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Abstract

This paper studies the impact of the intensity of market competition on firms' willingness to adopt green technologies. We exploit the staggered rollout of different ride-hailing platforms (most notably, Uber) across metropolitan areas in Spain as a natural experiment that provides time and city-specific exogenous variation in the intensity of competition to study the impact on taxi drivers' decisions to purchase "green" or "dirty" vehicles. We show that the entry of these platforms significantly increased the takeout of green vehicles among professional drivers in incumbent (dominant) conventional taxi companies, while maintaining or slightly decreasing that of dirty ones. Some evidence of the opposite effect is observed in the cities where these platforms were extremely unlikely to enter. These results speak directly to the recent debate on whether competition policies should be relaxed to achieve certain environmental targets.

Keywords: Technological change; Green technology adoption; Market competition; Diffusion of technology; Environmental externalities.

JEL Classification Numbers: D22, K32, L20, O30, O33, Q55, R11.

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1 Introduction

The question on the role of the intensity of competition and market structure in firms' willingness to innovate and in the diffusion of technology has sparked an intense debate that dates back to the 1940s. Two major conflicting theories find their roots in the pioneer contributions on this topic. Namely, while Shumpeter (1942) argues that monopolies favor innovation and, hence, "market power is the price that society must pay" for technological progress, Arrow (1972) claims that competition is the main driving force of innovation and, eventually, of the adoption of new technologies. More recent contributions, such as Aghion et al. (2005), provide some middle ground between these two conflicting theories.

However, this question has been asked with increasing force in the past few years. This is because the diffusion of innovation and technological change are critical not only for economic growth and productivity, but they have also become key towards the fighting of environmental externalities —greenhouse gas emissions, climate change, etc. To facilitate the adoption of new environmentally-friendly technologies, many countries have implemented regulatory measures that make it less attractive to use highly pollutant technologies —e.g., environmental taxes or artificial markets that allow firms to internalize the cost of emissions—, and that incentivize the use cleaner ones —e.g., "green" subsidies.

Unfortunately, it is well-acknowledged that governments are usually not effective in inducing the adoption of new clean technologies —Bénabou and Tirole (2010). Among the major barriers that hinder this process, one that is often cited —which is in line with Arrow's theory— is precisely the lack of market competition —Götz (1999), Vives (2008), Correa and Ornaghi (2014). Therefore, if firms with market power are less prone to adopt new technologies, pro-environmental policies will become ineffective. Conversely, authors with a more Schumpeterian view would argue just the opposite. While many papers have presented numerous theoretical models to discuss the role of competition on technology adoption, ultimately this is an empirical question for which there is no clear-cut answer (perhaps due to the lack of natural experiments that allow researchers to claim causality) despite the huge policy implications —for example, at the time of writing, the European Union discusses whether competition policies should be relaxed to achieve the environmental targets in the *European Green deal*.[1]

Acknowledging the importance of this policy-relevant issue, in this paper we study whether an

[1] This debate triggered the "Competition Policy Contributing to the European Green Deal" conference, organized by the European Commission on February 4, 2021. Some of the key ideas that were discussed are in Fabra (2021).

(exogenous) increase in market competition induces suppliers to adopt new, green technologies. To answer this question, we use a unique dataset on the universe of vehicles purchased by professional taxi drivers in Spain between December 2014 and February 2020. We study the impact of the entry of well-known ride-hailing platforms —such as Uber and Cabify (a Spanish-based company)— across major metropolitan areas on their vehicle purchase decisions according to the type of engines, i.e., whether they are purely fossil-fuel based or, alternatively, if they use cleaner engines (e.g. electric, hybrid, etc.). The staggered rollout of these platforms, which provides variation across metropolitan areas over time, allows us to study this question using a generalized difference-in-differences (*DiD*) approach.

As in many other countries, alternative-fueled (green) vehicles offer in Spain evident and substantial per-mile savings in comparison to those that run solely on petroleum products, which are heavily taxed. However, the takeout of green vehicles is still very low among professional taxi drivers.[2] Bearing in mind that the entry of the aforementioned ride-hailing platforms posed a thread on the dominant position of the (long-lasting) incumbent taxi companies, we view their staggered rollout as a natural experiment to study whether a shock in the level of competition induced taxi drivers to purchase green vehicles more often. Importantly, contrary to previous works, our setup abstracts away from some other features, such as firms' strategic (marketing-related) "green investments" (i.e. those incurred to satisfy consumers' preferences for cleaner technologies), to focus solely on the impact that an increase in the intensity of competition has on the incumbent suppliers' technology choices.

We find that in metropolitan areas in which Uber or Cabify entered the average monthly purchases of green vehicles increased by 27% relative to the "control" ones, suggesting that these platforms causally induced one extra green vehicle purchase by taxi drivers in every four. This finding is remarkable stable across different specifications —some of which control for a potential anticipation effect. Moreover, it proves robust to the use of propensity-score matching —which corrects for differences between metropolitan areas in which Uber or Cabify entered and those in which they did not—; and also when we use the "stacked-*DiD*" approach by Cengiz et al. (2019) that addresses recent concerns raised on the validity of the two-way fixed effects *DiD* estimator with staggered treatment —Goodman-Bacon (2021). We also provide compelling evidence that this effect is not stemming from a previously existing trend.

Then, we further confirm that the increase in the takeout of green vehicles solely responds to a spe-

[2] According to the data from the Spanish Directorate-General for Traffic, 85% of the vehicles purchased in Spain by taxi drivers between December 2014 and February 2020 are powered solely by either diesel fuel (56%) or unleaded gasoline (29%).

cific shock in the taxi business, as we show that this effect is not observed in other (vehicle-dependent) sectors unlikely to be affected by the entry of Uber or Cabify —namely, driving schools and car rentals. Finally, we also rule out that this result responds to a change in the timing patterns of vehicle purchases. In particular, we show that total purchases by taxi drivers are constant over time across treated and control metropolitan areas —i.e., the result is not due to more frequent (overall) purchases— and, consistently, we do not observe either a delay in the scrappage decisions following the entry of the platforms. Likewise, we do not find a change in the purchases of second-hand vehicles among taxi drivers.

In Spain, the number of cars that ride-hailing companies can operate is limited by local regulation —as a rule of thumb, one vehicle is allowed per every thirty taxi licenses.[3] Since Uber and Cabify typically deployed the maximum number of vehicles allowed at the regional level whenever they enter a metropolitan area, then it was extremely unlikely for them to enter also into another metropolitan area within the same province. As a consequence, taxi drivers in these latter metropolitan areas could anticipate that competition will be less intense for them. Consistently, we find that the effect on the takeout of different types of vehicles in these metropolitan areas is just the opposite to that found in cities where Uber and Cabify entered: taxi drivers did not significantly increase the purchase of green vehicles. In fact, our estimates suggest that they were slightly more likely to buy dirty ones.

As mentioned above, our setup constitutes a close-to-ideal setting with which to study the effect of market competition on the adoption of new, green technologies by dominant suppliers for several reasons. First, in all the metropolitan areas that we consider, the incumbent taxi companies enjoyed a dominant position with very limited (or no) competition —CNMC (2017). Thus, the entry of Uber or Cabify constitutes a shock in the intensity of competition that significantly reduced their market share. Moreover, this shock can be interpreted as an exogenous one, as the entry of these platforms usually responds to tourism or demographic-related reasons —Berger et al. (2018).[4]

Second, the entry of these platforms is unlikely to drive changes in the key characteristics of the incumbent firms (prices, number of suppliers, etc.). Note that conventional taxi businesses are regulated by a limited number of medallions, which very rarely changes. Similarly, fares and drivers' working

[3]Uber and Cabify cannot operate as peer-to-peer ride-sharing platforms in this country. Instead, similarly to taxi companies, they provide ride-hailing services with professional drivers.

[4]In the same vein, we document that the entry of Uber and Cabify also respond to similar reasons in Spain. In particular, we find that they entered in cities with greater population, travelers, and unemployment —see Table A.1 in Appendix A.

hours of conventional taxi companies remain the same in the post-platforms period.[5] This might not be the case in previous studies that rely on other type of shocks, such as trade liberalization or deregulation, in which additional competition might induce incumbent firms to reallocate production or to exit the market, potentially overestimating the effect of competition on the diffusion of green technologies.

Third, taxi drivers' incentives to purchase green vehicles are unlikely driven by marketing-related purposes, that is, to attract customers with particular environmental preferences. This is because taxi drivers are anonymous: when a consumer hails a taxi, she cannot choose it according to the engine the driver uses —Hall and Krueger (2018). Again, this might not be the case in other sectors, in which customers might choose one supplier over another due to environmental concerns, creating thus a strategic-investment incentive on suppliers.[6] Four, as Aghion et al. (2020) explain, the use of vehicles data is ideal and appropriate in this kind of studies, since the distinction between "clean" and "dirty" technologies is clear and highly relevant. This distinction is even easier in the Spanish context, as there is a government-approved sticker-based classification that easily identifies environmentally-friendly vehicles.

This paper makes a contribution that is relevant for different strands of the literature. First, it is related to the recent and growing literature on market structure and green technologies. Perhaps the closest paper is by Aghion et al. (2020). These authors develop a model to document that firms innovate green to escape market competition, and validate their theoretical findings using data from the European automobile sector. However, part of their story is precisely that firms innovate green to attract consumers with certain environmental preferences. We abstract away from the role of consumers' preferences to focus solely on the market competition effect on technology choices. Another related paper is by Nesta et al. (2014), which documents that renewable energy subsidies are more effective in fostering green innovation in countries with liberalized energy markets. However, using liberalization as a proxy for the intensity of competition might also present some challenges, as discussed above.

Our paper also adds to the literature that examines the effects of competition on technology diffusion in the presence of regulatory policies. Aghion et al. (2015) address the issue of complementary between market competition and industrial policies along the lines of recent Schumpeterian models on

[5] Anecdotal evidence suggest that one of the reactions of the incumbent taxi companies was to develop apps (similar to those used by Uber and Cabify) to hail a taxi. This favors the main argument of the paper, that is, that an increase in competition induces the adoption of new technologies, as no conventional taxi company used apps before the arrival of Uber/Cabify.

[6] As explained by Woo and Zarnikau (2019), many electricity companies offer retail pricing plans with more renewable energy content (usually for a premium fee) to attract customer with certain environmental preferences.

innovation and competition —Aghion et al. (2001), Aghion et al. (2005). They find that policies targeted at sectors with higher technological potential have a larger effect on firms' innovative efforts. These theoretical findings are in line with our empirical results. In this strand of the literature, many papers have focused on the role of environmental regulation in liberalized energy sectors —Sanyal and Ghosh (2013), Jamasb and Pollitt (2011), and Asane-Otoo (2016). However, identification in these papers might be threatened if, as Lee (2020) documents, liberalization increases the likelihood of adopting policies that promote green investments. In addition, it might be potentially complicated to disentangle whether their results are driven just by market competition, or also as reaction to the liberalization policies.

Finally, we also contribute to a recent literature on the economic impact of ride-hailing digital platforms. However, many of these papers —with some notable exceptions, such as Berger et al. (2018) and Anderson and Davis (2021)— focus on the consequences just for either riders or drivers —Cohen et al. (2016), Cramer and Krueger (2016). To the contrary, we focus on the impact of these platforms on the technology choices of incumbent suppliers to draw implications that go beyond this sector.

The rest of the paper proceeds as follows. Section 2 provides some background. Section 3 discusses the data and the empirical strategy. Section 4 presents the empirical results, and Section 5 concludes.

2 Background

In this section we provide some background on the taxi industry in Spain, and on the staggered rollout of ride-hailing platforms across different metropolitan areas.

2.1 The taxi business in Spain

In many countries, the conventional taxi business is a tightly regulated one with notorious entry barriers and restrictions —Cramer and Krueger (2016). In Spain, the regulation of the local taxi companies is implemented at the municipal level or at the metropolitan-area level either by the city hall or by some other local government agency.[7] The most commonly observed regulatory restrictions are as follows.

First, the number of taxi drivers at the metropolitan area is limited by a system of medallions or licenses, which grant them the right to ride passengers. The number of medallions rarely changes.

[7]For example, the city hall regulates the taxi company that operates in the metropolitan area of Málaga, but in Pamplona this is done by the *Mancomunidad de la Comarca de Pamplona* —a supra-municipal body.

Therefore, drivers that are interested in joining the taxi business need to purchase these medallions in the secondary market.[8] Second, taxi drivers are restricted from picking up consumers outside of the jurisdiction that issued their license. Third, fares are heavily regulated, and common across all taxi drivers within a metropolitan area. These fares are usually established in a two-part tariff fashion —a minimum fee plus a per-mile fee—, with some price discrimination according to the area (e.g., airport fares) or time of use (e.g., night and bank holidays fares). Some other features, such as the color of the vehicles, are also regulated in some cities.[9] All these restrictions create huge entry barriers in the ride-hailing services market. As a consequence, local taxi companies have traditionally enjoyed a (*de facto*) monopolist position in this market in all major cities across Spain, as the Spanish National Commission on Markets and Competition (CNMC, in its Spanish acronyms) documents —see CNMC (2017).

The vast majority of the drivers that work for the local taxi companies in Spain are either self-employed or they work on behalf of an entrepreneur in exchange of a revenue-based commission. In both cases, either the taxi drivers themselves (or the entrepreneurs) choose and purchase the vehicle they use in the business, considering not only the upfront payment but also the per-mile cost as the key ingredients to calculate the expected return of the vehicle.[10] Both the number of hours and also (in many cities) the days in which taxi drivers are allowed to work are restricted and regulated. Therefore, taxi drivers cannot increase the number of hours or days they serve passengers to increase the return on their vehicles. In addition, they are not allowed to partner with Uber or Cabify —and they cannot do this undercover, since taxis are painted with easily identifiable colors in most cities.

2.2 The entry of ride-hailing platforms across Spanish metropolitan areas

To compete with conventional taxi companies, two app-based platforms entered different metropolitan areas in Spain, namely Uber —a renowned multi-national company based in San Francisco— and Cabify —the locally founded alternative to Uber.[11]

Uber launched its peer-to-peer ride-sharing service for the first time within Spain in Barcelona in April 2014. Then, this service was also introduced the same year in the metropolitan areas of Madrid

[8] As Angrist et al. (2017) explains, the limited supply of taxi medallions has made them a valuable asset.

[9] For example, taxis must be painted white with a yellow stripe in Sevilla, and they must be white with a stripe containing red and white squares (similar to those in the Croatian flag) in Murcia.

[10] In a few cities (e.g., Palma de Mallorca), the set of vehicles that taxi drivers are allowed to purchase is restricted. However, the choice set is sufficiently ample, and includes vehicles of all sizes and types of engines.

[11] In other countries there are app-based local firms similar to Uber, such as Ola (India), Didi (China), and 99 (Brazil).

