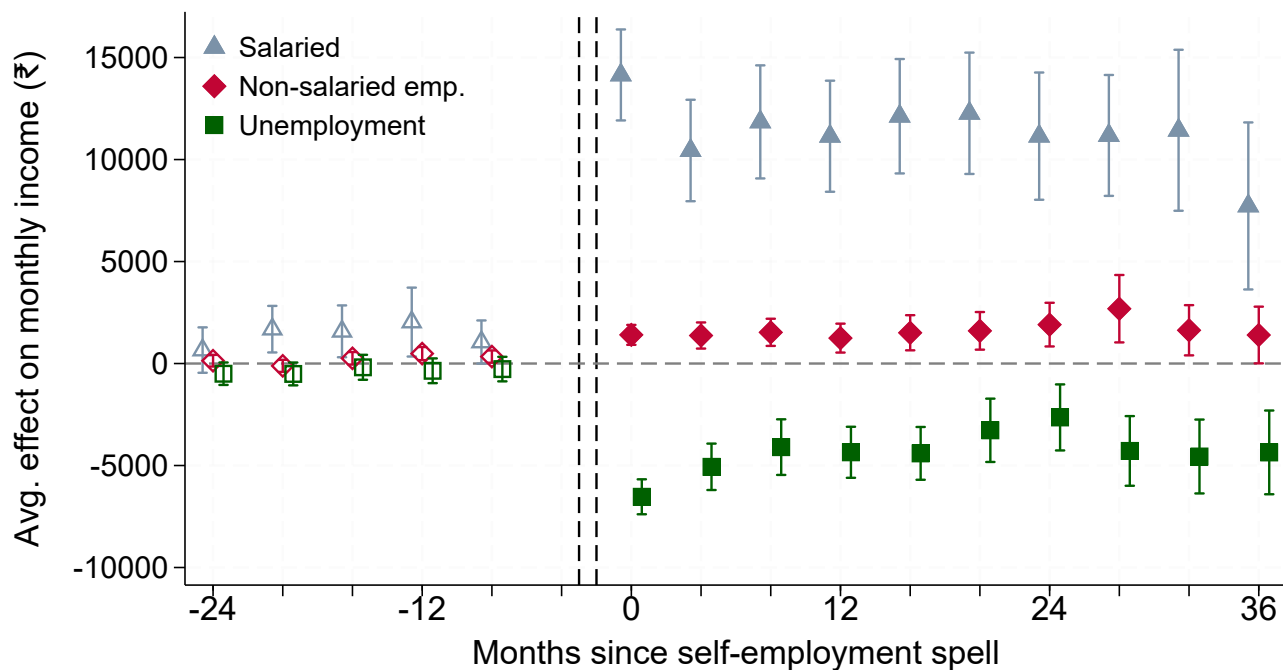


(a) From non-salaried backgrounds



(b) From salaried backgrounds

Figure A.19: Earnings trajectories of long-spell former entrepreneurs by whether they were classified as “businessman” (the CPHS *nature of occupation* code for those who own or manage business premises and capital) at the first wave of their self-employment spell, versus other self-employment occupations. Panel (a) shows long-spell entrepreneurs from non-salaried backgrounds; panel (b) those from salaried backgrounds.



Long-spell non-salaried-origin entrepreneurs only.
By wave-0 destination: salaried / non-salaried emp. / other (unemployment/OLF).
Standard errors two-way clustered at household and matched-group level.

Figure A.20: Earnings trajectories of formerly non-salaried entrepreneurs with a long spell by post-entrepreneurship employment arrangement (including unemployment). Compare to Figure 5

Table A.9: OLS estimations predicting monthly earnings for formerly non-salaried entrepreneurs with a longer spell, including destination industry FEs.

	Baseline		+ Dest. industry FE	
	(1)	(2)	(3)	(4)
	k1	k2	k3	k4
Post-entrepreneurship	1,911*** (317)	3,472*** (353)	1,125** (381)	1,351** (416)
Post-ENT X stay in industry		-2,050** (677)		-1,140 (812)
Constant	9,788*** (49)	10,236*** (50)	11,037*** (46)	11,176*** (47)
Obs	67,375	58,085	33,333	31,063
N matched groups	1513	1289	1000	914
R2	0.56	0.55	0.64	0.63
Match group X surv. wave FEs	Yes	Yes	Yes	Yes
Dest. industry FEs	No	No	Yes	Yes

All long-spell non-salaried entrepreneurs.

