Supporting Platform Complementors From Disadvantaged Communities in Times of Crisis

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Abstract

This phenomenological study investigates the effectiveness of a training initiative aimed at supporting platform complementors (i.e., fintech agents) based in disadvantaged communities. A field experiment involving 293 agents of a Brazilian fintech company that serves poor communities shows that training has an average positive but highly nuanced effect on agents' sales. While gains from training are stronger for agents operating in communities with more limited access to regular banking services, I also find that local economic conditions and the pre-existing size of the local customer base also shape these effects. Accordingly, I show that the intervention reduced the sales of agents with larger existing customer bases who operated in communities subject to stronger Covid-19 restrictions. All the other groups—agents with smaller customer bases or operating in communities where Covid-19 restrictions were weaker or nonexistent-derived positive gains from the training. I then examine a potential mechanism underlying these resultsnamely, the entry of new agents in communities where at least one incumbent agent participated in the intervention. Lastly, I show that the results are consistent with a simple model that accommodates network effects and heterogeneity in demand-side conditions. The paper closes with a discussion about theoretical and practical contributions, including implications for research on strategy in contexts of disenfranchisement, stakeholder theory, and platforms.

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INTRODUCTION

Strategic management research has recognized the social impact of businesses as a critical dimension of interest (e.g., Barnett, Henriques, & Husted; 2020; Barney & Rangan, 2019; Durand & Huysentruyt, 2022; George, Howard-Grenville, Joshi, & Tihanyi, 2016; Kaul & Luo, 2018; 2019; Lazzarini, 2020; Luo, Kaul, & Seo, 2018; McGahan, Zelner, & Barney, 2013). This obviously includes platforms, an increasingly important type of organization whose business model revolves around managing interconnections and facilitating exchanges among two or more interdependent parties (e.g., Li & Agarwal, 2017; McIntyre & Srinivasan, 2017; Rietveld & Eggers, 2018; Rietveld & Schilling, 2021; Rysman, 2009; Zhu & Furr, 2016; Zhu & Iansiti, 2012; Zhu & Liu, 2018). This paper explores a rich intersection between these two large bodies of work.

Taking a phenomenological approach (e.g., Flammer & Ioannou, 2021; Hambrick, 2007; Pongeluppe, 2022), I study how platforms may support complementors from disenfranchised communities in times of economic hardship. I report the results of a stratified randomized field experiment involving 293 small entrepreneurs who are *agents* of a Brazilian fintech that operates in poor communities across the country. The company owns and manages an app-based digital platform used by agents to offer basic financial services to their own communities. In the intervention, agents were randomly assigned to receive sales and marketing training (and mentoring) aimed at increasing their capacity to sell the fintech's services locally. The training initiative was conducted by analysts who work in the fintech's team and a team leader was responsible for overseeing it internally.

In the analyses, I combine administrative and survey data with information from IBGE (the Brazilian Bureau of Statistics), the Brazilian Central Bank, and other secondary sources. The main dataset has three data points: a pre-intervention (October 2019) and two post-intervention

(February and May 2020) observations. Survey data, in turn, was collected in October 2019 and October 2020. Importantly, the Covid-19 pandemic unexpectedly became a global crisis immediately after the end of the intervention (January 2020). Thus, exploring variation in regional restrictions to economic activity, I use the Covid shock to examine how the effects of training change when agents are more or less exposed to a crisis situation.

The baseline results suggest that training offered to platform complementors based in disadvantaged communities leads, on average, to an increase of approximately 25% in their appbased sales. Nevertheless, I also find that this result is exceptionally nuanced. First, I show that the effect is stronger in cities where the access to regular banking services— proxied by the number of bank stores per inhabitant—is more limited. These are precisely the communities where fintech services are more needed and valuable to local consumers.

Second, I also find that, on average, agents with smaller existing customer bases (measured prior to the intervention) benefit relatively more from training. This finding suggests that demandside factors might play a critical role in explaining the treatment effects stemming from training. Thus, I delve deeper into the role of demand-side conditions by distinguishing between communities that are more or less exposed to Covid-19 restrictions. And the results once again show important heterogeneity. In essence, I find that training negatively impacts the app-based sales of agents with larger existing customer bases who are also subject to stronger Covid-19 restrictions. The other groups—that is, agents with smaller existing customer bases or subject to weaker Covid-19 restrictions—all benefit (on average) from the training intervention.

Given these results, I then investigate potential underlying mechanisms. Informal interviews with the analyst responsible for overseeing the intervention in the fintech's team suggested that the entry of new agents could play a role. In essence, the analyst reported that

several agents were reluctant to more actively advertise the fintech's services because, in doing so, they could induce other local entrepreneurs to also become fintech agents. To examine plausibility of this mechanism, I use an alternative sample—defined at the community level—comprising all agents that were ever active in the fintech's system between October 2019 and May 2020. The results seem to support the analyst's claim: in communities exposed to stronger Covid-19 restrictions, the training is associated with agents' entry only when the treated agent has a larger customer base—that is, only when it negatively impacts agents' sales.

In light of all the empirical findings, the final section proposes a simple theoretical framework that extends and generalizes the arguments developed in the paper. Albeit parsimonious, the model accommodates network effects and heterogeneity in demand-side conditions. It describes, essentially, how the provision of training interacts with local demand-side (and more general economic) conditions to shape entry into local markets, which then affects sales outcomes.

This paper proceeds as follows. In the next section, the field experiment and the empirical approach are described in detail. I then move to discuss the baseline results and two key moderators: communities' access to banking services and agents' pre-existing customer bases. Next, I examine how the effects vary with communities' exposure to Covid-19 restrictions, and study the role of agents' entry as a potential underlying mechanism. Finally, I describe a theoretical framework that extends my findings and discuss implications of my results for both theory and practice.

