Data sharing and tax enforcement: Evidence from short-term rentals in Denmark

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Abstract

Airbnb and other home-sharing platforms have been facing increasing regulation over the past years, mainly in the form of restricting short-term rentals through day caps. In contrast, as one of the first countries in the world, Denmark applied a collaborative strategy: In 2018, the government negotiated an agreement with Airbnb about the transmission of income data from the platform to the tax agency. We analyze how this data-sharing agreement affected hosts' behavior on the platform, using a difference-in-differences approach with Sweden as a counterfactual. We find that the agreement reduced hosts' propensity to list property on the platform by 14%, while increasing listing prices by 11%. Our results indicate that platform exits were mostly limited to single-property hosts. In contrast, hosts with many properties and those in areas with initially low Airbnb penetration made their rental objects more often available and managed to increase the number of bookings. Overall, the findings imply that the data-sharing agreement not only helped to increase tax compliance but also led to a commercialization and spatial re-organization of short-term renting in Denmark.

Keywords: Airbnb; DAC7; digital platforms; home sharing; income tax; tax enforcement

JEL classification: H26; L86; M38; R12; R31

1. Introduction

Over the past decade, home-sharing platforms have become increasingly popular for short-term renting of accommodation, which has helped to use scarce housing space efficiently. As the world's leading home-sharing platform, Airbnb has grown vividly, counting 4 million hosts in 100,000 cities by 2022 – with Copenhagen among the top ten European cities in terms of listings (Airbnb, 2022a). The success of these platforms has been met with criticism from various sides though. For example, the hotel industry fears unfair competition and an erosion of safety and quality standards, while residents feel negatively affected by streams of noisy visitors. In large cities, there have been concerns that the excessive commercial use of home sharing contributes to increasing housing prices and the displacement of residents from their neighborhoods. Critics also argue that taxes related to activities on home-sharing platforms are difficult to enforce because the authorities in charge cannot trace online transactions but must trust that hosts themselves report income generated on these platforms.

Local governments have therefore introduced restrictions to short-term rentals, especially to limit excessive commercial activities. In most cases, these restrictions involve caps on the number of days an object can be rented out within a given period (e.g., in Amsterdam, Berlin, London, and Paris), as well as bans of short-term rentals within certain areas of a city (e.g., in Barcelona, Los Angeles, and New York); see Gauss et al. (2022). More recently, various countries have started to collaborate with Airbnb in terms of reporting of transactions made on the platform. For example, between 2018 and 2019, governments in Denmark, Estonia, and Norway negotiated data-sharing agreements with Airbnb to better enforce hosts' tax obligations. Similarly, EU Council Directive 2021/514 (commonly known as DAC7), which will take effect in 2023/24, makes it mandatory for platforms to share information about sales of goods and services with relevant member states. Accordingly, Airbnb and other home-sharing platforms that conduct business in the EU will be required to report earnings of hosts to local tax agencies. To this date, evidence on how data sharing affects transactions on short-term rental platforms is scarce, and little is known about potential side effects.

For this reason, we analyze the effects of the data-sharing agreement reached between Denmark and Airbnb in 2018 on hosts' behavior on the platform. Our focus on Denmark is motivated by the scope of this agreement. The information provided by Airbnb made it possible for the Danish Tax Agency to retrieve social security numbers for most taxable hosts. In contrast to arrangements made in other countries[1], the data sharing led to a steep increase in the probability of detecting tax evasion – much like the scenario that can be expected throughout the EU once Council Directive 2021/514 comes into force.

Estimating the effects of the data sharing on hosting is difficult because of omitted variables and simultaneity issues. To address this challenge, we employ a difference-in-differences strategy, using Airbnb hosts in Sweden as a control group. Sweden shares many similarities with Denmark regarding tourism, business cultures, and housing markets. However, Sweden did not change its regulation regarding short-term rentals or entered collaboration scenarios with Airbnb, which makes the country an ideal benchmark to estimate treatment effects in Denmark.

According to our baseline estimates, we find that the data-sharing agreement reduced the propensity of hosts to list property for rent by 14%, while increasing listing prices by 11%. As discussed throughout the paper, these effects were mostly driven by a strong increase in the probability of detecting tax evasion. Hence, the costs of renting out property increased for those hosts that did not comply with the tax system before the data sharing (i.e., in the form of income and value added taxes). The cost increase made it unprofitable for a fraction of hosts to rent out their property, causing them to leave the platform as reflected by the overall decrease in listing propensity. In contrast, hosts remaining on the platform increased their prices, likely in response to the decrease in competition caused by platform exits, and to pass (part of) the cost related to tax liabilities to the customer.

The reduction in the listing propensity was mostly driven by single-property hosts. This group also experienced a drop in realized bookings. In contrast, hosts with many properties expanded the daily availability of their objects and managed to slightly increase their number of realized bookings. Since multi-property hosts are often those that use Airbnb primarily for business reasons, these results point toward a commercialization of short-term rentals through the platform due to the data-sharing agreement.

[1] For instance, upon its start in 2019, the data-sharing agreement between Airbnb and Norway did not involve any systematic reporting of income data. The tax authority was merely allowed to request information in individual cases (Airbnb, 2022b), implying a minimal change in the detection probability (Garz and Schneider, 2023). In Estonia, the agreement mostly led to a reduction in hosts' administrative costs of filing their taxes, without implications for the detection probability (Airbnb, 2022c).

As another side effect, we find evidence of a spatial re-organization in rental activity. Hosts in areas with initially low levels of Airbnb penetration made their properties more often available and realized more bookings, whereas we find the opposite for hosts in areas with high penetration of Airbnb before the implementation of the data-sharing agreement. Airbnb penetration – measured as the overall number of listed properties in an area relative to the population size – is highly correlated with population density. Our findings therefore imply that the data sharing led to a shift in short-term renting from urban to rural areas.

We contribute to existing research in three ways. First, we extend the literature that evaluates the effects of regulations on short-term rentals.[2] For instance, Valentin (2021) finds evidence that restrictions of short-term rentals in New Orleans reduced listings on Airbnb and decreased housing prices by approximately 30% in the relevant neighborhood. Exploiting a quasi-experimental design in Los Angeles County, Koster et al. (2021) find that Airbnb listings dropped by 50% due to home-sharing ordinances, while housing prices increased by 2%. Gauss et al. (2022) show that those ordinances affect occasional and commercial hosts in different ways. Chen et al. (2022) evaluate a reform that allows hosts in New York and San Francisco to rent out no more than one property via Airbnb, and provide evidence of a reduction of long-term rents and home values. While these studies investigate regulations aiming to restrict the quantity of short-term rental supply, we evaluate an entirely different policy category: the sharing of income data with the tax authority. Since the data sharing targets taxpayers, we differ from the above research by conducting our investigation at the level of individual hosts instead of properties. In addition, the above literature investigates the effects of city-level interventions, whereas our study pertains to an entire country. To the best of our knowledge, we are therefore the first to investigate the causal impact of short-term rental interventions outside of big cities, which allows us to characterize differences in treatment effects between rural and urban areas.

Second, the data sharing investigated in our study is closely related to voluntary tax-collection agreements between Airbnb and US metropolitan areas. Between 2014 and 2017, Airbnb entered multiple agreements with selected tax jurisdictions which made the platform a tax remitter of a 10% sales and hotel tax. Bibler et al. (2021) show that 2.4% of the tax burden is carried by Airbnb

[2] Other studies investigate the implications of home-sharing more generally, for example, regarding housing prices (e.g., Horn and Merante, 2017; Garcia-López et al. 2020; Barron et al., 2021; Franco and Santos, 2021), the hotel industry (e.g., Zervas et al., 2017; Dogru et al., 2020; Farronato and Fradkin, 2022), and local amenities (e.g., Xu and Xu, 2021; Basuroy et al., 2022; Hidalgo et al., 2022).

hosts while 7.6% of the burden is carried by the guests. The authors calculate an upper bound of pre-treatment tax compliance of 24%. Our set up does not allow us to calculate how the tax burden is split between hosts and customers, nor is it possible to quantify levels of tax compliance, because income tax rates in Denmark differ across hosts. However, we are able to provide evidence in a situation where it is not feasible to withhold taxes at the income source but where tax enforcement is instead implemented through data sharing.