Std errors clustered by matched group and household.

B All variables

Salaried. Binary variable indicating whether a worker works in permanent, salaried employment, based on the survey question on *employment arrangement*. Answers include salaried employment (permanent), salaried (temporary), casual wage work / day labor, and self-employment. More recent survey waves also identify whether respondents' work takes place on digital platforms. The variable takes the value 1 for responses in permanent, salaried employment only. *Self-employment* is based on the same underlying survey item.

Demographic variables. The self-reported gender of the respondent is used as a binary variable and takes the value of 1 if the respondent is a *woman*. The survey also reports each individual's *age*, *household size* and *education*. The latter is constructed as a categorical variable, distinguishing between those with less than 5th grade education, those who reached the 5th-9th grade, 10th or 11th, a secondary education (high school) degree, or a university degree. Respondents also report their *religion*. As the vast majority of the sample is Hindu, I report this as a binary variable indicating Hinduism as one's religion. The survey also provides detailed information on *caste*. I use a binary indicator for belonging to one of the intermediate or upper castes, as categorized by CMIE, in some of the additional matching specifications in the robustness checks. When identifying majority caste membership, I use a more detailed classification with over 400 different castes and tribes.

Financial variables. I measure *income* as an individual's survey-wave's most recent month's income in Indian Rupees (Rs). This measure includes all income attributable to the individual including wages, dividends, and earned interest. For those who re-enter entrepreneurship, it also includes income attributable to a business.³⁴ The CPHS captures these at the household-level. When multiple household members are self-employed, I evenly divide the business-income between them. Since all of my main analyses estimate an individual's earnings *outside* of times of entrepreneurship, concerns about difficulties in attributing household earnings to an individual remain minimal.

Other financial variables are collected at the household level and include *types of savings*, *assets*, and *expenditures*. These variables are mainly used to assess the extent to which the matching procedure ensures balance on dimensions not included in the matching procedure, as well as in robustness checks. Household savings are measured as binary variables, indicating the existence of savings of a certain kind. They include fixed deposits, life insurance, business, gold, real estate, and other. The household asset variables indicate the ownership of cattle, air conditioners, a fridge, two-wheeler, car, tractor, or computer. Lastly, respondents report their monthly *total household expenditures* which CMIE provides in raw format as well as after adjusting for discrepancies between monthly and weekly data. One part of these monthly expenditures are loan payments in the form of equal monthly installments, *EMIs*.

³⁴CMIE confirmed that this measure closely resembles take-home pay, rather than profits that get re-invested into the venture.

Other work-related measures. The survey also provides information on occupation and industry. The primary occupation variable reported by CMIE, *nature of occupation*, is based on ISCO standards and includes 21 different occupations. The reported industry variable contains 39 levels.

C Exploratory analyses

C.1 What explains the penalty for short-spell entrepreneurs?

The earnings penalty for short-spell entrepreneurs from casual employment backgrounds qualitatively differs from the penalty observed in prior work. In the Global North, entrepreneurship sends a signal that the entrepreneur is not committed to working in a traditional organization or might not fit in (Botelho and Chang, 2023; Kacperczyk and Younkin, 2022). Casual employment neither produces nor demands such signals. Instead, I proposed that leaving casual employment for a short spell acts like a disruption. The process of continuous job search is interrupted and the self-employed forgo the specific experiences and workplace contacts that help them land the next job.

The penalty resembles the cost of switching work. I leverage information about the employment spells of control cases to assess whether the earnings penalty is driven by losing continued access to the same type of work. The disruption mechanism hinges on workers in casual employment finding future jobs through their current one, by building recent, demonstrable experience and the workplace contacts who vouch for them and pass along openings. It thus assumes continuity in a similar line of work, whereas a worker who moves between different types of paid work, say from agricultural work to manufacturing, loses that same continuity. I divide control cases into those who switched occupations, or industries, as the entrepreneur returned to paid employment, and those who remained in the same type of employment. If the penalty comes from this lost continuity, it should be larger against control cases who stayed in the same line of work and smaller against controls who themselves switched.³⁵