TRAINING COMPLEMENTORS FROM DISADVANTAGED COMMUNITIES IN A TIME OF CRISIS: A FIELD EXPERIMENT

Context and experimental design

To study the impact of complementors' training on platform sales in disadvantaged communities, I partnered with a Brazilian fintech company that owns and operates a multi-service mobile app. This platform provides a variety of distinct services such as utility and bill payments, phone recharges, digital content purchases, public transportation tickets, and so forth. The key complementors in the company's business model are *small business owners* based in disadvantaged, resource-constrained communities across Brazil. These *agents* partner with the firm and use its mobile app to offer the above-mentioned services to their local customer bases, receiving in return a fixed proportion of the total app sales.

Our field experiment involved 306 agents whose main businesses belong to one of five sectors in retail: cyber cafés, clothing stores, stationery stores, computer shops and grocery stores. The intervention consisted of a training and mentoring effort carried out by employees of the fintech company and aimed at increasing the service provision capacity of "treated" agents. Accordingly, agents were randomly assigned to short training sessions conducted every two weeks, by phone, between November 2019 and January 2020. In each session, a general theme related to app-based services or general sales and marketing skills was discussed, with a special focus on showing agents how this knowledge could be leveraged to increase their app sales and improve their operational efficiency. Agents were also taught to use advertising and marketing materials, and received guidance about the menu of services offered through (as well as the functionalities available in) the fintech's app. Lastly, the "coaches" also offered help with questions, issues, or difficulties faced by the agents.

The randomization procedure was stratified in two layers. First, I divided the sample into two groups according to the *development stage* of each agent's app-related customer base—i.e., the customers of an agent's store who purchase the fintech's app-based services. Accordingly, the *Larger Existing Customer Base* group included agents with more than 2,000 single transactions completed through the app, in the nine-month period *preceding* the beginning of the intervention. The *Smaller Existing Customer Base* group, in turn, included individuals with less than 2,000 transactions in the same period. In the next step, I divided each of these groups further according to the five business sectors listed above, thus forming ten distinct subgroups. Finally, within each subgroup, I randomly selected half of the agents to be treated and the other half to be controls.

Generally speaking, the main advantage of stratification is that it helps to ensure that treated and control groups are balanced in respect to relevant characteristics (Bruhn & McKenzie, 2009). This is particularly important in the context of my intervention. For instance, agents from distinct sectors can respond differently to the treatment due to sector-specific characteristics. Thus, if treatment and control groups are unbalanced in respect to business sector, eventual differences on average treatment effects could be driven by this lack of balance rather than by treatment itself (Athey & Imbens, 2017; Bruhn & McKenzie, 2009). The stratification design thus mitigates concerns of this sort (Chatterji et al., 2016). Also importantly, by stratifying within the two development stage groups described above, I ensure that the analyses comparing treatment effects between these groups still retain a causal interpretation. Table 1 summarizes the randomization process discussed above.

**************************Insert Table 1 about here******************************

The Covid-19 crisis

The Covid-19 outbreak grew into a global pandemic in early 2020. In Brazil, the health situation started deteriorating in March, one month after the end of our intervention and just a couple of weeks after our second data collection (the next subsection discusses the data collection process in detail). Between March and May 2020, several—though not all—state governments in Brazil

announced social distancing measures and severe restrictions to economic activity (Barberia & Gómez, 2020). While these policies were critical to contain the spread of the virus and prevent an even more tragic health crisis, they also contributed to serious economic damages, including sharp growth in inflation and unemployment rates, severe exchange rate depreciation, and a 4-percent GDP drop (IMF, 2021).

The Covid-19 restrictions observed after February 2020 in Brazil were obviously exogenous to our experiment. Thus, they offer a unique opportunity to study whether and how the training provided to complementors (i.e., the fintech's agents) impacted app sales in regions exposed to severe economic crisis. To that end, I explore variation across Brazilian regions' announcement and implementation of Covid-19 economic restrictions. The next subsection provides a detailed description of the database used in the study.

Sample definition and data description

Of the 306 agents initially participating in our study, 13 were dropped from the sample due either to a partnership discontinuation (with the fintech) or to issues of data incompleteness. The final sample comprises 293 agents located in 24 Brazilian states. Each of these agents is observed three times: prior to the beginning of the intervention and the Covid-19 pandemic (i.e., September 2019), immediately after the end of the intervention (February 2020) and three months later, when the pandemic was in full swing in Brazil (May 2020). The main database includes information on the agents' geographic locations (state, city, and ZIP codes), business sectors, total sales made through the app, and other community-level data.

This sample was further complemented with survey data collected before and after the intervention—i.e., in September 2019, and between November and December 2020. Through these surveys, I obtained exploratory data (also used to compose control variables), as well as data used

to examine the causal mechanisms at work in the intervention, in an attempt to "probe" the theory of change for the experiment. In short, the underlying theory of change is that the mentoring program will translate into improved service provision capacity, which should then affect agents' app sales. But if mentoring is indeed effective, the higher capacity of treated agents should lead to the differential adoption, by these treated individuals, of specific managerial practices, relative to the control group. To study this (potential) differential practice adoption, in the survey conducted at the end of 2020 agents were asked to report on: i) whether they had banners or other marketing materials related to the fintech's app services placed in their stores; and ii) the weekly frequency with which they actively offered the fintech's services to their customers. Unfortunately, due to the restrictions imposed by the pandemic, only (approximately) half of the agents in the sample responded to the questionnaires.

Furthermore, to study the Covid-19 crisis, I combine archival information on the announcement of social distancing measures—hand collected from state and city government websites—with data on the annual average state-level income, retrieved from IBGE (the Brazilian Bureau of Statistics). This approach allows me to identify regions where the official Covid-19 restrictions coincided with a significant economic downturn, proxied by reductions in annual average state-level income. I then compare agents based in these regions (*Stronger Covid-19 Restrictions*) with individuals from regions where restrictions where either not adopted or not sufficiently severe to reduce the average income (*Weaker Covid-19 Restrictions*).