(September 2014) and Valencia (October 2014). Even tough Uber became rapidly popular in these cities within a few months —Spain was one of the fastest growing markets for this company at the time[12]—, and following a fierce opposition from conventional taxi drivers, a court banned Uber in December 2014, on the grounds that peer-to-peer ride-sharing services were not a legal activity in Spain. Following this prohibition, Uber was re-launched in Spain as a ride-hailing company with professional drivers. In March 2016, Uber entered again Madrid, and it subsequently entered seven other major metropolitan areas, including Malaga (June 2018), Sevilla and Cordoba (October 2018), Valencia and Granada (January 2019), Cadiz (July 2019), and Bilbao (November 2019).

Cabify was founded in Spain in 2011, and it started to operate in Madrid in February 2012. Unlike Uber, Cabify was originally not a peer-to-peer ride-sharing platform, but a company offering in-advanced-booking rides with professional drivers. Therefore, the Spanish court did not ban it in December 2014. Initially, the company offered only the *Cabify Executive* service with high-end vehicles, intended for either corporate clients and/or relatively wealthy passengers. This service was introduced in a few cities —including Madrid, Barcelona, Bilbao, Tenerife, Vitoria, Sevilla, and Malaga—, although the company was relatively unknown within Spain.[13]

In mid-2013, the company launched *Cabify Lite*, a budget-friendly service provided with mid-range vehicles, with the possibility of instantly-booked rides —unlike Cabify Executive, which required in-advanced booking. The pilot version of this service was first introduced in some of the aforementioned cities (e.g., Madrid and Malaga), where it became more popular than Cabify Executive. In fact, and consistent with the information presented in Figure B.1 (Appendix B), the notoriety of Cabify gained momentum in 2017: the company multiplied by 6 the rides relative to that in 2016. Given the success of Cabify Lite, the company launched it in other metropolitan areas, including Alicante (June 2018), Coruña (December 2018), Barcelona (January 2019), Murcia (March 2019), and Santander (June 2019).

Importantly, both Uber and Cabify Lite entered different cities unexpectedly (as a surprise), and the time between the announcement of entry and the actual entry was usually short (about a month), limiting thus the possibility of substantial anticipation effects.[14] A summary of the staggered rollout of

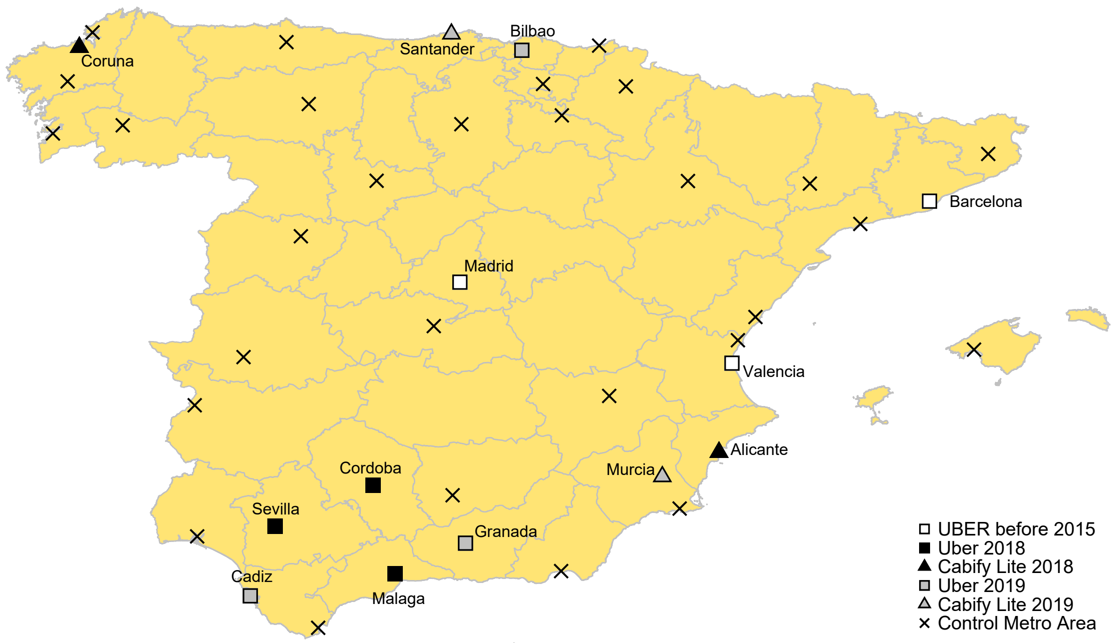
[12]Niall Wass, Uber's senior vice president for Europe, stated in November 2014 at the European E-commerce Conference (EEC) that the Spanish cities were among the fastest growing cities in the history of the company.

[13]In Appendix B, we provide evidence of the relatively lack of awareness of this company before 2017. In particular, Figure B.1 displays the Google Trend Index in Spain for the period covered by our sample for both words "Uber" and "Cabify". As discussed, the word Cabify was relatively little searched before 2017.

[14]Still, we control for a potential anticipation effect the month before the entry of these companies in the empirical analysis.

these companies across Spain is provided in Figure 1.

Figure 1. Spatial diffusion of Uber and Cabify Lite in Spain



Note: This figure provides a map with the metropolitan areas in Spain that we consider (all those with population greater than 100,000), indicating the rollout of both Uber and Cabify Lite across them. Squares represent metropolitan areas in which Uber entered —white squares indicate the metropolitan areas in which Uber was launched as a peer-to-peer ride-sharing company in 2014 (banned in December 2014). Triangles represent those in which Cabify Lite entered —we do not indicate the metropolitan areas in which Cabify Executive was previously present. Black crosses represent the control metropolitan areas —those in which neither Uber nor Cabify Lite entered. Two additional control metropolitan areas located in the Canary Islands are not included in this map for the sake of space limitations. Thin gray lines indicate the boundaries of the provinces.

Following a fierce opposition from drivers in conventional taxi companies, both Uber and Cabify Lite were also heavily regulated in Spain. More precisely, these companies are abided by the regulation of ride-hailing, professional driver companies —in Spanish, *Vehículo de Transporte con Conductor* or VTC. Among other things, the number of vehicles that these companies can operate is capped at the regional level. The general rule is that the number of VTC vehicles cannot exceed one every thirty taxi licenses — CNMC (2019). However, despite the regulatory efforts to prevent the growth of these companies, there is substantial evidence that their entry provided an unprecedented shock in the intensity of competition in the taxi industry in Spain.[15] In the following we study to what extent this unexpected shock in competition induced professional taxi drivers to switch from dirty vehicles to green ones.

[15] Akimova et al. (2020) document that Cabify and Uber had a significantly negative effect on the profitability of the traditional taxi companies in Spain. Similar evidence is also found in other countries —e.g., USA and Canada (Ngo, 2015).

3 Data description and empirical strategy

In this section, we discuss the data and present the empirical strategy that we use to estimate the impact of the entry of ride-hailing platforms on taxi drivers' vehicles purchase decisions.

3.1 Data

We combine publicly available secondary data from disparate sources together with the set of dummies that capture the entry of Uber and Cabify Lite in different metropolitan areas in Spain —see Figure 1— and some other indicators. By doing so, we obtain a novel panel data at the metropolitan area-month level, covering the period from December 2014 to February 2020.

Data on vehicles purchased. To begin with, data on all the new vehicles purchased by professional taxi drivers was obtained from the Spanish Directorate-General for Traffic (DGT, in its Spanish acronyms).[16] This dataset includes information on the type of vehicle purchased (engine, size, brand, etc.) and also provides other characteristics of the buyer —city of residence, purchase date, etc. We focus on all the purchases of regular (5-passenger) vehicles by taxi drivers in the metropolitan areas included in the map in Figure 1 between December 1, 2014 and February 28, 2020, which we aggregate by month.

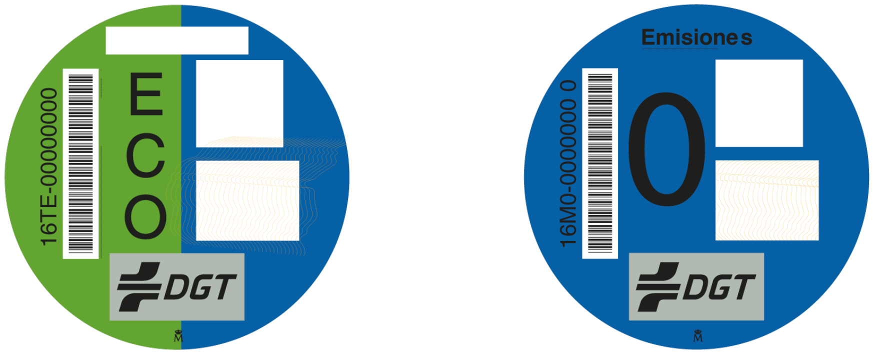
To distinguish between different types of vehicles, we use the system of stickers implemented (nationwide) by the Spanish government, which identifies green vehicles —i.e., those that are not powered exclusively by diesel or unleaded fuel— with either an "Eco" sticker or a "Zero" sticker —see Figure 2.[17] The Eco sticker is granted to plug-in hybrid electric vehicles (PHEV) that can travel up to 40 kilometers (around 25 miles) without using its combustion engine, to hybrid electric vehicles (HEV) —those that combine an internal combustion engine with electric propulsion—, and to all natural gas powered vehicles —i.e., those that use compressed natural gas (CNG) or liquefied natural gas (LNG).[18] The Zero sticker is granted to battery electric vehicle (BEV), range-extended electric vehicles (REEV), hydrogen internal combustion engine vehicles (HICEV), plug-in hybrid electric vehicles (PHEV) that can travel more than 40 kilometers (25 miles) without using its combustion engine, and to fuel cell vehicles (FCV).

[16] This dataset includes only the vehicles purchased by professional drivers in conventional taxi companies. It does not capture those purchased by professional drivers working in some other sectors or companies (including Uber and Cabify).

[17] A similar system of stickers exists in other countries in the European Union —Haq and Weiss (2016), Alberini et al. (2019).

[18] Recent research provides evidence that emissions from CNG and LNG are substantially lower compared to those by gasoline and diesel fueled vehicles —Takeuchi et al. (2007), Rood Werpy et al. (2010), Murphy (2010), Knittel (2012).

Figure 2. Stickers granted to green vehicles in Spain



(A) Eco sticker

(B) Zero sticker

Note: The figure provides the stickers used in Spain to identify green vehicles. The Eco sticker —Subfigure 2(A)— identifies plug-in hybrid electric vehicles (PHEV) that can travel up to 40 kilometers without using its combustion engine, hybrid electric vehicles (HEV), and all natural gas powered vehicles —compressed natural gas (CNG) or liquefied natural gas (LNG) vehicles. The Zero sticker —Subfigure 2(B)— identifies battery electric vehicles (BEV), range-extended electric vehicles (REEV), hydrogen internal combustion engine vehicles (HICEV), plug-in hybrid electric vehicles (PHEV) that can travel more than 40 kilometers without using its combustion engine, and fuel cell vehicles (FCV).

In our empirical analysis, dirty vehicles are those not allowed to carry one of these stickers.[19]

Even tough green vehicles are slightly more expensive (upfront) than their gasoline and diesel counterparts, it is well-documented that they provide substantial per-mile savings —particularly to relatively high-kilometer drivers, such as taxi drivers (Hoekstra et al., 2017; Li et al., 2017). This is because driving dirty vehicles in Spain is penalized through a tough taxation of petroleum refined products.[20] In addition, some Spanish cities (namely, Madrid and Barcelona) implemented low-emission zones (LEZ), in which only green vehicles are allowed to circulate.[21] Despite these regulatory efforts, aggregate figures suggest that the takeout of green vehicles is still relatively low among professional and non-professional drivers. For example, according to the DGT, about 85% of the vehicles purchased in Spain by taxi drivers between December 2014 and February 2020 were powered solely either by diesel or unleaded gasoline.

Additional variables. To confer robustness to our estimation strategy, we add variables to control for some other characteristics that might potentially affect taxi drivers' vehicle purchase decisions. First,

[19]Biofuel-powered vehicles are not considered green vehicles in Spain. In any case, there is only one biofuel-powered vehicle purchased by a taxi driver in our sample, which we drop.

[20]Some other authors document that the savings of a green vehicle *vis-à-vis* a dirty one still remain in the presence of other cost differences (e.g., taxes, repair, maintenance, and insurance costs) —see Wu et al. (2015).

[21]For these particular cities, one might be concerned that the takeout of green vehicles among taxi drivers was driven not by the intensity of competition, but due to the implementation of the LEZs, as suggested by Wolff (2014). Thus, we remove from our main sample data from Madrid and Barcelona.

to control for socio-economic conditions and the purchasing power that might affect taxi drivers in different regions in Spain, we obtained data on both the unemployment rate and the mean income per-capita at the province level from (government-sponsored) official sources.[22] In addition, to control for political preferences, we also include a set of dummies that capture both the political party in power in the city and also at the State level.[23]

Then, we also add information on the State-level fuel tax rate to capture regional differences in the fiscal policies aimed at reducing the purchase of petroleum products for transportation purposes. Next, to capture idiosyncratic preferences for different types of vehicles across regions, we include the percentage of new "Eco" and "Zero" cars purchased by households (i.e., by non-professional drivers) at the metropolitan area level . This information was also obtained from the DGT. Finally, due to the strong interdependence between tourism and the taxi business —and also bearing in mind that tourism is a prominent industry in Spain—, we add data on the monthly number of travelers (i.e., those that stayed at least one night) at the province level, obtained from the National Statistics Institute.

Sample. We use data from all the metropolitan areas in Spain with population greater than 100,000 —see Figure 1. However, we drop from our sample those in which Uber entered before December 2014 (the first month included in our dataset) —namely, Madrid, Barcelona, and Valencia—, as the lack of pre-treatment data for these "always treated" metropolitan areas does not allow us to identify the effect of the entry of the ride-hailing platforms on the takeout of vehicles by taxi drivers in them.

There are three additional concerns if include these cities in our sample. First, Madrid and Barcelona implemented ample and stringent LZEs, where access by dirty vehicles is restricted. These LZEs are likely to discourage *per se* the purchase of dirty vehicles, as documented by Wolff (2014). Second, as explained by Albalate and Rosell (2021), both Madrid and Barcelona also implemented subsidies for professional drivers (including taxi drivers) to purchase green vehicles —we are not aware of the existence of similar subsidies in other cities in our sample. Again, for these two cities, one might be concerned that the impact on the takeout of green vehicles is not driven by market competition, but rather by these subsidies. Finally, for the case of Barcelona and Valencia, even though Uber re-entered these cities as a ride-hailing company in 2018 and 2019, respectively, it exited them a few months later, as a re-

[22] Income data at the province level (obtained from survey data) is not available for all the months included in our sample.

[23] Since the metropolitan areas in our sample comprise multiple cities (municipalities), we consider the party in power in the most populated one.

sponse to the mandatory 15-minute wait time rule for passengers to book a ride imposed to ride-hailing platforms. These multiple entry and exit events in these two cities makes it even more complicated to correctly identify the time effect of these platforms on taxi drivers purchase decisions.