Table A.3 shows estimates based on a regression including solely entrepreneurs from non-salaried backgrounds with a short spell (and their matched controls), similar in structure to Table 2. Columns 1 through 4 of Table A.3 provide suggestive evidence supporting a switching cost explanation. The earnings penalty decreases and loses statistical significance when comparing entrepreneurs to control cases who switched to a new industry (column 1) or occupation (column 3) during the return transition. Short-spell entrepreneurs experience a larger penalty compared to those who remained in the same type of paid work throughout this time (columns 2 and 4).³⁶

The penalty is driven by those experiencing the most disruption. If the penalty reflects disruptive frictions akin to job loss, interrupting the continuous search that casual work depends on and the contacts that come with it, it should be driven by those who

³⁵This assumes that entrepreneurs engage in different work as entrepreneurs than they otherwise would have. This appears to be borne out in the data. Over 75% of previously non-salaried entrepreneurs enter self-employment in a different industry compared to their prior employment. Even those who remain in the same industry should still find their exposure to future employers decreased, are less likely to engage in the exact same work as asked for by employers, and might spend more time on general business activities.

³⁶Note that my matching procedure identified up to four control cases for each entrepreneur, which is why some, but not all, entrepreneurs appear both in columns with stayers and switchers. If all control cases remained in the same industry during the return transition, the entire matched group would only be part of column 2 but not 1.

struggle to find paid work upon their return. Therefore, I divide short-spell entrepreneurs by whether they directly returned to paid employment or they experienced an unemployment spell upon their return in Appendix Figure A.17. Those who immediately return to paid work experience an initial earnings premium over control cases that disappears after about two years. Those who experience unemployment first face an earnings penalty that persists. While later point estimates are statistically insignificant, they are consistent in direction and magnitude and, at times, marginally significant. These patterns are consistent with a disruption account: the costs fall on those who struggle to re-enter the continuous chain of casual employment.

The data do not separate the underlying channels. Whether this penalty is primarily driven by lost demonstrable experience or lost workplace contacts lies beyond the scope of this paper. Exploratory analyses in Appendix Table A.2 investigate whether alternative information mechanisms (other employed household members or being a majority caste member) might alleviate the earnings discount, but do not find conclusive evidence (columns 1 and 2). Neither is there a significant difference in the discount for entrepreneurs who entered in the same (vs different) industry compared to their prior work (column 3), consistent with the penalty reflecting lost continuity rather than sector-specific capital.

Alternative explanation: unemployment in disguise. Another explanation for the observed pattern could be that entrepreneurship is reported as a labor-force status to disguise unemployment, which, in turn, would impact future earnings. I leverage earnings during the entrepreneurial spell to guard against that possibility. Approximately 10% of respondents report having earned no income during their (short) entrepreneurial spell. One might argue that these are disguising unemployment. Removing these, however, does little to change the earnings trajectories of these entrepreneurs (see Figure A.5 in the Appendix).

Taken together, the evidence is most consistent with a short spell interrupting the continuity that job search in casual work depends on. On returning, workers must re-establish the recent, demonstrable experience and the workplace contacts that lead to the next job. People who remained in casual employment continued their search and have a head start. A short stint in self-employment thus carries costs resembling those of switching jobs or occupations (Rider et al., 2019).

D Robustness checks

Alternative (TWFE robust) estimator. Several sources of bias plague difference-in-differences and two-way fixed-effects (TWFE) event studies, particularly staggered ones (see e.g. de Chaisemartin and d’Haultfoeuille, 2022). While I rely on matched group, rather than individual, fixed effects, it is still possible for negative weights or ‘forbidden comparisons’ to occur. I corroborate results using an alternative estimator by Borusyak et al. (2024) that avoids these concerns. This estimator imputes the potential outcomes for treated units by extrapolating from fixed effects estimated for never- and not-yet-treated units. Aside from avoiding some of the core issues of TWFE regressions, it has several desirable properties with regard to the theoretical quantity of interest of this study. First, it can flexibly incorporate various fixed effects structures and thus allows for the estimation of matched group fixed effects, while other estimators are derived with the individual fixed effects from a canonical TWFE setup in mind (de Chaisemartin and d’Haultfoeuille, 2022). Second, the estimator uses the average of pre-treatment periods as a baseline, rather than just the last pre-treatment period. de Chaisemartin and d’Haultfoeuille (2022) show that the imputation estimator by Borusyak et al. (2024) is therefore less biased than other options under anticipation effects.³⁷ Third, the estimator is easily combined with matching methods and allows for matching on pre-treatment outcomes.³⁸

I show results using the estimator by Borusyak et al. (2024) in Appendix Figure A.6. I estimate returns both for former salaried and non-salaried entrepreneurs. The graph looks very similar to Figure 2, limiting concerns that results are driven by a biased estimator.

