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**Peaceful Entry: Entrepreneurship Dynamics
During Colombia's Peace Agreement**

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PEACEFUL ENTRY: ENTREPRENEURSHIP DYNAMICS DURING COLOMBIA'S PEACE AGREEMENT

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ABSTRACT. The end of internal conflict are often shaped by political uncertainty and threats of violence recurrence. This implies that the effects of conflict termination on economic activity and specifically entrepreneurship can go in either direction, and we know little about this relationship. Studying Colombia's recent peace agreement with the FARC guerrilla, and using a *difference-in-differences* empirical strategy, we document that dynamics of entrepreneurship in traditionally violent areas closely mapped the politics that surrounded the peace agreement. When the agreement was imminent after a 5-decade conflict and violence had plummeted, local investors from all economic sectors established new firms and created jobs. Instead, when the agreement was rejected in a referendum, the party that promoted this rejection raised to power, and violence re-escalated, the rate of firms' creation rapidly reversed.

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1. INTRODUCTION

A large literature documents how violent conflict hinders economic activity by destroying physical capital and infrastructure, diverting resources away from productive activities, shattering human capital, and thwarting trust, social capital, and market efficiency. Indeed, the aggregate effects of conflict are substantial. For example, Collier (1999) estimates that civil war reduces the annual growth of GDP by 2.2 percent, and Rodrik (1999) argues social conflicts largely explain the instances of growth collapse observed since the 1970s.¹ The NGO *Institute for Economics & Peace* reckons that the economic impact of violence in 2021 was U.S. \$16.5 trillion, equivalent to roughly 10 percent of the global GDP and \$2,117 per person.² The effect of violent conflict on private entrepreneurship is especially daunting as entrepreneurship and business formation are strongly correlated with economic growth, productivity, investment, and employment in a wide range of countries (Aghion et al., 2004; Mueller, 2006; Acs and Varga, 2005; Ashcroft and Love, 1996; Carree and Thurik, 2008; Fritsch and Mueller, 2004; Van Stel and Suddle, 2008; Boeri and Cramer, 1992).³

Conceptually, the negative effects of conflict on entrepreneurship can be decomposed into two broad factors: those associated with the incidence and intensity of violence, which destroys capital and market infrastructure; and those associated with general uncertainty, which hurts investment. When a conflict ends, each of these channels may or may not be attenuated or completely shut depending on contextual factors such as the political legitimacy of the peace agreement or the presence of additional sources of violence. This implies that conflict termination should not unambiguously promote entrepreneurship, especially in the short run. For instance, as conflict exacerbates poverty, inequality, and social discord, it often creates the seeds for its recurrence (Blomberg and Hess, 2002; Koubi and Böhmelt, 2014). Moreover, peace “spoilers” recurrently attempt to undermine peace agreements by exerting political or economic pressure (Stedman, 1997; Newman et al., 2006; Hoddie and Hartzell, 2010; Le Billon, 2012). Finally, post-conflict is often shaped by a general lack of state capacity and scant institutional presence in the territory (Prem et al., 2022, 2020).

We study the effect of the recent efforts to end the five-decade-long conflict in Colombia on business

¹Similarly, de Groot et al. (2022) estimate that the incidence of violent conflicts since 1970 reduced global GDP in 2014 by 12 percent on average, with the largest burden due to civil war.

²See <https://rb.gy/z14pv> (last accessed 05/30/2023).

³Our paper focuses on the creation of firms both because it is a good proxy of the aforementioned concepts and because of data limitations regarding variables that capture other firms' dynamics.

formation. In October 2012, the Colombian government started peace negotiations with the *Revolutionary Armed Forces of Colombia* (FARC from the Spanish acronym), the largest and oldest guerrilla organization in the Western Hemisphere. After normal ebbs and flows, the main milestone of the peace process came on December 20 of 2014, when FARC declared a permanent ceasefire as a way to signal internal cohesion and a credible commitment to lay down their weapons. The ceasefire was largely met until replaced, on August 29 of 2016, by the definitive bilateral ceasefire and, shortly afterward, by the peace agreement. FARC violence almost completely disappeared. The peace agreement, however, was rejected in a low-turnout national referendum by a 0.5 percentage points vote margin. Moreover, the political party that campaigned for the ‘No’ vote also won the 2018 presidential election, following a platform that promised to ‘tear apart’ the peace agreement. At the same time, violence in former FARC strongholds re-intensified as other armed groups that did not take part of the peace negotiations attempted to contest those territories to expand their influence. In short, the political events that started with FARC’s ceasefire entailed significant variation in both violence levels as well as in political and economic uncertainty. We argue that this variation shaped the post-conflict dynamics of entrepreneurship in former FARC strongholds.

Specifically, using a difference-in-differences empirical strategy as well as detailed firm-level data, we find that the 2014 ceasefire and the subsequent violence reduction triggered an 8 to 13 percent differential increase in the entry of new firms in municipalities formerly affected by FARC violence. However, consistent with the increased uncertainty introduced by the results of the referendum, the threat of right-wing political leaders to sink the agreement, and a violence resurgence in former FARC strongholds, we also find that such positive effect was only short-lived: after two years, the excess firm creation completely disappeared.

Our findings are robust to a battery of tests, including the introduction of municipal and department-year fixed effects as well as controlling for differential trends parametrized by various pre-treatment controls. They are also unaffected by selecting the municipal controls with machine learning techniques to maximize their capacity to predict firm creation (Belloni et al., 2014) and their inclusion via inverse probability weighting (Abadie, 2005) or in a doubly-robust way (Sant’Anna and Zhao, 2020). The results are also unchanged by collapsing the pre and post-ceasefire periods to avoid serial correlation (Bertrand et al., 2004), to increase the overlap between treated and control areas

(Crump et al., 2009), to using different definitions of the treatment and the dependent variables, to restricting the control groups to areas affected by other guerilla organization with a similar ideology and tactics, and to performing a permutation test that randomizes the treatments across municipalities to obtain a distribution-free probability that our main result is driven by pure chance.

We also explore the characteristics of the economic dividend brought upon by the creation of new businesses in areas affected by FARC violence. To that end, we investigate the effect of the ceasefire on job creation, the size of the new firms, and their economic sector. Regarding employment, we document that incumbent firms in FARC-affected places do not experience any differential post-ceasefire change in the number of employees, as measured by two independent proxies. In contrast, new firms do differentially increase employment by a magnitude of between 20 and 30 percent. This suggests that the documented excess firm creation was concomitant with employment gains, rather than just triggering jobs reallocation from incumbent to new firms. We also find that the differential business creation takes place across the board, both in low value-added sectors such as agriculture and retail, and in sectors that entail higher value-added, investment and human capital such as manufacturing and tourism. However, most of the new firms created in FARC-affected areas are, at least in the short-run, micro businesses with low capital levels.

We further explore the extent to which key active post-conflict recovery programs implemented since 2017 amplify the economic benefits of the peace agreement. Conflict-affected places that benefited from either tax cuts targeted at the creation of formal employment or community-driven development programs experienced a post-ceasefire surge in firms' creation that persisted after 2016. This implies that government initiatives have the potential to offset, at least partially, the uncertainty generated by the political turmoil around the implementation of the peace agreement.

Regarding the mechanisms, we provide both quantitative and qualitative evidence suggesting that the short-lived peace dividend was the result of political events that generated economic and political uncertainty. Indeed, section 2.2 describes how some of the main institutional changes procured by the peace agreement were dismantled or threatened by the Colombian government, especially after the candidate whose main campaign promise was to dismantle the agreement won the presidency in 2018. It also discusses how a faction of FARC sabotaged the agreement by re-taking the arms and returning to illegality, pledging a lack of commitment from the government to implement the agreed policies and institutional changes. Moreover, we document that conflict levels

decreased in 2015 and 2016 but rapidly re-escalated since 2017, mimicking our findings regarding entrepreneurial activity. Finally, survey evidence that we analyze in that section suggests that these events negatively affected people’s optimism about the future as well as their perceptions about the improvement of security.⁴

To study the potential alternative mechanisms that could drive the post-ceasefire dynamics of entrepreneurship, we investigate the extent to which business competition explains the reversion in firm creation in the later post-ceasefire period. To this end, we look at temporal patterns of firm *exit* and find no evidence supporting this channel. We also explore whether pent-up demand in FARC-affected areas could explain the short-lasting economic recovery and we find no evidence of this. As a third alternative, we test the hypothesis that all the observed firm entry is driven by the formalization of pre-existing businesses. We conclude this is unlikely the case. Finally, we look at the possibility that internal migration may explain our results and reject this idea.

The contribution of this paper is multifaceted. First and foremost, there is very little research on how violence and uncertainty explain the relationship between conflicts and entrepreneurship. Our case study, in which following a ceasefire both violence and uncertainty experienced large variations, allows us to study these dimensions –even if we cannot disentangle them– and their policy implication for a successful conflict termination and a persistent peace. Indeed, we know little about the impact of conflict termination on firms’ creation and performance.⁵

Instead, most of the existing literature focuses on the effect of conflict on entrepreneurship.⁶ Collier and Duponchel (2013) show that the intensity of conflict is negatively associated with the size of firms in Sierra Leone, and Camacho and Rodriguez (2013) find that it increases the probability that relatively small and young firms shut down in Colombia. Also for that country, Roza (2018) estimates that violence lowers the prices of both inputs and outputs of exposed firms. Since the drop in output prices is larger, firms are forced to adjust their production down or leave the market. The fact that conflict pushes some firms to exit, while others remain in business can increase the

⁴Brück et al. (2011) suggests that extreme events such as natural disasters or terrorism can affect entrepreneurship directly via people’s opportunities and indirectly via people’s perceptions.

⁵One notable exception is Guidolin and La Ferrara (2007), who incidentally suggest that the end of a conflict is not unambiguously good for firms. A recent paper that also studies Colombia’s peace agreement but focuses in agricultural investments and exploits a different source of variation is de Roux and Martinez (2020), which finds demand-driven positive effects.

⁶Two thorough reviews include Blattman and Miguel (2010) and Brück et al. (2013). The latter introduces a special issue of the *Journal of Conflict Resolution* on entrepreneurship and conflict.

market share of the surviving firms. Indeed, [Prete and Di Maio \(2021\)](#) show that the relationship between conflict exposure and the firms' performance in Libya is convex for such firms. [Amodio and Di Maio \(2018\)](#) find that, in Palestine during the Second Intifada, conflict disrupted the supply of inputs, pushing exposed firms to substitute domestically produced materials for imported ones and reducing their output value. [Utar \(2020\)](#) documents that, in the context of the Mexican Drug War, urban violence reduced firms' output, product scope, employment, and capacity utilization. The fall in employment is driven by a drop in both labor demand (via output reduction) and labor supply (via death or disability). Moreover, the effect is larger for relatively small firms and for those intensive in female employment. Similar results are documented by [Ksoll et al. \(2021\)](#) for the case of Kenya, where electoral violence reduced exports primarily via workers' absence and especially for smaller firms. [Klapper et al. \(2013\)](#) show that conflict in Cote d'Ivoire reduced firms' total factor productivity, especially for establishments either owned by or employing foreigners. Finally, conflict also affects trade flows and exchange, both across firms and with external markets ([Hjort, 2014](#); [Korovkin and Makarin, 2021a,b](#)). Notwithstanding the massive evidence about the negative effects of conflict on the economy, we know much less about conflict termination.

Another contribution is our capacity to observe informal as well as formal economic activity. Our detailed firm-level data set records firms' creation based on a new business registry with the local chamber of commerce. Importantly, however, this does not imply formalization in terms of either social security contributions, tax contributions, or compliance with sanitary regulations. Instead, registration in the local chamber of commerce provides firms with benefits such as training workshops in accounting and marketing, and participation in product-specific market fairs. In sharp contrast to most of the existing literature, this implies that our results are valid for both formal and informal entrepreneurial activity.⁷

Finally, we also contribute to the recent literature on the (positive and negative) effects of conflict termination, most of which has focused on the case of Colombia. For instance, [Prem et al. \(2023b\)](#) and [Guerra-Cújar et al. \(2022\)](#) document that the ceasefire improved educational outcomes and increased fertility rates in formerly FARC-affected areas, and [Prem et al. \(2023a\)](#) and [Perilla et al.](#)

⁷Previous research on Colombia, that explores the effect of violence on firms exit uses a different data set that is however more restricted in the type of firms it allows to study. Indeed, [Camacho and Rodriguez \(2013\)](#) is limited to industrial plants with more than ten employees or with a production of at least US\$ 50,000 (in 2005 dollars). Unfortunately, however, our data do not allow us to separate formality from informality, to explore heterogeneous effects in this dimension. In addition, this does not imply that all informal economic activity is captured by our data set, as new businesses may choose not to register in the chamber of commerce despite the mentioned benefits.

(2023) find positive socio-economic effects of post-conflict landmine demining efforts, but Prem et al. (2020) show that it also increased large-scale deforestation and Prem et al. (2022) suggest that it triggered selective violence targeted toward local community leaders in areas disputed by illegal armed groups other than FARC. For the case of Somalia, Shortland et al. (2013) find a significant peace dividend for low-income households that varied according to the intensity of the conflict. A similar heterogeneity is documented by Serneels and Verpoorten (2015) for the case of Rwanda, and the authors argue that this is because conflict intensity shapes the pace of post-conflict recovery.

2. CONTEXT

2.1. Colombia's civil conflict and the peace process. Colombia's civil conflict started with the foundation of left-wing guerrillas FARC and the *National Liberation Army* (ELN from the Spanish acronym) in the mid-1960s. Guerrillas claim to represent the rural poor and fought for over 50 years with the stated aim of overthrowing the government. To finance their insurgency, both groups have been profiting from several forms of illegal activities, localized within the Colombian territory (Richani, 1997). Thus sub-national territorial dominance is an important intermediate objective of the illegal armed groups, and the infliction of violence on both military and civilian targets is a means of achieving it.