Table 1. Summary statistics by the time Uber/Cabify Lite is introduced

	(1) All observations	(2) Always control	(3) Treated (before)	(4) P-val	(5) Treated (after)	(6) P-val
			Outcome variables			
Green vehicles (taxi)	1.825	1.408	2.402	0.000	5.681	0.000
Dirty vehicles (taxi)	3.847	2.984	6.994	0.000	4.841	0.000
			Baseline characteristics			
(log)-Unemployment	11.363	11.183	11.925	0.000	11.902	0.000
(log)-Income	7.035	7.038	6.999	0.013	7.148	0.001
(log)-Travelers	11.610	11.463	12.015	0.000	12.251	0.000
Fuel duties (diesel)	4.362	4.319	4.471	0.000	4.586	0.000
% Eco cars (priv. use)	5.021	5.031	3.913	0.000	8.838	0.000
% Zero cars (priv. use)	0.498	0.520	0.282	0.000	0.960	0.000
N. Observations	2,583	1,953	492		138	

Note: Summary statistics for all the variables included in our final dataset, except for the political party dummies (both at the State and at the city level). "Green vehicles (taxi)" captures the number of vehicles purchased by taxi drivers that are suitable to carry either the Zero sticker or the Eco sticker, while "Dirty vehicles (taxi)" captures the vehicles purchased by taxi drivers that are not allowed to carry one of these stickers. All the variables are captured at the metropolitan area-month level, expect unemployment, income, travelers —which are captured at the province-month level—, and fuel taxes —which is captured at the State-month level.

Our final sample, thus, covers data from 41 major metropolitan areas at the month level. Each metropolitan area comprises all the suburbs and cities (municipalities) in which the local taxi company is allowed to pick up passengers. For instance, for the particular case of Sevilla, the metropolitan area is defined not only by the boundaries of the city of Sevilla itself, but it also includes the 21 municipalities in the so-called *Aglomeración urbana de Sevilla*. A complete list of all the municipalities included in each of the metropolitan areas in our sample is in the Appendix —see Table B.1. Table 1 contains summary statistics of the variables that we use in the empirical analysis for all these metropolitan areas. Notice that, for the treated ones, the monthly average number of dirty vehicles purchased by taxi drivers is higher before the entry of Uber and Cabify Lite than that of green ones. However, once these platforms were launched, this pattern is reversed. In the next section we explain the techniques that we use to establish the casual link between the entry of these platforms and the emergence of this new pattern.

3.2 Regression model

To study the impact of the rollout of ride-hailing platforms across Spain on the fleet in conventional (incumbent) taxi companies, we examine the aggregated purchases of new vehicles by taxi drivers at the metropolitan area-month level. Our empirical strategy relies on variation both in the timing and place of entry of Uber and Cabify Lite. Metropolitan areas without ride-hailing platforms during the period of our sample provide a natural control group, allowing us to identify the effect on vehicle purchases before and after the entry of these platforms in the treated ones. Our empirical model controls for some other underlying demand and supply-related variables described above.

To accommodate the count data nature of the dependent variable (number of new vehicles purchased by taxi drivers in a metropolitan area-month) in our panel data setting, we use a fixed-effects Poisson quasi-maximum-likelihood estimation with standard errors clustered at the province level that account for arbitrary patterns of correlation among the observations for each province —Wooldridge (1999), Fabrizio (2013), McCabe and Snyder (2015).[24] Our main regression model is as follows:

$$E\left(\text{Taxi}^X_{m,p,s,t}\middle|Z_{m,p,s,t},\lambda_m,\theta_t\right)=\exp\left(\beta\,\text{Uber/Cabify Lite}_{m,p,s,t}+Z_{m,p,s,t}+\lambda_m+\theta_t\right), \tag{3.1}$$

where $\text{Taxi}^X_{m,p,s,t}$ is the aggregated number of type-X vehicles purchased by taxi drivers in metropolitan area m, province p, state s, and month t, where $X \in \{\text{Green}, \text{Dirty}\}$; [25] $\text{Uber/Cabify Lite}_{m,p,s,t}$ is a dummy that indicates whether Uber or Cabify Lite operate in metropolitan area m;[26] $Z_{m,p,s,t}$ is a vector of regressors, λ_m is a metropolitan area fixed effect controlling for all unobserved time-invariant differences across metropolitan areas, and θ_t are year-month indicators that control for common patterns of purchases of vehicles over time (e.g., incentives and policies that apply to all metropolitan areas). In an alternative specification of equation (3.1), we additionally include state-specific time trends to control for other factors that may influence purchases decisions differently across states over time.

The previous regression provides the standard generalized *DiD* setup to estimate the impact of a

[24]As Wooldridge (1999) and Fabrizio (2013) explain, this method does not rely on the often-violated assumption of mean variance equivalence, while it provides consistent estimates under general conditions.

[25]The distinction between green and dirty vehicles is based on the system of stickers explained above. A similar distinction between dirty and green cars to study innovations in the automobile sector is in Aghion et al. (2016).

[26]For the sake of expositional clarity, and bearing in mind that there is no substantial overlap of these companies across metropolitan areas —see Figure 1—, we use just one dummy for both of them. However, we show as a robustness check that our results are practically unchanged if we use a different dummy variable for each of these platforms.

shock in the level of competition —captured by the entry of Uber or Cabify Lite— on the takeout of green/dirty vehicles among taxi drivers relative to the metropolitan areas in which these companies did not enter. We base our analysis on two major identifying assumption. First, that the pattern of purchases of different types of vehicles among taxi drivers in treated metropolitan area would have evolved in a similar way as in the control ones had the entry of Uber or Cabify Lite not occurred. Although this common trend assumption is not directly testable, we provide the evolution of pre-trends in Section 4.2 to confirm the validity of our identifying strategy.

Second, we also rely on the assumption that the entry of these platforms is not correlated with unobserved factors that affect the takeout of green/dirty vehicles, after controlling for different underlying drivers of these purchases that vary across metropolitan areas and over time. This assumption is likely satisfied if we consider that, as discussed above, Uber and Cabify Lite entry decisions are based on socio-demographic characteristics —e.g., population and number of visitors. These characteristics are, in principle, uncorrelated with taxi drivers' vehicle purchase decisions, which are presumably rather affected by environmental policies and fuel prices.

We remark, tough, that recent literature has raised concerns on the validity of two-way fixed effects estimators in settings in which the treatment is staggered over time —Athey and Imbens (2018), Callaway and Sant'Anna (2020), De Chaisemartin and d'Haultfoeuille (2020), Sun and Abraham (2020), Baker et al. (2021)—, as is in our case. We address this issue in Section 4.3 by presenting results using the alternative "stacked-*DiD*" approach proposed by Cengiz et al. (2019). In the same section, we also discuss some other potential concerns regarding our empirical regression model, and present additional results that prove the robustness our main conclusions.

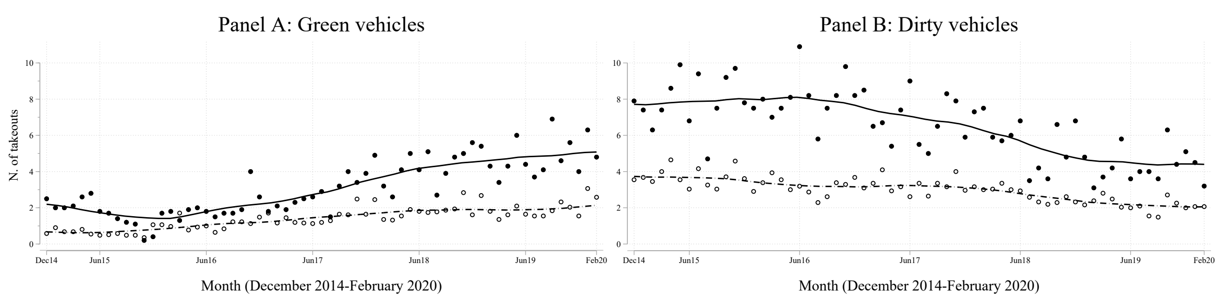
Finally, recall that the number of vehicles that Uber and Cabify Lite are allowed to operate is capped at the regional level and, in those cities where they entered, they typically deployed the maximum number of vehicles they are allowed to (one in every in every thirty taxi licenses). Therefore, taxi drivers in the cities in which these platforms did not enter but located within the same province in which they did so might expect to remain in a monopolistic position. Consequently, one should expect to find the opposite effect in the takeout of different types of vehicles by taxi drivers to that obtained for the cities in which Uber or Cabify Lite actually entered. To test this hypothesis, we also estimate an augmented version of equation (3.1), which includes a dummy equal to 1 if Uber or Cabify Lite did not entered metropolitan area m in province p but entered some other metropolitan area m' in province p, and equal to 0 otherwise.

4 Empirical results

4.1 Main results

We start by providing graphical evidence on the evolution of vehicle purchases by taxi drivers throughout the period covered in our sample using the raw data. Figure 3 shows the total number of new vehicles purchased both in metropolitan areas where Uber or Cabify Lite entered (solid line) and in those in which these platform did not enter (dashed-dotted line) plotted against time —these lines represent local linear regressions separately estimated for both subsets of metropolitan areas.

Figure 3. Number of green and dirty vehicles purchased by taxi drivers in treated and control metropolitan areas against time



Note: The lines represent local linear regressions separately estimated for treated (solid line) and control (dashed-dotted line) metropolitan areas. Mean values by month-year for treated (black) and control (white) metropolitan areas shown as circles.

Panel A reports the results for green vehicles, while Panel B does so for dirty ones. In the former panel, the raw data suggests an increase in the monthly purchases of green vehicles across all metropolitan areas over time. However, this increase is much more pronounced in those in which Uber or Cabify Lite entered, particularly, from the end of 2017 on. Conversely, Panel B displays a decline in the takeout of dirty vehicles among taxi drivers in all the metropolitan areas, being this decline particularly pronounced in those ones where Uber or Cabify Lite entered. These "raw" data comparisons —which do not take into account geographical characteristics nor other controls— already suggest that taxi drivers reacted to the entry of these ride-hailing platforms by purchasing green (dirty) vehicles more (less) frequently relatively to the control metropolitan areas.

We present next the empirical results from equation (3.1) in Table 2. We show the estimates of the

Table 2. The effect of Uber/Cabify Lite entry on takeout of different types of vehicles by taxi drivers

	(1)	(2)	(3)	(4)	(5)
			Panel A: Green vehicles		
Uber/Cabify Lite	0.271***	0.306***	0.284***	0.317***	0.300***
	(0.100)	(0.077)	(0.087)	(0.081)	(0.090)
Uber/Cabify Lite (t-1)			0.153	0.138	0.186
			(0.195)	(0.186)	(0.186)
(log)-Unemployment			0.714*	0.362	0.177
			(0.433)	(0.320)	(0.311)
(log)-Travelers			0.114	0.051	-0.033
			(0.119)	(0.098)	(0.101)
Fuel duties (diesel)			-0.552	-0.309	-0.531
			(0.354)	(0.305)	(0.340)
% Eco cars (priv. use)			0.003	-0.001	-0.009
			(0.013)	(0.015)	(0.017)
% Zero cars (priv. use)			0.022	0.003	-0.042
			(0.040)	(0.037)	(0.042)
(log)-Income					0.043
					(0.074)
Mean dep. var. non-treated	1.408				
N. metro. areas	41	41	41	41	41
N. Observations	2,576	2,576	2,505	2,505	2,061
			Panel B: Dirty vehicles		
Uber/Cabify Lite	-0.079	-0.016	-0.079	-0.001	0.004
	(0.099)	(0.085)	(0.096)	(0.088)	(0.103)
Uber/Cabify Lite (t-1)			-0.097	-0.097	-0.160
			(0.142)	(0.141)	(0.148)
(log)-Unemployment			0.174	0.078	0.188
			(0.238)	(0.177)	(0.185)
(log)-Travelers			0.040	0.018	0.046
			(0.067)	(0.055)	(0.062)
Fuel duties (diesel)			-0.470	-0.067	-0.165
			(0.326)	(0.180)	(0.201)
% Eco cars (priv. use)			-0.003	0.008	0.005
			(0.016)	(0.013)	(0.013)
% Zero cars (priv. use)			-0.012	-0.054	-0.024
			(0.035)	(0.038)	(0.040)
(log)-Income					0.080
					(0.073)
Mean dep. var. non-treated	2.984				
θ_t	✓	✓	✓	✓	✓
λ_m	✓	✓	✓	✓	✓
Pol-parties FE	✓	✓	✓	✓	✓
State $\times$ Trend FE		✓			✓
N. metro. areas	41	41	41	41	41
N. Observations	2,583	2,583	2,511	2,511	2,067

Note: Column (1): DiD with month-year, metro area and political parties (City and State) fixed effects. Column (2): DiD with previous fixed effects and a state-specific time trend. Column (3): DiD with fixed effects of columns (1) and control variables. Column (4): DiD with fixed effects of columns (2) and control variables. Column (5): DiD with month-year, metro area and political parties (City and State) fixed effects, a state-specific time trend, and all control variables —including income. The significance levels are as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the province level.

effect of the entry of Uber and Cabify Lite on the purchase of new vehicles by taxi drivers, distinguishing between green —Panel A— and dirty vehicles —Panel B. Column (1) presents the estimates with no control variables other than the dummies for the political parties, and the metropolitan area and month-year fixed effects. For the case of green vehicles, the coefficient of interest is positive and significant at the 1% level, suggesting that the entry of Uber and Cabify Lite increased the takeout of these vehicles among taxi drivers by 27%. A positive, significant, and slightly higher effect (around 31%) is obtained also if we add State-specific time trends that account for additional unobserved factors that vary within states over time —Column (2). However, the same coefficient is found to be slightly negative (close to zero) but not significant in Panel B for dirty vehicles in both columns.

Next, Column (3) presents the estimates including the full set of control variables described above but income —due to the substantial number of missing observations—, and also a dummy equal to 1 for the month before the entry of Uber or Cabify Lite in the corresponding metropolitan area, to capture a potential anticipatory effect by taxi drivers.[27] Then, Column (4) additionally includes state-specific time trends. Finally, column (5) includes all the above plus income at the province level as an additional control variable. In all these columns in Panel A, the estimated effect remains positive, significant at the 1% level, and very similar in magnitude to the previous ones, suggesting that the entry of these platforms increased by about 29-32% the takeout of green vehicles among taxi drivers. However, the coefficient for dirty vehicles remains not significant and close to zero.

4.2 Research design validity

The evidence in the previous section suggests that the entry of Uber and Cabify Lite causally induced taxi drivers to purchase green cars more often. In this section, we provide additional estimates to strengthen the validity of this result. First, we show no evidence of differential pre-trends in the purchases of both types of vehicles before the entry of these firms. Second, following Berger et al. (2018), we show using two different strategies that the increase in the takeout of green vehicles is only found among taxi drivers, while purchases made by other (vehicle-dependent) professionals who were unlikely affected by the entry of ride-hailing platforms remain unchanged at the time of their entry.