Third, we contribute to the literature that investigates the use of information provided by external sources for the verification of tax declarations (i.e., "third-party reporting"). For example, several studies investigate the implications of information sharing by credit card companies about business transactions for tax compliance (e.g., Slemrod et al., 2017; Adhikari et al., 2021; 2022). Others study charitable organizations (Gillitzer and Ebbesen Skov, 2018; Clifford and Mavrokonstantis, 2021) and accounting firms (Edwards et al., 2021) as sources of third-party information, as well as situations where tax authorities create data by combining different sources available to them, such as registers, customs records, and filings of other taxpayers (Carillo et al., 2017; Brockmeyer et al., 2019; Eerola et al., 2019). According to this literature, third-party information is generally helpful to increase tax compliance. However, the magnitude of this effect depends on the context and varies across taxpayers. There may also be counteractions, such as taxpayers attempting to offset the cost associated with compliance by reporting higher expenses or avoiding taxes in other areas. We differ from this research by providing evidence on third-party reporting in the context of digital platforms, which have not been investigated as an external source of income data so far. The transmission of data about income and transactions from digital platforms to tax authorities is gaining importance, given the continued growth of gig- and sharing economies, and legislative responsive such as EU Council Directive 2021/514. We confirm that third-party reporting cannot be assumed to affect taxpayers equally, as illustrated by our evidence of differential effects on single- vs. multi-property hosts and those in rural vs. urban areas. Hence, third-party reporting may change competitive structures.

The paper proceeds as follows. In the next section, we provide background information about Airbnb and the regulation of short-term rental markets in Denmark and Sweden. In Section 3, we propose a theoretical framework used to structure the empirical analysis. Afterwards, in Sections 4 and 5, we describe the data and estimation strategy, followed by a discussion of the results in Section 6. Section 7 concludes.

2. Background

At the beginning of this study, we compiled a timeline of changes in the conditions that affected short-term rentals in Denmark and Sweden, primarily by accessing information published by Airbnb and the countries' tax authorities. To avoid missing any pertinent events or legislation, we also conducted a systematic search in all Danish and Swedish media sources archived in Retriever Mediearkivet[3] during our sample period, by using translations of applicable keywords (e.g., Airbnb, short-term rental, regulation, rental income, day cap).

We summarize the timeline of events in Denmark in Figure 1. Demands for a regulation of short-term rentals were first raised by the hotel industry in 2014, followed by an initiative of opposition parties in 2016 with the goal to force Airbnb to report hosts' income data to the Danish Tax Agency (e.g., Øland Frederiksen and Steensberg, 2016). In May 2018, rather than making data sharing a legal requirement, the Danish Tax Minister and Airbnb's director of public policy in Europe jointly announced the signing of an agreement, according to which Airbnb would automatically share information about all income generated by hosts residing in Denmark with the Tax Agency (e.g., Airbnb, 2018; Skatteministeriet, 2018). The government incentivized Airbnb to agree to the data sharing by granting hosts using the platform a tax-free allowance on rental income of 28,000 DKK per year (ca. 4,000 EUR or USD). To our knowledge, other home-sharing platforms did not participate. Apart from the allowance and other minor exceptions, rental income is subject to both income tax and 25% value added tax (PWC, 2019).[4] Income tax liabilities vary across hosts due to individual differences in income tax rates and tax deductions.

The agreement was implemented in two stages. As of July 2019, with the beginning of the first stage, the Tax Agency would receive information including hosts' real names, birth dates, property addresses, and earnings on the platform (Airbnb, 2019). The Tax Agency used this data to identify most hosts, retrieve associated social security numbers, and contact these citizens with a request to confirm the information (Skattestyrelsen, 2020). We argue that these steps substantially increased the probability of detecting non- or underreporting of income earned on Airbnb.

[3] See https://www.retrievergroup.com/. The database is maintained by TT Nyhetsbyrån, which is Scandinavia's largest news agency. The archive includes approximately 2,200 media sources from Denmark and 3,900 from Sweden.
[4] Before 2018, all short-term rental income below 1.333% of the property value or less than DKK 24,000 was not considered as taxable income. Value added tax only applies to income realized on Airbnb if the turnover exceeds DKK 50,000 within a 12-month period.

While the chances of detecting tax evasion increased substantially during the first stage of the agreement, full tax enforcement was not achieved until the start of the second stage in January 2021. With the beginning of the second stage, hosts were required to register a unique code provided by the Tax Agency in their Airbnb account. This procedure ensures that taxpayers are unambiguously identified, which makes it possible to directly enter any income realized on Airbnb in hosts' tax declarations (Airbnb, 2022d). Note that the second stage of the agreement started after the outbreak of the Covid-19 pandemic and is not included in our sample period, given the lack of a valid counterfactual at this point.

In addition to the data-sharing agreement, a cap on the number of days that a property can be rented out through short-term rental platforms in Demark was introduced in January 2019, restricting rentals of the same object to a maximum of 70 days per calendar year. This rule applies to entire properties but is not relevant for shared objects. However, in practice it remains unclear to what degree this day cap is enforced, as the relevant information is not part of the data-sharing agreement (Horesta, 2020). In Section 6.4 and Table A9, we provide complementary evidence that the introduction of the day cap was largely inconsequential for the supply of short-term rentals.

Short-term rentals in Sweden are regulated by rental housing rules that have been in place since before the launch of Airbnb, and have remained unchanged throughout: With single-family houses, there are no restrictions to subletting but the resulting revenues may be subject to income tax and value added tax (PWC, 2018).[5] Tax rules also apply to apartments, but here the landlord or tenant-owner association need to give their permission to any rentals, a rule confirmed by the Stockholm Rental Board to be applicable to short-term renting (e.g., Hellekant, 2015). Regulations specifically targeting short-term rentals have not been issued, nor have there been any collaborations between Swedish authorities and online lodging platforms.

We argue that Sweden provides an ideal counterfactual to evaluate the impact of the data-sharing agreement in Denmark because the two countries share many similarities regarding tourism trends, business cultures and, not least, housing markets (Torstensen and Roszbach, 2019). For example, both countries have equally high home ownership rates of around 60%. The segment for rental housing in Denmark and Sweden is divided into commercial and non-commercial rentals, which

[5] Rental income below or equal to SEK 40,000 is considered tax free. Income above this threshold is subject to a 30% capital tax. In addition, there is a 12% value added tax on income realized with the supply of accommodation if the rental period exceeds 113 days per year and the realized turnover exceeds SEK 50,000.

are typically supplied by for-profit corporations and subsidized building societies. Subsidized rental housing accounts for approximately one fifth of the housing stock. Both countries have experienced increasing frictions during the last decade, with steeply increasing housing prices and housing shortages, especially in the metropolitan areas of Copenhagen and Stockholm.

3. Theoretical framework

In this section, we develop a conceptional framework to illustrate how data-sharing agreements affect the market for short-term rentals. The main mechanism discussed in this section is the change in the probability of detecting tax evasion. We abstract from other possible mechanisms, such as shifts in social norms, changes in perceptions of shaming penalties, or improved knowledge of the tax code due to the letter sent by the Tax Agency (e.g., Fellner et al., 2013; Pomeranz, 2015) or news coverage of the agreement (e.g., Garz and Pagels, 2018; Battiston et al., 2020). We believe that those alternative mechanisms at best play a minor role because the issue of tax evasion related to short-term rental income had been widely discussed in the media and made salient by the Tax Agency long before the data-sharing agreement. Similarly, we can rule out a reduction of taxpayers' administrative costs as a dominant mechanism because hosts' responsibility to retrieve, organize, and declare rental income did not change with the announcement and first stage of the data-sharing agreement.