The conflict escalated during the 1990s, fueled by the guerrillas' involvement in illegal drug trafficking and the consolidation of anti-insurgent right-wing paramilitary groups. The three-sided, protracted Colombian conflict resulted in 8.8 million people formally registered with the state as victims of the conflict.⁸

In October 2012, the Colombian government and FARC, the largest guerrilla, started peace negotiations in Cuba. While constant ebb and flow characterized the four-year-long process, one of the most significant milestones was establishing a permanent ceasefire by FARC on December 20, 2014. As a result of the ceasefire, FARC withdrew their troops to more remote areas where military contact with government security forces and other armed groups was unlikely to occur. This explains why FARC's offensive activities dropped by 98% during this period (CERAC, 2016). The

⁸Source: Victims' Registry, from the Unit for the Victims Assistance and Reparation (<https://www.unidadvictimas.gov.co/en>).

negotiations were completed in June 2016 and the final peace agreement was signed in September that year.

2.2. Rejection of the agreement and implementation hurdles. President Juan Manuel Santos sought public ratification of the peace agreement via a national referendum that was held on October 2, 2016. Former president Alvaro Uribe, who was elected in 2002 under a mandate of ending the internal conflict through military means, successfully led the ‘No’ campaign. In an election shaped by a record-low turnout, the agreement was rejected by a vote margin of 0.5 percentage points.

These events led to large economic and political uncertainty.⁹ While Santos sought to mitigate the referendum blow by making changes to the agreement (incorporating key concerns of the opposition and having Congress ratify the revised document), the political legitimacy of the peace agreement was already heavily undermined. The referendum exacerbated the political polarization that the peace negotiation had created (with the right accusing Santos of being a traitor), and the party that led the ‘No’ campaign canvassed for the upcoming presidential elections (2018) by promising to ‘tear apart’ the agreement and halt its implementation. Their candidate (Ivan Duque) ultimately won and the structural reforms that the agreement vowed were indeed either blocked or significantly slowed down.

But failures and delays in the implementation of the peace agreement were already underway since the early 2017 –before Duque took office–both because of administrative slowness and lack of political support. For instance, the government failed to timely equip with basic infrastructure and services the areas designated by the agreement for the demobilization and reincorporation of FARC combatants; judges throughout the country were slow to apply the mechanisms incorporated in the amnesty law designed by the agreement; and the Special Investigation Unit of the Office of the Attorney General conceived by the agreement to dismantle paramilitary groups and their economic and political support structures was widely criticized by its lack of leadership and efficiency.¹⁰ Moreover, as a measure of reparation to the victims of the armed conflict, the peace agreement instructed that the House of Representatives should host transitory seats for 16 victims, representing the 16 areas of the country that were most affected by the armed conflict. However,

⁹On the economic front, in the aftermath of the referendum, the Colombian Peso depreciated, the yield of public bonds dropped, and the stock market shrank. See <https://rb.gy/b7eht> (last accessed 05/30/2023).

¹⁰See <https://rb.gy/987om> and <https://tinyurl.com/mryra4kz> (last accessed 05/30/2023).

at the end of 2017, the Congress blocked the directive. The opposition feared that this would give additional congressional power to FARC –outside the 10 seats that the agreement secured for its leadership—since the victims would represent their former strongholds.¹¹

As a silver lining, 2017 marked the creation of two policies that sought to provide support and financial resources to the post-conflict economic and social reconstruction. As a salient exception, these policies have been at least partially implemented. On the one hand, the *Development Programs with a Territorial Approach* (PDET) was launched by Decree 893 of 2017. Effectively, PDET are community development programs targeted at rural areas affected by conflict and featuring illicit economies.¹² They are planned to last for 10 years and their objective is to provide institutional and financial support to the targeted communities to help them design and implement comprehensive rural reform programs, aimed at the transformation of the rural sector and focusing on areas such as rural property, infrastructure, and land use; agricultural production; rural education, housing, and basic services; food security; reconciliation and peace; and environmental sustainability. On the other, through Decree 1650 of 2017, the Ministry of Finance, the National Planning Department, and the Territory Renewal Agency identified 344 municipalities (about a third of the country) as the *Most Affected Areas by the Armed Conflict* (ZOMAC from its Spanish acronym).¹³ The government offered tax benefits for ten years to companies that developed their entire production process in these areas and complied with minimum investment and job creation levels.

However, upon taking office in 2018, President Duque sent clear signals that the anti-agreement campaign promises were to be fulfilled. Among other gestures, Duque objected to the Special Jurisdiction for Peace, a high court designed by the agreement to administer the transitional justice regarding crimes committed as part of the armed conflict. The president also appointed a known right-wing intellectual and conflict denier as director of the National Center of Historic Memory, tasked with the objective of recovering, compiling, and conserving all the written material and oral testimonies regarding the violations that occurred in the context of the armed conflict.

Beyond these salient examples, the fact of the matter is that the peace agreement has been poorly

¹¹See <https://tinyurl.com/35vxt34t> (last accessed 05/30/2023).

¹²A total of 170 municipalities in 19 departments were selected as part of this program.

¹³The methodology used for the selection of these areas is explained in: <https://rb.gy/y10mu> (last accessed 05/30/2023).

implemented (especially so during our sample period). University of Notre Dame's Kroc Institute for International Peace Studies is the institution in charge of the technical verification and monitoring of the implementation of the peace accord, and it releases periodic assessment reports. According to the Kroc Institute, the share of the implementation goals that the agreement had set for the first four years was only met by the end of 2020 in 28 percent. Moreover, this statistic merely grew two percentage points from December 2019, when it had reached 26 percent. This suggests that the implementation of the accord was very modest, both in levels and growth.¹⁴ The Kroc Institute also highlights that the pace of implementation has significantly decelerated since Duque took office. Indeed, the only implementation project that the Duque administration submitted to Congress was the creation of an Agrarian Jurisdiction, aimed at resolving land conflicts in rural areas. However, the government purposefully did not lobby to include the initiative in the agenda, and so it sank as the timing for discussing it expired. Duque also reduced the budget of the National Land Agency, whose main goal is following the guidelines of the peace accord regarding rural development policies.¹⁵ By and large, the evidence from the institution in charge of the verification of the implementation suggests that the Duque administration largely obeyed its campaign promise of boycotting the peace agreement.

In this context, several of the FARC commanders who had signed the agreement—led by a.k.a. Iván Marquez, who was the chief negotiator of FARC in Havana—deserted and re-took the arms, pledging a lack of commitment from the government to implement the accord as well as the increased vulnerability of the accord signatories. Indeed, by the end of our sample period about 130 demobilized FARC members had been killed since the peace agreement was signed at the end of 2016.¹⁶ The return to the illegal armed struggle of the at least 3,000 combatants that joined them constitutes a threat to the stability of the accord.

But the FARC ex-combatants are not the only social group that has been disproportionately targeted since the agreement was reached: by the end of our sample period, over 500 human rights activists and social leaders were killed. As shown by Prem et al. (2022), this phenomenon responds to territorial disputes that the FARC retreat triggered on its former strongholds, especially among other illegal armed groups and in a context of little territorial state presence.

¹⁴See <https://tinyurl.com/kftp2tve> (last accessed 05/30/2023).

¹⁵See <https://tinyurl.com/2s35bmeb> (last accessed 05/30/2023).

¹⁶See <https://rb.gy/fag5b> (last accessed 05/30/2023).

These patterns are related to the dynamics of conflict-related violence in Colombia, which largely decreased in 2015 and 2016 following the ceasefire and then re-escalated starting in 2017, after the agreement was reached (see Figure 1, Panel A). Relative to the pre-peace negotiation period (2011-2014), in 2015 and 2016 (the period between the declaration of the ceasefire and the attainment of the agreement) most types of violence significantly dropped, especially that perpetrated by FARC (which suggests that the ceasefire was largely met, and any remnant violence was that exerted by the FARC dissidence). During this period the violence perpetrated by armed groups other than FARC remained stable, but it surged in 2017 and 2018.

Ultimately, the political uncertainty caused by: the rejection of the agreement in the referendum; the opposition of the right-wing political coalition to the implementation of the agreement; the return of a faction of FARC to the illegal armed struggle; the assassination of local activists and of ex-combatants; and the restoration of a high violence equilibrium makes the “implementation” period (from 2017 onward) one characterized by high levels of uncertainty. We argue that this is the main reason for the empirical patterns that we uncover in this paper, namely that, following the ceasefire, the creation of new firms differentially increased in FARC-affected areas only in 2015 and 2016, but then differentially plummeted in 2017 and 2018.

Qualitatively, several polls suggest that general satisfaction with the peace agreement decreased in the second period relative to the first. For instance, the Americas’ Barometer (a poll of the Los Andes University’s Democracy Observatory) surveyed in 2015 and 2017 a sample of rural households from four regions heavily exposed to the armed conflict and with historic presence of FARC. The proportion of respondents that supported the peace *process* in 2015 was 63%, but that that supported the peace *agreement* two years later was 10 percentage points smaller.¹⁷ Moreover, in Panel B of Figure 1, we aggregate for our substantive periods of interest the findings of DATEXCO’s periodic nationally representative public opinion poll, *Pulso País*. The share of respondents that report being positive about the future follows an inverted-U pattern similar to the one we document for the dynamics of firm entry: it increases (from 35 to 50 percent) in 2015-2016 relative to the pre-ceasefire period, and then decreases (to 28 percent) in 2017-2018. A similar pattern emerges with the share of respondents that report that security is improving: it increases (from 12 to 22 percent) in 2015-2016 relative to the pre-ceasefire period, and then decreases (to 15 percent) in

¹⁷See <https://rb.gy/3c2t9> (last accessed 05/30/2023).

2017-2018.

3. DATA

3.1. Firms. Our main outcome is the creation of new firms. As mentioned in the introduction, business formation has been shown to be strongly correlated with economic growth, technical change, investment, and employment. Thus our main outcome is a broad proxy of economic performance. The data come from the business demographics of a database called Unique Business and Social Registry (RUES from its Spanish acronym), which includes the country’s Registry of Commerce.¹⁸ RUES is maintained by *Confecámaras*, the umbrella organization that coordinates Colombia’s 57 local Chambers of Commerce. The Registry of Commerce includes the location of the firm’s establishments, its legal representation and statutory audit, the firm’s economic activity, some financial information, and the current state of the firm (whether active or canceled). Each new firm must register with its local Chamber of Commerce within one month of starting its commercial activity. Moreover, they must renew their registry during the first three months of each year. This legal obligation allows *Confecámaras* to engage in a longitudinal monitoring of all private firms. Importantly, registration does not entail that the firm must pay either social security contributions or taxes, and registered firms may or may not comply with sanitary regulations. This implies that not all registered firms are formal (according to various formalization definitions), and so we can capture both formal and informal economic activity.¹⁹ This is important as FARC-affected areas are mainly rural, and informality is generally more prevalent among smallholder farmers.

RUES encompasses a total of six systems that consolidate the information of the 57 local Chambers of Commerce. Five of them are managed by the Chambers of the largest five cities, but host the records of 292 municipalities (26% of the country, holding 56% of the firms). The remaining system is directly maintained by *Confecámaras* and hosts the information of the rest of the country.

Following the *Manual on Business Demography Statistics*, RUES records the movement of firms

¹⁸RUES’ business demographics variables strictly follow the *Manual on Business Demography Statistics* developed by the European Union and the OECD. This ensures standardization of firm demographic events such as births or firm creation.

¹⁹In Colombia there are four formalization dimensions, and firms can (and often do) comply with a subset only: i) formalization of its creation (registering with the chamber of commerce); ii) formalization in inputs (e.g., paying social security for the firms’ employees); iii) formalization in production and commerce (e.g. complying with sanitary regulations); and iv) fiscal formalization (paying taxes). See the “Business Formalization Policy” document of the *National Council of Economic and Social Policy* (CONPES from the Spanish acronym). CONPES Policy Document No. 3956, available in the original Spanish form <https://tinyurl.com/4wew6r5m> (last accessed 01/14/2022).

over time. For example, if a firm cancelled its commercial registration to transfer its commercial address to the jurisdiction of another Chamber, both registries will appear in RUES with the same firm's unique ID. This avoids counting such transfers as firm creation. Similarly, a firm that stops its commercial activity for less than three years and then re-registers with a Chamber is registered with the old ID. Thus, it is not counted as a new firm. Finally, the firm creation statistic does not include entries due to statutory reforms such as mergers, break-ups, or split-off.

Therefore, a *new firm* in our data is a business that began its commercial activity for the first time in a given year, regardless of its size, economic activity, or whether it has any employees. Such a firm must register to the Registry of Commerce and obtain a unique ID. We had access to yearly RUES cross-sections from 2011 to 2018. This defines our sample period. To identify the firm's location in our sample, we used the firm's most recent record. It is also worth mentioning that, in addition to firm entry, we also use RUES to compute a range of additional variables that we use in the paper for robustness and to explore potential mechanisms. These include the firm's size, the economic sector of new firms, and various employment variables. We explain each of these additional measures in their respective paper section.

3.2. Conflict. To construct a measure of exposure to FARC violence prior to the start of the cease-fire, we use the conflict dataset originally compiled by Restrepo et al. (2004), and updated through 2014 by Universidad del Rosario. This dataset codes violent events recorded in the *Noche y Niebla* reports from the NGO *Centro de Investigación y Educación Popular* (CINEP) of the Company of Jesus in Colombia, which provides a detailed description of the violent event, its date of occurrence, the municipality in which it took place, the identity of the perpetrator, and the count of the victims involved in the incident.²⁰

We first created a discrete variable that measures the extensive margin of FARC violence. This variable takes the value one if there was at least one FARC attack from 2011 to 2014 in a municipality. This is the period elapsed after President Juan Manuel Santos took office and before the beginning of the permanent ceasefire. We also present results using two variants of this definition. The first one is a continuous measure based on the total number of FARC attacks over

²⁰*Noche y Niebla* sources include "1. Press articles from more than 20 daily newspapers of both national and regional coverage. 2. Reports gathered directly by members of human rights NGOs and other organizations on the ground such as local public ombudsmen and, particularly, the clergy." (Restrepo et al. 2004, p. 404). Notably, since the Catholic Church is present in even the most remote areas of Colombia, we have extensive coverage of violent events across the entire country.