[27] Uber and Cabify Lite usually entered different cities in Spain unexpectedly. However, they usually announce their entry a few days in advanced (according to the World Bank, the time required to start a business in Spain is about 13 days). Therefore, we should not expect a substantial anticipation effect beyond one month before the entry of these companies.

Evolution of pre-trends. As in any *DiD* setup, a key assumption needed to identify the effect of interest is that outcomes in treated and control locations would follow the same time trend in the absence of the entry of Uber or Cabify Lite. However, it is not straightforward to compare both units around the beginning of the treatment, given the staggered rollout of the platforms across treated metropolitan areas, implying that there is not a clear point in time serving as cut-off date to distinguish the *before* and *after* periods. Nevertheless, we can test the existence of differential trends in the takeout of both types of vehicles prior to the Uber or Cabify Lite entry to confirm the validity of our identifying strategy.

To do so, we extend our main regression model —equation (3.1)— by including several leads and lags of the entry of Uber/Cabify Lite variable, in the spirit of Autor (2003). In particular, we estimate the following regression model:

$$E\left(\text{Taxi}^{X}_{m,p,s,t} \middle| Z_{m,p,s,t}, \lambda_m, \theta_t\right) = \exp\left(\sum_{t=-18}^{-1} \beta_t Lead_{m,p,s,t} + \sum_{t=0}^{18} \beta_t Lag_{m,p,s,t} + Z_{m,p,s,t} + \lambda_m + \theta_t\right), \quad (4.1)$$

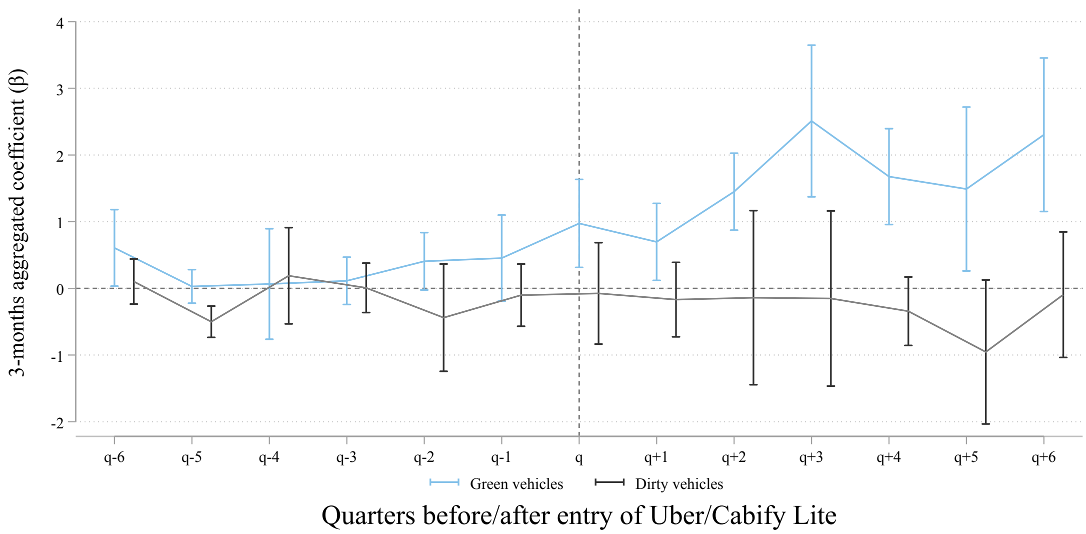
where the dependent variables are the same as before, $Lead_{m,p,s,t}$ are dummies equal to 1 for the corresponding months before the entry of Uber or Cabify Lite, and $Lag_{m,p,s,t}$ are similar dummy variables but for the corresponding months following the launch of these platforms. This specification allows us to check not only the existence of potential pre-trends, but also the impact on the takeout of different types of vehicles before and after market entry.[28]

The results of this exercise are included in Figure 4, in which the lead and lag estimates are aggregated and displayed by quarter.[29] The light blue line captures the impact on the takeout of green vehicles by taxi drivers six quarters before and after the ride-hailing platforms were launched in the corresponding local market, while the dark gray line captures the same but for dirty cars. In both cases, we include 95% confidence intervals as vertical bands. Both lines show that the effect from six quarters to one quarter before the entry of these platforms is close to zero and not significant, ruling thus out the possibility of pre-trends or an anticipation effect. In fact, the gap in the takeout of both types of vehicles widens from the quarter in which Uber or Cabify Lite enter on. This result further suggests that these

[28] As explained above, pre-trends due to an anticipation effect are unlikely to occur, considering that the time between the announcement and the actual entry of Uber and Cabify in a city is usually very short.

[29] We do so for two main reasons. First, due to the substantial number of zeros in the dependent variables, the estimation of the leads and lags by month is more "noisy" —the coefficients (and their corresponding standard errors) substantially change, for instance, for months in which there are many zeroes. Second, we do so for the sake of expositional clarity, as presenting monthly leads and lags for one and a half year before and after market entry would require a substantially larger figure.

Figure 4. Estimated impact on the takeout of different types of vehicles by taxi drivers relative to the time of entry of Uber and Cabify Lite



Note: The figure captures the impact that Uber/Cabify Lite had on the takeout of green (light blue) and dirty (dark gray) vehicles by taxi drivers six quarters before and after the quarter in which these platforms entered the corresponding local markets. The vertical dashed line represents the quarter in which Uber/Cabify Lite entered the market. Estimates are obtained using equation (4.1). Vertical bands represent $\pm$ 1.96 times the standard error of each point estimate.

platforms triggered more purchases of green vehicles among taxi drivers relative to that of dirty ones.

Placebo tests and DDD estimates. We can also estimate a placebo *DiD* test by using as dependent variable aggregated purchases by professionals in two different vehicle-dependent sectors, namely, in driving schools and in rental car companies. The intuition behind is that differences in the purchase decisions for those professionals are not expected, as none of them work in a sector directly affected by the entry of Uber or Cabify Lite. Hence, any *DiD* estimates different from 0 would suggest that the parallel trend assumption is violated. We show in Table 3 that this is not the case both for green cars —Panels A1-A2— and for dirty ones —Panels B1-B2—, thus offering support to the claim that our results are driven by the entry of Uber or Cabify Lite rather than other omitted factors.[30]

Finally, we add a third difference to our regression model by computing the difference in the aggregated purchases of green/dirty cars by taxi drivers and by professionals in the other two sectors used

[30] Notice that the number of metropolitan areas —and thus the number of observations— in Panels A1 and A2 is smaller. This is because, for some metropolitan areas, there are no purchases of green cars in both sectors. We address this issue using a triple difference strategy explained just below.

Table 3. Placebo effect of Uber/Cabify Lite entry on takeout of different types of vehicles by other drivers

	(1)	(2)	(3)	(4)	(5)
			Green cars		
			Panel A1: Driving schools		
Uber/Cabify Lite	0.053	-0.412	0.254	-0.207	-0.672
	(0.371)	(0.518)	(0.388)	(0.479)	(0.480)
Mean dep. var. non-treated	0.063				
N. metro. areas	30	31	31	31	30
N. Observations	1,298	1,381	1,381	1,381	1,018
			Panel A2: Rental cars without driver		
Uber/Cabify Lite	-0.238	0.264	-0.054	0.419	0.535
	(0.398)	(0.506)	(0.472)	(0.526)	(0.729)
Mean dep. var. non-treated	0.616				
N. metro. areas	30	30	30	30	29
N. Observations	1,343	1,340	1,313	1,317	883
			Dirty cars		
			Panel B1: Driving schools		
Uber/Cabify Lite	-0.020	-0.102	-0.073	-0.115	-0.057
	(0.146)	(0.163)	(0.160)	(0.167)	(0.200)
Mean dep. var. non-treated	0.797				
N. metro. areas	41	41	41	41	41
N. Observations	2,583	2,583	2,542	2,542	2,098
			Panel B2: Rental cars without driver		
Uber/Cabify Lite	-0.364	-0.340	-0.377	-0.401	-0.345
	(0.256)	(0.343)	(0.279)	(0.336)	(0.394)
Mean dep. var. non-treated	23.170				
N. metro. areas	41	41	41	41	41
N. Observations	2,583	2,583	2,542	2,542	2,098
λ_m and θ_t	✓	✓	✓	✓	✓
Pol-parties FE	✓	✓	✓	✓	✓
Time-varying control vars.			✓	✓	✓
$Log(income)$					✓
State $\times$ Trend FE		✓			✓

Note: Dependent variables: Number of takeouts made by $i)$ driving schools; $ii)$ rental cars companies. Panels A1-A2: Green cars. Panels B1-B2: Dirty ones. Column (1): DiD with month-year, metro area and political parties (City and State) fixed effects. Column (2): DiD with previous fixed effects and a state-specific time trend. Column (3): DiD with fixed effects of columns (1) and control variables. Column (4): DiD with fixed effects of columns (2) and control variables. Column (5): DiD with month-year, metro area and political parties (City and State) fixed effects, a state-specific time trend, and all control variables —including income. The significance levels are as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the province level.

in the previous placebo tests. Specifically, we estimate the following triple-difference (*DDD*) model:

$$E\left(\text{Taxi}^X_{m,p,s,t} - \text{Other}^X_{m,p,s,t} | Z_{m,p,s,t}, \lambda_m, \theta_t\right) = \exp\left(\beta \, \text{Uber/Cabify Lite}_{m,p,s,t} + Z_{m,p,s,t} + \lambda_m + \theta_t\right), \quad (4.2)$$

where $\text{Taxi}^X_{m,p,s,t} - \text{Other}^X_{m,p,s,t}$ is the difference in the number of type-X —green or dirty— vehicles purchased by taxi drivers and by *(i)* driving schools and *(ii)* rental car companies, for each metropolitan area m, province p, state s, and month t. By introducing this additional difference, we now compare purchasing decisions between metropolitan areas with and without ride-hailing platforms, before and after their entry, and between affected and not affected sectors. Therefore, this specification allows us to control for additional unobserved time-varying factors that are correlated with the entry of Uber/Cabify Lite, as the identifying variation now relies solely on the relative difference in the purchasing decisions of professionals in the taxi business relative to those in other (vehicle-dependent) sectors.

The results of this empirical exercise are presented in Table 4, which follows a similar structure to that in Table 3. Panels A1-A2 show that the estimated impact of the entry of Uber/Cabify Lite on the relative takeout of green cars by taxi drivers is positive and very similar in magnitude and significance to our baseline *DiD* estimates in Table 2, irrespective of whether we use the difference in aggregated purchases by taxi drivers with those by driving schools or by rental car companies. In addition, Panels B1-B2 show no significant effect on the relative takeout of dirty cars, which again is consistent with our baseline estimates.[31] Therefore, the estimates in Table 4 suggest that our results are not driven by unobserved time-varying factors at the local level correlated with the rollout of Uber and Cabify Lite.

4.3 Robustness checks

In this section we show that our main results also prove robust to the use of alternative estimation methods that address potential issues in our baseline empirical strategy.

Stacked-*DiD*. To begin with, recent literature has raised concerns on the validity of the two-way fixed effects *DiD* estimator in settings in which the treatment (in our case, the entry of the platforms) is staggered over time —Athey and Imbens (2018), Callaway and Sant'Anna (2020), De Chaisemartin and

[31] Notice that the number of metropolitan areas is the same that the ones used in the main *DiD* specification. However, the number of observations is smaller, specifically, in Panels B1-B2. This is due to the count data nature of the dependent variable: we must discard a few negative observations to estimate our fixed-effects Poisson regression via quasi-maximum-likelihood.

Table 4. DDD effect of Uber/Cabify Lite entry on takeout of different types of vehicles by taxi drivers relative to other drivers

	(1)	(2)	(3)	(4)	(5)
	Green cars				
	Panel A1: Difference vs driving schools				
Uber/Cabify Lite	0.253**	0.305***	0.253**	0.305***	0.261***
	(0.102)	(0.081)	(0.102)	(0.081)	(0.095)
Mean dep. var. non-treated	1.400				
N. metro. areas	41	41	41	41	41
N. Observations	2,535	2,535	2,535	2,535	2,062
	Panel A2: Difference vs rental cars without driver				
Uber/Cabify Lite	0.222**	0.289***	0.222**	0.289***	0.253***
	(0.099)	(0.086)	(0.099)	(0.086)	(0.098)
Mean dep. var. non-treated	0.616				
N. metro. areas	41	41	41	41	41
N. Observations	2,486	2,486	2,486	2,486	2,024
	Dirty cars				
	Panel B1: Difference vs driving schools				
Uber/Cabify Lite	-0.184	-0.096	-0.184	-0.096	-0.088
	(0.122)	(0.107)	(0.122)	(0.107)	(0.112)
Mean dep. var. non-treated	2.859				
N. metro. areas	41	41	41	41	41
N. Observations	2,229	2,229	2,229	2,229	1,814
	Panel B2: Difference vs rental cars without driver				
Uber/Cabify Lite	0.009	0.003	0.009	0.003	-0.125
	(0.142)	(0.125)	(0.142)	(0.125)	(0.183)
Mean dep. var. non-treated	1.797				
N. metro. areas	40	40	40	40	40
N. Observations	1,497	1,497	1,497	1,497	1,220
λ_m and θ_t	✓	✓	✓	✓	✓
Pol-parties FE	✓	✓	✓	✓	✓
Time-varying control vars.			✓	✓	✓
$Log(income)$					✓
State × Trend FE		✓			✓

Note: Dependent variables: Difference in the number of takeouts made by taxi drivers and $i)$ driving schools; $ii)$ rental cars companies. Panels A1-A2: Green cars. Panels B1-B2: Dirty ones. Column (1): DDD with month-year, metro area and political parties (City and State) fixed effects. Column (2): DDD with previous fixed effects and a state-specific time trend. Column (3): DDD with fixed effects of columns (1) and control variables. Column (4): DDD with fixed effects of columns (2) and control variables. Column (5): DDD with month-year, metro area and political parties (City and State) fixed effects, a state-specific time trend, and all control variables —including income. The significance levels are as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the province level.

d'Haultfoeuille (2020), Sun and Abraham (2020), Baker et al. (2021). The reason is that, in contrast to a standard *DiD* setting with a single treatment, in the staggered setup already-treated units also act as effective control units. That is, in our case, metropolitan areas with Uber or Cabify Lite are compared at some point to those in which Uber or Cabify Lite already entered —i.e., they act as incorrect controls. Thus, the two-way fixed effects *DiD* estimator does not capture the impact of the treatment, but it rather reflects a weighted average of all possible two-by-two *DiD* estimators —Goodman-Bacon (2021).