3.1 Rental choices

Owner i of properties $j = 1, .., n_i$ decides whether to rent out the properties on Airbnb or to offer them for long-term rent.[6] In the short-term case, an owner that complies with the tax system maximizes the following total profit π_i:

$$\max \pi_i(p_1, .., p_{n_i}) = \sum_{j=1}^{n_i} \left[\underbrace{(p_j - c_j - \tau_i) b_j}_{=\pi_{ij}} \right] - F_i \tag{1}$$

where p_j denotes the price for which a property j is offered (i.e., the listing price) and b_j captures the respective number of days the property is booked. Renting out on Airbnb comes with both

[6] For simplicity, and because home ownership rates are relatively high in our context, we abstract from hosts who do not own the property that they offer on Airbnb.

marginal cost c_j and fixed cost F_i.[7] We assume for simplicity that a host's marginal cost is constant but may vary across properties. One may, for example, assume that the marginal cost is higher for a property where the host resides compared to a property that is solely used to rent out. There is an additional heterogeneity in part of the fixed cost. The fixed cost may, for example, capture administrative costs coming with setting up an account on Airbnb and vary across hosts. In addition, a host that complies with the tax system faces an individual income tax and possibly value added tax, which we jointly capture by the variable τ_i. The tax rate depends on individual factors (e.g., income generated elsewhere). For simplicity reasons, we assume that the tax rate is constant. π_{ij} denotes the variable profit that host i realizes with property j (i.e., profit neglecting the fixed cost).

Since properties offered on Airbnb differ in various characteristics, we assume that the market is a market with imperfect competition and product differentiation. Following the seminal paper on price competition with product variety by Shapley and Shubik (1969), we assume the following demand function:

$$b_j(p_j, \bar{p}) = \frac{a - \bar{p}}{2s} - \frac{n}{2\varepsilon}(p_j - \bar{p}) \tag{2}$$

where $\bar{p}$ denotes the average market price. Competition is captured by the number of active properties n. The parameter ε controls the degree of product differentiation, with $\varepsilon = 0$ capturing the case where all properties are perfect substitutes. Finally, a is a constant that denotes the price at which bookings are zero and s is the slope of the inverse demand function. As before, p_j denotes the price for which a property j is offered and b_j the sum of booked days. If property j is offered at a price larger than the average market price, this property is booked less often than the average property. This effect is more pronounced the larger the level of competition (i.e., the larger the number of active properties n) and the more similar the properties (i.e., the smaller the value of ε).

A host that evades taxes faces the following objective:

$$\max\ \pi_i(p_1^e, \dots, p_{n_i}^e) = \sum_{j=1}^{n_i} \underbrace{\left[\varphi\left[(p_j^e - c_j)b_j^e - \phi\right] + (1-\varphi)\left[(p_j^e - c_j)b_j^e\right]\right]}_{=\pi_{ij}^e} - F_i \tag{3}$$

[7] We assume that the fixed cost only appears at the host level. The main model predictions remain the same if additional fixed cost at the property level arises.

where p_j^e denotes the price at which property j is offered, while b_j^e captures the respective number of days the property is booked. The exponent e indicates tax evasion. The demand function is the same as in Equation (2) but uses the exponent e. ϕ denotes the fine a host must pay if the tax authority detects the evasion. For simplicity, we assume that the probability φ of detection is exogenously given. In reality, this probability might depend on individual characteristics, such as the number of listing days and the location of the property.

We do not make detailed assumptions about the income m_j that owners may realize with property j in the long-term housing market because this market is not in the focus of our paper. Specifically, we are agnostic about whether this income involves tax evasion, as the underlying decision is not affected by the data-sharing agreement. Hosts enter the platform and offer a first property if:[8]

$$\max\{(\pi_{i1} - F_i), (\pi_{i1}^e - F_i)\} > m_1 \quad \text{and} \quad \max\{b_1; b_1^e\} > 0, \tag{4}$$

That is, the maximum utility that can be realized by renting out on Airbnb (with or without tax evasion) must be higher than the utility that can be realized on the long-term housing market and higher than the utility from making the property available for the host's personal use (which would be captured by $b_1 = b_1^e = 0$). Analogously, a host offers additional properties $j = 2,..,n_i$ on the platform if:

$$\max\{\pi_{ij}, \pi_{ij}^e\} > m_j \quad \text{and} \quad \max\{b_j; b_j^e\} > 0. \tag{5}$$

Once a host becomes active on the platform, it becomes ceteris paribus easier to offer additional properties since the fixed costs that appear when entering the platform are already paid.

3.2 Prices and market equilibrium

The main goal of this subsection is to characterize the optimal price-setting behavior of host i. We simplify the derivation of the market equilibrium by assuming that host i's market share is small enough for any effect on the average market price, i.e., $\partial\bar{p}/\partial p_j = \partial\bar{p}/\partial p_j^e = 0$.

Differentiating Equation (1) with respect to p_j gives the first-order condition for the optimal price for a host that complies with the tax system:

[8] Note that we order the properties of each host based on the additional profit the host can realize by offering a property for short-term rent instead of offering it on the long-term housing market. That is, property *j=1* is the one that generates the highest benefits when offering on the market for short-term rentals.

$$\left(p_j - c_j - \tau_i\right)\frac{\partial b_j}{\partial p_j} + b_j = 0. \tag{6}$$

Taking the demand function as in Equation (2) into account, we can solve for the optimal price that is set by a tax-compliant host:

$$p_j = \frac{c_j+\tau_i}{2} + \frac{a\varepsilon}{2sn} + \frac{sn-\varepsilon}{2sn}\bar{p}. \tag{7}$$

Note that the optimal pricing strategy does not depend on the fixed cost and is therefore, ceteris paribus, identical for single- and multi-property hosts. If hosts are identical with respect to costs c and tax obligations τ, if everyone complies with the tax system (i.e., $\bar{p} = p_j$), and if the market is perfectly competitive (i.e., $n \to \infty$), Equation (7) gives the standard result for the optimal price ($p = c + \tau$) where the price equals marginal cost including marginal tax payments.

Analogously, by differentiating Equation (3) with respect to p_j^e, equalizing to zero and solving for p_j^e, we obtain the optimal price a host offers if they evade taxes:

$$p_j^e = \frac{c_j}{2} + \frac{a\varepsilon}{2sn} + \frac{sn-\varepsilon}{2sn}\bar{p}. \tag{8}$$

Comparing Equations (7) and (8), we observe that non-compliant hosts offer, ceteris paribus, their properties at a lower price. This also implies that the average market price is lower the greater the number of hosts evading taxes. Differentiating Equations (7) and (8) with respect to the number of active properties n, we obtain:

$$\frac{\partial p_j}{\partial n} = \frac{\partial p_j^e}{\partial n} = -\frac{\varepsilon(a-\bar{p})}{2sn^2} < 0 \tag{9}$$

which indicates that a larger number of active properties implies lower prices at which the properties are offered on Airbnb, implying lower mark-ups realized by hosts.

3.3 Impact of the data-sharing agreement

As discussed in Section 2, the Danish authorities did not fully enforce taxes during the first stage of the data-sharing agreement but increased the chances of detecting tax evasion, as reflected in the model by an increase in φ. Hosts evading taxes prior to the agreement face a decrease in their expected profit due to the agreement (see Equation 3), which increases their likelihood of compliance. With decreasing profits, some hosts may not find it beneficial to offer their property for short-term rental under the data-sharing agreement, i.e., Equations (4) and (5) are not fulfilled anymore. The agreement therefore has two primary effects on the market: First, the average price $\bar{p}$ increases due to the fact that some initial non-compliers now comply with the tax system and therefore offer their properties at a higher price (cp. Equations 7 and 8). Second, some hosts that initially evade taxes exit the market. Thus, the number of active properties n is reduced, which leads to further price increases (see Equation 9). Overall, the data-sharing agreement should therefore lead to a decrease in active properties/hosts and an increase in the average listing price.