10,000 inhabitants that took place from 2011 to 2014 in a municipality. The second is a discrete measure that identifies municipalities' highly exposed to FARC violence. To compute the latter we defined a dummy that takes the value of one for places above the bottom quartile of the continuous measure, thus not including municipalities with few events relative to their population. Finally, we created another measure of conflict intensity by defining a dummy that takes the value of one for municipalities that experienced in 2014 FARC attacks above the median.

3.3. Descriptive statistics. Table A.1 reports descriptive statistics of our main variables and exposure measures during the period 2011-2014. On average there are 83 new firms entering a municipality every year, with a large standard deviation of 191, which is consistent with the large heterogeneity of the country's municipalities and in terms of economic activity. In turn, 9% of the municipalities experienced at least one attack by FARC in the period 2011 to 2014.

Table A.2 in the Appendix presents the average of the characteristics during the period 2011-2014 for municipalities non-exposed to FARC violence and the difference between municipalities exposed and not exposed. FARC-exposed municipalities have, on average, a larger number of firms entering the market, a higher population, a higher share of the rural population, a higher poverty index, and are further away from the department's capital.²¹

4. EMPIRICAL STRATEGY

4.1. Main specification. Our identification strategy exploits the timing of the permanent ceasefire announced by FARC on December 20, 2014, as well as the spatial distribution of the exposure to FARC violence across municipalities prior to the ceasefire. More formally, using the sub-index m to denote municipalities, d to denote departments, and t to denote time, we estimate the following *difference-in-differences* regression model:

$$(4.1) \quad y_{mdt} = \alpha_m + \lambda_{dt} + \beta(Cease_t \times FARC_m) + \sum_{c \in \mathbf{X}_m} \gamma'(c \times \delta_t) + \varepsilon_{mdt}$$

where y_{mdt} is our measure of firm creation in the baseline specification, or any of the additional outcomes we study to understand the potential mechanisms. $FARC_m$ measures pre-ceasefire exposure to FARC violence in municipality m , and $Cease_t$ is a dummy that takes the value one after

²¹The information on municipal characteristics comes from an annual panel of Colombian municipalities, constructed by the *Center of studies on Economic Development* (CEDE by the Spanish acronym), a think-tank at Universidad de los Andes.

the start of the permanent ceasefire. α_m are municipality fixed effects and λ_{dt} are department \times year fixed effects. These control, respectively, for any observed or unobserved municipal-level time-invariant heterogeneity and for any temporal shock that affects simultaneously all the municipalities of the same department. X_m are municipality characteristics measured before the ceasefire that we interact with the full set of time fixed effects to flexibly control for differential time changes parametrized by each one of the municipal attributes.²² Finally, $\varepsilon_{m dt}$ is the error term, which we cluster at the municipality level. As a robustness, we estimate p-values using a variance-covariance matrix that takes into account cross-sectional dependence in the error term following Conley (1999) and Conley (2016).

Our coefficient of interest, β , captures the differential change before and after the ceasefire in firm entry in municipalities exposed to FARC violence versus those that were not exposed to FARC violence. We also estimate a variant of equation (4.1) where we split the ceasefire period into two, based on the peace agreement plebiscite that took place at the end of 2016. In that specification, the coefficient β of equation (4.1) becomes β_1 for the years 2015 and 2016, and β_2 for the years 2017 and 2018. 2018 is the end of our sample period due to data limitations (see section 3.1).

4.2. Identifying assumption. The main assumption behind our *difference-in-differences* model is that, in the absence of the ceasefire, firm creation in municipalities exposed to FARC violence would have evolved similarly to firm creation in non-exposed municipalities. The validity of this “parallel trends” assumption can be partially assessed by estimating the following equation:

$$(4.2) \quad y_{m dt} = \alpha_m + \lambda_{dt} + \sum_{j \in T} \beta_j (FARC_m \times \delta_j) + \sum_{c \in \mathbf{X}_m} \gamma'(c \times \delta_t) + \varepsilon_{m dt}$$

where δ_j are year dummies and T includes all years in our sample except from 2014, which is the year before the ceasefire. Therefore, the parameters β_j can be interpreted as the difference in firm creation in municipalities exposed to FARC violence and municipalities non-exposed, in year j relative to the year at the end of which the ceasefire started.

4.3. Potential mechanisms. We can use variation across municipal-level characteristics to estimate heterogeneous effects that may shed light on the underlying mechanisms of the effect of the ceasefire on firm creation. To that end, we augment the main specification (equation 4.1) by adding a third interaction term. Specifically, let the municipal characteristic Z_m (measured before

²²The set of characteristics includes the logarithm of population, the share of rural population, a poverty index, and the log distance to the department capital.

the ceasefire) be a potential mechanism of interest. We then estimate:

$$(4.3) \quad y_{m dt} = \alpha_m + \delta_{dt} + \tau_1(Cease_t \times FARC_m \times Z_m) + \tau_2(Cease_t \times Z_m) + \tau_3(FARC_m \times Cease_t) \\ + \sum_{c \in \mathbf{X}_m} \gamma'(c \times \delta_t) + \mu_{m dt}$$

Our coefficient of interest, τ_1 , captures the differential change in firm creation in places exposed to FARC violence in municipalities with characteristic Z_m . The set of characteristics, Z_m , includes a pre-ceasefire conflict intensity indicator, the average transfers received by the municipality from a higher government level, population density, and the distance to the main near markets. Note that the results coming from this test are suggestive about potential mechanisms, but not necessarily causal. They have to be interpreted with caution.

Using the above specifications, we estimate the impact of the December 2014 permanent ceasefire on firm entry in areas previously exposed to FARC violence (equation 4.1), the differential pre-trends and dynamic persistence of this effect (equation 4.2), and key heterogeneous effects (equation 4.3). The next section reports the estimated results and robustness tests.

5. MAIN RESULTS

Table 1 reports the coefficients resulting from estimating equation (4.1) using firms' entry as the dependent variable. In Columns 1 to 3, we bundle the ceasefire period from 2015 to 2018. Instead, Columns 4 to 6 split the ceasefire between the periods 2015-2016 and 2017-2018. Municipality and year fixed effects are included in all specifications. Columns 2 and 5 control for control for differential changes in firms' entry parametrized by several pre-ceasefire municipality characteristics (as discussed in Section 4.1). Columns 3 and 6 further add department×time fixed effects. The standard errors in parentheses are clustered at the municipality level, while square brackets report the p-values for standard errors that allow for spatial and first-order time correlation (see Conley, 1999, Conley, 2016). In addition, in Columns 4 to 6, we present the p-values of a test of the difference in the coefficients associated with the interaction between FARC and years 2015-2016, and the interaction between FARC and years 2017-2018.

The first three columns suggest that there is a positive, small, and insignificant increase in firms' creation in places traditionally affected by FARC violence, after the start of the ceasefire. Based

on Column 3, the magnitude of the differential increase in firm creation is 4.1 percent.²³ However, this effect masks a large heterogeneity between the early and later years after the start of the ceasefire. Over the first two years, the ceasefire triggered a differential increase in firms' creation in FARC-affected areas of between 8.3 and 12.7 percent depending on the model specification. Instead, starting in 2017, this trend reverses and we find an insignificant effect that ranges between 0 and -7.7 percent. Moreover, regardless of the specification, the difference between these coefficients is statistically significant (with **p-values** ranging from 0.00 to 0.06).

We interpret this heterogeneity as consistent with the political dynamics described in section 2.2 and summarized in Figure 1. Initially, positive expectations and optimism about an approaching peace agreement, coupled with a substantial reduction in the violence levels, incited new investments in municipalities traditionally exposed to FARC violence. Indeed, these same expectations and safety, were manifest in a large improvement in the educational outcomes of these same places (Prem et al., 2023b). However, the fiasco of the rejected peace referendum and the uncertainty that was concomitant to the empowerment of the political losers of the peace agreement (Fergusson, 2019) and to the new violence surge led by other armed groups, led to a subsequent differential decrease in firms' entry.

5.1. Identifying assumption. To partially validate the parallel trends assumption and to study the temporal dynamics of firms' creation after the start of the ceasefire, we estimate equation (4.2). The estimated coefficients for our baseline specification are plotted in Figure 2. We find no differential change in the entry of new firms in municipalities exposed to FARC violence before the ceasefire, with the estimated coefficients moving around zero. This points to the absence of differential pre-trends in firm creation and thus provides support for the use of a *difference-in-differences* empirical strategy. In addition, the figure also reveals visual evidence consistent with the reported time heterogeneity in the differential formation of new businesses following the ceasefire, with a large differential increase in treated municipalities in 2015 and 2016 and a reversion thereafter.

We also conduct a more parametric test for the existence of differential trends during the pre-ceasefire period (2011-2014), in the spirit of Muralidharan and Prakash (2017). Specifically, we interact a linear trend with our measure of exposure to FARC violence and test for the significance

²³As suggested by Bellemare and Wichman (2020), we compute the percentage change in the outcomes subject to a hyperbolic sine transformation (such as firms' entry) as $e^{\beta} - 1$.

of this coefficient prior to the ceasefire.²⁴ The results are reported in column 1 of Table 2 and point to the lack of differential trends in the rate of firms' creation before the ceasefire.

Finally, we also perform a placebo exercise using the date on which the government and FARC achieved the first important milestone of the peace process. On May 26, 2013, the parties reached an agreement on the first point of the peace negotiation agenda, namely the need for a comprehensive rural reform including regulating land use and access, discouraging unproductive land, improving land property titles, investing in rural infrastructure, and providing technical assistance and subsidies to improve agricultural production. This was the first out of six partial agreements reached prior to signing the final peace accord in September 2016.

The regressions for this exercise follow the structure of equation (4.1) but, instead of a *Cease* time indicator, we include a *Placebo Cease* one, which takes the value of one for the years 2013 and 2014, and another that takes the value one for the years 2012 to 2014. For this analysis, we focus on the sample period between 2011 and 2014, so as to capture potential pre-ceasefire trends. We find that there is no differential change in firm entry in areas exposed to FARC violence relative to FARC-free areas after this agreement was reached (see Table 2 columns 2 and 3). These results are consistent with the absence of differential pre-ceasefire trends and provide credibility to our main finding, namely that the differential evolution of firm creation between these two types of municipalities is driven by the ceasefire.

5.2. Further robustness. We now assess the robustness of our main findings to a series of empirical exercises that we present in this subsection.

5.2.1. *Municipality covariates.* We begin by augmenting our most demanding specification by adding a larger list of potential municipality characteristics. To that end, we follow Belloni et al. (2014) and select the set of pre-ceasefire municipality characteristics using machine learning. Column 4 of Table 2 shows that the results are robust to this exercise.

5.2.2. *Standard errors.* To assess the robustness of our standard errors, we follow Bertrand et al. (2004) and collapse our data before the ceasefire, during early years after the start of the ceasefire,

²⁴The specification we run is $y_{m dt} = \alpha_m + \lambda_{dt} + \beta(FARC_m \times Trend_t) + \epsilon_{m dt}$, where $Trend_t$ is a linear trend and we restrict the sample to the years 2011 to 2014. Our parameter of interest, β , captures whether there are differential trends between municipalities exposed and not exposed to FARC.

and over the later years. We do so to deal with potential serial correlation. Column 5 of Table 2 shows that our results are also robust to this practice.

5.2.3. *Comparison municipalities.* One threat to our identification is that municipalities exposed to FARC violence are different from areas not exposed and that in 2014 there was some “shock” (other than the ceasefire) that differentially affected these municipalities because of such characteristics but not because of the prior exposure to FARC violence. To alleviate this concern, we estimate our main model using a different set of control municipalities, which we select following Crump et al. (2009). Based on an estimated propensity score, we truncate the sample to increase the overlap of treated and control municipalities in terms of various municipality characteristics.²⁵ We perform this truncation using the optimal cut-off suggested by Crump et al. (2009), which in our case is 4.3%. Column 6 of Table 2 shows that our results are also robust to this sample truncation strategy.

In addition, we estimate a version of the treatment effects weighting by a propensity score of the probability of being exposed to FARC violence. First, we follow Abadie (2005) and use an inverse probability weighting for our difference-in-differences estimator, which considers that there may be differences in observed characteristics between treated and control municipalities. Then, we follow Sant’Anna and Zhao (2020) and estimate a doubly-robust version of our estimate that is robust to either a misspecification of the propensity score used for weighting or a misspecification of the linear model. Again, we find effects of similar magnitudes and significance (see Columns 7 and 8 of Table 2).

Finally, we use municipalities affected by the other main guerrilla group, namely ELN, as the control group. Areas affected by ELN are also exposed to violence from a left-wing non-state actor, with an ideology and guerrilla tactics similar to those of FARC. However, since the peace agreement did not include ELN, these areas remained violent after the start of the ceasefire. We conduct two exercises. First, we use ELN-affected areas as the control group. Second, we use them as a placebo treatment in an estimation that excludes FARC-affected areas.

The results of both these exercises are reported on Table A.8, and they confirm our findings. That is, even when restricting the control sample to ELN areas we find a significant increase in business’ formation (and of the same size) only in the first two years after the start of the ceasefire. Moreover,

²⁵ The set of potential controls includes the logarithm of population, the distance to department capital, the share of rural population, and a poverty index.

when we drop FARC-affected areas and use ELN violence as the cross-sectional placebo treatment, we obtain a very small and insignificant effect, both when looking at the entire post-ceasefire period and at the first two years.²⁶

5.2.4. *Measurement of the dependent variable.* We now check whether our results are robust to different ways of measuring our outcome variable. First, instead of using the hyperbolic transformation, which helps us to deal with cases when in a municipality-year level there was no firms' creation, we use the logarithm of firm entry plus one. Second, instead of using a logarithmic transformation, we use the number of entrants as our dependent variable. Columns 9 and 10 of Table 2 show that our results are robust to the way we define our dependent variable.