It is important to remark, though, that the aforementioned issue is not that relevant in our setting for two reasons. First, because the size of the control group —i.e., metropolitan areas without Uber or Cabify Lite— is substantially larger than the size of the treated group —those in which they entered. Therefore, a greater weight is placed on the comparison between treated and ("clean") control metropolitan areas when we use the two-way fixed effects estimator.[32] Second, because the entry of Uber and Cabify Lite in different metropolitan areas occurred close in time from each other. That is, all the metropolitan areas are treated towards the end of the period covered by our sample, leaving just a little window of time for a possible comparison between treated and already-treated metropolitan areas.

However, as a sanity check, we address the aforementioned concerns by additionally estimating our main regression model using the so-called "stacked-*DiD*" approach —see Cengiz et al. (2019). That is, we create "event-specific" datasets using the observations of the treated and the ("clean") control metropolitan areas in which a ride-hailing platform did not enter within a 8-month window —i.e., from $t = -8$ to $t = 8$, relative to the entry period $t = 0$.[33]We then stack these (relative time) event-specific datasets to calculate an average effect across all of them using a single set of treatment indicators. This approach is thus similar to a standard one in which the treatment happens contemporaneously for all treated units and, hence, it prevents already treated metropolitan areas to act as controls.

The empirical results using this alternative approach are included in Table 5, which follows a similar structure to the one that contains our baseline results. For the case of green vehicles —see Panel A— , the coefficients obtained in columns (1) to (3) are similar to those obtained in the same columns in Table 2. However, once we include the full set of controls and also the state-specific time trends, the estimated coefficient is positive, significant, and slightly higher in magnitude to those obtained in Table

[32]We show in Appendix A.1 evidence on this fact by providing the so-called Goodman-Bacon decomposition —Figure A.1.

[33]As explained in Baker et al. (2021), the length of time in the event windows might impact the stacked-*DiD* estimator. However, we show in Figure A.2 (Appendix A.1) that our results do not change when we choose alternative lengths: all estimates are significant and similar in magnitude for the case of green cars, while none is significant for the case of dirty cars.

Table 5. The effect of Uber/Cabify Lite entry on takeout of different types of vehicles by taxi drivers using stacked data

	(1)	(2)	(3)	(4)	(5)
	Panel A: Green vehicles				
Uber/Cabify Lite	0.235*	0.321**	0.263*	0.352***	0.344**
	(0.138)	(0.134)	(0.136)	(0.136)	(0.145)
Mean dep. var. treated	1.897				
N. metro. areas	41	41	41	41	41
N. Observations	5,248	5,248	5,141	5,141	3,813
	Panel B: Dirty vehicles				
Uber/Cabify Lite	0.078	0.098	0.089	0.124	0.163
	(0.086)	(0.089)	(0.085)	(0.089)	(0.106)
Mean dep. var. treated	2.430				
λ_m and θ_t	✓	✓	✓	✓	✓
Pol-parties FE	✓	✓	✓	✓	✓
Time-varying control vars.			✓	✓	✓
Log(*income*)					✓
State × Trend FE		✓			✓
N. metro. areas	41	41	41	41	41
N. Observations	5,236	5,236	5,141	5,141	3,818

Note: Column (1): DiD with month-year, metro area (both interacted with stack-specific indicators) and political parties (City and State) fixed effects. Column (2): DiD with previous fixed effects and a state-specific time trend. Column (3): DiD with fixed effects of columns (1) and control variables. Column (4): DiD with fixed effects of columns (2) and control variables. Column (5): DiD with month-year, metro area and political parties (City and State) fixed effects, a state-specific time trend, and all control variables —including income. The significance levels are as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the province level.

2. In particular, these estimates suggest an increase in the takeout of green vehicles among taxi drivers of about 34-35% following the entry of Uber or Cabify Lite. Finally, for the case of dirty vehicles —Panel B—, all the estimated coefficients of interest are again close to zero and not significant.

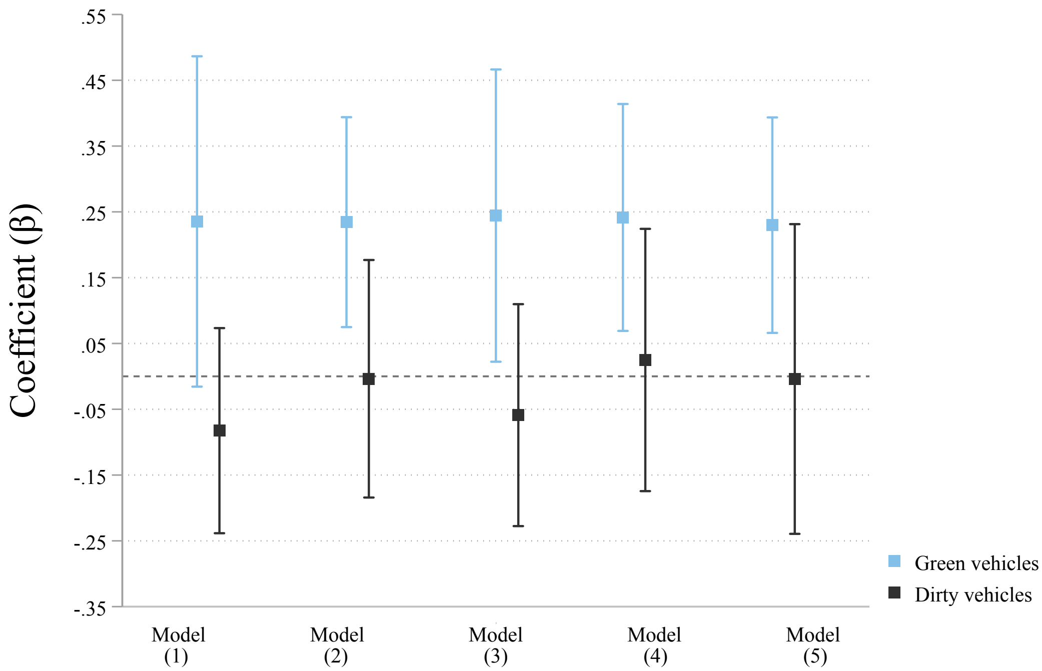
Matching *DiD*. We also acknowledge that the metropolitan areas in which Uber or Cabify Lite were launched are not well matched with those in which they were not. In fact, we provide some evidence that these firms entered metropolitan areas that have, on average, higher population, more tourists, and higher unemployment rates —see Table A.1 (Appendix A.2). We address this concern by using matching techniques that can help resolve the unbalanced sample problem. In particular, we proceed as follows.

We create a dataset with one observation per metropolitan area in order to implement a "greedy" matching procedure. We first compute the propensity score of being in the treated group —i.e., those with Uber or Cabify Lite— as a function of *(i)* the (log of) population at the metropolitan area level, *(ii)* the (log of) the number of tourists, *(iii)* the (log of) the unemployment rate, and *(iv)* state-level diesel duties.[34] Then, we match every treated metropolitan area to the control one that has the closest propensity

[34]Table A.1 (Appendix A.2) provides the standardized differences of the four variables used to calculate the propensity score, before and after matching metropolitan areas. This table suggests that the matching procedure helps to reduce the imbalances in the original dataset: after doing so, the standardized differences of all variables are no longer significant at the 5% level.

score based on *(i)-(iv)*. We do so applying an algorithm that selects the closest matching first, then the closest remaining matching, until there are no further pairings considered acceptable. After doing so, we obtain 10 pairs of matched treated and control metropolitan areas. Finally, we plug all this information regarding the matching cases back into the original dataset, and we re-estimate equation (3.1) using only the paired metropolitan areas, while clustering the standard errors at the matched-pair level.

Figure 5. Estimated impact of Uber/Cabify Lite entry on takeout of different types of vehicles by taxi drivers using a matching technique



Note: The figure displays the estimated coefficient of interest (β) in equation (3.1) by using a different subsample of metropolitan areas, obtained from a propensity score matching technique. In particular, we compute the propensity score of being in the treated group (i.e., metropolitan areas where Uber or Cabify Lite were launched) as a function of population, number of tourists, unemployment, and diesel tax rates, and we match them to control metropolitan areas using a "greedy" matching procedure. The coefficients were obtained by estimating equation (3.1) using the five specifications of our regression model presented in Table 2 for green vehicles (light blue) and dirty ones (dark gray). Standard errors are clustered at the matched-pair level. Vertical bands represent $\pm$ 1.96 times the standard error of each point estimate.

The estimated coefficient on interest —β in equation (3.1)— using this procedure for the five specifications considered in Table 2 —Models (1) to (5)— are displayed in Figure 5 for green vehicles (light blue squares) and for dirty ones (dark gray squares). In both cases, we include 95% confidence intervals as vertical bands. The estimates are very similar to those obtained with the full sample. In particular, the coefficient of interest is positive, significant, and around to 0.25 using the green vehicles data. This result is obtained if no control variables are included —Model (1)—, if we also add the state-specific time trends —Model (2)—, if we include the full set of control variables (except income) —Models (3) and (4)—, and if we also include income as an additional control —Model (5). However, for all these model specifications, β is close to zero and not significant when we use the data on dirty vehicles.[35]

Heterogeneity across platforms. Finally, we check the effect of the entry of Uber and Cabify Lite separately, rather than using the same variable to capture both. To do so, we modify equation (3.1) by including one dummy for each of them. The results of this additional exercise are presented in Table 6.

Table 6. Separate effect of Uber/Cabify Lite entry on takeout of different types of vehicles by taxi drivers

	(1)	(2)	(3)	(4)	(5)
	Panel A: Green vehicles				
Uber	0.227**	0.306***	0.273***	0.333***	0.305***
	(0.105)	(0.099)	(0.100)	(0.101)	(0.108)
Cabify Lite	0.338***	0.306***	0.246**	0.239**	0.222*
	(0.131)	(0.081)	(0.101)	(0.093)	(0.128)
Mean dep. var. non-treated	1.408				
N. metro. areas	41	41	41	41	41
N. Observations	2,576	2,576	2,495	2,495	2,051
	Panel B: Dirty vehicles				
Uber	-0.235**	-0.108	-0.206*	-0.074	-0.076
	(0.095)	(0.097)	(0.109)	(0.104)	(0.106)
Cabify Lite	0.152	0.146*	0.107	0.141	0.167
	(0.130)	(0.078)	(0.126)	(0.094)	(0.105)
Mean dep. var. non-treated	2.984				
λ_m and θ_t	✓	✓	✓	✓	✓
Pol-parties FE	✓	✓	✓	✓	✓
Time-varying control vars.			✓	✓	✓
$Log(income)$					✓
State × Trend FE		✓			✓
N. metro. areas	41	41	41	41	41
N. Observations	2,583	2,583	2,501	2,501	2,057

Note: Column (1): DiD with month-year, metro area and political parties (City and State) fixed effects. Column (2): DiD with previous fixed effects and a state-specific time trend. Column (3): DiD with fixed effects of columns (1) and control variables. Column (4): DiD with fixed effects of columns (2) and control variables. Column (5): DiD with month-year, metro area and political parties (City and State) fixed effects, a state-specific time trend, and all control variables —including income. The significance levels are as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the province level.

First, we display the results obtained using green vehicles data in Panel A. The separately estimated coefficients for both platforms are positive, significant, and similar in magnitude to those obtained in the previous tables. In the specification of the model that does not include control variables (just the fixed effects and the political party dummies), these coefficients suggest that the entry of Uber and Cabify Lite increased the purchase of green cars by about 23% and 34%, respectively —Column (1). However, in

[35] We also provide in Figure A.3 (Appendix A.2) evidence that these estimates are not driven by the choice of the model used, nor by differences in the propensity-score between treated and control metropolitan areas, nor by the number of matches used.

the most complete specification of the model —including the full set of controls and state-specific time trends—, the magnitude of the coefficients for both platforms flip, suggesting an increase in the takeout of green vehicles of about 22% for the Cabify Lite case, and of 31% for the Uber case —Column (5).

Then, Panel B in Table 6 contains the effect of the entry of Uber and Cabify Lite (separately estimated) on the purchase of dirty vehicles by taxi drivers. On the one hand, the Uber dummy is negative in all the model specifications. This effect is found to be slightly significant in Column (1) —no controls other than the fixed effects and the political party dummies— and Column (3) —all controls (except income)—, suggesting a decline in the takeout of dirty vehicles following the entry of this platform. However, this effect is not significant and close to zero once we add income and the state-specific time trends —Columns (2), (4), and (5). On the other hand, the Cabify Lite dummy is positive but not significant in all models specifications —except in Column (2), in which the effect is marginally significant at the 10% level. All in all, our main results remain unchanged by considering separately the entry of Uber and Cabify Lite, as we find evidence of an increase in the purchase of green vehicles by taxi drivers, while the purchases of dirty ones did not change —or, if anything, they slightly decreased for the Uber case.

4.4 Additional results: spillover effects

In this final section, we speculate that there might be also valuable information in the metropolitan areas close to those in which Uber or Cabify Lite entered. As we extensively explained in Section 2.2, the number of vehicles that they are allowed to operate is capped at the regional level. In addition, in the metropolitan areas in which they entered, they deployed the maximum number of vehicles they are allowed to —one in every thirty taxi licenses. Therefore, taxi drivers in the metropolitan areas in which these platforms did not enter but located within the same province in which they did so should expect to remain in a monopolistic position: with the current limitation, is very unlikely for Uber or Cabify Lite to enter them. Does this fact affect the vehicle purchasing behavior of these taxi drivers as well?

To answer this question, we estimate an augmented version of equation (3.1) that also includes a dummy equal to 1 if Uber or Cabify Lite did not enter metropolitan area m in province p but entered other metropolitan area m' in the same province p, and equal to 0 otherwise. The results of this additional empirical exercise are included in Table 7. Once again, estimates using the data on green vehicles are reported in Panel A, while those obtained using the data on the dirty ones are included in Panel B.

Table 7. Spillover effect of Uber/Cabify Lite entry in cities within the same province on takeout of different types of vehicles by taxi drivers

	(1)	(2)	(3)	(4)	(5)
	Panel A: Green vehicles				
Uber/Cabify Lite	0.259**	0.290***	0.253***	0.294***	0.280***
	(0.105)	(0.088)	(0.094)	(0.093)	(0.099)
Uber/Cabify Lite in province	-0.310	-0.245	-0.336	-0.248	-0.147
	(0.339)	(0.336)	(0.376)	(0.385)	(0.351)
Mean dep. var. non-treated	1.408				
N. metro. areas	41	41	41	41	41
N. Observations	2,576	2,576	2,495	2,495	2,051
	Panel B: Dirty vehicles				
Uber/Cabify Lite	-0.062	0.001	-0.062	0.028	0.038
	(0.108)	(0.091)	(0.106)	(0.095)	(0.110)
Uber/Cabify Lite in province	0.202*	0.136	0.170	0.162	0.298*
	(0.113)	(0.127)	(0.125)	(0.145)	(0.174)
Mean dep. var. non-treated	2.984				
λ_m and θ_t	✓	✓	✓	✓	✓
Pol-parties FE	✓	✓	✓	✓	✓
Time-varying control vars.			✓	✓	✓
$Log(income)$					✓
State $\times$ Trend FE		✓			✓
N. metro. areas	41	41	41	41	41
N. Observations	2,583	2,583	2,501	2,501	2,057

Note: Column (1): DiD with month-year, metro area and political parties (City and State) fixed effects. Column (2): DiD with previous fixed effects and a state-specific time trend. Column (3): DiD with fixed effects of columns (1) and control variables. Column (4): DiD with fixed effects of columns (2) and control variables. Column (5): DiD with month-year, metro area and political parties (City and State) fixed effects, a state-specific time trend, and all control variables —including income. The significance levels are as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the province level.