The effects on bookings are ambiguous. Totally differentiating Equation (2) with respect to the listing price, p_j, the average price, $\bar{p}$, and the number of active properties, n, the change in the number of days a property is booked can be described as follows:

$$-\frac{1}{2s}\underbrace{\Delta\bar{p}}_{>0}-\frac{n}{2\varepsilon}\underbrace{\Delta\left(p_j-\bar{p}\right)}_{\gtrless 0}-\frac{1}{2\varepsilon}\underbrace{\Delta n}_{<0}\underbrace{\left(p_j-\bar{p}\right)}_{\gtrless 0}\lesseqgtr 0 \tag{10}$$

where Δ denotes changes of the respective variables. The equation shows that the total change in bookings consists of three market adjustments. First, there is an increase in the average market price that reduces total demand (see first term of Equation 10). Second, hosts might attract or lose additional market shares, depending on how their property price changes compared to the average market price (see second term of Equation 10). Third, the reduction of active firms lowers competition and leads to additional demand for properties that are offered at a price above the average market price, while decreasing the demand for properties offered at a price below the average market price (see third term of Equation 10). The effect of the data sharing on the number of booked days depends on the relative importance of each adjustment mechanism in the real world and therefore remains an empirical question.

3.4 Differences in mark-ups

Properties offered on Airbnb are heterogenous and hosts differ with respect to the costs and tax liabilities that they face if they rent out on the platform. Thus, we expect heterogeneity in prices and the number of days properties are booked. This kind of market variety is confirmed by the descriptive statistics presented in Section 4 and Table 1. As described by Equations (7) and (8), hosts' mark-ups (i.e., $p_j - c_j - \tau_i$ and $p_j^e - c_j$, respectively) decrease in the marginal cost. Hence, hosts with initially higher marginal costs are more likely to exit the market after the implementation of the data-sharing agreement.

We have already argued that single- and multi-property hosts differ since the fixed cost a host faces appears when entering the platform, that is, when offering the first property on Airbnb. There are further arguments pointing to lower costs for multi-property hosts. First, it is plausible to assume that hosts that offer a single property on the platform are more likely to use the property, at least some part of the year, by themselves. This should imply higher marginal costs since most individuals experience some inconvenience sharing their private sphere with strangers. In addition, multi-property hosts might face lower costs as they are often commercial hosts with a higher level of professionalization (Xu and Xu, 2021; Gauss et al. 2022). Finally, commercial hosts often purchase properties specifically with the goal to rent out on the short-term market. Their properties are therefore more likely located in areas with relatively low competition and high mark-ups. The descriptive statistics presented in the next section and Table A2 confirm this argument as well. For these reasons, it is plausible to assume that multi-property hosts have higher mark-ups and are therefore less likely to exit the market in response to the data-sharing agreement.

3.5 Summary of predictions

As a result of the implementation of the data-sharing agreement we expect that

1. some hosts exit the platform,
2. the average listing price increases,
3. the effect on the number of booked days is ambiguous, and
4. multi-property hosts are less likely to exit the market than single-property hosts.

4. Data

4.1 Sources and variables

Information on Airbnb hosts come from AirDNA, a US- and Spain-based commercial data provider specialized on short-term rental markets. AirDNA scrapes information about rental properties listed on Airbnb and similar platforms, including bookings, prices, and host and property characteristics. Our sample includes all Airbnb hosts from Denmark and Sweden that listed a property on the platform at least once between January 1, 2015, and December 31, 2019. We chose the end date of this sample period in consideration of the outbreak of the Covid-19 pandemic. The pandemic had a strong impact on short-term rentals worldwide and was arguably differently handled in Sweden than Denmark (e.g., Garz and Zhuang, 2022), which invalidates hosts from Sweden as a control group during the pandemic. We chose the start data of the sample by weighing the benefits of having as many pre-treatment observations as possible, while excluding the somewhat volatile early times of emergence of Airbnb as a leading platform for short-term rentals in the two countries.

For each host, we observe the location, type, daily availability, listing price, and booking status of their property or properties. We use this information to compile a panel data set of Airbnb hosts. Our motivation to choose hosts as the unit of observation rather than individual properties or localities – as previously done in the literature – is that the data-sharing agreement targeted the taxation of individuals' income. While previous studies often use monthly observations on Airbnb activity (e.g., Barron et al., 2021; Bibler at al., 2021), we choose a quarterly frequency for our panel because our outcome variables exhibit excess variation when measured at a monthly frequency. That is, with monthly observations, graphs would be difficult to interpret, and the standard errors of the estimates would be larger. Our estimation sample includes a total of 148,064 hosts that we observe over a period of 20 quarters. See Table 1 for details.

To measure the supply of rentals, we compute the number of properties per host, counting those objects that are made available at least one day in a given quarter. This measure is an overdispersed count variable with excess zero counts because in 73% of observations (i.e., host-quarter combinations) hosts do not offer any property. The second most common value is 1, as ca. 24% of hosts offer exactly one property. As Table 1 shows, the maximum number of properties listed by a host in a quarter is 805. Less than 3% of hosts list multiple properties, which implies that supply decisions mostly take place at the extensive margin. Hence, we construct a binary variable (*listing propensity*) that takes the value 1 if any number of properties are listed – for at least one day in a given quarter – and 0 if not. To evaluate the compliance with the day cap introduced in Denmark in January 2019, we also construct a binary variable that takes the value 1 if hosts rent out an entire property for more than 70 days per calendar year and 0 otherwise. As Table 1 shows, approximately 8–9% of hosts have a property with bookings above this ceiling. We also calculate each host's average daily *listing price* in a quarter. This variable captures the monetary value in USD at which the host is willing to rent out a property for a night. In case of multiple properties per host, we take the average over properties. We also measure supply of rentals in terms of the *sum of listing days* per host and quarter. For multi-property hosts, we calculate this sum over all properties. This variable refers to the number of days a property is made available for rent, hence including both vacant and occupied days. In addition, we measure the *sum of booked days* – again over all properties if applicable – as a measure of realized renting in equilibrium. As Table 1 shows, the sample maximum of this variable is 23,407 booked days. These bookings were realized by a single host who listed 371 objects in the second quarter of 2019, which illustrates the extend of (presumably commercial) renting on Airbnb. Note that the sum of listing days, listing price, and sum of booked days are only observed in quarters where the host made a property available for rent.

We match the geo coordinates provided by AirDNA with information from the Global Administrative Areas Database (GADM, version 3.6; see https://gadm.org/) to determine the municipality in which a property – and by extension a host – is located. For hosts with multiple properties, we define the host's primary location as the municipality where the host generates most rental revenue. We complement this information with yearly data from Statistics Denmark and Statistics Sweden, respectively, to capture various characteristics of the host's (primary) municipality, including population density, average age, average income, share of employed citizens, share of citizens with higher education degree, and share of citizens with an immigration background. We also construct a measure of local Airbnb penetration, which is defined as the number of listed properties per 1,000 inhabitants. This measure allows us to evaluate whether hosts' reactions to the data-sharing agreement differed depending on the relative extent of local supply of short-term rentals. We plot the pre-treatment distribution of this variable in Figure A1, while Table A2 summarizes differences between municipalities by Airbnb penetration.

To address the possibility of demand spillovers from Denmark to Sweden, we compute the distance between hosts in Sweden to the Danish border. This distance ranges from 0 to 1,255 km. We categorize hosts in bins 0 < 200 km, 200 < 400 km, 400 < 600 km, 600 < 800 km, and 800 km or more. See Figure A2 for a map of both countries.