5.2.5. *Measurement of exposure to FARC violence.* Columns 11 and 12, instead, report the robustness of our baseline results to using two alternative measures of exposure to FARC violence. The first one (column 11) is a continuous measure of conflict based on the total number FARC attacks between 2011 and 2014 over the municipal population. For interpretation, this measure is standardized. The second (column 12) is a more stringent measure of "high exposure" to FARC violence. It is an indicator that takes the value of one for municipalities above the bottom quartile of the empirical distribution of the per capita FARC attacks, conditional on experiencing at least one attack. In column 11, we find that a one standard deviation in FARC violence per capita increases firms' entry by 5.5 percent, while in column 12, we find a similar but somewhat larger effect in municipalities with high exposure to FARC violence prior to the start of the ceasefire.

5.2.6. *Randomization inference.* We also conduct a permutation test by randomly assigning the FARC exposure indicator across municipalities over 200 times, with the random assignment being consistent with the observed distribution of municipalities exposed to FARC violence. This provides us with a distribution-free estimate of the probability that our coefficient of interest arises by chance. Reassuringly, our estimated coefficient (red vertical line) is above the 99th percentile of the resulting distributions of the effect of the ceasefire on firms' entry during the early post-ceasefire period (Panel A of Appendix Figure A.2).

5.2.7. *Influential geographical units.* Finally, we check whether our findings are driven by a particular treated municipality or by one specific department.²⁷ Appendix Figure A.1 presents the

²⁶The effect for the 2017-2018 period is however negative and significant, suggesting that first creation differentially dropped in these areas during the last two years of our sample period.

²⁷Colombia has 32 departments, equivalent to US states.

robustness of our results to both of these tests. By and large, all coefficients remain stable and statistically significant.

6. ADDITIONAL RESULTS

In this section, we explore additional outcomes that help us understand the magnitude and nature of the economic dividend implied by the documented differential increase in the number of firms. We do so in terms of job creation and regarding the economic sector and the size of the new businesses. Understanding these dimensions is important insofar as the impact of entrepreneurship on well-being and economic growth depends on the capacity of new firms to create employment, on the productivity of the entrant businesses (Van Stel and Suddle, 2008; Mueller, 2006), and on the size of the new establishments (Baldwin and Picot, 1995). Further, to appreciate the potential complementarity between private investment incentives during post-conflict and public policy support to economic recovery, we also explore the extent to which government programs can help harvest the economic peace dividend in municipalities formerly affected by violence.

6.1. Firm creation and employment. We start by studying the effect of the ceasefire on two different employment measures. The first is the number of employees reported by the new firms on the registry (Panel A of Table 3). While this measure is self-reported, it has the advantage of potentially encompassing all types of employment, including informal jobs. The second comes from merging RUES with Colombia’s employer-employee administrative data set –*Planilla Integrada de Liquidación de Aportes* (PILA). This measure only picks up the formal employment –for which social security contributions are made–(Panel B). Importantly, we divide total employment into that of incumbent firms (Columns 1 and 3) and that of newly created businesses (Columns 2 and 4). This distinction is key to understand if the arrival of new businesses is concomitant to the *creation* of new jobs or if employment simply reallocates from old to new businesses, with no actual job creation gains.

We find that the number of employees does not change in incumbent firms after the start of the ceasefire in FARC-affected areas relative to other municipalities. While the estimated coefficients are mostly positive, their magnitude is very small (up to 5 percent), both for the entire post-ceasefire period (Column 1) and for each of the substantive sub-periods (Column 3). This suggests

that incumbent firms are not losing employees, but they are not creating new jobs either. Instead, there is a substantial differential employment increase in new businesses, created after the start of the ceasefire. This is true for both employment measures. Moreover, this effect is present in both the substantial ceasefire sub-periods (Column 4). Focusing on the self-reported employment measure (Panel A), we find a differential increase in FARC-affected areas of 31 (23) percent in the 2015-2016 (2017-2018) period. Regarding the administrative measure of formal employment (Panel B), the differential increase in FARC-affected areas is 20 percent in both periods. We conclude that the documented differential firm entry came along with employment gains in former FARC-affected municipalities. This is consistent with the effects of firms' entry that have been previously found in the context of developed countries such as Germany and The Netherlands (Boeri and Cramer, 1992; Van Stel and Suddle, 2008).

6.2. Sector composition of new firms. We also explore potential heterogeneity according to the economic sector of the new investments. Table 5 suggests that the entry of new firms takes place across a wide range of economic activities with different employment requirements and value-added. For instance, Column 2 indicates that there is a 17 percent differential increase in entrepreneurship in the agricultural sector during the first two years following the ceasefire. This result is in line with a differential increase in deforestation in areas previously affected by FARC after the ceasefire, documented by Prem et al. (2020). In addition, Column 4 shows that investments in tourism differentially rose by 12 percent in these areas over the same period. This may be explained by a higher willingness to visit remote areas once they become safer (Maldonado et al., 2018). Indeed, to the extent that more tourism increases internal demand, this may also explain the differential increase found for businesses in the retail sector (see Column 8). Finally, Columns 6 and 10 convey that the creation of firms that followed the ceasefire is not specific for low value-added sectors, which are also easier to develop in rural areas. Indeed, we also find an increase in entrepreneurship in sectors that entail expectations of high economic growth, such as construction and manufacturing. Firms in these sectors differentially increased by 14 percent in FARC-affected areas during 2015 and 2016. This is good news insofar as it suggests that the peace dividend encompasses sectors that are capable of generating positive externalities in terms of technical change, human capital, and agglomeration economies.

The effect of the ceasefire on the creation of business during the later post-ceasefire period (after

the victory of the ‘No’ vote in the plebiscite) is positive and significant for firms in agriculture (Column 2) and manufacturing (Column 6). Indeed, the average effect for the entire post-ceasefire period is positive and significant in both cases (see Columns 1 and 5, respectively). This finding is important, as it implies that the aggregate dynamics documented in Table 1 mask some sector-specific nuance.²⁸

6.3. Firms size. An additional dimension of the new firms, that can shed light on the nature of the economic dividend of the ceasefire is firm size. RUES classifies firms across different sizes, from micro firms to small, medium, and large firms. This is done according to the firm’s assets value and number of employees, both self-reported in the firm’s yearly registration (first year) or renewal (thereafter) form, submitted to the local Chamber of Commerce.²⁹ Micro firms are those that report total assets of less than 500 legal monthly minimum wages (LMMW), or else less than 10 employees. Small firms are those that report total assets between 501 and 5,000 LMMW, or else between 11 and 50 employees. Medium-sized firms report between 51 and 200 workers; and the remaining firms are classified as “large.”

Table 4 re-estimates the baseline specification for the different sub-samples of firms according to their size. We find that our results are driven by micro-businesses, the smallest firms. During the first two years following the ceasefire, the number of micro-firms differentially grew in 13 percent in FARC-affected areas (Column 2). There are two possible (but complementary) interpretations of this finding. The first is that new investments take place mainly in rural and peripheral areas (where FARC was mostly active). According to the Micro-Business survey conducted by the National Statistics Bureau in 2019, micro firms can develop their economic activity in any location (e.g. home, premises, door-to-door, etc.). This suggests that the new firms that are entering the market after the start of the ceasefire are likely being constituted by settlers of the same municipalities previously affected by FARC’s violence. Once the group’s offensive activity plummeted, they likely saw the opportunity to undertake new activities. These investors, moreover, come from small and relatively poor peripheral areas, and thus the large majority is unlikely to create larger size firms.³⁰

²⁸The results from estimating the dynamic specifications of the differential firm entry by economic sector, in newly created firms in FARC-affected municipalities before and after the ceasefire, is reported in Figure 5.

²⁹This classification follows the ruling of Law 905 of 2004. Its original in Spanish can be accessed here: <https://rb.gy/66ybw> (last accessed 05/30/2023).

³⁰According to Colombia’s National Household Budget Survey, the PPD-adjusted average monthly income of Colombian households in 2016-2017 was COP\$ 1,773,262 (US\$ 1,335 in 2017 prices).

The second interpretation of the dynamics of micro-firms is that, as employment, firm assets are self-reported. Even if the Chambers of Commerce are not a tax authority, firm owners may likely tend to under-report their assets. For instance, the price that firms must pay to register is proportional to their size. Consistently, the reported assets value bunches at COP\$1.³¹

We also find a statistically significant albeit very small (2 percent) increase in the number of medium-sized firms in the post-referendum period (Column 6). The results from estimating the dynamic specifications of the differential firm creation in FARC-affected municipalities before and after the ceasefire by size is reported in Figure 3. By and large, these dynamics map the results reported in Table 4.

6.4. The role of post-conflict recovery programs. A policy-relevant addition to our main finding is the analysis of the extent to which active government programs aimed at promoting economic development at the local level strengthened (or not) the creation of new business in FARC-affected areas after the start of the ceasefire. We described the two most important such policies, PDET and ZOMAC, in section 2. Within our sample, 176 municipalities received ZOMAC but not PDET, one benefited from PDET but not ZOMAC, and 166 obtained both. Moreover, only 90 of the 344 ZOMAC municipalities belong to our baseline definition of FARC exposure, and 11 treated municipalities are not in the ZOMAC group. Also, 71 PDET municipalities belong to our treatment, and 31 treated municipalities are not part of PDET. This gives us plenty of variation to estimate heterogeneous effects by the presence of the two flagship post-conflict recovery programs of the Colombian government.

Table 6 reports these heterogeneities. Columns 2 and 4 include a dummy that takes the value of one if the municipality benefits from one of the programs (ZOMAC and PDET respectively). We are interested in the triple interaction of this indicator with the FARC-exposure dummy and the late ceasefire period (years 2017 and 2018). Since the policies started in 2017, we cannot explore this heterogeneity for the early ceasefire period (2015-2016). We find a significant differential increase in firms' entry in FARC-exposed municipalities that benefited from either of these policies during the later post-ceasefire period. The effect is also larger in magnitude for these municipalities than the average effect on FARC-exposed municipalities over the same years, and it has the opposite

³¹For this same reason, we do not explore assets' value as a dependent variable. Instead, recall that for the case of employment, RUES includes both the self-reported number of workers and that available from the employer-employee administrative data set.

sign. This suggests that during the period of greater disappointment and uncertainty around the future of the peace process, investors in formerly FARC-affected areas decided to enter the market only in municipalities that benefited from tax cuts and community-driven comprehensive rural reforms. Put differently, active government post-conflict development policies were able to offset the negative effect that the peace fiasco has on entrepreneurship.

7. MECHANISMS

We have argued that the post-ceasefire dynamics of entrepreneurship in Colombia fluctuated with the evolution of violence and with the uncertainty of the political landscape, shaped by both the lack of legitimacy of the peace agreement and the empowerment of the anti-agreement right-wing elite. Additional to the evidence discussed to support this interpretation in sections 2.2 and 5, we also document that the baseline effect is larger in areas traditionally more affected by the conflict (prior to the ceasefire) and thus likely to catch-up faster after violence plummeted. We do so in Column 1 of Table A.4, where we estimate equation model 4.3 adding to the baseline specification the triple interaction with an indicator of whether a municipality experienced FARC violence above the median of the empirical distribution in 2014, the last year before the start of the ceasefire. We find a differential increase in firm's entry both in the early and the late post-ceasefire years. This suggests that the post-referendum investment dismay was offset by the potential economic catch-up of municipalities heavily affected by conflict.

In the rest of this section, we explore the empirical relevance of several alternative explanations of these dynamics. In particular, we study the extent to which the firms' entry reversion can be explained by the dynamics of business competition; the extent to which the short-term investment surge could be driven by the pre-ceasefire pent-up demand in FARC-affected areas; the possibility that the observed entrepreneurship may be driven by the formalization of existing businesses rather than by new investments; and the role of internal migration in driving local demand shifts.

7.1. Increased business competition. Instead of responding to the deterioration of safety and to the political uncertainty that rose after the peace referendum, the reduction in local business formation observed since 2017 could be explained by the initial post-ceasefire increase in firms' creation and the implied burden on incumbent firms. That is, in the absence of large frictions or

market power, the initial entry of new firms increased competition, pushing out of business the least productive ones. This simple conceptual framework maps into an initial post-ceasefire differential firm entry followed by a subsequent differential firm *exit* (driven by the least productive firms of FARC-affected municipalities). Thus, if our findings were explained by this alternative mechanism, an observable implication would be that, after the ceasefire, FARC affected municipalities should experience a differential closure of businesses, potentially lagged relative to the immediate differential opening of firms. Unfortunately data availability prevents us to look into this using firm-level investment.

To test this idea, we follow our baseline empirical strategy and study differential patterns of firms' *exit* in FARC-affected areas, before and after the start of the ceasefire. However, there is an important caveat. Various legislative changes have led to inconsistencies in how the Registry of Commerce computes the volume of firms that shut down. For instance, the number of years a firm can fail to renew its commercial license before it gets dissolved has changed a few times.³² There are two implications of this: first, the firms' exit information lacks the quality of the firms' entry counterpart, and therefore the results using this outcome should be interpreted with caution. Second, we lack sufficiently reliable firms' closure data before 2013. This implies we must forgo the first two years of the sample period, which in turn undermines the analysis of the parallel trends assumption for this outcome. With these admonitions in mind, we now described our empirical results of the effect of the ceasefire on the closure of firms.