Measures and econometric specifications

Dependent Variables

The main dependent variable used in this study is the log of the average daily app-based service sales (*App Sales per Day*). I also use three additional dependent variables to study the underlying

mechanisms at work. The first two variables were collected in the survey described above (conducted at the end of 2020). *Uses Marketing Material* is a dummy indicating agents who reported the use of banners or other marketing materials related to the fintech's app services. *Frequency Offers Service* is a 5-point (ordered) categorical variable describing the reported weekly frequency with which agents actively offer the fintech services to the customers in their stores. These variables capture whether the mentoring intervention induces marketing-related practices that favor app service sales, in line with an improved service provision capacity. Lastly, I also examine agents' entry in a given community—defined as the region represented by the agent's ZIP code—as a key underlying mechanism. Hence, *Number Entrepreneurs in the Community* counts the number of agents who sell the fintech's app services in a given ZIP code, at a given period.

Independent Variables

I define several independent variables. *Treated* is a dummy that indicates agents randomly assigned to receive the service provision mentoring. *Larger Existing Customer Base* equals one if the agent had more than 2,000 transactions completed through the app, in the nine-month period preceding the beginning of the intervention (and equals zero, otherwise). A third dummy indicates observations in the post-intervention period (*Post October 2019*). Furthermore, I also rely on event study specifications operationalized using period-specific dummies (i.e., *February 2020* and *May 2020* dummies). Lastly, one premise in the literature is that the training support offered to complementors in disadvantaged communities would be more important in those communities where the availability of services is particularly limited (e.g., Pongeluppe, 2022). In my context, this is equivalent to a claim that the training intervention should be more effective in communities where access to traditional banking services is more limited. To test this assertion, I collected data

on city-level population from IBGE (the Brazilian Bureau of Statistics) and number of bank stores (from the Brazilian Central Bank), both for the year 2019. I then created a measure that proxies for the local availability of banking services: *Relative Number of Bank Stores* is calculated as the natural logarithm of the ratio between the number of bank stores and the total population of a given city at year 2019.

Descriptive statistics and specifications

Table 2 presents descriptive statistics for the main sample (left panel), as well as comparisons between treatment and control groups (right panel). These comparisons are based on group averages calculated in the *pre-intervention* period. In sum, there are no statistically significant differences between treated and control agents, in respect to any covariates, thereby suggesting that the randomization process successfully generated a control group that is highly comparable to the treated group.

Moreover, despite the seemingly successful randomization process, I try to mitigate other potential concerns with unobserved heterogeneity by applying a difference-in-differences methodology (e.g., Wooldridge, 2002) that also controls for agent and period fixed effects (e.g., Hamilton & Nickerson, 2003). The baseline econometric specification has the following functional format:

App Sales per Day_{i,t} =
$$\beta$$
(Treatedt, x Post October 2019_t) + $\gamma_{s,t}$ + γ_{i} + $\varepsilon_{i,t}$,

where γ_t denotes period fixed effects, γ_i denotes agent fixed effects, and $\varepsilon_{i,t}$ is an idiosyncratic error term. The coefficient β then describes the change in App Sales per Day for treated (versus control) agents, observed after the intervention. Lastly, some specifications will also include sector-period fixed effects to account for time-varying shocks affecting the distinct sectors (e.g., Hamilton & Nickerson, 2003).

BASELINE RESULTS: THE EFFECT OF SERVICE PROVISION TRAINING ON AGENTS' APP SALES

Is the theory of change supported?

Before describing the baseline results, I briefly examine whether the experiment impacts agents' service provision capacities as expected. As mentioned above, the underlying theory of change is that the mentoring program improves service provision through differential adoption of specific business practices by treated agents, which should then increase their app-based sales (relative to the control group). Table 3 provides some evidence for these differences in practice adoption.

Models 1 and 2 present OLS regressions with *Uses Marketing Material* and *Frequency Offers Service* as dependent variables, respectively. Both specifications control for the app service sales of the agents (both at the individual and at the ZIP levels, and in a monthly basis), the number of agents operating in the ZIP code, as well as for agents' (self-reported) gender and race classifications. The control variables are all set at their *pre-treatment* values.

According to Model 1 in Table 3, in the post-intervention period, treated agents were significantly more likely (p-value = 0.027) to have a poster, banner or other visual advertising material about the fintech's app-based services placed in their stores. Similarly, Model 2 suggests that treated agents more frequently offered the fintech's app-based services to their customers, although this result is only marginally significant (p-value = 0.068). Together, these findings suggest that, on average, treatment was associated with differential adoption of marketing and sales practices by the agents, in line with the presence of improved service provision capacities

among treated individuals. Hence, I conclude that the results offer some support for the theory of change underlying our intervention. I now describe the baseline results.

Baseline results

Table 4 reports the baseline results for the average agent in our sample. Models 1-4 estimate a regular difference-in-differences specification varying the layers of fixed effects and control variables included, while Model 5 is an event-study specification that includes agent and period fixed effects. Together, the results from models 1-4 suggest that, on average, providing training and mentoring to the agents leads to an economically (and statistically) significant increase in average daily app sales. More specifically, the estimates across all four specifications point to an increase of approximately 25% in sales, with a statistically significant coefficient in all regressions that include agent fixed effects (p-value < 0.05). Model 5, in turn, suggests that there are consecutive and (marginally) significant increases in the daily app sales of treated agents, following the intervention. These results are also plotted in Figure 1.

Taken together, the results in Table 4 suggest that the training and mentoring program has a positive and economically meaningful impact on agents' average daily sales. Yet, as mentioned above, this training support intervention should be particularly impactful in communities where access to financial services is most problematic. Model 6 in Table 4 tests this prediction by adding to the specifications the interactions with our measure of local access to banking services (*Relative Number of Bank Stores*). The negative and statistically significant coefficient in the three-way interaction reported in Model 6 (p-value = 0.012) confirms our expectations: as the availability of

classical banking services increases, the treatment effects reported in models 1-5 become smaller in size.