Finally, we obtain quarterly averages of the USD/DKK and USD/SEK exchange rates from the OCED Main Economic Indicators Database (cp. Figure A3). These variables allow us to evaluate the possibility that shocks to the value of the local currency and downstream effects on foreign demand for short-term rentals affect hosts' behavior.

4.2 Overall trends

In Figure 2, we compare developments of the outcome variables between Denmark and Sweden over time. Overall, the graphs offer initial support for the notion that relevant outcomes followed similar trends in both countries before Denmark's announcement of signing the data-sharing agreement with Airbnb. The raw data indicate that the listing propensity (Panel A) may have decreased in Denmark after the interventions (relative to the development in Sweden), whereas listing days (Panel B), listing prices (Panel C), and booked days (Panel D) may have increased.

5. Empirical strategy

5.1 Estimation approach

Estimating the effects of the data sharing on hosting is difficult because of omitted variables and simultaneity issues. For example, adjustments in rentals observed after the announcement do not necessarily have to be driven by the announcement but could be a result of seasonal influences and shocks to local housing or labor markets. To address this challenge, we employ a difference-in-differences strategy, using Airbnb hosts in Sweden as a control group to evaluate treatment effects in Denmark. As discussed in Section 2, the two countries share many similarities regarding tourism trends, business cultures, and housing markets. However, Sweden has not changed its regulation regarding short-term rentals or entered collaboration scenarios with Airbnb, which makes the country an ideal benchmark. Hosts in Sweden can be considered "never-treated" units, which implies that the difference-in-differences estimator is consistent even in the case of heterogenous treatment effects (Goodman-Bacon, 2021).

We estimate versions of the following model to compare developments in Denmark to Sweden:

$$y_{i,m,q,c} = \alpha_1 DK_c \times t_q^{2018q2} + \alpha_2 DK_c \times t_q^{2019q3} + \mu_i + \rho_q + \theta DK_c \times q + \varepsilon_{i,m,q,c} \qquad (11)$$

where y denotes the value of the outcome variable pertaining to host i, generating their primary revenue in municipality m and country c (Denmark or Sweden) during quarter q. DK is a binary variable that takes the value 1 for hosts in Denmark and 0 for Sweden.

We capture the implementation of the data sharing with the treatment dummy t^{2019q3}, which equals 1 during the time 2019q3 – 2019q4, and 0 otherwise. We also include the binary indicator t^{2018q2} to model treatment effects due to anticipation. This indicator takes the value 1 during the time between Denmark's announcement of the data-sharing agreement and its implementation (i.e., 2018q2 – 2019q2), and 0 otherwise. As Figure 3 indicates, Google searches in Denmark for the query "Airbnb tax" peaked in the second quarter of 2018, when the government announced the agreement. Hence, it is possible that hosts changed their behavior before the agreement took effect, for instance, by preemptively adjusting their supply in line with the expected changes in tax enforcement. Following methodological guidelines, we allow for anticipation effects by including t^{2018q2}, which ensures that the effects of the actual implementation are consistently estimated (Abadie, 2021; Borusyak et al., 2022).

The terms μ_i and ρ_q denote host fixed effects and the full set of time fixed effects[9], respectively, which capture time-invariant differences in rental supply and bookings across hosts, as well as common trends in the outcome variables over time. It would be possible to include municipality fixed effects, but these are absorbed by the host fixed effects. The coefficients α_1 and α_2 on the interactions between the Denmark dummy and t^{2018q2} and t^{2019q3} capture the effect of the announcement and implementation of the data-sharing agreement, respectively, on Airbnb hosts in Denmark relative to hosts in Sweden.

Following Bilinski and Hatfield (2019), we also include differential time trends ($DK_c \times q$) in the model (linear, quadratic, or cubic) to allow for potential differences in the development of the outcome variables between Denmark and Sweden before the treatment. Hence, identification of the effects of the data sharing comes from deviations from potentially pre-existing country-specific trends caused by the data sharing. If there are minor trend differences in the outcome variables, including $DK_c \times q$ reduces a potential estimation bias of α_1 and α_2, but this approach also yields unbiased estimates of the treatment effects if trends are parallel.

We estimate Equation (11) by using OLS and cluster the standard errors by the municipality where a host generates most of their income. It would also be possible to compute standard errors clustered by host but clustering by municipality yields more conservative confidence intervals because the latter accounts for error correlation both within hosts (over time) and within municipalities.

As discussed below, we do not find strong evidence of pre-existing trends in the outcome variables, and there are no statistically significant differences in their mean values (see Table 1). Hence, we

[9] That is, we include one dummy for each of the 20 quarters in the sample period minus the reference category. As Table A3 shows, the results are similar when we use year fixed effects instead.

refrain from matching on observables and do not include any covariates in our baseline specifications. However, robustness checks confirm that we obtain very similar results when we estimate Equation (11) conditional on municipality-specific characteristics and other factors (see Table A4).

5.2 Parallel-trends assumption and model selection

We test whether the outcome variables exhibit parallel pre-trends by estimating models that include interactions between the country dummy DK and lags l and leads f of the initial treatment date:

$$y_{i,m,q,c} = \sum_{j=-l}^{f} \beta_j t_{q+j}^{2018q2} \times DK_c + \mu_i + \rho_q + \varepsilon_{i,m,q,c} \tag{12}$$

where μ_i and ρ_q and are again host and time fixed effects. The β_j's measure differences in outcome y between Denmark and Sweden at different quarters during the sample period. Statistically significant β_j's before the first point of treatment could be indicative of non-parallel pre-trends, whereas insignificant pre-treatment β_j's would be compatible with the notion that the parallel-trends assumption holds. We consider the second quarter of 2018, when Denmark declared the signing of the data-sharing agreement with Airbnb, as the initial point of treatment due to possible anticipation effects. We use the quarter before the initial treatment as the reference period (i.e., 2018q1), against which the values of the β_j's are compared. The results of testing for parallel pre-trends can be conveniently visualized in the form of event study plots.

As Figure 4 shows, there are generally no significant pre-treatment effects in our outcome variables. An exception is the second lag – corresponding to a hypothetical effect in 2017q3 – in the listing price regression shown in Panel C, which yields a borderline significant coefficient. However, in economic terms, this coefficient has a similar or smaller magnitude than most other pre-treatment coefficients in this graph. In settings where many pre-treatment periods are available (here: 13), some out of many coefficients may be statistically significant just by chance. That is, in any regression, 1 of 10 coefficients will be coincidentally significant when applying the 10% significance level. Hence, focusing on the economic rather than statistical significance of violations of pre-trends, we do not reject the parallel-trends assumption here (Roth et al., 2022).

Tests for parallel pre-trends based on event study plots have been criticized for being sensitive to sample choices – especially the number of pre-treatment periods – and for offering low power to detect violations of the parallel-trends assumption. In turn, well-powered studies that rely on millions of observations (as in our case) may find statistically significant violations that are economically unimportant (e.g., Freyaldenhoven et al., 2019; Kahn-Lang and Lang, 2020; Rambachan and Roth, 2023).

For that reason, we complement our event study plots with results of "non-inferiority" tests that allow us to quantify potential violations of parallel trends in terms of their economic rather than statistical significance. For that purpose, we follow Bilinski and Hatfield (2019) and evaluate to what degree differential trends between treated and control hosts affect the size of the treatment effect estimates by comparing simple to more complex models.[10] Given that the raw data (Figure 2) and the event study plots (Figure 4) suggest minor violations of parallel pre-trends, we compare models that include differential linear trends (simple specification) with models that include differential quadratic trends (complex specification), following the "one step up" procedure of the non-inferiority approach. We repeat this procedure for a comparison of models that include differential quadratic trends (simple specification) with models that include differential cubic trends (complex specification). We then evaluate how much the estimated treatment effects (α_1 and α_2 in Equation 11) change from the simple to the more complex specification. Changes in treatment effect sizes between these models would indicate that a fraction of the effect estimated in the simple model is driven by bias due to trend violations, where the magnitude of the change in the effect size reflects the magnitude of the bias. If the change in the size of the treatment effect exceeds a certain threshold, the more complex model would be favored over the simple model. Specifically, we evaluate whether the magnitude of the change is larger than the estimated treatment effect size (Bilinski and Hatfield, 2019).