Table A.3 reproduces our baseline firm entry results (reported on Table 1), but for firm exit. The smaller number of observations in the new table reflects the fact that, as mentioned above, the sample period for this outcome starts with a two-year delay. We find no differential effect in the number of firms closing business, neither when we look at the entire ceasefire period (Columns 1 to 3) nor when we distinguish between the two substantive post-ceasefire periods (Columns 4 to 6). The point estimates are, in most cases, smaller than those estimated for firms' entry. In addition, Figure A.3 of the Appendix plots the coefficients of the dynamic specification to visually explore the differential evolution of this outcome in FARC-affected municipalities. While noisy, the firms' exit results fail to convey any specific pattern, especially one consistent with the alternative mechanism

³²Regrettably, we cannot use a data-driven approach to resolve this because *Confecámaras* only provides pre-processed data that forbids us to separate firm cancellations due to the administrative penalties from actual business closures.

of increased business competition. Albeit through the lens of an indirect test that is based on far from ideal data, we conclude this alternative account is unlikely to explain our main results.

7.2. Pent-up demand. A second alternative mechanism is that conflict-affected areas had pent-up demand, and that once violence dropped after the ceasefire the held back economic activity rose to catch up with that of conflict-free areas. This would explain both the initial differential surge of investment and its subsequent reversion. The idea behind such dynamics is that, in rural contexts, the economic catch is unlikely to take a long time. The empirical implication of this argument depends on the underlying economic model (which in turn hinges on key fundamentals of the economy). On the one hand, convergence theory suggests that places with larger economic potential would probably have smaller—and probably shorter—recovery after the start of the ceasefire. On the other hand, in the presence of sources of increasing returns, places that are initially larger in economic terms could better reap the business opportunities created by the ceasefire, and experience a larger and more sustained post-conflict investment.

The above discussion implies that empirically estimating potential heterogeneous effects by the ‘economic size’ of municipalities could shed light on the validity of this alternative mechanism (and on the underlying structure of the economy). In the absence of municipal-level GDP statistics in Colombia, we can proxy for such a characteristic with a number of variables. These include population density, the predetermined number of firms per capita, and the predetermined tax revenue (or value-added) per capita. We then estimate equation 4.3 and report the results of these heterogeneities in Columns 2 to 5 Table of A.4.

We find that the excess increase in the number of firms entering the market in FARC-affected municipalities in 2015-2016 is neither differentially larger in municipalities more densely populated (Column 2), nor in municipalities with a high pre-determined number of firms per capita (Column 3), nor it is in municipalities with larger tax revenue or value-added per capita (Columns 4 and 5). This suggests that the short-term effect that we find for the creation of new firms is unlikely explained by a rebound effect in which conflict-driven pent-up demand catches up after violence decreases.

In addition, recall our findings regarding how the post-ceasefire increase in firms’ creation is longer lasting in areas that benefited from government investments (Table 6). We interpreted these findings

as suggesting that active government policies can make economic recovery more sustainable. An additional (complementary) interpretation is that in areas in which two of the key policies devised by the peace agreement were actually implemented the uncertainty regarding the fate of the agreement was much lower and hence the positive effect in terms of firm dynamics persisted in 2017 and 2018.

7.3. Formalization of pre-existing firms. One alternative interpretation of the documented patterns regarding the creation of new firms after the *de facto* end of the conflict with FARC is that the ceasefire encouraged (or allowed, in terms of lifting potential restrictions associated with the presence of the non-state actor) existing but unregistered firms to seek a formal status in terms of registration in the local Chamber of Commerce. Perhaps these firms thought that this would facilitate access to some forms of post-conflict-specific support. Due to data limitations, we can test this possibility only indirectly. Specifically, as discussed in section 3, our entry measure relies on a new (previously non-existing) registration in RUES. That is, we cannot really distinguish between the creation of a new business or the registration of an existing one. In that section, we argued that registration with the local Chamber of Commerce does not entail any tax or employment obligation, but rather brings about several benefits to the firm. Still, it may be true that a violent FARC presence may have prevented existing firms from registering.

Our indirect test of the possibility that our firm entry measure fundamentally captures the registry of existing firms rather than investment in new ones relies in employment formalization. To the extent that the registry of existing firms is correlated with employment formalization (in terms of making social security contributions on behalf of employees) then we can check how the employment formalization rate (that is the share of formal employees over total employees) varies with the ceasefire in FARC-affected areas versus control municipalities. Further, we explore this proxy in both incumbent and newly registered firms.

In Table A.5, we document that, while the labor formalization rate is not differentially larger for newly entrant firms, it is so (albeit if the estimates are somewhat noisy and fail to achieve statistical significance at standard levels) for incumbent (that is previously registered) firms. This is so especially over the first two years after the start of the ceasefire. Put it differently, there are differentially new businesses being registered in FARC-affected areas after the start of the ceasefire, and recall that these firms generate *new* jobs, even if this is measured via formal employment (recall Panel B of Table 3). At the same time (but not instead of), the already existing firms seek to

formalize their employees. This implies that the formalization mechanism is at best complementary, but not a substitute, for the creation of new businesses.

7.4. Internal migration. Colombia experienced large internal displacement flows over the course of the civil war, with millions fleeing their residency (Ibáñez and Velásquez, 2009). Today, almost 8,5 million people are officially recognized as victims of displacement, and have been included in the registry of Colombia’s Victims Unit.³³ Given the scale of internal forced migration in Colombia, it could be the case that the end of the conflict with FARC may have triggered large flows of households seeking to return to their homes and land. In turn, these households may have decided to invest in these areas, generating the documented surge in the creation of firms.

Tables A.6 and A.7 in the Appendix address this possibility into two different ways. First, Table A.6 relies on our most demanding specification to investigate, in Column 1, the effect of the ceasefire on forced displacement in FARC-affected municipalities versus other parts of the country. Consistent with our finding that security improved in the first two years of the start of the ceasefire and deteriorated thereafter, we find that forced displacement differentially and significantly dropped in FARC-affected areas only in 2015 and 2016. Moreover, in Columns 2 and 3, we look at the 2018 population census (the first after 2005) and show that, in the cross-section, our measure of FARC exposure is not correlated with the arrival of migrants, neither in the past five years (Column 2) nor in the last 12 months (Column 3). This suggests that the differential increase in business formation is unlikely explained by a differential in-migration to FARC municipalities after the ceasefire. Indeed, consistent with this interpretation, in Table A.7, we re-estimate our baseline specification controlling for the number of forcibly displaced individuals. Our results are unchanged.

8. CONCLUSION

In this paper, we have argued that, from a conceptual perspective, the negative effects of conflict on economic outcomes can be divided in two broad mechanisms: reduced security and increased uncertainty. One implication of this observation is that the end of a conflict does not necessarily imply positive and long-lasting economic gains, as post-conflict societies are usually subject to large variations in both the incidence of violence and the scope of political and economic uncertainty. Empirical evidence of the dynamics of entrepreneurship after the end of a conflict is however scarce.

³³See <https://rb.gy/hwpty> (last accesses 05/30/2023).

We start filling this gap by studying firm creation in Colombia after the end of the conflict with the FARC insurgency, terminating a period of over 50 years of violent struggle. Our findings have important implications for the way we think about the economic dividend of conflict termination, and the type of policies that can maximize this premium.

First, and related to a strand of the political science literature about conflict termination, our findings imply that peace ‘spoilers’ (see, e.g., [Stedman, 1997](#)), political or economic losers from the end of a conflict, likely hinder the economic return of peace. In our context, a large faction of the political right first opposed the agreement, then campaigned to reject it in a referendum, and finally opposed its implementation (see section 2.2). This suggests that peace negotiations should probably involve, in addition to the warring parties, representatives a broad spectrum of the political arena, including certainly the main political opposition of the party in power when peace is being negotiated.

Second, and related to the previous point regarding broad base legitimacy, peace agreements should procure judicial and political mechanisms to guarantee their survival to political turnover. They also should *ex-ante* secure the economic means to allow for a speedy implementation of the agreed programs and policies. In retrospect, the Colombian context clearly lacked the institutional design to seal the agreement from the will of its political opponents.

Third, peace deals that fail to secure the mechanisms to fill in with institutional means the governance vacuum that the demobilization of a powerful non-state actor leaves in the territory, are less likely to successfully and persistently reduce violence ([Prem et al., 2022](#)). This may ultimately play against the realization of the full economic dividend of the agreement.

Finally, our findings also leave room for hope. Particularly, the post-referendum investors’ disappointment was partially offset by active government recovery policies aimed at promoting local economic activity and community-driven rural development. Again, one key implication is that the implementation of peace agreements should be taken seriously, and should hopefully not be politicized.

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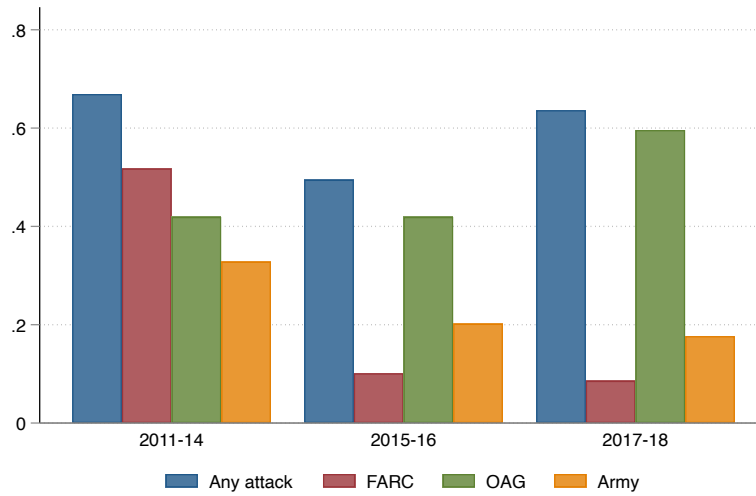
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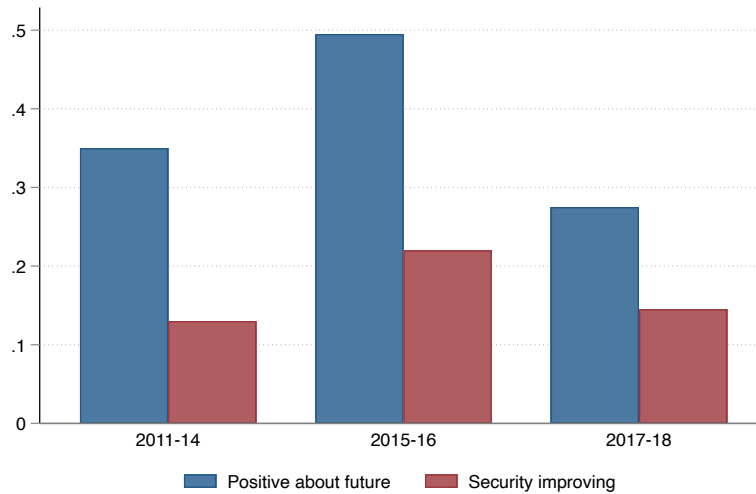
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FIGURE 1. The evolution of conflict and people's perceptions



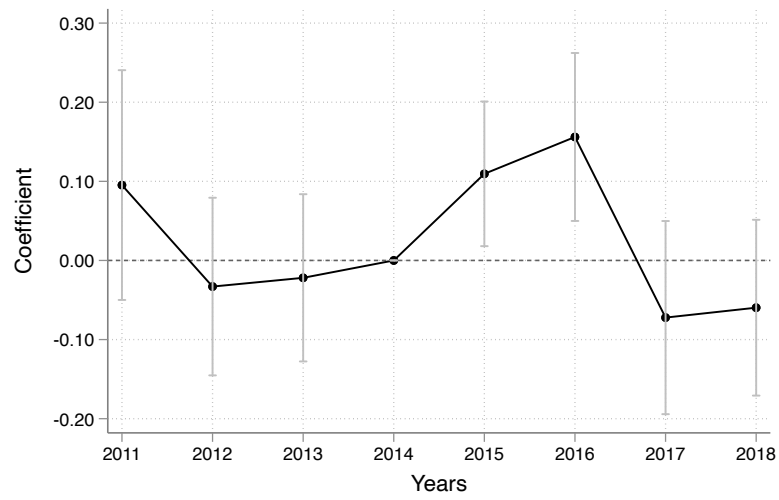
A. Conflict



B. Survey perceptions

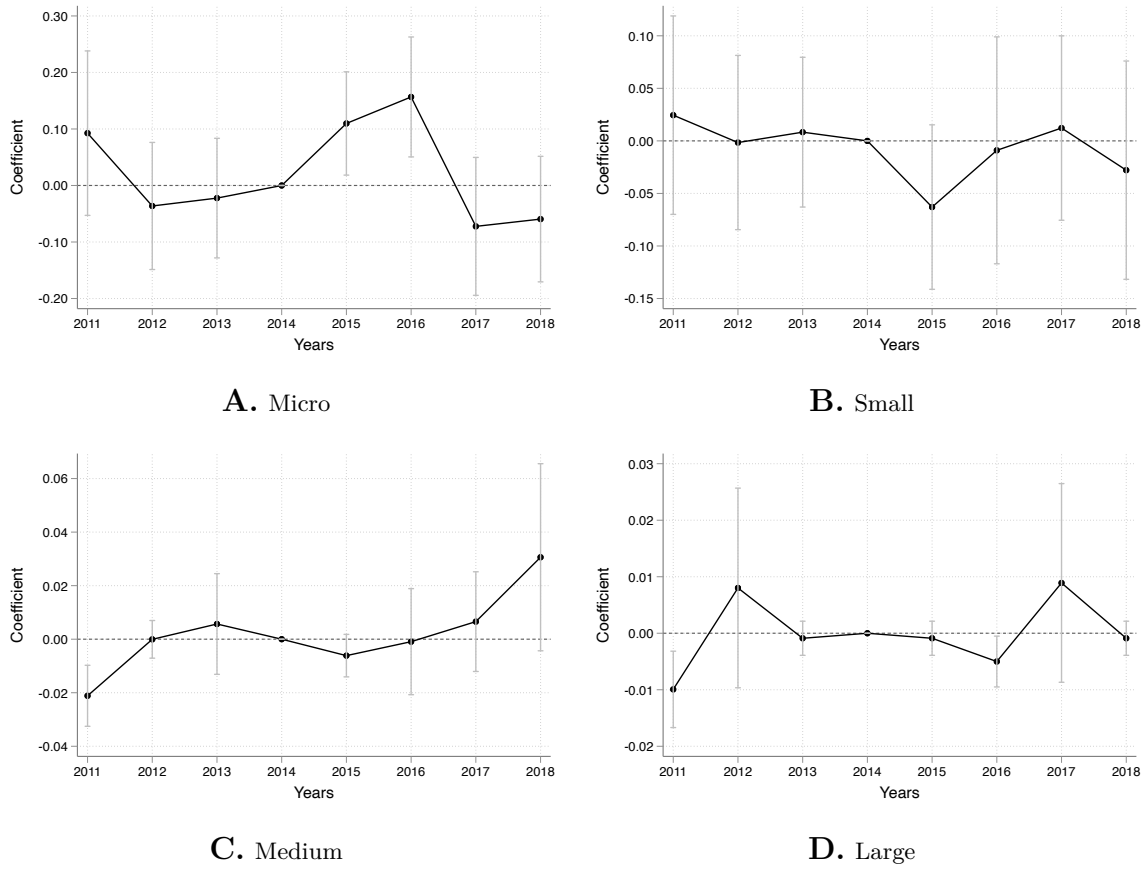
Notes: This figure presents the evolution of aggregate conflict and public perception from 2011 to 2018. We divide the period into 2011-2014, 2015-2016, and 2017-2018. In Panel A, we present the the average number of attacks by municipality for four categories: any violent attack, any attack perpetrated by FARC, any attack perpetrated by another illegal armed group (OAG), and any attack by the army. In Panel B, we present the average of the share of survey respondent's who answer 'Yes' to the question: "Are you positive about the future?" (blue) and to the question "Do you think that the security is improving?" The survey was conducted by DATEXCO and the numbers were taken from the 2018 "Pulso País" report.