Alternative matching criteria. Next, I assess whether findings are sensitive to alternative matching criteria in Appendix Figure A.8. I start by matching on an extended history of employment arrangements in Panel A.8a. Here, I ensure that both treated and control cases engage in the same type of work (salaried / non-salaried) during the four waves before the treated units enter self-employment. Since the employment arrangement variable was not available from the beginning of the survey, estimates over longer horizons become noisy, so I plot four (instead of five) years post-entrepreneurship. In Panel A.8b, I further ensure that the previously (non-)salaried consistently engaged in (non-)salaried employment for four waves before entrepreneurial entry. In other words, this condition removes cases that experienced an unemployment spell or any time in the opposite (non-/salaried) employment type leading up to treatment. Next, I match on religion (Hindu versus other), caste (upper/middle castes versus other backward castes and scheduled tribes), whether other household members were self-employed, as well as three waves of (non-)salaried employment type and three waves of occupation status in Panel A.8c. Panel A.8d additionally matches on dummies

³⁷If people are particularly likely to enter entrepreneurship to stop downward mobility (O’Donnell et al., 2024) or facilitate upward mobility (Borchhardt and Sorenson, 2022), comparisons to the last pre-treatment observation might produce biased effects.

³⁸Prominent alternatives, e.g. Callaway and Sant’Anna (2021), combine an inverse-probability treatment weighting with an outcome regression and require having the same variables in both. Matching on pre-treatment earnings is core to minimizing bias absent an exogenous shock (Ham and Miratrix, 2022). However, that would imply including the outcome variable in the outcome regression when using Callaway and Sant’Anna (2021) and produce erroneous results.

for home- and cattle-ownership, as well as the number of different basic saving types of the household (fixed deposits, life insurance, gold, real estate) and the number of other types of savings (savings in post office, NSC bonds, KVP, provident funds, mutual funds, listed shares, chit funds, and other financial instruments). Lastly, panel A.8e replicates the original matching approach but matches on the industry and occupation of employment during the pre-entry wave.

These alternative approaches show findings that appear roughly comparable with the original analysis, but get increasingly noisy as matching criteria increase in stringency. For example, formerly non-salaried entrepreneurs appear to reap more consistently positive returns in Panel A.8a compared to A.8b. The majority of point estimates in Panels A.8b through A.8d are small and insignificant for those from more precarious backgrounds, similar to the main findings of this paper. For previously salaried entrepreneurs, more stringent matching criteria add noise, turning more estimates insignificant. However, in magnitude the earnings penalty appears comparable to the main findings above (note that the larger standard errors require a wider range on the y-axis in panels (b) through (d)).³⁹, ⁴⁰

Entropy balancing. The main matching approach prioritizes achieving tight balance on pre-treatment earnings levels and trends, which I argue are the most important confounders as they likely correlate with numerous unobserved predictors of income including ability, effort, social capital, and job market access. Alternative matching methods that simultaneously balance multiple variables face an inherent trade-off: while they efficiently achieve balance across many covariates without dropping as many observations, this comes at the cost of potentially weaker alignment on any single matching dimension.

To assess whether my findings depend on the specific matching estimator, I re-estimate the main equations using entropy balancing (Hainmueller, 2012). This method reweights control cases so that they match treated units on chosen covariates. I maintain the original sampling logic by pairing each treatment cohort (those treated in wave t) with all never-treated controls, so control individuals appear multiple times in the dataset. Each control observation is assigned a treatment-group specific cohort identifier, which enters the regression as fixed effects interacted with survey wave, allowing every treatment cohort comparison to have its own time effects. Standard errors are clustered at the household and treatment-wave levels. I induce balance on the same variables as the main procedure (without coarsening age or education) and add pre-treatment earnings level and slope.

³⁹The more stringent matching criteria also make it more difficult to identify control cases with similar pre-treatment earnings, which is why I exclude matches for previously non-salaried entrepreneurs whose Mahalanobis difference exceeds 1. Not doing so shifts up both pre- and post-treatment coefficients but does not meaningfully change the interpretation of results.

⁴⁰I also show results using fewer control cases in Appendix A.10. Results look roughly comparable to the main findings.