Results of the model comparison exercise are presented in Table A1. When comparing the effect sizes between the linear and quadratic trend specifications (Columns 1 and 2), we find three cases where the absolute difference between coefficients is larger than the absolute treatment effect (i.e., once when using the sum of listing days as the outcome and twice when using the sum of booked days). These results indicate that violations of parallel pre-trends are too large to rule out a difference-in-differences specification with a linear trend differential. When comparing treatment effect

[10] Rambachan and Roth (2023) propose an alternative approach to evaluate the parallel trends assumption based on the economic significance of pre-trends. However, this approach does not inform the model selection process in the way the non-inferiority approach does.

sizes between the quadratic and cubic trend specifications (Columns 2 and 3), the changes in coefficients are generally lower and consistently smaller in magnitude than the treatment effects. Hence, the possible reduction of bias of the treatment effect estimates is negligible when moving from a quadratic to a cubic trend model. In this case, the non-inferiority approach suggests using the simpler quadratic trend specification, as the more complex cubic trend model produces larger standard errors. The estimation results presented in the next section are therefore all based on models with differential quadratic trends.[11]

6. Results

6.1 Baseline estimates

We summarize the results of estimating Equation (11) in Table 2. As Column (1) shows, we find significant effects of the data-sharing agreement on hosts' listing propensity. The point estimates indicate that the likelihood to make any number of properties available decreased by 2.8 percentage points after the government's announcement of signing the agreement with Airbnb, relative to developments in Sweden. The listing propensity was 3.8 percentage points lower after the start of the data sharing than in the pre-treatment period. These effect sizes imply reductions by 0.028 / 0.269 = 10.4% and 0.038 / 0.269 = 14.1% compared to the mean listing propensity or 6.2% and 8.4% compared to the standard deviation. Columns (2) and (4) of Table 2 indicate that we do not find significant effects of the interventions on listings days or booked days. According to Column (3), we find a significant impact on listing prices though (at the 10% and 5% levels). Hosts in Denmark increased these prices by approximately \$9.90 after the data-sharing announcement and \$12.90 after its implementation, relative to the price development in Sweden. These amounts imply effects sizes in the magnitude of 8.5% and 11.1% compared to the mean listing price, and 10.2% and 13.3% compared to the standard deviation.[12]

We interpret these results as follows: In line with the theoretical predictions formulated in Section 3, the decrease in listing propensity suggests that a fraction of hosts left the platform because the

[11] As Panel B of Table A3 indicates, models that do not include any differential trend variables produce qualitatively similar results. However, these estimates are less trustworthy than estimates based on models with differential trend variables when there are minor violations of parallel pre-trends.

[12] The price effect could be driven by actual increases in hosts' listing prices or be an algebraic result if hosts with low listing prices left the platform. As Figure A4 indicates, we do not find any differential effects on hosts' listing propensity depending on their average pre-treatment listing price. Hence, the post-treatment price increase in Table 2, Column (3), was likely driven by actual price adjustments.

data-sharing agreement and the associated increase in the probability of detecting tax evasion made it unprofitable to rent out their property via Airbnb.[13] The null effects regarding the sum of listing days may indicate that hosts – on aggregate – did not attempt to offset the costs of complying with the tax system by renting out their property more often. Instead, the increase in listing prices is compatible with the notion that hosts pass the costs to the customer. However, the increase in listing prices could also be the result of decreased competition due to the exit of hosts from the platform. The null effect on the number of booked days is more difficult to interpret because this variable refers to renting in equilibrium.

6.2 Robustness

It could be argued that our baseline specification produces biased results because it does not account for time-varying confounders or country-specific shocks. In Table A4, we include controls for characteristics of the host's primary municipality, including yearly measures of average age, average income, population density, share of employed citizens, share of citizens with higher education degree, and share of citizens with an immigration background. In addition, we add the USD/DKK and USD/SEK exchange rates to account for the possibility that differential trends in the value of the local currencies and related changes in foreign demand for short-term rentals affected hosts.[14] The results remain similar to the baseline specification when we condition on these factors.

We also re-estimate the baseline specifications for the sum of listing days and the sum of booked days using Poisson models.[15] The reason is that both variables are count variables, which can cause OLS to be inconsistent. Results are shown in Table A6, Columns (1) and (2). The estimates mostly confirm the null results regarding these outcomes. An exception is the sum of booked days, which

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[13] Due to the lack of data, we cannot evaluate whether these hosts switched to other home-sharing platforms. However, data from Google Trends do not indicate an increase in web searches for alternative providers of short-term rentals after the announcement or start of the data-sharing agreement (cp. Figure A5), likely because alternative platforms did not qualify for the tax-free allowance on rental income. Hence, it is plausible to assume that hosts with unprofitable objects exited the short-term rental market completely.

[14] The local currencies followed fairly similar trends throughout the investigation period (Figure A3). A placebo test confirms the absence of currency-related, country-specific shocks. That is, we do not find any "treatment effects" of the data sharing on the exchange rates (Table A5).

[15] We also consider applying a log transformation or inverse hyperbolic sine transformation to these variables but discard this option because of estimation inconsistencies in the presence of many zeros (Bellemare and Wichman, 2020; Aihounton and Henningsen, 2021; Norton, 2022).

is now estimated to be significantly lower after the start of the data sharing. According to the incidence-rate ratio of 0.739, the magnitude of the decline equals 1 – 0.739 = 26.1% of the mean. Columns (3) and (4) of Table A6 show OLS estimates when trimming the top 0.01% of the listing days and booked days variables. The results remain similar, which suggests that extreme values do not play an important role here.

When using the listing propensity as an outcome variable, we may underestimate the true magnitude of the reduction in short-term rental supply if hosts' reactions are not limited to the question of whether or not to use Airbnb but also involve decisions about how many properties to list. In Table A7, we therefore investigate how the data-sharing agreement affected the number of listed properties. The estimates in Columns (1) and (2) indicate that the negative effects on listings remain statistically significant and similar in magnitude when using this alternative outcome variable, both with OLS and Poisson regression. In the case of OLS, the point estimates imply reductions in the number of listed properties by 0.044 / 0.353 = 12.5% after the announcement of the data sharing and 0.055 / 0.353 = 15.6% after the start. The Poisson estimates indicate effect sizes of 1 – 0.912 = 8.8% and 1 – 0.851 = 14.9%, respectively.[16] Columns (3) and (4) present estimates pertaining to the number of listings when excluding observations with zero listings. This exercise allows us to evaluate whether the data-sharing agreement caused a regrouping of properties across hosts. For example, single-property hosts may have decided to engage the services of a short-term rental agency or sell their property to multi-property hosts. If a "small-to-large" regrouping took place, we should observe an increase in the number of listed properties among those hosts that remained active on the platform. This is not the case though; we do not find any significant effects here.