FIGURE 2. Dynamic difference-in-differences



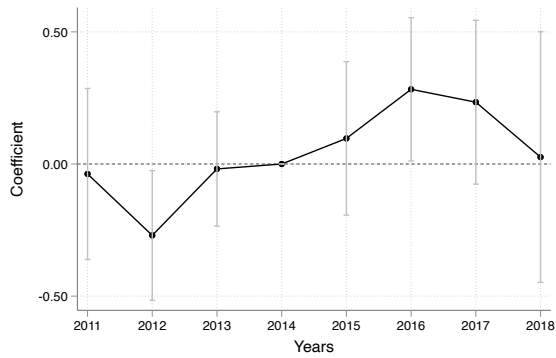
Notes: This figure presents the point estimates from equation (4.2) for our baseline specification. We present the point estimates of the regression and the confidence of interval at the 95%.

FIGURE 3. Dynamic difference-in-differences: Firm entry by size of the firm

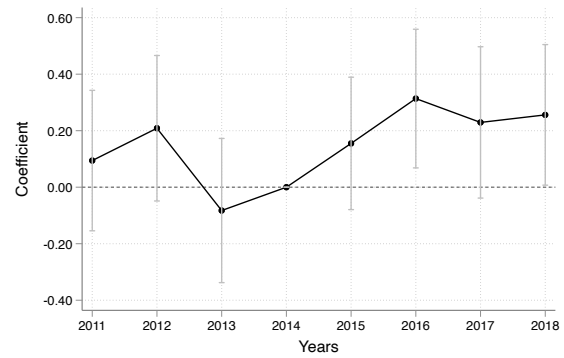


Notes: This figure presents the point estimates from equation (4.2) for each firm size. Panel A presents the results for micro-enterprises, Panel B shows small firms, Panel C shows medium-size firms, and Panel D shows the results for large firms. We present the point estimates of the regression and the confidence of interval at the 95%.

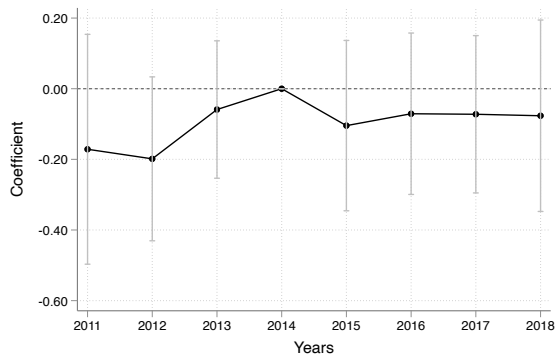
FIGURE 4. Dynamic difference-in-differences: Employment by entrants and incumbent firms



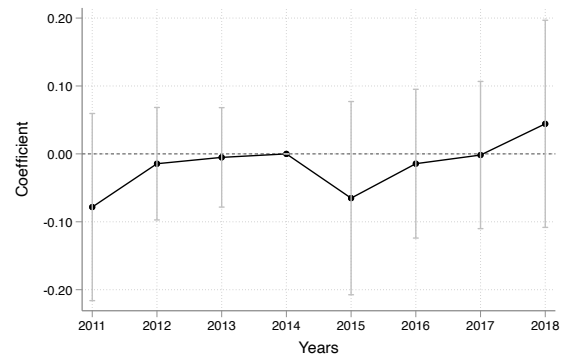
A. Employment for entrants



B. Formal employment for entrants



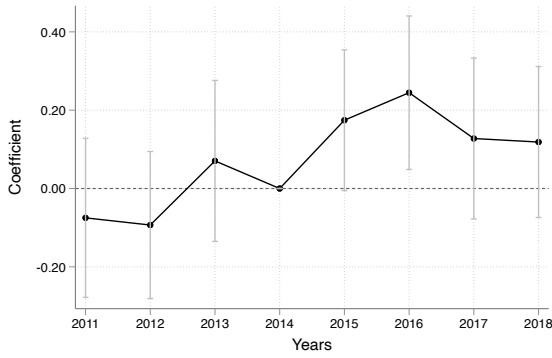
C. Employment for incumbents



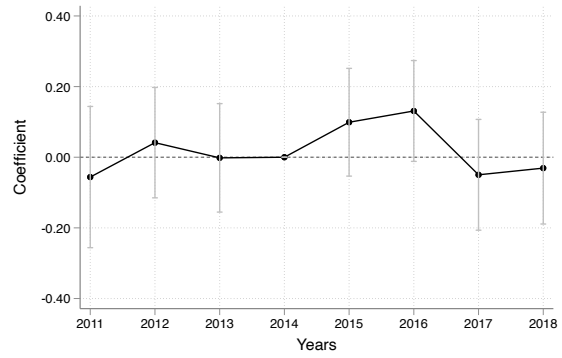
D. Formal Employment for incumbents

Notes: This figure presents the point estimates from equation (4.2) for the employment outcomes. Panel A presents the results of employment reported by the firm, and Panel B for employment reported on PILA for entering firms. Panel C shows those results but for incumbent firms, while Panel D shows the same but employment reported in PILA. We present the point estimates of the regression and the confidence of interval at the 95%.

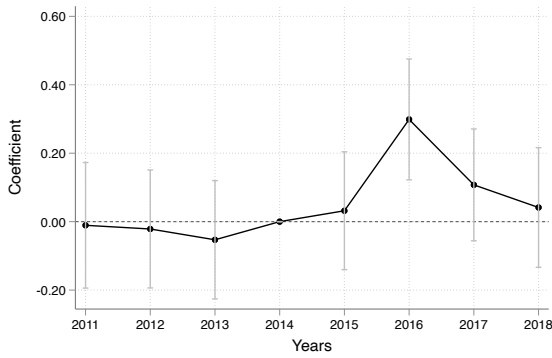
FIGURE 5. Dynamic difference-in-differences: By sector



A. Agriculture



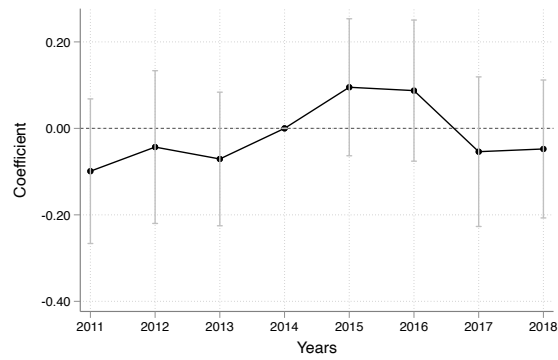
B. Tourism



C. Manufacturing



D. Retail



E. Construction

Notes: This figure presents the point estimates from equation (4.2) by sector of the firm. We present the point estimates of the regression and the confidence interval at the 95%.

TABLE 1. The effect of the ceasefire on firms' entry

The dependent variable is the hyperbolic transformation of firm entry

	(1)	(2)	(3)	(4)	(5)	(6)
(1) FARC × Years 2015 and 2016				0.12*** (0.04) [0.00]	0.09*** (0.04) [0.01]	0.08** (0.04) [0.01]
(2) FARC × Years 2017 and 2018				-0.08 (0.05) [0.09]	-0.05 (0.05) [0.23]	0.00 (0.05) [0.96]
FARC × Ceasefire	0.02 (0.04) [0.52]	0.02 (0.04) [0.53]	0.04 (0.04) [0.44]			
Observations	8,736	8,736	8,736	8,736	8,736	8,736
R-squared	0.927	0.935	0.935	0.927	0.929	0.935
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Dept-Year FE	No	No	Yes	No	No	Yes
Controls-Year FE	No	Yes	Yes	No	Yes	Yes
Municipalities	1092	1092	1092	1092	1092	1092
Mean DV	3.871	3.871	3.871	3.871	3.871	3.871
SD DV	1.569	1.569	1.569	1.569	1.569	1.569
p-values diff between (1) and (2)				0.00	0.00	0.06

Notes: This table presents the results from the main specification in equation (4.1). *FARC* is defined as a dummy that takes the value one if the municipality experiences any FARC attacks for the period 2011 to 2014. *Ceasefire* is a dummy that takes the value one for the period after 2014. *Years 2015 and 2016* is a dummy that takes the value one for years 2015 and 2016, while *Years 2017 and 2018* is a dummy that takes the value one for the years 2017 and 2018. Columns 2 and 5 add predetermined municipal controls interacted with year fixed effects. This controls include logarithm of the population in 2010, share of rural population, poverty index, and distance to the department capital. Columns 3 and 6 add department×year fixed effects. In addition, in columns 4, 5 and 6, we show the **p-values** of the difference between the coefficients of the interaction between FARC and the early and later years. Robust standard errors are clustered at the municipality level and presented in parenthesis. In square brackets, we present the p-values for standard errors control for spatial and first-order time correlation (see Conley, 1999, Conley, 2016). We allow spatial correlation to extend to up to 416 km from each municipality's centroid, which is the average distance from one municipality to all the rest. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

TABLE 2. Robustness exercises

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Pre trend	Placebo ceasefire		Machine learning controls	Collapse periods	Increase overlap	Abadie (2005)	Sant'Anna and Zhao (2020)	Dependent variable in:			FARC definition:
		2012	2013						Logarithm	Levels	Attacks over population	Drop bottom quartile
FARC × Linear trend	-0.02 (0.03)											
FARC × Cease placebo		-0.08 (0.07)	-0.02 (0.06)									
(1) FARC × Years 2015 and 2016				0.09** (0.04)	0.12*** (0.04)	0.11*** (0.04)	0.09** (0.04)	0.09** (0.04)	0.11*** (0.03)	7.94*** (3.00)	0.05** (0.03)	0.13*** (0.04)
(2) FARC × Years 2017 and 2018				-0.04 (0.05)	-0.08 (0.05)	-0.03 (0.05)	-0.04 (0.05)	-0.04 (0.05)	-0.06 (0.04)	5.77 (4.78)	-0.02 (0.02)	-0.08 (0.05)
Observations	4,368	4,368	4,368	8,736	3,276	5,552	4,368	4,368	8,736	8,736	8,736	8,736
R-squared	0.945	0.948	0.948	0.930	0.967	0.935	Yes	Yes	0.934	0.969	0.929	0.927
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipalities	1092	1092	1092	1092	1092	694	1092	1092	1092	1092	1092	1092
Mean DV	3.885	3.885	3.885	3.871	3.885	4.126	3.885	3.885	3.271	76.35	3.871	3.871
SD DV	1.557	1.557	1.557	1.569	1.502	1.570	1.557	1.557	1.457	159	1.569	1.569
p-values diff between (1) and (2)				0.000	0.000	0.000			0.000	0.560	0.000	0.000

Notes: This table presents the results for a series of robustness exercises. See section 5.2 for details about each exercise. *FARC* is defined as a dummy that takes the value one if the municipality experiences any FARC attacks for the period 2011 to 2014. *Years 2015 and 2016* is a dummy that takes the value one for years 2015 and 2016, while *Years 2017 and 2018* is a dummy that takes the value one for the years 2017 and 2018. Column 2, defines *Placebo Cease* as a dummy that takes the value one for the years 2012, 2013 and 2014, while column 3 defines *Placebo Cease* as a dummy that takes the value one for the years 2013 and 2014. Columns 1, 2 and 3 are estimated in the sample before the ceasefire. Column 3 collapses the data into pre-ceasefire, years 2015-16, and years 2017-18. Column 4 selects the controls using machine learning following Belloni et al. (2014). Column 5 collapses the data into three periods pre-ceasefire, 2015-16, and 2017-18. Column 6 performs a sample selection to ensure a higher overlap following Crump et al. (2009). Columns 7 and 8 estimates the main model following Abadie (2005) and Sant'Anna and Zhao (2020), respectively. Columns 8 and 9 replace the dependent variable by the logarithm of entry plus one and for the level of entry, respectively. Columns 10 and 11 use as exposure to FARC a standardized measure of FARC attacks between 2011 and 2014 over population and a dummy based on this continuous measure that takes the value one for municipalities above the bottom quartile of the empirical distribution. Robust standard errors are clustered at the municipality level and presented in parenthesis. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