On the other hand, Table 5 examines how these treatment effects vary as a function of the agents' development stage. According to Model 1 (Table 5), the training has a substantive positive impact on the average daily app sales of agents with smaller customer bases: treated individuals in this group see, on average, an increase of more than 50% in their app sales, relative to control agents (p-value < 0.01). Model 2, on the other hand, shows a markedly different picture: large-customer-base agents do not seem to benefit from the training, on average. Model 3 further supports this conclusion, as the three-way interaction term (*Treated* x *Post October 2019* x *Larger Existing Customer Base*) is negative and highly significant, while Model 4 shows that this result also holds in an event-study specification.

Lastly, as seen in Figure 2, similar conclusions are derived when event-study estimates of the average treatment effects of each group are plotted against each other. Overall, the results in this section suggest that, while training seemingly benefits the average agent, these benefits are in fact reaped by agents with smaller customer bases. Across all specifications, I find no evidence that large-customer-base agents benefit from the intervention. In the next section, I study how exposure to the economic impacts of the Covid-19 pandemic may help explain the result patterns found so far.

To study how economic crises might affect the treatment effects estimated and described in the previous section, we start with the specifications shown in Table 6. Model 1 shows a negative and marginally significant (p-value = 0.065) difference between the average treatment effects for agents in regions subject to stronger Covid-19 restrictions (that is, regions subject to formal economic restrictions and which are located in states that saw a decline in average income between 2020 and 2019), vis-à-vis agents located elsewhere. In other words, the first specification in Table 6 suggests that exposure to Covid-19 restrictions reduces the average treatment effects reported in the previous section. Models 2 and 3, in turn, split the sample according to the agents' stage of development. These specifications suggest that the results shown in Model 1 are primarily driven by agents with larger customer bases, although standard deviations are large and the difference between the three-way coefficients is only significant at the 10% level.

*************************Insert Table 6 about here*******************************

According to the results in Table 6, Covid-19 restrictions gravely impacted treated agents with large customer bases, while the average effect on small-customer-base agents seems lower and only partially reduces the gains reported in Table 5. Figure 3 sheds further light into this discussion by splitting the sample according to the strength of Covid-19 restrictions faced by the agents. In each panel, we plot the estimated treatment effects for small- (blue, continuous line) and large-customer-base agents (green, dashed line).

As seen in Figure 3, in regions subject to weaker (or to no) Covid-19 restrictions (right panel), both large- and small-customer-base agents reap positive gains from the training and mentoring program, although the effect loses statistical significance in May 2020 for the former

group. These results are in stark contrast to the panel at the left-hand side: in regions subject to stronger Covid-19 restrictions, the treatment (on average) has a negative impact on the group of large-customer-base agents (green, dashed line). We estimate a reduction of more than 60% in average daily sales in February 2020, and a smaller but still substantive reduction of around 50% in May 2020.

In turn, the group of agents with smaller customer bases seems to benefit from training despite the economic downturn, although their gains are no longer statistically significant in this context (possibly due to large standard errors). It is safe to say, however, that the negative effects of training are restricted to large-customer-base agents who face harsher Covid-19 economic restrictions. This finding suggests that the heterogeneity in treatment effects shown in Figure 2 is likely driven by a negative impact of training on large-customer-base agents who face highly salient crisis conditions. In all other scenarios—namely, in the case of agents with smaller customer bases or larger customer bases but facing weaker Covid-19 restrictions—training arguably helps agents increase their daily app-based sales. The key question, therefore, is why training is detrimental to "larger" agents in a context of crisis. The next section sheds light on a potential mechanism underlying this result, namely, the entry of new agents in communities where treated agents are based.

UNDERSTANDING THE NEGATIVE EFFECTS OF TRAINING ON LARGE-

CUSTOMER-BASE AGENTS SUBJECT TO STRONGER COVID-19 RESTRICTIONS

As a first step to understanding why large-customer-base agents may be "harmed" by the training intervention during times of crisis, I conducted informal open interviews with the fintech team, particularly with the analyst responsible for overseeing the experiment internally. The key takeaway from those interactions is that, according to the analyst, agents were often reluctant to applying the knowledge acquired during the training program because doing so could lead to local entry spillovers. By actively promoting the services sold through the fintech's app, agents would naturally increase the visibility of the app locally, potentially inducing other entrepreneurs in the community to also become agents (i.e., fintech partners). For instance, according to the analyst, "many agents avoid using ads and other marketing materials provided by us because they expect this active promotion of the services will bring more agents to the platform and increase competition" [free translation; original quotes in Portuguese]. Interestingly, as shown in Table 3 (above), I find that the treatment correlates, on average, with a greater use of marketing and advertising materials by the agents. I suspect, therefore, that this entry spillover mechanism can indeed play a role in explaining our results.

Yet entry per se might not necessarily lead to reductions in sales, due to potential network effects: as additional entrepreneurs become agents, local customers who did not know the app's services beforehand may now start demanding these services (i.e., a "conversion" effect). Nonetheless, such effects should also depend on local conditions. For example, the potential entrants may use the incumbent agent's customer base size as a proxy for the size of the local market. Thus, when the agent who receives training has a larger pre-existing customer base for the platform services, the potential entrants in the community may overestimate the size of the local market, and therefore, be more willing to enter. The potential increases in sales, on the other hand, could be muted due, for instance, to Covid-19 economic restrictions that might limit the potential conversion effect by reducing local demand.

To investigate the possibility that entry contributes to explaining the results reported above, I use an alternative sample comprising all agents who operated in the fintech's app between October 2019 and May 2020. I then focus on the communities where these agents were baseddefined as the ZIP codes associated with their profiles—and redefine all key variables at the community level. Thus, in these analyses, I distinguish between communities where at least one agent is treated and communities where no agents are treated using a dummy variable: *Treated Agent in the Community*. Similarly, I also define a dummy indicating communities where at least one agent has a larger customer base. This variable serves as a proxy for regions with more developed markets for the fintech's app-based services. Lastly, the dependent variable used in the analyses counts the number of agents in each community, and all specifications are estimated via Poisson pseudo-maximum likelihood (e.g., Wooldridge, 1999). The final sample consists of 291 communities in all 27 Brazilian states. Basic descriptive statistics for this sample are shown in Table 7.