In line with our previous results, we find that the coefficient of the dummy "Uber/Cabify Lite" is positive, significant, and similar in magnitude to those obtained above for the case of green vehicles. However, the coefficient of the new dummy variable "Uber/Cabify Lite in province" is negative (but not significant) in all the different specifications of the models considered. That is, we do not find evidence that the taxi drivers in metropolitan areas without ride-hailing platforms but located in provinces in which Uber or Cabify Lite were launched increased the takeout of green vehicles —to the contrary, if anything, the data suggests that the purchases of these vehicles decreased.

Moreover, for the case of dirty vehicles, we also find some evidence of the opposite effect. That is, while taxi drivers in cities in which Uber or Cabify Lite entered did not increase the purchase of dirty vehicles —the corresponding coefficient is very close to zero and not significant in all the specifications—, those serving in cities within the same province just started to do so. This latter effect is significant at the 10% level both in the model with no controls —Column (1)— and also in the model that includes the full set of controls and the state-specific time trends —Column (5)—, suggesting that taxi drivers increased the average monthly takeout of dirty vehicles by about 20-30%. The effect is also positive but slightly lower in magnitude (and significant only at the 12% level) in the rest of the columns.

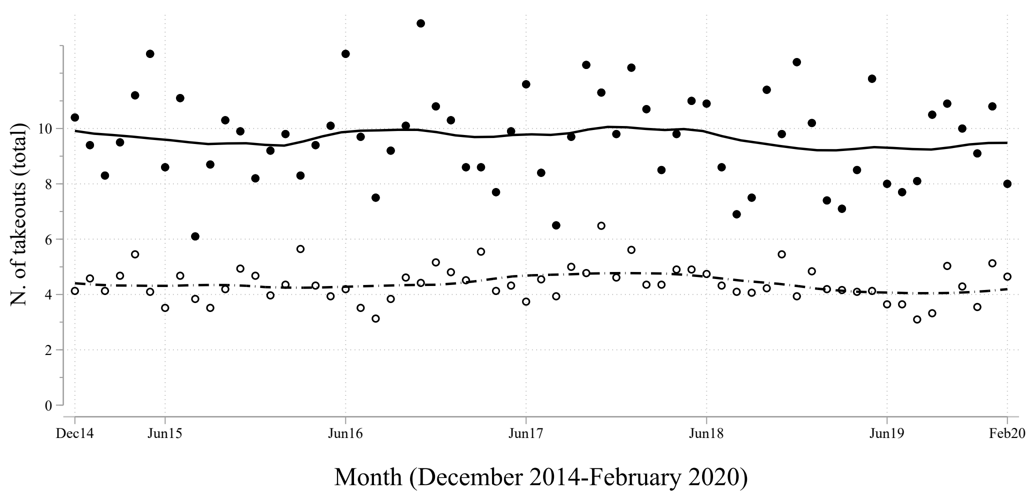
The reader should be aware, though, that the standard errors of the estimated coefficients of this new "Uber/Cabify Lite in province" dummy variable might be large —reducing, thus, their significance level— as a result of the small sample size: there are only five metropolitan areas in our sample that are located within the same province of another ones in which the ride-hailing platforms were launched. Still, these additional results are consistent with the main story of this paper. That is, while taxi drivers that experienced a shock in the intensity of market competition were more likely to purchase green cars, those that expected to remain in a monopolistic position did not increase the takeout of these vehicles —in fact, the data suggests that they more likely to purchase dirty ones.

4.5 Discussion

In this section we use additional data and present new estimates to understand the mechanisms behind our main empirical results. More precisely, we explore whether the increase in the takeout of green vehicles documented above is potentially explained by a temporal adjustment or by other changes in the pattern of purchases of taxi drivers following the entry of the ride-hailing platforms.

To begin with, Figure 3 provides the total number of vehicles (including green and dirty ones) purchased by taxi drivers per month both in cities where Uber or Cabify Lite were launched (solid line) and in which they were not (dashed-dotted line) for the period covered by our sample.[36] The local linear regressions plotted against time show that the evolution of monthly aggregated purchases is flat and almost perfectly parallel for both subsets of metropolitan areas. This fact suggests that our main results are unlikely explained by a temporal adjustment in the purchasing patterns of vehicles, as taxi drivers were not more likely neither to buy new cars more or less often, nor to delay (anticipate) the purchase of new cars after (before) the entry of these platforms.

Figure 6. Total number of vehicles purchased by taxi drivers in treated and control metropolitan areas against time



Note: The lines in this figure represent local linear regressions separately estimated for treated (solid line) and control (dashed-dotted line) metropolitan areas. Mean values by month-year for treated and control metropolitan areas shown as black and white circles, respectively.

Next, to further show that our main results are unlikely driven by a change in the timing of purchases of new cars, we estimate again equation (3.1) but using instead as the outcome of interest the total number of vehicles scrapped by taxi drivers at the month-metropolitan area level.[37] The results of this empirical exercise are included in Table 8 for the same five specifications of the regression model that we considered in the tables above. Consistent with the previous results, we do not observe either a change in the timing of vehicle scrappage in cities where Uber or Cabify Lite entered: the estimated coefficient

[36]Note that in this figure we do not distinguish between green and dirty cars, as we did in Panels A and B in Figure 4.

[37]This data was also obtained from the Spanish DGT for the same period —December 2014 to February 2020— and for the same set of metropolitan areas described above.

of interest is close to zero and not significant in all the specifications of the model.[38]

Table 8. The effect of Uber/Cabify Lite entry on the aggregate number of vehicles scrapped by taxi drivers

	(1)	(2)	(3)	(4)	(5)
Uber/Cabify Lite	-0.011	-0.039	-0.008	-0.057	0.008
	(0.070)	(0.075)	(0.078)	(0.081)	(0.081)
Mean dep. var. non-treated	2.286				
N. metro. areas	41	41	41	41	41
N. Observations	2,583	2,583	2,511	2,511	2,067
λ_m and θ_t	✓	✓	✓	✓	✓
Pol-parties FE	✓	✓	✓	✓	✓
Time-varying control vars.			✓	✓	✓
$Log(income)$					✓
State × Trend FE		✓			✓

Note: Dependent variable: Aggregate number (green + dirty) of vehicles scrapped by taxi drivers. Column (1): DiD with month-year, metro area and political parties (City and State) fixed effects. Column (2): DiD with previous fixed effects and a state-specific time trend. Column (3): DiD with fixed effects of columns (1) and control variables. Column (4): DiD with fixed effects of columns (2) and control variables. Column (5): DiD with month-year, metro area and political parties (City and State) fixed effects, a state-specific time trend, and all control variables —including income. The significance levels are as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the province level.

Still, one could very well be concerned that the entry of Uber or Cabify Lite triggered additional purchases of used vehicles (rather than new ones) among taxi drivers that are not captured by our dataset. This potential switch towards the second-hand vehicles market, if exists, might also explain our main results —e.g., the constant or slight drop in the takeout of (new) dirty vehicles following the entry of the platforms could be due to an increase in the purchases of used ones— or even affect them —e.g., we might be underestimating the takeout of green vehicles if taxi drivers are more likely to purchase used green vehicles in treated cities in the post-treatment period. To address this concern, we estimate again equation (3.1) but using as the outcome of interest the total number of used vehicles purchased by taxi drivers at the month-metropolitan area level.[39] Panel A in Table 9 displays the empirical results using the data on (used) green cars, while Panel B contains those using the data on (used) dirty cars.

In the former case, we find that the coefficient of the Uber/Cabify Lite dummy is positive in Column (1) (no controls case), and negative in all the other columns —being close to zero and not significant in Column (3), but marginally significant at the 10% level in Column (4). Given these contradictory coefficients obtained in different specifications of our regression model, and bearing in mind that the number of non-zero observations is very low —as indicated in the table, the mean of the dependent variable is 0.48—, these results do not provide compelling evidence of a significant change in the purchases of

[38]Similar results are obtained if we separately estimate the regressions for green and dirty vehicles.
[39]Likewise, this data was obtained from the DGT for the same period and for the same metropolitan areas described above.

green vehicles in the second-hand market. The same conclusion applies for the case of used dirty cars: as shown in Table 9 (Panel B), the coefficient of interest is close to zero and not significant in all the columns.

Table 9. The effect of Uber/Cabify Lite entry on the takeout of different types of used (second-hand) vehicles by taxi drivers

	(1)	(2)	(3)	(4)	(5)
	Panel A: Green vehicles				
Uber/Cabify Lite	0.042	-0.262	-0.063	-0.327*	-0.394
	(0.184)	(0.169)	(0.174)	(0.190)	(0.241)
Mean dep. var. non-treated	0.485				
N. metro. areas	40	40	40	40	40
N. Observations	2,520	2,520	2,450	2,450	2,012
	Panel B: Dirty vehicles				
Uber/Cabify Lite	0.051	0.017	0.047	0.023	0.081
	(0.084)	(0.081)	(0.084)	(0.091)	(0.109)
Mean dep. var. non-treated	2.689				
N. metro. areas	41	41	41	41	41
N. Observations	2,583	2,583	2,511	2,511	2,067
λ_m and θ_t	✓	✓	✓	✓	✓
Pol-parties FE	✓	✓	✓	✓	✓
Time-varying control vars.			✓	✓	✓
$Log(income)$					✓
State $\times$ Trend FE		✓			✓

Note: Dependent variable: Number of purchased second-hand vehicles by taxi drivers. Panel A: Green vehicles. Panel B: Dirty ones. Column (1): DiD with month-year, metro area and political parties (City and State) fixed effects. Column (2): DiD with previous fixed effects and a state-specific time trend. Column (3): DiD with fixed effects of columns (1) and control variables. Column (4): DiD with fixed effects of columns (2) and control variables. Column (5): DiD with month-year, metro area and political parties (City and State) fixed effects, a state-specific time trend, and all control variables —including income. The significance levels are as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the province level.

5 Conclusions

Does market competition promote the adoption and diffusion of greener technologies? This question has become an extremely relevant one from a policy perspective. As countries have been (and still are) implementing substantial regulation to fight environmental externalities that discourage the use of older, dirty technologies and/or promote the use of innovative, greener ones, this regulation might be ineffective if monopolistic suppliers are unlikely to switch from the former to the latter. In fact, within the European Union, countries are currently profoundly discussing whether competition policy and antitrust should be modified (relaxed) to achieve the policy initiatives and environmental targets included in the so-called *European Green deal*. However, providing a clear answer to the question of interplay between the intensity of competition and the diffusion of green technologies has been usually challenging, due to the lack of settings that allow researchers to claim causality from one to the other.

In this paper, we aim at providing a straightforward answer to this question using an empirical setting that provides some of the key ingredients that allow us to isolate the effect of an (exogenous) shock in the intensity of competition on the adoption of clean technologies by dominant, incumbent suppliers. In particular, we study the takeout of green vehicles by professional taxi drivers before and after the entry of ride-hailing, app-based platforms —namely, Uber and its locally-founded rival Cabify Lite. This setting constitutes an ideal setting with which to explore this question, considering that *(i)* the rollout of these platforms is unrelated to the vehicle choices by professional drivers in conventional taxi companies, *(ii)* their entry is unlikely to trigger relevant changes in the key characteristics of the incumbent taxi companies (prices, number of licenses, productivity, etc.), and *(iii)* taxi drivers' incentives to purchase green vehicles are unlikely driven to attract customers with particular environmental preferences.

Using a unique panel data on all the vehicles purchased by taxi drivers between December 2014 and February 2020 (in combination with some other supply and demand-related variables) we document that the staggered rollout of Uber and Cabify Lite across major metropolitan areas in Spain substantially increased the takeout of green vehicles. More precisely, our results suggest that these platforms causally induced one extra green vehicle bought by taxi drivers in every four, while no change (or, if anything, a slightly decrease) is observed in the purchases of dirty cars. Moreover, our estimates provide some evidence of the opposite effect in the behavior of taxi drivers in metropolitan areas in which Uber and Cabify Lite are unlikely to enter: these taxi drivers did not increase the takeout of green vehicles, while they maintained (or slightly increased) that of dirty ones. Finally, we present additional evidence to rule out that these results are explained either by an adjustment in the timing pattern of purchases or by an increase/decrease in the takeout of used vehicles following the entry of the ride-hailing platforms.

In Spain, the cost of driving a green car are substantially lower than those associated to a dirty one (particularly for high-kilometer drivers) due to a stringent taxation of petroleum products. Therefore, what can explain our empirical findings? Previous literature indicates that they are likely consistent with a situation of *X-inefficiency* in the taxi business for three reasons. First, as Leibenstein (1966) explains, the X-inefficiency arises in the absence of a competitive pressure that energizes firms to produce at lower costs (at the risk of losing sales to more efficient rivals by not doing so). Second, Borenstein and Farrell (2000) and Perelman (2011) argue that another symptom of X-inefficiency is that suppliers are able to make adjustments quickly in response to new competition. Finally, Leibenstein (1966) discusses that the lack of competition makes it possible to use inefficient production techniques and still stay in business. These arguments are consistent with the fact that in the cities where Uber or Cabify Lite entered taxi drivers purchase more green vehicles, while in those cities without ride-hailing platforms they did not increase the takeout of these vehicles (maintaining or even increasing that of dirty ones).

Some other authors —most notably, Stigler (1976)— questioned the existence of X-inefficiency, arguing that production at a higher than technologically feasible cost should not per-se be associated with inefficiency. This argument might no longer hold if such production also entails (likely previously ignored) environmental costs not internalized by the firms but borne by the society. Thus, although market regulation and fiscal policies are usually acknowledged as necessary tools that allow firms to internalize the additional costs generated and, consequently, to effectively tackle negative environmental externalities, and to induce firms to adopt cleaner production processes, our results suggest that they might not be sufficient ones —particularly, if firms enjoy substantial market power. In this case, a more stringent and rigorous competition policy could actually be the most effective way to promote sustainability.