TABLE 3. The effect of the ceasefire on employment by type of firm

The dependent variable is the hyperbolic transformation of number of employees

	(1)	(2)	(3)	(4)
	Incumbents	Entrants	Incumbents	Entrants
Panel A: Self-reported employment				
(1) FARC × Years 2015 and 2016			0.02 (0.15)	0.27** (0.11)
(2) FARC × Years 2017 and 2018			0.03 (0.14)	0.21 (0.16)
FARC × Ceasefire	0.03 (0.14)	0.24** (0.12)		
Panel B: Formal employment				
(1) FARC × Years 2015 and 2016			-0.02 (0.05)	0.18** (0.07)
(2) FARC × Years 2017 and 2018			0.05 (0.06)	0.19** (0.08)
FARC × Ceasefire	0.03 (0.05)	0.18*** (0.07)		
Observations	8,736	8,736	8,736	8,736
Municipalities	1092	1092	1092	1092
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
R-squared (Panel A)	0.829	0.760	0.829	0.760
R-squared (Panel B)	0.829	0.835	0.955	0.835
Mean DV (Panel A)	4.439	3.283	4.439	3.283
Mean DV (Panel B)	4.689	2.591	4.689	2.591
SD DV (Panel A)	2.295	2.077	2.295	2.077
SD DV (Panel B)	2.244	1.896	2.244	1.896
p-values diff between (1) and (2) (Panel A)			0.880	0.660
p-values diff between (1) and (2) (Panel B)			0.250	0.910

Notes: This table presents the results from the main specification in equation (4.1) for employment reported by the firm (Panel A) and for formal employment reported in PILA (Panel B). Columns 1 and 3 (2 and 4) present the result for total employment of incumbent (entrant) firms. *FARC* is defined as a dummy that takes the value one if the municipality experiences any FARC attacks for the period 2011 to 2014. *Ceasefire* is a dummy that takes the value one for the period after 2014. *Years 2015 and 2016* is a dummy that takes the value one for years 2015 and 2016, while *Years 2017 and 2018* is a dummy that takes the value one for the years 2017 and 2018. In addition, in columns 3 and 4, we show the **p-values** of the difference between the coefficients of the interaction between FARC and the early and later years. Robust standard errors are clustered at the municipality level and presented in parenthesis. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

TABLE 4. The effect of the ceasefire on firms' entry - Effects by firm size

The dependent variable is the hyperbolic transformation of firm entry

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Micro		Small		Medium		Large	
(1) FARC × Years 2015 and 2016		0.12*** (0.04)		-0.04 (0.03)		0.00 (0.01)		-0.00 (0.00)
(2) FARC × Years 2017 and 2018		-0.07 (0.05)		-0.02 (0.04)		0.02** (0.01)		0.00 (0.01)
FARC × Ceasefire	0.03 (0.04)		-0.03 (0.03)		0.01** (0.01)		0.00 (0.00)	
Observations	8,736	8,736	8,736	8,736	8,736	8,736	8,736	8,736
R-squared	0.927	0.935	0.646	0.646	0.286	0.286	0.189	0.190
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipalities	1092	1092	1092	1092	1092	1092	1092	1092
Mean DV	3.869	3.869	0.119	0.119	0.012	0.012	0.004	0.004
SD DV	1.569	1.569	0.423	0.423	0.121	0.121	0.066	0.066
p-values diff between (1) and (2)		0.000		0.360		0.050		0.140

Notes: This table presents the results from the main specification in equation (4.1) by size of the firm. *FARC* is defined as a dummy that takes the value one if the municipality experiences any FARC attacks for the period 2011 to 2014. *Ceasefire* is a dummy that takes the value one for the period after 2014. *Years 2015 and 2016* is a dummy that takes the value one for years 2015 and 2016, while *Years 2017 and 2018* is a dummy that takes the value one for the years 2017 and 2018. Robust standard errors are clustered at the municipality level and presented in parenthesis. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

TABLE 5. The effect of the ceasefire on firms' entry - Effects by economic sector

The dependent variable is the hyperbolic transformation of firm entry

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Agriculture		Tourism		Manufacturing		Retail		Construction	
(1) FARC × Years 2015 and 2016	0.19*** (0.06)	0.23*** (0.06)	0.04 (0.05)	0.12** (0.05)	0.19*** (0.06)	0.10** (0.04)	0.00 (0.04)	0.10** (0.04)	0.07 (0.05)	0.14*** (0.05)
(2) FARC × Years 2017 and 2018		0.15** (0.07)		-0.04 (0.06)	0.10* (0.05)			-0.09* (0.05)		0.00 (0.05)
FARC × Ceasefire			0.04 (0.05)		0.14*** (0.05)		0.00 (0.04)		0.07 (0.05)	
Observations	8,736	8,736	8,736	8,736	8,736	8,736	8,736	8,736	8,736	8,736
R-squared	0.714	0.714	0.842	0.842	0.842	0.842	0.897	0.897	0.810	0.810
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipalities	1092	1092	1092	1092	1092	1092	1092	1092	1092	1092
Mean DV	0.903	0.903	1.886	1.886	1.469	1.469	3.115	3.115	0.924	0.924
SD DV	1.045	1.045	1.414	1.414	1.365	1.365	1.514	1.514	1.183	1.183
p-values diff between (1) and (2)		0.150		0.000	0.150		0.000	0.000		0.010

Notes: This table presents the results from the main specification in equation (4.1) by sector of the firm. *FARC* is defined as a dummy that takes the value one if the municipality experiences any FARC attacks for the period 2011 to 2014. *Ceasefire* is a dummy that takes the value one for the period after 2014. *Years 2015 and 2016* is a dummy that takes the value one for years 2015 and 2016, while *Years 2017 and 2018* is a dummy that takes the value one for the years 2017 and 2018. All Columns include municipality and year fixed effects. Robust standard errors are clustered at the municipality level and presented in parenthesis. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

TABLE 6. The effect of the ceasefire on firms' entry - Heterogeneity by presence of post conflict government programs

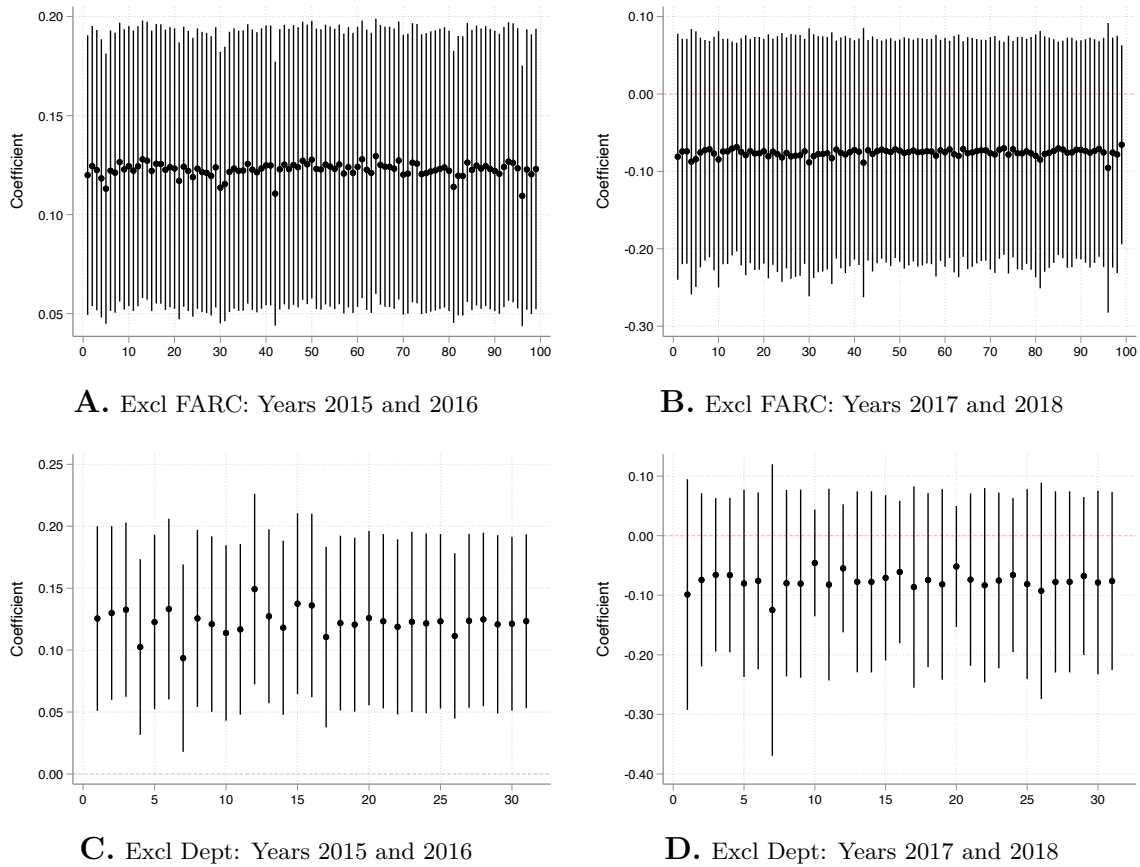
	(1)	(2)	(3)	(4)
	Tax benefits		PDET	
FARC \times Years 2017 and 2018 \times Z		0.31*** (0.06)		0.23*** (0.09)
FARC \times Years 2015 and 2016	0.12*** (0.04)	0.12*** (0.04)	0.12*** (0.04)	0.12*** (0.04)
FARC \times Years 2017 and 2018	0.00 (0.05)	-0.27*** (0.04)	-0.02 (0.05)	-0.15** (0.07)
Years 2017 and 2018 \times Z	-0.12*** (0.03)	-0.14*** (0.03)	-0.10*** (0.04)	-0.14*** (0.04)
Observations	8,736	8,736	8,736	8,736
R-squared	0.928	0.928	0.927	0.927
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Municipalities	1092	1092	1092	1092
Mean DV	3.871	3.871	3.871	3.871
SD DV	1.569	1.569	1.569	1.569

Notes: This table presents the results from the specification in equation (4.3). *FARC* is defined as a dummy that takes the value one if the municipality experiences any FARC attacks for the period 2011 to 2014. *Years 2015 and 2016* is a dummy that takes the value one for years 2015 and 2016, while *Years 2017 and 2018* is a dummy that takes the value one for the years 2017 and 2018. *Z* is a dummy that takes the value one if the municipality benefits from special post-conflict programs. In columns 1 and 2, *Z* is a dummy that takes value one if the municipality has tax benefits or not, while in columns 3 and 4, *Z* is a dummy variable that takes value one if the municipality benefits from the PDETs. Robust standard errors are clustered at the municipality level and presented in parenthesis. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Online Appendix: Not for Publication

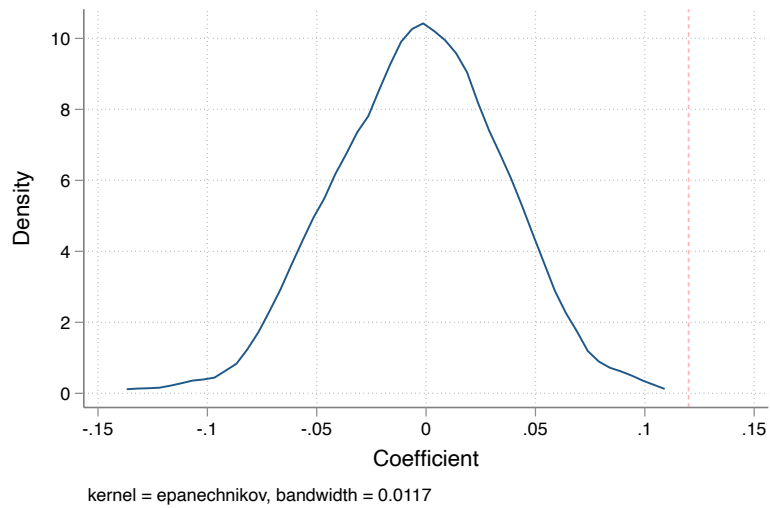
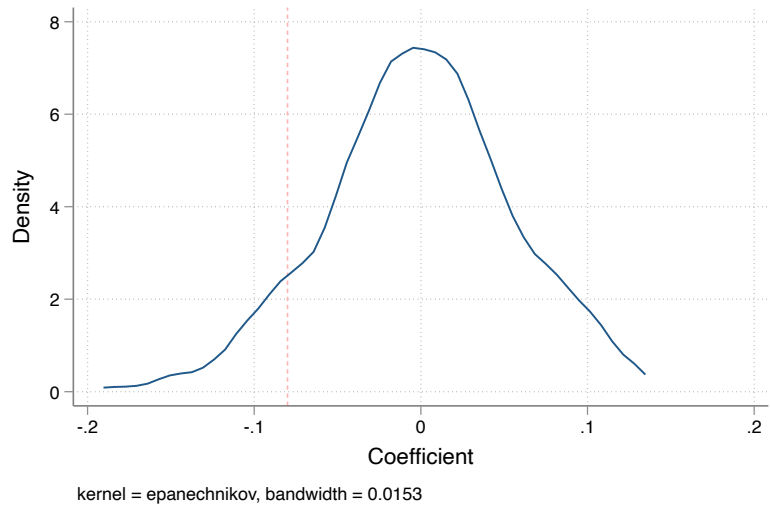
Peaceful Entry: Entrepreneurship Dynamics during Colombia's Peace Agreement

FIGURE A.1. Exclusion of one FARC municipality and one department at the time



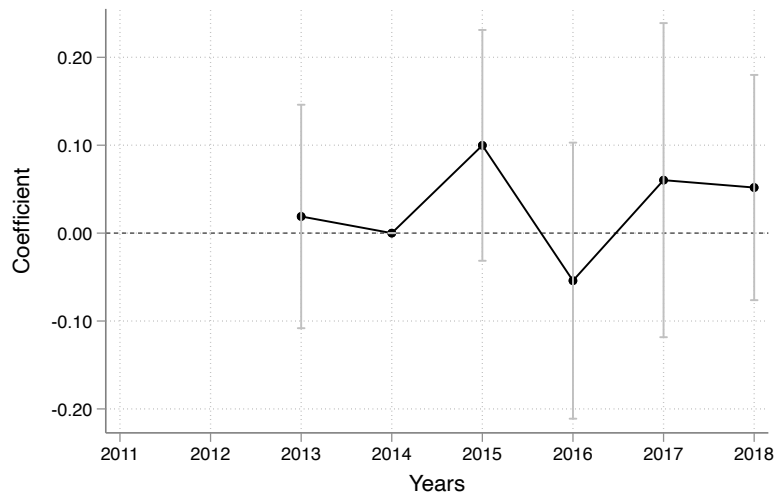
Notes: This figure presents the results our main specification. In Panels A and B we drop one of the FARC affected municipalities at the time, while Panels C and D remove one department at the time. Panels A and C show the results for the interaction between FARC and the dummy for years 2015 and 2016, while Panels B and D present the results for the interaction between FARC and the dummy for years 2017 and 2018.