I start by examining whether the presence of a treated agent in the community generates entry spillovers on average. Results in Table 8, below, suggest that this is indeed the case. According to Model 1, on average, following the intervention, the number of agents increased by 15% in communities where at least one agent was treated, relative to communities where only never-treated agents operated (p-value < 0.01). Event-study estimates in Model 2 offer additional support for this conclusion. Model 3, in turn, suggests that this effect was more pronounced in more developed markets—that is, in markets where at least one agent had a larger customer base– –although the interaction was no longer significant in May 2020.

The more critical question, though, is whether there are differences in agents' entry behavior as a function of their exposure to stronger Covid-19 restrictions. Figure 4 examines this possibility. The left-hand side panel in Figure 4 plots Poisson event-study estimates for the communities that faced stronger Covid-19 restrictions, whereas the right-hand side panel focuses on communities where these restrictions were weaker or non-existent.

As seen in Figure 4, in regions subject to stronger Covid-19 restrictions, the presence of a treated agent in the community is associated with a significant and sustained increase in the number of agents for the more developed local markets (represented by the green, dashed line). We fail to find any evidence for potential treatment-induced entry in communities where all agents have smaller customer bases (blue, continuous line). In contrast, in regions subject to weaker (or non-existent) Covid-19 restrictions (right-hand side panel), the presence of a treated agent is generally associated with post-intervention entry, regardless of how developed the local market is.

This finding suggests that the negative impact of training on larger-customer-base agents facing harsher Covid-19 restrictions (shown in Figure 3) might indeed be driven by potential treatment-induced entry spillovers—as previously suggested in my informal interviews with the fintech's team. A similar entry behavior is not observed among "smaller" agents. Interestingly, according to Figure 3, the latter group of agents is also not harmed by the training intervention (and may even benefit from it, although we do not find statistically significant effects). Hence, our community-level results further support the claim that potential treatment-induced entry may shape the outcomes of the training intervention in contexts marked by significant economic crisis. Lastly, the results depicted in Figure 4 also raise the possibility that treatment-induced entry may depend on an interaction between the existing size of an agent's customer base and the local consumer demand, which is a function of local economic activity. In the next section, I propose a simple theoretical framework that helps us make sense of the results from the empirical analysis.

SUPPORTING PLATFORM COMPLEMENTORS FROM DISADVANTAGED COMMUNITIES IN TIMES OF CRISIS: A FRAMEWORK

In this section, I introduce a simple framework that summarizes and reconciles the main empirical findings discussed in the previous sections. Here, I describe the setup and discuss the intuition and implications of the main results derived from this framework. More technical details can be found in Appendix A1.

Model setup and assumptions

A platform complementor I, who currently operates in a disadvantaged community as a monopolist, may be assigned to participate in a training program. If I does not participate in the program, they face an inverse demand function given by $p_I^0 = h + \varepsilon - q_I^0$, where h > 0 denotes the complementor's customer base size, q_I^0 denotes the quantity of platform services offered by I, and ε is a demand shock. For simplicity, I assume that ε is uniformly distributed in $[\varepsilon, \overline{\varepsilon}]$, where ε and $\overline{\varepsilon}$ are real numbers, and that ε 's distribution is common knowledge. In contrast, I's participation in the training effort has several important implications.

First, training enhances the complementor's access to basic sales capabilities that are particularly salient in disadvantaged contexts (e.g., Pongeluppe, 2022). This improvement in their sales capabilities leads to a marginal increase in their services demand of y > 0. Yet, as discussed above, stronger sales capabilities may also increase the platform's visibility locally. To capture this effect in a straightforward way, I assume that, when I participates in the training, another community member \in learns about the platform and considers becoming a complementor; \in , otherwise, never hears about the platform. This simple formulation captures training spillovers while also avoiding more technically complex discussions.

To enter the platform, \in must bear a fixed cost $\overline{c} > 0$ reflecting, for example, the necessity to acquire basic capabilities or to buy equipment. Our focus on disadvantaged communities is again important here: these costs can be particularly relevant in such contexts. Furthermore, \in 's entry may also impact the local demand for platform services due to network effects (e.g., Hagiu, 2006; Hagiu & Wright, 2015; Rochet & Tirole, 2006), which will be represented by a function b(n) that equals one if I is a monopolist (i.e., n = 1) and equals b > 2 if \in enters (i.e., n = 2). In essence, in my framework, network effects may boost the increase in demand from training participation—i.e., there is an interaction yb(n)—as well as strengthen the effects of demand shocks—modeled through an interaction $\varepsilon b(n)$.

All in all, the framework has a straightforward structure. After *I* participates in the training initiative, \in decides whether or not they will enter the platform. The demand shock ε is then realized and two possibilities follow: either *I* and \in engage in competition à la Cournot (in case \in enters) or *I* remains a monopolist (otherwise). In examining \in 's entry decision, I assume that the potential entrant does not know about the training and its benefits, and that $\underline{\varepsilon} \ge -h/b$. This last assumption ensures that the local demand for platform services is never negative. In what follows, I start by investigating \in 's entry decision and next move to examine the outcomes for *I*, using the no-training case as the baseline.

The entry spillovers of training

Player \in enters—that is, becomes a platform complementor in the community—if and only if they expect a non-negative payoff from this choice. Their expected payoff, in turn, is calculated given their expectations about the future local demand for platform services and their beliefs about *I*'s payoff structure. As shown in Appendix A1, \in enters if and only if the following condition holds:

$$h + \left(\frac{\overline{\varepsilon} + \underline{\varepsilon}}{2}\right) b \ge 3\sqrt{\overline{c}}.$$
 (1)

According to Equation 1, the state of local demand can have profound implications for \in 's entry choice. For simplification, let us fix $\underline{\varepsilon}$. Then, the role of (expected) local demand conditions can be examined by varying $\overline{\varepsilon}$. First, note that, when there are sufficiently benign demand conditions—that is, when $\overline{\varepsilon}$ is sufficiently high, meaning that the local demand is also expected to be high— \in always chooses to enter. In this scenario, regardless of the size of *I*'s customer base (parameter *h*), entry always occur.