6.3 Demand spillovers

It is possible that the data-sharing agreement in Denmark not only affected domestic hosts but also hosts in other countries, due to demand spillovers. That is, Airbnb guests may have reacted to the changes in Airbnb rental supply and prices by switching to rentals abroad. If hosts in Sweden were affected by demand spillovers, they would not constitute a valid control group, as the stable unit treatment value assumption (SUTVA) is violated. The coefficients α_1 and α_2 in Equation (11) would be biased upwards. If demand spillovers took place, hosts close to the Danish border and

[16] We interpret these results carefully though because tests for pre-trends (available on request) indicate that it is difficult to justify the parallel-trends assumption when using the number of listed properties as an outcome variable.

those in the larger cities (i.e., Stockholm, Gothenburg, and Malmö – all located in the south of Sweden) should arguably be more likely affected than hosts in more remote places. That is, it is not plausible for rental objects far away from the Danish border to serve as substitutes for rental objects in Denmark, especially not those in the very northern part of Sweden. For instance, the beeline distance between the Swedish city of Luleå and Copenhagen is more than 1,200 km (see Figure A2). It seems highly unlikely that guests renting in Copenhagen would switch to places such as Luleå due to the data sharing agreement.

Clarke (2017) and Butts (2021) propose a modification of the standard difference-in-differences model to obtain unbiased estimates in the presence of treatment spillovers, based on the idea that the magnitude of the spillover declines with the geographic distance between treatment and control units. We implement this approach by adding distance bins that capture the distance of hosts in Sweden from the Danish border to Equation (11), leaving out the bin that includes the most distant hosts as the reference category (i.e., more 800 km). This specification produces unbiased estimates as long as there are no spillovers in this most distant category.

Results are presented in Table A8. Accordingly, the coefficients capturing the effects of the announcement and start of the data sharing remain virtually unchanged. The coefficients on the distance bins are mostly insignificant, which is not compatible with the notion of demand spillovers between Denmark and Sweden. For example, if Airbnb guests that would normally rent in Copenhagen reacted to the lower supply and higher prices by switching to rentals in Stockholm, we expect the coefficient on the 400 < 600 km bin to be significant (the beeline distance between these cities is 470 km). This is generally not the case though. We obtain similar results when we use bins of alternative sizes, such as 150 km and 250 km instead 200 km, and when we measure the distance from Copenhagen instead of the border (results are available on request).

6.4 Compliance with the day cap in Denmark

As detailed in Section 2, the Danish government introduced a day cap in January 2019 for entire properties to be rented out via short-term rental platforms. This restriction prohibits rentals of the same property for more than 70 days per year, regardless of whether the property is rented by the same or multiple guests. In Table A9, we evaluate whether the introduction of the day cap affected the probability that hosts in Denmark rent out property in violation of the cap. As Column (1) shows, this probability significantly decreased after the rule was implemented, by a magnitude of approximately 1.0 percentage point or 0.010 / 0.083 = 12.0% compared to the mean probability. Hence, short-term rentals of entire properties with more than 70 booked days per year declined only by a moderate degree. In absolute terms, the renting of properties above this threshold actually increased in Denmark between 2018 (a total of 13,753 hosts) and 2019 (a total of 14,909 hosts), likely because compliance with the day cap could not be enforced in practice. We also check whether the day cap affected the average number of relevant properties but do not find any significant effects (Table A9, Columns 2 and 3).

6.5 Effect heterogeneity

6.5.1 Single- vs. multi-property hosts

A central question when investigating the supply of short-term rentals is whether the data sharing affected hosts with one vs. multiple properties in different ways. As argued in Section 3, multi-property hosts often use the platform for commercial reasons and may realize higher mark-ups than single-property hosts. Hence, we expect the effects on listings to be stronger among single-property hosts. To test this prediction, we use a triple-differences version (e.g., Olden and Møen, 2022) of Equation (11) and include interactions between the country indicator, the post-treatment indicators, and dummies that categorize hosts by the maximum number of properties they listed for rent at any point during the pre-treatment period. Specifically, we distinguish between hosts listing 1 property (the reference category), 2 properties, and 3 or more properties.

Results are summarized in Table 3. The table shows that the estimates for single-property hosts by and large mirror the aggregate results. That is, a fraction of these hosts exited the platform (Column 1), while the remaining hosts increased their listing prices (Column 3). In addition, single-property hosts experienced a significant decline in the number of booked days after the start of the data-sharing agreement (at the 5% level, see Column 3). We obtain different results for multi-property hosts, especially for those with 3 or more properties. While these hosts also increased their prices (Column 3), they did not exit the platform (Column 1), which is in line with the theoretical predictions formulated in Section 3. Instead, these hosts reacted by increasing the daily availability of their properties after the announcement of the agreement (Column 2), which could be an indication that they attempted to offset the costs incurred by the higher tax enforcement. In contrast to single-property hosts, those with 3 or more properties may have benefited after the announcement of the data sharing by an increase in the number of booked days, according to the borderline significant coefficient in Column (4). Considering that hosts with many properties often use Airbnb primarily for business reasons, the results in Table 3 suggest that the data-sharing agreement led to a stronger commercialization of Airbnb activities in Denmark.

6.5.2 Local Airbnb penetration

Home sharing has been criticized the most in areas with a high supply of short-term rentals, due to concerns about the crowding out of residents and unfair competition to the hotel industry. Previous studies have therefore focused on the impact of regulatory measures in areas with high levels of Airbnb penetration, especially big cities and urban regions (e.g., Valentin, 2021; Koster et al., 2021; Chen et al., 2022; Gauss et al., 2022). Hence, little is known about short-term rental markets in rural areas. For that reason, we investigate whether variation in pre-treatment Airbnb penetration across municipalities moderates the effects of the data-sharing agreement on listings and bookings. As Figure A1 shows, our measure of local Airbnb penetration – the number of listed properties per 1,000 inhabitants – approximately has a tri-modal distribution. On one end of the distribution, we observe a number of (mostly rural) municipalities with a relatively low supply of Airbnb rentals. On the other end, we find Copenhagen, Stockholm, and a few other urban municipalities with relatively high numbers of listed properties per 1,000 inhabitants. We therefore categorize hosts in low (< p25), medium (>= p25 < p75), and high (>= p75) Airbnb-penetration areas according to the average pre-treatment number of listings per 1,000 inhabitants in their municipality, which allows us to estimate specifications with triple interactions between the country indicator, the post-treatment indicators, and penetration dummies.

Results are presented in Table 4. As Column (2) shows, hosts in low-penetration areas expanded the number of listing days both after the announcement and start of the data-sharing agreement, whereas hosts in high-penetration areas tended to make their property less often available.[17] Similarly, we find that the number of bookings increased for hosts in low-penetration areas but decreased in high-penetration areas (Column 4). It is difficult to discuss the reasons behind the

[17] The triple-differences estimates shown in Table 4 capture post-treatment drifts between areas relative to each other. Re-estimating plain difference-in-differences models (Equation 11) with a sample restricted to hosts in low-penetration areas indicates an increase in the availability of objects in absolute terms too (cp. Table A10).

differential effects without additional data, especially on hosts' mark-ups (which are not available to us). However, the patterns in Table 4 offer important descriptive insights when looking at the characteristics of low- vs. high-penetration municipalities: As Table A2 shows, places with higher levels of Airbnb penetration tend to be characterized by higher income, education, and employment levels. Hosts request higher prices and make their property less often available in these areas. In addition, there is a large overlap between the level of Airbnb penetration and population density. Low-penetration municipalities are often sparsely populated areas, whereas high-penetration municipalities tend to be densely populated, such as Copenhagen and Stockholm. Hence, the data-sharing agreement led to a spatial reorganization of short-term rental supply and bookings from initially high-penetration (urban) to initially low-penetration (rural) areas. This shift was most likely not an objective of the data-sharing agreement but can be considered an unintended side effect.

6.5.3 Pre-treatment earnings

Our primary interpretation of the main results is that the costs of renting out property increased for hosts that did not comply with the tax system prior to the data-sharing agreement. If this interpretation is correct, we should observe differential effects for hosts that are liable for taxes and hosts that are not. As discussed in Section 2, hosts with annual earnings below a certain threshold (e.g., 28,000 DKK in 2018) are in principal exempt from the income tax. These hosts should be largely unaffected by the data-sharing agreement, whereas hosts with earnings above the threshold should be particularly responsive. In 2017, the last tax year before the treatment, 20.1% of hosts in Denmark accumulated annual earnings above 28,000 DKK on Airbnb.