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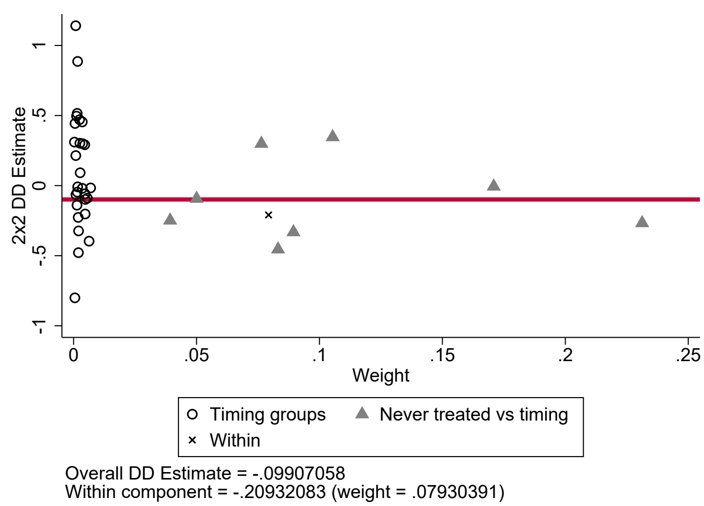
Appendix A (For Online Publication). Additional Robustness Checks

A.1 Stacked-*DiD*

In this appendix we provide additional figures and results regarding the Stacked-*DiD* estimator discussed in Section 3.2, and whose empirical results are presented in Section 4.3.

First, as we extensively explain in Section 3.2, recent literature has raised concerns on the validity of the two-way fixed effects *DiD* estimator in settings in which the treatment is staggered over time — Athey and Imbens (2018), Callaway and Sant'Anna (2020), De Chaisemartin and d'Haultfoeuille (2020), Sun and Abraham (2020), Baker et al. (2021). This is because, in these settings, already-treated units also act as effective controls. Consequently, the two-way fixed effects estimator turns to be a weighted average of all possible two-by-two *DiD* estimators in the data —Goodman-Bacon (2021). Nevertheless, we argue that this issue is not that relevant in our setting for two reasons. First, because the size of the control group —i.e., metropolitan areas without Uber or Cabify Lite— is substantially larger than the size of the treated group —those in which these platforms entered. Second, because the entry of Uber and Cabify Lite in different metropolitan areas occurred close in time from each other.

Figure A.1. Two-by-two *DiD* decomposition of the staggered treatment (i.e., Uber or Cabify Lite entry) over time in different metropolitan areas in our sample

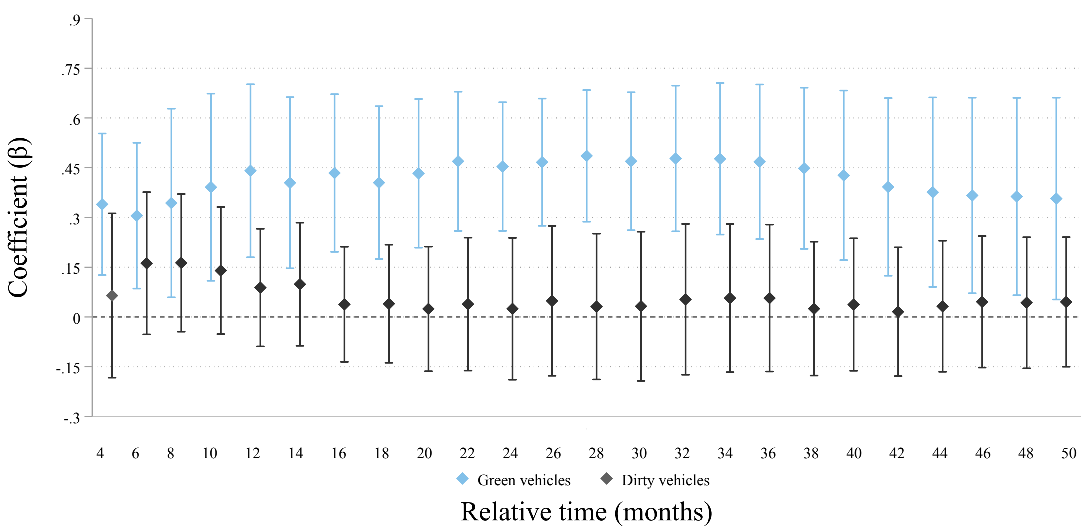


Note: The figure includes each two-by-two *DiD* components of the staggered treatment (i.e., Uber or Cabify Lite entry) from the decomposition theorem by Goodman-Bacon (2021) against their weight for the analysis of monthly purchases of green/dirty vehicles by taxi drivers using our sample. The circles represent terms in which one timing group acts as the treatment group and the Uber/Cabify Lite metropolitan areas act as the control group. The triangles capture terms in which one timing group acts as the treatment group and the metropolitan areas in which Uber or Cabify Lite never entered act as the control group. The crosses represent the timing-only terms. We obtain this figure following the implementation by Goodman-Bacon et al. (2019).

To further show that these concerns on the validity of the two-way fixed effects estimator are not that relevant in our empirical setting, we provide in Figure A.1 the so-called Goodman-Bacon decomposition. This figure displays the weights placed in each of the two-by-two components of the *DiD* estimator for the staggered treatment (i.e., Uber or Cabify Lite entry) in our sample obtained from the decomposition theorem by Goodman-Bacon (2021). As argued above, the greatest weights are placed on the *DiD* components in which one timing group acts as the treatment group and the metropolitan areas in which Uber or Cabify Lite never entered act as the control group (represented by triangles). Moreover, little weight is placed on those components in which one timing group acts as the treatment group and the metropolitan areas with Uber/Cabify Lite act as the control group (represented by circles). That is, a greater weight is placed on the comparison between treated and (clean) control units than to the comparison between treated and already-treated units when we use the two-way fixed effects estimator.

Still, as a sanity check, we estimate in Section 4.3 our main regression model using the so-called stacked-*DiD* approach —see Cengiz et al. (2019). To do so, we create event-specific datasets using the observations of the treated and the (clean) control metropolitan areas in which Uber or Cabify Lite did not enter within a 8-month window —i.e., from $t = -8$ to $t = 8$, relative to the entry period $t = 0$. We then stack these (relative time) event-specific datasets to calculate an average effect across all of them using a single set of treatment indicators. The results of this empirical exercise are included in Table 5.

Figure A.2. Estimated impact of Uber/Cabify Lite entry on takeout of different types of vehicles by taxi drivers using different time windows in the stacked-*DiD* approach



Note: The figure displays the estimated coefficient of interest (β) in equation (3.1) by using a stacked-*DiD* approach. The light blue color indicates the estimates obtained for green cars, while those obtained using the data on dirty cars are indicated in dark gray. The horizontal axis represents the different lengths of the time window (i.e., the number of months) around the treatment considered to create the "event-specific" dataset using the observations of the treated and the ("clean") control metropolitan areas, relative to the entry period $t = 0$. Month-year, metropolitan area (both interacted with stack-specific indicators), and (City and State) political party fixed effects, as well as state-specific time trend and the full set of control variables are included in all regressions. Standard errors are clustered at the province level. Vertical bands represent $\pm$ 1.96 times the standard error of each point estimate.

However, as explained by Baker et al. (2021), the choice of the length of the time window to create the event-specific datasets might also influence the outcome of the stacked-*DiD* estimator. To address this concern, we provide in Figure A.2 the estimated coefficient of the "Uber/Cabify Lite" dummy using both the data on green cars (light blue) and dirty cars (dark gray) for alternative lengths of the time window (i.e., for alternative number of months) around the treatment to create the event-specific datasets. These estimates are obtained for the most complete specification of the regression model —column (5) in Table 5—, including month-year and metropolitan area fixed effects (both interacted with stack-specific indicators), the political party dummies, the state-specific time trend, and the full set of control variables. As shown in this figure, our results do not change when we choose alternative lengths: all estimates are significant and similar in magnitude for the case of green vehicles, while none is significant for the case of dirty cars.

A.2 Matching-*DiD*

We acknowledge in Section 4.3 that some of the observable characteristics used in the empirical analysis are not well-balanced across metropolitan areas with and without Uber/Cabify Lite. This is why we use matching techniques before the *DiD* estimation (also explained in that Section) in order to solve this unbalanced sample problem.

In this appendix we check the quality of the matching by comparing the standardized differences of the variables used in the matching procedure between treated and control metropolitan areas before implementing the matching —Panel A1 in Table A.1—, and the standardized differences of the same variables between treated and control units after the matching —Panel 2 in the same Table. Specifically, we show in both panels the mean value of these variables for the cities with Uber/Cabify Lite (treated group), the mean value of them for the cities without one of these ride-hailing platforms (control group), the standardized differences between them, and the p-value of the test under the null hypothesis that this difference is equal to zero.

Table A.1. Balance of baseline characteristics of metropolitan areas before-after matching

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A1: Before matching				*Panel A2: After matching*			
	Mean		Stan.	P-value	Mean		Stan.	P-value
	Treated	Control	Diff.		Treated	Control	Diff.	
(log)-Population	13.329	12.392	1.780	0.000	13.329	12.885	0.838	0.061
(log)-Travelers	12.067	11.463	0.916	0.019	12.067	12.017	0.092	0.837
(log)-Unemployment	11.920	11.183	1.355	0.000	11.920	11.668	0.539	0.228
Fuel duties (diesel)	4.496	4.319	0.449	0.299	4.496	4.295	0.418	0.350

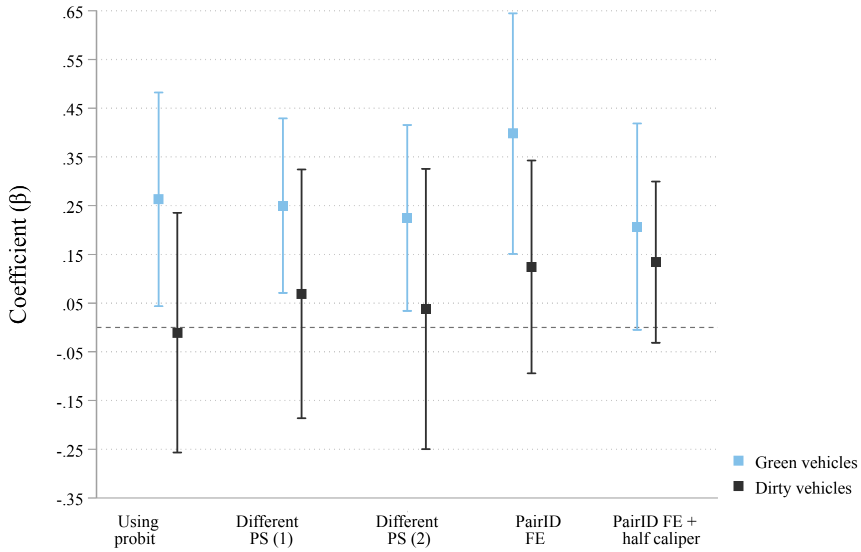
Notice that a perfectly balanced variable has a standardized difference equal to zero. Hence, we observe in Panel A1 that all variables but the diesel fuel duties are not balanced in the original sample. However, Panel A2 shows that all imbalances are fixed after implementing the matching, in which we only keep pairs of metropolitan areas with similar propensity scores.

Finally, we also check that our matching-*DiD* estimates obtained in Section 4.3 are not driven by the choice of the model used to estimate the propensity score, nor by differences in the propensity score between treated and control units, nor by the number of matches used. To do so, we re-estimate our main regression model by *(i)* using a probit regression instead of a logit one to estimate the propensity scores; *(ii)* using different combinations of the variables included in the matching procedure; *(iii)* estimating the specification using a metropolitan area-pair fixed effect; and *(iv)* reducing the magnitude of the difference in the propensity score —i.e., the caliper.

The results of these final robustness checks are displayed in Figure A.3. As in Figure 5, we present the coefficient of interest (β) in equation (3.1) for green vehicles (light blue squares) and for dirty ones (black squares), while vertical bands represent 95% confidence intervals. But conversely to the original figure, each model shows a separate regression.[40] The first specification shows the coefficients using a

[40] Month-year, and political parties (City and State) fixed effects, as well as state-specific time trend and all controls variables are included in all regressions.

Figure A.3. Robustness check of the estimated impact of Uber/Cabify entry on takeout of different types of vehicles by taxi drivers using a matching technique

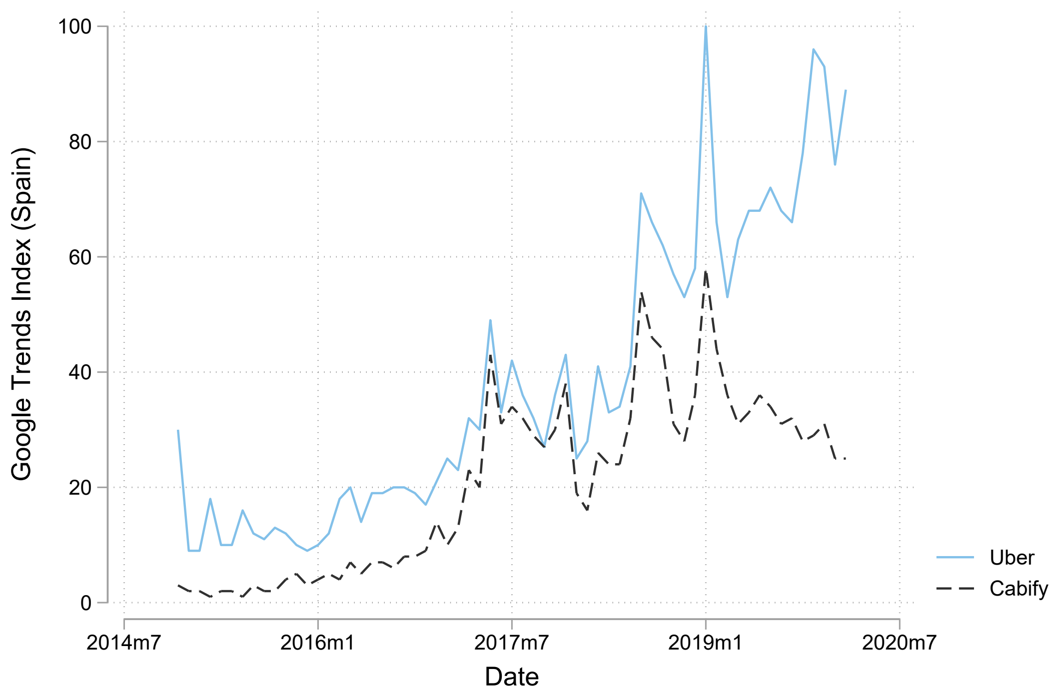


Note: The figure displays several robustness checks of the matching-*DiD* performed in Figure 5. Each model is from a separate regression, where the light blue(dark gray) color indicates estimates for green(dirty) cars. Month-year, metro area and political parties (City and State) fixed effects, as well as state-specific time trend and all controls variables are included in all regressions. Standard errors are clustered at the matched-pair level. Vertical bands represent $\pm$ 1.96 times the standard error of each point estimate.

probit model to estimate the propensity score. The second and the third ones display the coefficients after estimating the propensity score using population, tourism and unemployment in the first case and population and tourism in the second one. The fourth specification displays the coefficients after estimating the specification using a metropolitan area-pair fixed effect —instead of a metropolitan area fixed effect—, while the last one further reduces the magnitude of the difference in the propensity score to half the original one —i.e. reducing the caliper, which further limits the number of matches but ensuring that their quality is higher. In all cases, results remain practically unchanged. Hence, we can conclude that our main results prove robust to the alternative strategy of combining the original *DiD* and matching techniques, where the *DiD* takes care of selection on time-invariant unobservables while matching deals with the selection into treatment based on observables.