Nonetheless, in situations of economic downturn—when demand is expected to be lower, corresponding to lower values of $\overline{\epsilon}$ —*I*'s customer base size may still drive entry. This is because, while weaker economic activity may reduce prospects for the expected local demand, the size of the complementor's existing customer base signals (to the entrant) the size of the potential local market for platform services. Therefore, in such contexts, training offered to complementors with larger customer bases may still translate into entry, while a similar effect is no longer observed among "smaller" trained incumbents. The following proposition summarizes this first result:

Proposition 1. Under favorable economic conditions, training provided to a platform complementor leads to entry of new complementors in the same local market. In contrast, in situations of economic downturn, training leads to entry only if the trained complementor has a sufficiently large pre-existing customer base for platform services.

The effects of training on the incumbent's platform sales

I now examine the impact of training on *I*'s sales. Let $\tilde{\varepsilon}$ be the realization of the demand shock ε , and define the effects of training (TE) as the difference between *I*'s sales when they are assigned to the training initiative and *I*'s sales when they do not undertake training. Again, I explore two basic scenarios, one with more benign and the other with harsher, unfavorable economic conditions.

As described above, under more favorable economic conditions, \in always enters the platform. Entry can obviously be detrimental to *I*'s position, but the net effect on the incumbent's sales will also depend on the strength of network effects. If the local economic conditions are sufficiently benign (i.e., if $\tilde{\varepsilon}$ is sufficiently high), and if network effects are also present, the total impact on *I*'s sales can compensate for \in 's entry. In this case, training leads to a net increase in *I*'s sales, regardless of \in 's entry decision.

On the other hand, under sufficiently poor economic conditions, the effects of training are more nuanced and depend on the size of the incumbent's customer base. More specifically, as detailed above, if *I* has a sufficiently large pre-existing customer base for platform services, \in may enter the platform despite the unfavorable economic conditions. Yet, in this case, network effects might not be sufficient to offset the impact of \in 's entry decision. Furthermore, because the direct sales increase stemming from training (i.e., represented by the parameter *y*) tends to be small, it will also likely not be sufficient to compensate *I*'s sales loss. Therefore, in this scenario, training can lead to a reduction in the incumbent's sales.

Lastly, if *I* has a sufficiently small customer base, training will not lead to \in entering the platform (as per Proposition 1). While, in this case, there are no direct benefits from network effects, the direct sales increase caused by the intervention (*y*) means that the training may still have a positive—albeit small—effect on *I*'s sales. These insights are summarized in two propositions.

Proposition 2a. Under favorable economic conditions, and in the presence of sufficiently strong network effects, training provided to a platform complementor leads to an increase in their sales.

Proposition 2b. In situations of economic crisis, training leads to: (i) a reduction in the sales of the trained complementor, when there are local entry spillovers; or (ii) a (small) increase in the sales of the trained complementor, in the absence of entry.

Proof: see Appendix A1.

DISCUSSION

Literature on platform strategy and governance has emphasized the importance of demand-side factors, including the key role of network externalities in explaining platform performance (e.g., Li & Agarwal, 2017; McIntyre & Srinivasan, 2017; Rietveld & Eggers, 2018; Rysman, 2009). Nonetheless, despite the rich body of research in this area, scholars acknowledge that the drivers and consequences of complementors' entry are still to be fully explored and better understood (e.g., Rietveld & Schilling, 2021). The field-experimental results reported in this paper, together with the framework developed in the previous section, suggest that complementors' entry can be a critical mechanism to explain the performance of platforms aimed at serving disadvantaged populations, particularly during times of economic hardship. They also reveal, however, a complex interplay with network effects and local demand conditions.

My results indicate, first, that offering training support to fintech platform complementors operating in disenfranchised communities may lead to important entry spillovers. But the existence of network externalities implies that the net effect of these supporting policies on complementors' performance also crucially depends on demand-side factors. On the one hand, under regular or more favorable demand-side conditions—proxied, in my experiment, by communities not subject

to harsh Covid-19 restrictions—exposure to training support leads to an increase in both agents' app-based sales and in the number of agents operating in the community. As shown in my simple theoretical framework, the network effects stemming from the entry of new agents may interact positively with favorable demand-side conditions, thereby helping incumbents convert new customers and keep their sales growing, despite the new entrants.

On the other hand, a more nuanced story is observed in contexts of economic downturn. In such settings—exemplified by communities exposed to harsher Covid-19 restrictions—the impact of training on complementors' entry depends on the incumbents' existing customer base. According to my framework, the size of the customer base may be used by the entrant as a proxy for the size of the (potential) local market. Hence, even if economic conditions are unfavorable, the entrant may still decide to join the platform as a new complementor, in case the incumbent complementor has a sufficiently large customer base. Of course, the opposite should be expected—and is also observed in my data—when the incumbent has a small customer base.

As a result, in contexts of economic crisis, larger-customer-base complementors will likely fail to capture benefits from training. They might even be harmed by these interventions—as my experiment showed—since their inherently lower capacity to convert new customers combined with the presence of new entrants might end up reducing their sales potential. In turn, smaller-customer-base complementors may still benefit from training support initiatives: their improved customer-conversion potential (stemming from sales capabilities that were enhanced by training) coupled with null entry spillovers may lead to higher sales, following the intervention, despite the unfavorable economic conditions.