FIGURE A.2. Distribution of placebo treatments

**A.** Years 2015 and 2016**B.** Years 2017 and 2018

Notes: This figure presents the distribution of placebo treatments. We randomize the assignment of a municipality to have FARC presence before the ceasefire based on the number of municipalities exposed (99). We run the regressions using the specification from Column 5 in Table 1. Panel A shows the results for the interaction between the placebo FARC and the dummy for years 2015 and 2016, while Panel B presents the results for the interaction between the placebo FARC and the dummy for years 2017 and 2018. The red line presents the coefficient of Column 4 of Table 1. In Panel A the **p-values**, i.e. the number of cases where the placebo effect shows a larger decrease in firm creation after the ceasefire, is <0 , while in Panel B is 0.10.

FIGURE A.3. Dynamic difference-in-differences: Firm exit



Notes: This figure presents the point estimates from equation (4.2) for firm exit for our baseline specification. We present the point estimates of the regression and the confidence of interval at the 95%. We present the point estimates of the regression and the confidence of interval at the 95%.

TABLE A.1. Summary statistics

	(1)	(2)	(3)	(4)
	Average	Standard deviation	90th percentile	10th percentile
Number of firms entry	82.56	186.73	187.00	4.00
HB firm entry	3.93	1.55	5.92	2.09
FARC	0.09	0.29	0.00	0.00
Poverty index	70.30	15.72	89.35	48.23
Log population	9.46	0.97	10.70	8.25
Share of rurality population	0.59	0.23	0.86	0.23
Log distance to department capital	0.04	0.91	1.07	-1.07

Notes: This table presents summary statistics for the main variables of interest.

TABLE A.2. Differences by exposure to FARC's violence

	(1) Avg without FARC presence	(2) FARC Presence
HB firm entry	3.89 (1.56)	0.48*** (0.08)
Poverty index	69.53 (15.76)	8.51*** (0.70)
Log population	9.41 (0.96)	0.58*** (0.04)
Share of rurality population	0.58 (0.23)	0.05*** (0.01)
Log distance to department capital	0.03 (0.91)	0.14*** (0.05)

Notes: This table presents univariate regressions based on municipality characteristics before the ceasefire. Column 1 presents the average of each variable before the ceasefire for municipalities non-exposed to FARC violence. Column 2 presents the estimated coefficient and standard errors from univariate regressions for the discrete treatment. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A.3. The effect of the ceasefire on firms' exit

The dependent variable is the hyperbolic transformation of firm exit

	(1)	(2)	(3)	(4)	(5)	(6)
(1) FARC \times Years 2015 and 2016				0.01 (0.06)	-0.01 (0.06)	0.02 (0.05)
(2) FARC \times Years 2017 and 2018				0.05 (0.06)	0.03 (0.06)	0.07 (0.06)
FARC \times Ceasefire	0.03 (0.05)	0.01 (0.05)	0.04 (0.05)			
Observations	6,552	6,552	6,552	6,552	6,552	6,552
R-squared	0.889	0.892	0.919	0.889	0.892	0.919
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Dept-Cease FE	No	No	Yes	No	No	Yes
Controls FE	No	Yes	Yes	No	Yes	Yes
Municipalities	1092	1092	1092	1092	1092	1092
Mean DV	3.740	3.740	3.740	3.740	3.740	3.740
SD DV	1.884	1.884	1.884	1.884	1.884	1.884
p-values diff between (1) and (2)				0.610	0.470	0.380

Notes: This table presents the results from the main specification in equation (4.1) for firm exit. *FARC* is defined as a dummy that takes the value one if the municipality experiences any FARC attacks for the period 2011 to 2014. *Ceasefire* is a dummy that takes the value one for the period after 2014. *Years 2015 and 2016* is a dummy that takes the value one for years 2015 and 2016, while *Years 2017 and 2018* is a dummy that takes the value one for the years 2017 and 2018. Robust standard errors are clustered at the municipality level and presented in parenthesis. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

TABLE A.4. Heterogeneity by municipal characteristics

	(1) Conflict intensity	(2) Population density	(3) Number of firms over population	(4) Industrial taxes collected over population	(5) Value-added over population
FARC \times Years 2015 and 2016 \times Z	0.14** (0.07)	0.08 (0.11)	-0.06 (0.04)	-0.06 (0.05)	-0.04 (0.03)
FARC \times Years 2017 and 2018 \times Z	0.14 (0.09)	0.19 (0.15)	-0.05 (0.05)	-0.01 (0.06)	-0.02 (0.05)
FARC \times Years 2015 and 2016	0.05 (0.04)	0.14*** (0.04)	0.10*** (0.03)	0.12*** (0.03)	0.11*** (0.04)
FARC \times Years 2017 and 2018	-0.14** (0.06)	-0.04 (0.06)	-0.10** (0.04)	-0.09** (0.05)	-0.09* (0.05)
Years 2015 and 2016 \times Z		0.00 (0.01)	-0.06*** (0.01)	-0.03** (0.01)	-0.04*** (0.01)
Years 2017 and 2018 \times Z		-0.03*** (0.01)	-0.13*** (0.01)	-0.07*** (0.02)	-0.04** (0.01)
Observations	8,736	8,736	8,736	8,528	8,736
R-squared	0.927	0.927	0.929	0.921	0.928
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Municipalities	1092	1092	1092	1066	1092
Mean DV	3.871	3.871	3.871	3.958	3.871
SD DV	1.569	1.569	1.569	1.490	1.569

Notes: This table presents the results from the specification in equation (4.3). *FARC* is defined as a dummy that takes the value one if the municipality experiences any FARC attacks for the period 2011 to 2014. *Years 2015 and 2016* is a dummy that takes the value one for years 2015 and 2016, while *Years 2017 and 2018* is a dummy that takes the value one for the years 2017 and 2018. *Z* is a characteristic of the municipality measured before the ceasefire. In column 1, *Z* is a dummy variable that takes values one if the municipality is above the median of conflict intensity. In column 2, *Z* is a dummy that takes value one if the municipality had the population density above the lower quartile in 2014. In column 3, *Z* is the total number of firms in the municipality over population in 2014 standardized by the mean and standard deviation. In column 4, *Z* is the log of taxes industrial related taxes collected by the municipality over population in 2014 standardized by the mean and standard deviation. Finally, in column 5, *Z* is the log of value-added produced in the municipality over population in 2014 standardized by the mean and standard deviation. Robust standard errors are clustered at the municipality level and presented in parenthesis. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

TABLE A.5. The effect of the ceasefire on the share of formal employees

The dependent variable is the number of formal employees over self-reported ones

	(1)	(2)	(3)	(4)
	Incumbents	Entrants	Incumbents	Entrants
(1) FARC × Years 2015 and 2016			0.29 (0.46)	0.05 (0.18)
(2) FARC × Years 2017 and 2018			0.05 (0.45)	0.07 (0.18)
FARC × Ceasefire	0.17 (0.40)	0.06 (0.16)		
Observations	8,207	7,601	8,207	7,601
R-squared	0.426	0.381	0.426	0.381
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Municipalities	1073	1061	1073	1061
Mean DV	2.542	0.829	2.542	0.829
SD DV	4.257	1.477	4.257	1.477
p-values diff between (1) and (2)			0.580	0.930

Notes: This table presents the results from the main specification in equation (4.1) for formal employment over self-reported employment. Columns 1 and 3 (2 and 4) present the results using the total employment of incumbent (entrant) firms. *FARC* is defined as a dummy that takes the value one if the municipality experiences any FARC attacks for the period 2011 to 2014. *Ceasefire* is a dummy that takes the value one for the period after 2014. *Years 2015 and 2016* is a dummy that takes the value one for years 2015 and 2016, while *Years 2017 and 2018* is a dummy that takes the value one for the years 2017 and 2018. Robust standard errors are clustered at the municipality level and presented in parenthesis. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

TABLE A.6. Migration as an alternative account

	(1)	(2)	(3)
	Forcibly	2018 Census migrants	
	displaced	5 years ago	1 year ago
(1) FARC \times Years 2015 and 2016	-3.05***		
	(0.89)		
(2) FARC \times Years 2017 and 2018	-1.32		
	(1.86)		
FARC		0.50	0.08
		(0.43)	(0.15)
Observations	8,736	19,487,792	19,487,792
R-squared	0.427	0.027	0.007
Municipality FE	Yes	No	No
Department FE	No	Yes	Yes
Baseline controls	Yes	Yes	Yes
Municipalities	1092	1092	1092
Mean Dep. Var.	2.787	9.623	2.838
Std. Dev. Dep. Var.	6.955	29.49	16.60
p-values diff between (1) and (2)	0.240		

Notes: This table presents the results from two different specifications. In column 1, we present results using the main specification in equation (4.1). The dependent variable in this column is the share of the number of people forcibly displaced over population. In columns 2 and 3, we present a cross-section regression using data from the 2018 Census. The dependent variable in column 2 (3) is a dummy that takes the value 100 if an individual was in a different municipality to the one where it is being surveyed five (one) years ago. *Cease* is a dummy that takes the value for the period after 2014. *FARC* a dummy variable that takes the value one if there was at least one violent case by FARC. All columns add predetermined municipal controls and in column 1 we interact them with the ceasefire dummy. These controls include share of rural population, distance to the department capital, poverty index, and logarithm of the population in 2010. In columns 2 and 3, we also add individual level controls that include age, a dummy for urban location, and gender. Clustered robust standard error at the municipality level are presented in parenthesis. **p* is significant at the 10% level, ***p* is significant at the 5% level, ****p* is significant at the 1% level.

TABLE A.7. The effect of the ceasefire on firms' entry, controlling for forced migration

The dependent variable is the hyperbolic transformation of firm entry

	(1)	(2)
(1) FARC \times Years 2015 and 2016		0.09** (0.04)
(2) FARC \times Years 2017 and 2018		-0.05 (0.05)
FARC \times Ceasefire	0.02 (0.04)	
Observations	8,736	8,736
R-squared	0.927	0.927
Municipality FE	Yes	Yes
Year FE	Yes	Yes
Baseline controls	Yes	Yes
Municipalities	1092	1092
Mean DV	2.787	2.787
SD DV	6.955	6.955
p-values diff between (1) and (2)		0.000

Notes: This table presents the results from the main specification in equation (4.1). *FARC* is defined as a dummy that takes the value one if the municipality experiences any FARC attacks for the period 2011 to 2014. *Ceasefire* is a dummy that takes the value one for the period after 2014. *Years 2015 and 2016* is a dummy that takes the value one for years 2015 and 2016, while *Years 2017 and 2018* is a dummy that takes the value one for the years 2017 and 2018. All columns control for average forced migration in the municipality between 2011 and 2014 over population interacted with year fixed effects. In addition, in column 2, we show the **p-values** of the difference between the coefficients of the interaction between FARC and the early and later years. Robust standard errors are clustered at the municipality level and presented in parenthesis. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

TABLE A.8. The effect of the ceasefire on firms' entry using ELN presence as the control group or as a placebo

The dependent variable is the hyperbolic transformation of firm entry

Sample:	(1) FARC or ELN	(2)	(3) Excl. FARC	(4)	(5) Full sample	(6)
FARC × Years 2015 and 2016		0.11** (0.05)				0.12*** (0.04)
FARC × Years 2017 and 2018		0.01 (0.07)				-0.06 (0.05)
FARC × Ceasefire	0.06 (0.05)				0.03 (0.04)	
ELN × Ceasefire			-0.03 (0.04)		-0.03 (0.04)	
ELN × Years 2015 and 2016				0.01 (0.04)		0.02 (0.03)
ELN × Years 2017 and 2018				-0.10* (0.05)		-0.10** (0.05)
Observations	1,744	1,744	7,944	7,944	8,736	8,736
R-squared	0.924	0.924	0.927	0.927	0.927	0.927
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipalities	218	218	993	993	1092	1092
Mean DV	3.727	3.727	3.871	3.871	3.871	3.871
SD DV	1.551	1.551	1.569	1.569	1.569	1.569

Notes: This table presents the results from the main specification in equation (4.1) for the hyperbolic transformation of firm entry. Columns 1 and 2 restrict the sample to municipalities affected by FARC or ELN attacks for the period 2011 to 2014, columns 3 and 4 exclude municipalities effects by FARC attacks, and columns 5 and 6 show the results for the full sample. *FARC* (ELN) is defined as a dummy that takes the value one if the municipality experiences any FARC (ELN) attacks for the period 2011 to 2014. *Ceasefire* is a dummy that takes the value one for the period after 2014. *Years 2015 and 2016* is a dummy that takes the value one for years 2015 and 2016, while *Years 2017 and 2018* is a dummy that takes the value one for the years 2017 and 2018. Robust standard errors are clustered at the municipality level and presented in parenthesis. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.