Appendix B (For Online Publication). Additional Figures and Tables

Figure B.1. Google Trend Index using "Uber" and "Cabify" as search terms (Spain, December 2014-February 2020)



Note: This figure presents the monthly Google Trend Indices for the words "Uber" (light solid line) and "Cabify" (dark gray dashed line) in Spain between December 2014 and February 2020.

Table B.1. List of the metropolitan areas considered in this study and the municipalities included in each of these metropolitan areas

Metropolitan Area	Municipalities
Albacete	Albacete, Barrax, Balazote, Chinchilla de Monte-Aragón, La Gineta, Fuensanta, La Herrera, Madrigueras, Mahora, Montalvos, Motilleja, Pozo Cañada, La Roda, Tarazona de la Mancha, Valdeganga.
Algeciras	Algeciras, Barrios, Los, Castellar de la Frontera, Jimena de la Frontera, Línea de la Concepción, La, San Roque, Tarifa.
Alicante	Agost, Aigües, Albatera, Alicante, Busot, El Campello, Catral, Crevillent, Dolores, Elche, Mutxamel, Sant Joan d'Alacant, San Vicente del Raspeig/Sant Vicent del Raspeig, San Fulgencio, San Isidro, Santa Pola.
Almería	Almería, Benahadux, Gádor, Huércal de Almería, Níjar, Pechina, Rioja, Santa Fe de Mondújar, Viator.
Asturias	Avilés, Carreño, Castrillón, Corvera de Asturias, Gijón, Gozón, Illas, Langreo, Llanera, Mieres, Morcín, Noreña, Oviedo, Las Regueras, Ribera de Arriba, Riosa, San Martín del Rey Aurelio, Siero.
Badajoz	Alburquerque, La Albuera, Badajoz, La Codosera, Guadiana del Caudillo, Pueblonuevo del Guadiana, San Vicente de Alcántara, Talavera la Real, Valdelacalzada, Villar del Rey.
Bilbao	Abanto y Ciérvana-Abanto Zierbena, Alonsotegi, Arrankudiaga, Arrigorriaga, Barakaldo, Barrika, Basauri, Berango, Bilbao, Derio, Erandio, Etxebarri, Galdakao, Getxo, Gorliz, Larrabetzu, Leioa, Lemoiz, Lezama, Loiu, Muskiz, Ortuella, Plentzia, Portugalete, Santurtzi, Sestao, Sondika, Sopela, Ugao-Miraballes, Urduliz, Valle de Trápaga-Trapagaran, Zamudio, Zaratamo, Zeberio, Zierbena.
Burgos	Albillos, Alfoz de Quintanadueñas, Arcos, Buniel, Burgos, Carcedo de Burgos, Cardeñadijo, Cardeñajimeno, Castrillo del Val, Cavia, Cayuela, Cogollos, Hurones, Ibeas de Juarros, Merindad de Río Ubierna, Modúbar de la Emparedada, Orbaneja Riopico, Quintanaortuño, Quintanilla Vivar, Las Quintanillas, Revillarruz, Rubena, Saldaña de Burgos, San Mamés de Burgos, Sarracín, Sotragero, Tardajos, Valle de las Navas, Villagonzalo Pedernales, Villalbilla de Burgos, Villariezo, Villayerno Morquillas.
Cáceres	Aldea del Cano, Arroyo de la Luz, Cáceres, Casar de Cáceres, Malpartida de Cáceres, Sierra de Fuentes, Torreorgaz, Torremocha, Torrequemada.
Cádiz	Cádiz, Chiclana de la Frontera, Jerez de la Frontera, El Puerto de Santa María, Puerto Real, San Fernando.
Cartagena	Los Alcázares, Cartagena, Fuente Álamo de Murcia, Mazarrón, San Javier, San Pedro del Pinatar, Torre-Pacheco, La Unión.
Coruña	Abegondo, Arteixo, Betanzos, Bergondo, Cambre, Carral, A Coruña, Culleredo, Oleiros, Sada.
Córdoba	Almodóvar del Río, Guadalcázar, La Carlota, Córdoba, Obejo, Villafranca de Córdoba, Villaharta, Villaviciosa de Córdoba.
Castellón	Castelló de la Plana, Benicasim/Benicàssim, Borriana/Burriana, Almassora, Vila-real, Vilanova d'Alcolea, Borriol, Benlloc, Alqueries, les/Alquerías del Niño Perdido, Betxí.
Ferrol	Ares, Cabanas, A Capela, Cariño, Cedeira, Cerdido, Fene, Ferrol, Mañón, Moeche, Monfero, Mugardos, Narón, Neda, Ortigueira, Pontedeume, As Pontes de García Rodríguez, San Sadurniño, As Somozas, Valdoviño.
Girona	Aiguaviva, Fornells de la Selva, Girona, Quart, Salt, Sant Gregori, Sarrià de Ter, Vilablareix.
Gran Canaria	Agüimes, Arucas, Firgas, Gáldar, Ingenio, Moya, Las Palmas de Gran Canaria, Santa Brígida, Telde, Teror, Valleseco, Valsequillo de Gran Canaria.
Granada	Albolote, Alfacar, Alhendín, Armilla, Atarfe, Cájar, Cenes de la Vega, Chauchina, Churriana de la Vega, Cijuela, Colomera, Cúllar Vega, Dílar, Fuente Vaqueros, Las Gabias, Gójar, Granada, Güevéjar, Huétor Vega, Jun, Láchar, Maracena, Monachil, Ogíjares, Peligros, Pinos Genil, Pinos Puente, Pulianas, Santa Fe, Valderrubio, Vegas del Genil, Villa de Otura, Víznar, La Zubia.
Huelva	Aljaraque, Gibraleón, Huelva, Moguer, Palos de la Frontera, Punta Umbría, San Juan del Puerto, Trigueros.
Jaen	Fuensanta de Martos, Fuerte del Rey, La Guardia de Jaén, Jaén, Jamilena, Mancha Real, Martos, Mengíbar, Pegalajar, Torredelcampo, Torredonjimeno, Valdepeñas de Jaén, Villardompardo, Los Villares, Villatorres.
León	Ardón, Benavides, Boñar, Carrizo, Chozas de Abajo, Cimanes del Tejar, Cuadros, Garrafe de Torío, Gradefes, Hospital de Órbigo, León, Mansilla de las Mulas, Matallana de Torío, Onzonilla, La Pola de Gordón, La Robla, San Andrés del Rabanedo, Sariegos, Santas Martas, Santovenia de la Valdoncina, Soto y Amío, Turcia, Valdefresno, Valdepolo, Valdevimbre, Valverde de la Virgen, Vega de Infanzones, Vegas del Condado, Villadangos del Páramo, Villaquilambre, Villasabariego, Villaturiel.
Lleida	Aitona, Els Alamús, Albatàrrec, Alcanó, Alcarràs, Alcoletge, Alfarràs, Alfés, Alguaire, Almacelles, Almatret, Almenar, Alpicat, Artesa de Lleida, Aspa, Benavent de Segrià, Corbins, Gimenells i el Pla de la Font, La Granja d'Escarp, Llardecans, Lleida, Maials, Massalcoreig, Montoliu de Lleida, La Portella, Puigverd de Lleida, Rosselló, Sarroca de Lleida, Seròs, Soses, Sudanell, Sunyer, Torrebesses, Torrefarrera, Torres de Segre, Torre-serona, Vilanova de la Barca, Vilanova de Segrià.
Logroño	Agoncillo, Albelda de Iregua, Alberite, Arrúbal, Cenicero, Entrena, Fuenmayor, Lardero, Logroño, Murillo de Río Leza, Navarrete, Nalda, Ribafrecha, Villamediana de Iregua.
Málaga	Alhaurín de la Torre, Almogía, Benalmádena, Málaga, Rincón de la Victoria, Torremolinos, Totalán.
Murcia	Alcantarilla, Alguazas, Beniel, Archena, Ceutí, Librilla, Lorquí, Molina de Segura, Murcia, Las Torres de Cotillas, Santomera.
Ourense	Amoeiro, Barbadás, Coles, Esgos, Nogueira de Ramuín, Ourense, O Pereiro de Aguiar, A Peroxa, San Cibrao das Viñas, Taboadela, Toén, Vilamarín.
Palma de Mallorca	Andratx, Bunyola, Calvià, Esporles, Llucmajor, Marratxí, Palma, Puigpunyent, Santa María del Camí.
Pamplona	Adiós, Ansoáin/Antsoain, Anue, Añorbe, Atez/Atetz, Aranguren, Barañáin/Barañain, Basaburua, Belascoáin, Beriáin, Berrioplano/Berriobeiti, Berriozar, Bidaurreta, Biurrun-Olcoz, Burlada/Burlata, Cendea de Olza/Oltza Zendea, Ciriza/Ziritza, Cizur, Echarri/Etxarri, Enériz/Eneritz, Esteribar, Etxauri, Ezcabarte, Galar, Goñi, Guirguillano, Huarte/Uharte, Ibargoiti, Imotz, Iza/Itza, Juslapeña, Lantz, Legarda, Monreal/Elo, Muruzábal, Noáin (Valle de Elorz)/Noain (Elortzibar), Odieta, Olaibar, Pamplona/Iruña, Tiebas-Muruarte de Reta, Tirapu, Ucar, Ultzama, Uterga, Valle de Egüés/Eguesibar, Valle de Ollo/Ollaran, Villava/Atarrabia, Zabalza/Zabaltza, Zizur Mayor/Zizur Nagusia.
Pontevedra	Baiona, Cangas, Gondomar, Marín, Moaña, Mos, Nigrán, Pontevedra, O Porriño, Poio, Redondela, Soutomaior, Vilaboa, Vigo.

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Metropolitan Area	Municipalities
Sagunto	Albalat dels Tarongers, Algímia d'Alfara, Benavites, Benifairó de les Valls, Canet d'En Berenguer, Estivella, Faura, Gilet, Petrés, Puçol, El Puig de Santa Maria, Quart de les Valls, Quartell, Sagunto/Sagunt, Segart, Torres Torres.
Salamanca	Aldealengua, Aldearrubia, Aldeatejada, Arapiles, Babilafuente, Barbadillo, Buenavista, Cabrerizos, Calvarrasa de Abajo, Calvarrasa de Arriba, Calzada de Valdunciel, Carbajosa de la Sagrada, Carrascal de Barregas, Castellanos de Moriscos, Castellanos de Villiquera, Doñinos de Salamanca, Florida de Liébana, Galindo y Perahuy, Gomecello, Machacón, Martinamor, Miranda de Azán, Monterrubio de Armuña, Moriscos, Mozárbez, Parada de Arriba, Pelabravo, El Pino de Tormes, Salamanca, San Cristóbal de la Cuesta, Santa Marta de Tormes, San Morales, San Pedro del Valle, Terradillos, Valdemierque, Valverdón, La Vellés, Villagonzalo de Tormes, Villamayor, Villares de la Reina, Villaverde de Guareña, Villoria, Villoruela.
San Sebastián	Andoain, Astigarraga, Donostia/San Sebastián, Errenteria, Hernani, Hondarribia, Irun, Lasarte-Oria, Lezo, Oiartzun, Orio, Pasaia, Urnieta, Usurbil, Zarautz.
Santander	Alfoz de Lloredo, El Astillero, Cabezón de la Sal, Camargo, Cartes, Castañeda, Los Corrales de Buelna, Marina de Cudeyo, Medio Cudeyo, Miengo, Piélagos, Polanco, Puente Viesgo, Reocín, Ribamontán al Mar, Santa Cruz de Bezana, Santander, Santillana del Mar, Suances, Villaescusa.
Santiago de Compostela	Ames, Boqueixón, Brión, Oroso, O Pino, Santiago de Compostela, Teo, Trazo, Val do Dubra, Vedra.
Sevilla	Alcalá de Guadaíra, Almensilla, Bormujos, Camas, Castilleja de Guzmán, Castilleja de la Cuesta, Coria del Río, Dos Hermanas, Espartinas, Gelves, Gines, Isla Mayor, Mairena del Aljarafe, Palomares del Río, La Puebla del Río, La Rinconada, Salteras, San Juan de Aznalfarache, Santiponce, Sevilla, Tomares, Valencina de la Concepción.
Tarragona	L'Aleixar, Alcover, Alforja, Almoster, Altafulla, Les Borges del Camp, Botarell, Cambrils, La Canonja, Castellvell del Camp, El Catllar Constantí, Creixell, Duesaigües, Els Garidells, Maspujols, La Masó, Mont-roig del Camp, Montbrió del Camp, El Morell, La Nou de Gaià, Els Pallaresos, Perafort, La Pobla de Mafumet, Reus, La Riera de Gaià, Riudecanyes, Riudecols, Riudoms, Roda de Berà, El Rourell, Salou, La Secuita, La Selva del Camp, Tarragona, Torredembarra, El Vendrell Vila-seca, Vinyols i els Arcs.
Tenerife	El Rosario, Santa Cruz de Tenerife, San Cristóbal de La Laguna, El Sauzal, Tacoronte, Tegueste.
Toledo	Argés, Bargas, Burguillos de Toledo, Cobisa, Layos, Magán, Mocejón, Nambroca, Olías del Rey, Toledo, Villamiel de Toledo.
Valladolid	Arroyo de la Encomienda, Cistérniga, Dueñas (Palencia), Laguna de Duero, Medina del Campo, Palencia, Tordesillas, Valladolid.
Vitoria	Alegría-Dulantzi, Arminõn, Arraia-Maeztu, Arratzua-Ubarrundia, Asparrena, Barrundia, Berantevilla, Bernedo, Campezo/Kanpezu, Zigoitia, Kuartango, Elburgo/Burgelu, Harana/Valle de Arana, Iruña Oka/Iruña de Oca, Iruraiz-Gauna, Lagrán, Lantarón, Peñacerrada-Urizaharra, Erriberabeitia, Erriberagoitia/Ribera Alta, Añana, Agurain/Salvatierra, San Millán/Donemiliaga, Urkabustaiz, Valdegovía/Gaubea, Vitoria-Gasteiz, Zalduondo, Zambrana, Zuia.
Zaragoza	Alfajarín, Alagón, Bárboles, Botorrita, El Burgo de Ebro, Cabañas de Ebro, Cadrete, Cuarte de Huerva, Farlete, Fuentes de Ebro, Figueruelas, Grisén, La Joyosa, Leciñena, María de Huerva, Mediana de Aragón, Mozota, Muel, La Muela, Nuez de Ebro, Osera de Ebro, Pastriz, La Puebla de Alfindén, Pinseque, Pedrola, Perdiguera, San Mateo de Gállego, Sobradiel, Torres de Berrellén, Utebo, Villamayor de Gállego, Villanueva de Gállego, Zaragoza Zuera.