Contributions and limitations

This paper contributes to two distinct ongoing debates in the literature. First, while there is growing interest in social impact among strategy scholars (e.g., Barnett et al., 2020; Durand & Huysentruyt, 2022; George et al., 2016; Kaul & Luo, 2018; 2019; Luo, Kaul, & Seo, 2018), this research generally highlights firms' support to stakeholders—particularly those with disadvantaged backgrounds—as a crucial step to deepen their own social impact (e.g., Pongeluppe, 2022). My results, in contrast, suggest that some caution is warranted. Studying the case of a fintech platform that serves disadvantaged Brazilian communities, I show that complementors with more limited capacity to convert new customers, and who also faced unfavorable economic conditions—namely, strong Covid-19 restrictions—derived net losses from a training support initiative.

This finding highlights the complexity of effectively offering strategic support to stakeholders. Even in the case of disenfranchised stakeholders facing a crisis situation—a case where the potential benefits of supporting initiatives are enormous—these initiatives may backfire. Therefore, in creating and managing stakeholder support initiatives, a clear understanding of mechanisms at work is critical to avoid unpleasant surprises. Furthermore, the theory framework proposed in the paper shows that these arguments may be extended to other types of platforms—i.e., beyond fintech platforms—so long as complementors' demand functions are still locally defined.

In addition, this paper also speaks to the large body of research on platform strategy (e.g., Li & Agarwal, 2017; McIntyre & Srinivasan, 2017; Rietveld & Eggers, 2018; Rysman, 2009; Zhu & Iansiti, 2012; Zhu & Liu, 2018). As mentioned above, this literature has just begun to understand the drivers and consequences of complementors' entry (Rietveld & Schilling, 2021), a mechanism that, according to this paper, may be critical for platforms aimed at serving disadvantaged communities. Indeed, as mentioned above, my results suggest that complementors' entry can shape

the outcomes of strategic initiatives oriented to support existing complementors in contexts marked by severe economic crisis. Lastly, I also show that exposure to treatment—i.e., participation in a training and mentoring initiative—seemingly generated important entry spillovers in the community; this finding empirically documents a potentially relevant driver of complementor entry that future research can explore further.

Of course, while the theoretical framework I propose here offers insights into mechanisms that may be relevant more generally, it is worth noting that experiments can have limited external validity. The reader should, therefore, be cautious in generalizing my findings to other contexts, and more research is needed to ensure that they are applicable elsewhere. Similarly, the platform studied in this paper follows a model wherein complementors serve well-defined local markets. It is up for future research to determine whether the mechanisms explored here are also relevant in the case of platforms for which this local aspect is not as relevant.

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Table 1. Randomization summary

	Larger Custo	r Existing mer Base	Smalle Custo	er Existing mer Base	Sample
Business Sector	Control	Treatment	Control	Treatment	Total
Cyber Café	12	11	15	15	53
Clothing Store	10	9	13	15	47
Computer Shop	11	11	22	23	67
Grocery Store	16	15	26	25	82
Stationery Store	10	9	12	13	44
Sample Total	59	55	88	91	293

Table 2. Descriptive statistics and intergroup comparisons

]	Full sampl	e	Means - treated versus control				
Variables								
	Mean	Median	St. Dev.	Control	Treated	Difference	p-value	
Number of Employees in October 2019	1.69	2.00	1.26	1.71	1.68	0.03	0.40	
Monthly Revenues in October 2019 (log)	8.82	8.62	1.65	8.87	8.74	0.13	0.27	
Business Age (in months)	74.99	48.00	82.95	73.45	74.79	-1.34	0.45	
Agent's Schooling Level	4.41	4.00	1.25	4.39	4.46	-0.07	0.35	
Number of Agents in the Community	5.52	2.00	7.28	5.12	5.48	-0.36	0.32	
App Sales per Day (log)	1.66	1.73	1.16	1.60	1.67	-0.07	0.33	
Larger Existing Customer Base	0.39	0.00	0.49	0.38	0.37	0.01	0.33	
Relative Number of Bank Stores (log)	-9.18	-9.26	0.78	-9.18	-9.18	0.00	0.49	
Treated	0.50	1.00	0.50	-	-	-	-	

Variables	DV: Uses Marketing Materials	DV: Frequency Offers Service
	OLS	OLS
	(1)	(2)
Treated	0.2061*	0.3194+
	(0.0930)	(0.1738)
Total App Service Sales (month)	-0.0002	0.0005
	(0.0002)	(0.0004)
Total App Serv. Sales in the Community	0.0001	-0.0001
	(0.0002)	(0.0003)
Number of Agents in the Community	0.0031	0.0078
	(0.0120)	(0.0235)
Gender	-0.2144	-0.2806
	(0.1530)	(0.2372)
Race	-0.0649	0.0061
	(0.0401)	(0.0795)
Sector dummies	Yes	Yes
State dummies	Yes	Yes
Number of observations	143	145
Number of entrepreneurs	143	145
R-squared	0.27	0.29

Table 3. Is the theory of change supported?

Notes: Standard errors (in parentheses) are clustered at the agent level. Control variables are measured at the pre-treatment level. Gender is a dummy indicating respondents who identify themselves as males. Race is a categorical variable describing the race with which the respondents identify themselves. Dependent variables: Uses Marketing Material is a dummy indicating agents who reported the use of banners or other marketing materials related to the fintech's app services. Frequency Offers Service is a categorical variable describing the reported weekly frequency in which agents actively offer the app services to the customers in their stores. Significance levels: + p < 0.10; * p < 0.05; ** p < 0.01.

Variables	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.232					
	(0.192)					
Treated x Post October 2019	0.224 +	0.234*	0.230*	0.230*		-2.944*
	(0.132)	(0.110)	(0.110)	(0.111)		(1.241)
Treated x February 2020 Dummy					0.181+	
					(0.097)	
Treated x May 2020 Dummy					0.288+	
,					(0.148)	
Relative Number of Bank Stores x Pos	st October 2	2019				2.227*
						(1.103)
Treated x Relative Number of Bank St	tores					-0.346*
x I	Post Octobe	er 2019				(0.136)
Number of Employees in Oct. 2019	0.171					
	(0.106)					
Employees Hired in October 2019	-0.386*					
	(0.168)					
Business Age	0.003*					
	(0.002)					
Agent's Schooling Level	0.060					
	(0.075)					
Number of Agents in the Community	-0.194			-0.175		
	(0.126)			(0.152)		
Period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
City fixed effects	Yes	No	No	No	No	No
Sector fixed effects	Yes	No	No	No	No	No
Sector x Period fixed effects	No	No	Yes	Yes	No	No
Agent fixed effects	No	Yes	Yes	Yes	Yes	Yes
Number of observations	834	861	861	852	861	852
R-squared	0.63	0.74	0.75	0.75	0.74	0.75

Table 4. Baseline results: Average treatment effects

Standard errors (in parentheses) clustered at the agent level. The constant is omitted.

+ p < 0.10; * p < 0.05; ** p < 0.01.

Figure 1. Event-study plot for specification (5) in Table 4



	(1)	(2)	(3)	(4)
Variables	Small Customer Base	Large Customer Base	Full Sample	Full Sample
Treated x Post October 2019	0.464** (0.159)	-0.111 (0.134)	0.460** (0.158)	
Treated x Post October 2019 x Larger Existing Customer		-0.578** (0.208)		
Treated x February 2020 dummy x Larger Existing Customer	Base			-0.523** (0.188)
Treated x May 2020 dummy x Larger Existing Customer	Base			-0.635* (0.276)
Sector x Period fixed effects	Yes	Yes	Yes	Yes
Agent fixed effects	Yes	Yes	Yes	Yes
Number of observations R-squared	524 0.65	337 0.77	861 0.75	861 0.75

Table 5. Baseline results comparing large- and small-customer-base agents

Standard errors (in parentheses) clustered at the agent level. All other interactions and the constant are omitted. + p < 0.10; * p < 0.05; ** p < 0.01.

Figure 2. Effects of training on large- versus small-customer-base agents



	(1)	(2)	(3)	
Variables	Full Sample	Smaller Customer Base	Larger Customer Base	
Treated x Post October 2019	-0.440+	-0.126	-0.948**	
x Stronger Covid-19 restrictions	(0.241)	(0.341)	(0.289)	
Sector x Period fixed effects	Yes	Yes	Yes	
Agent fixed effects	Yes	Yes	Yes	
Number of observations	861	524	337	
R-squared	0.74	0.65	0.78	

Table 6. The role of the Covid-19 restrictions

Standard errors (in parentheses) clustered at the agent level. All other interactions and the constant are omitted. + p < 0.10; * p < 0.05; ** p < 0.01.

Figure 3.	. Comparing	treatment	effects	according	to	exposure	to	Covid-19	restrictions	and	agent
developn	nent stage										



	Community-level sample			
Variables				
	Mean	Median	St. Dev.	
Number of Agents in the Community	2.14	1.00	3.86	
Treated Agent in the Community	0.51	1.00	0.50	
At Least One Agent has Large Customer Base	0.40	0.00	0.49	
Stronger Covid-19 Restrictions	0.33	0.00	0.47	

Table 7. Descriptive statistics for the community-level sample

Table 8. Poisson community-level regressions. DV: Number of Agents in the Community

Variables	(1)	(2)	(3)
Treated Agent in the Community	0.140**		
x Post October 2019	(0.044)		
Treated Agent in the Community		0.127*	0.032
x February 2020 dummy		(0.052)	(0.072)
Treated Agent in the Community	0.153***	0.112*	
x May 2020 dummy		(0.044)	(0.044)
Treated Agent in the Community x February 2020 dummy		0.201*	
x At Least One Agent has Large Customer B		(0.102)	
Treated Agent in the Community x May 2020 dummy		0.084	
x At Least One Agent has Large Customer I	Base		(0.086)
Period fixed effects	Yes	Yes	Yes
Community fixed effects	Yes	Yes	Yes
Number of observations	831	831	831

Standard errors (in parentheses) clustered at the community level. Other interactions and the constant are omitted. * p < 0.05; ** p < 0.01; *** p < 0.001.



Figure 4. Poisson event-study regressions; stronger versus weaker Covid-19 restrictions. DV: Number of Agents in the Community

APPENDIX A1

Player \in 's (ex-ante) payoff is $\pi_{\in} = (h + \varepsilon b - q_I - q_{\in})q_{\in} - \overline{c}$, where q_{\in} if the quantity offered by \in and the other parameters were defined in the main text. Taking expectations, and maximizing the resulting expression, given that \in does not know about the training, leads to expected optimal quantities of $q_{\in}^* = q_I^* = (h + (\overline{\varepsilon} + \underline{\varepsilon})/2)/3$. Furthermore, it is easy to see that $\pi_{\in} = q_{\in}^{*2} - \overline{c}$. This leads to the condition expressed in Equation 1.

Proposition 1

Proof: by inspection of Equation 1.■

Propositions 2A and 2B

Proof: If \in enters, both players compete à la Cournot. The inverse demand functions are given by $p_I = h + \tilde{\varepsilon}b + yb - q_I - q_{\epsilon}$ and $p_{\epsilon} = h + \tilde{\varepsilon}b - q_I - q_{\epsilon}$. Pinning down the best response functions and solving simultaneously leads to the optimal quantities $q_{I,1}^* = (h + \tilde{\varepsilon}b + 2yb)/3$ and $q_{\epsilon}^* = (h + \tilde{\varepsilon}b - 2yb)/3$. On the other hand, if *I* does not train, it produces the baseline quantity of $q_{I,B}^* = (h + \tilde{\varepsilon})/2$. Thus the training effect (TE) is given by:

$$TE = \frac{\tilde{\varepsilon}(2b-3)+4yb-h}{6} \tag{a1}$$

It is easy to see that, all else equal, TE > 0, if $\tilde{\varepsilon}$ is sufficiently large, and TE < 0 if $\tilde{\varepsilon}$ is sufficiently low.

In contrast, if \in does not enter, $q_{I,0}^* = (h + \tilde{\varepsilon} + y)/2$ and TE = y/2 > 0. This is true even when $\tilde{\varepsilon}$ is low.