Figure 5 presents results from a triple-differences setting where we include interaction terms between the Denmark indicator, the post-treatment indicators, and dummies for 2017 income bins. Accordingly, the listing propensity did not change for hosts in income classes below the threshold. However, we find reductions in the listing propensity for hosts with annual earnings above 28,000 DKK, especially after the start of the data sharing (Panel B). The propensity to list a property tended to decline more, the higher the pre-treatment earnings, which is compatible with the tax-compliance mechanism.

6.5.4 Existing vs. new hosts

Another assumption behind the tax-compliance mechanism is that a reduction in the listing propensity reflects decisions of existing hosts (i.e., those active on Airbnb prior to the data sharing) to leave the platform. However, it is equally possible that the agreement reduced the entry of new hosts, who may have been deterred to join Airbnb by the increased probability of detection of tax evasion. The estimates in Table A11 suggest that both types of reactions took place, although the impact on existing hosts may have been somewhat stronger than the impact on new entrants. That is, the announcement of the agreement reduced the probability of entry by 1 percentage point or 0.010 / 0.070 = 14.3% compared to the mean entry probability. In contrast, the exit probability increased by 1.3 percentage points or 0.013 / 0.059 = 22.0%.

6.6 Bunching

The tax threshold for rental income of 28,000 DKK in 2018 should create a kink in the marginal tax rate, which could induce hosts to target an annual income level just below the threshold and produce excess mass in that area of the earnings distribution (e.g., Kleven, 2016). As Figures A6 and A7 show, there are no visible indications of bunching though, likely due to multiple reasons. First, the income threshold takes different values in cases where hosts are co-owners or co-tenants of a property. For instance, if a host owns 50% of the property, or splits the rent of a property at 50%, the threshold amounts to 14,000 DKK. Hence, there are as many individual income thresholds as there are ownership and tenant constellations. Second, even if all hosts were subject to the same income threshold, they face different average tax rates depending on their other (non-rental) income, which arguably creates different incentives to target the threshold. Third, uncertainty about bookings makes it difficult for hosts to precisely realize their targeted annual earnings (e.g., due to unclear occupancy rates towards the end of the tax year or cancelations). Hence, the absence of bunching does not mean that hosts are irresponsive to the tax system, but that practical reasons and individual differences lead to a rather smooth earnings distribution.

7. Conclusion

In this paper, we analyze the effects of the data-sharing agreement between the Danish government and Airbnb announced in 2018. Using rich panel data on individual hosts and a difference-in-differences approach, we find that the agreement reduced the listing propensity by 14%, while increasing prices by 11%. These effects are compatible with the notion that the transmission of information about income earned by hosts on the platform to the Danish Tax Agency led to a substantial increase in the detection probability of tax evasion. The associated cost increase made it unprofitable for some formerly non-compliant hosts to rent out their property, as reflected by the reduction in listing propensity. Among hosts remaining active on the platform, the increased tax compliance led to in an increase in listing prices, likely caused by the reduction in competition due to the drop in rental supply and efforts to pass through some of the tax burden to the customer. We observe these effects not only at the time of implementation of the data-sharing agreement but upon its signing a year earlier, which suggests that forward-looking taxpayers found the announcement credible.

Our results are not without limitations. On the one hand, we cannot investigate any effects after the outbreak of the coronavirus pandemic because public health strategies in Denmark and Sweden differed substantially, which invalidates hosts in Sweden as control group at this point. Other countries that share similarities with Denmark could not be used as counterfactual either, because they implemented regulatory measures of short-term rentals themselves (e.g., Norway). This limitation implies that it is not possible to investigate the second stage of the data-sharing agreement between Denmark and Airbnb starting in January 2021, which featured the pre-filling of income earned on the platform in hosts' tax declarations. On the other hand, our setting does not allow us to quantify the extent of pre-treatment tax evasion, nor is it possible to determine how hosts and customers split the tax burden under the data sharing, given that income tax rates differ across individuals.

Our results have important implications despite these limitations. First, the lessons from the Danish data-sharing agreement give us an idea about what to expect from EU Council Directive 2021/514, which makes it mandatory for home-sharing platforms and other online marketplaces to transmit information about taxable income and sales to authorities in relevant member states. The directive likely has heterogenous effects throughout the EU because of differences in tax systems and initial levels of tax compliance across countries. Hence, the results from Denmark cannot be applied to the EU context in a one-to-one manner. However, it is plausible to expect for hosts with lower mark-ups to be more likely to exit short-term rental markets due to the EU-wide data sharing. Second, our finding that rental supply shifted from single- to multiple-property hosts stands in contrast to what most regulatory measures targeting short-term rentals intend to achieve. That is, policy-making related to home-sharing platforms typically aims to curb the negative effects of large-scale commercial renting (e.g., Valentin, 2021; Koster et al., 2021; Chen et al., 2022; Gauss et al., 2022). Assuming that multiple-property hosts are often those that use Airbnb primarily for business reasons, the effects of the data-sharing agreement in Denmark are in opposition to this goal: Single-property hosts experienced a reduction in realized bookings and were more likely to leave the platform, while multi-property hosts expanded the availability of their objects. Third, our finding that rental activities shifted from regions with initially high degrees of Airbnb penetration to low-penetration areas could imply that the data-sharing agreement brought some relief to urban areas and larger cities, where concerns about the scarcity of affordable housing are typically the strongest. Hence, this effect of the data-sharing agreement is more compatible with the goals of traditional regulatory approaches to short-term renting. In any case, those unintended consequences illustrate that future research is necessary to evaluate the implications and possible side effects of upcoming tax-enforcement policies targeting home-sharing and other digital platforms.

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Tables and figures

Table 1: Summary statistics of main variables

	Denmark		Sweden				
	Mean	SD	Mean	SD	Min.	Max.	Pre-treatment difference (*p*-value)
Host-level characteristics							
Number of properties	0.37	3.29	0.33	2.37	0.00	805.00	0.364
Listing propensity (binary)	0.27	0.45	0.26	0.44	0.00	1.00	0.151
Propensity for property with more than 70 booking days p.a. (binary)	0.09	0.38	0.08	0.27	0.00	1.00	0.328
Sum of listing days	70.74	451.92	72.25	324.15	1.00	72210.00	0.752
Average daily listing price (USD)	113.28	96.95	120.70	376.63	0.00	105453.39	0.571
Sum of booked days	30.38	227.85	27.16	150.21	0.00	23407.00	0.919
Municipality-level characteristics							
Average age (years)	38.62	3.42	40.56	2.63	35.90	53.60	0.356
Average income (USD)	47298.26	7699.83	35733.76	5193.18	25208.95	97663.13	0.013
Population density (inhabitants per km^2)	5713.97	3254.53	2065.44	2180.61	0.22	12073.22	0.069
Share of employed people	0.51	0.04	0.50	0.03	0.36	0.57	0.910
Share with higher educ. degree	0.18	0.08	0.20	0.07	0.02	0.33	0.575
Share with immigration backgr.	0.19	0.07	0.27	0.10	0.04	0.60	0.136
Number of observations		1,827,880		1,133,400			
Number of quarters		20		20			
Number of hosts		91,394		56,670			
Number of municipalities		98		290			

Notes: The observations are quarterly data on all Airbnb hosts that listed a property at least once between 2015q1 and 2019q4. The rightmost column report *p*-values (adjusted for clustering by municipality) for the null hypothesis of no differences in means in the pre-treatment period (2015q1 – 2018q1). The number of observations is lower for the sum of listing days, listing price, and sum of booked days because these variables are missing when hosts did not offer any property.

Table 2: Effect of data sharing on listings and bookings

	(1) Listing propensity	(2) Sum of listing days	(3) Listing price (USD)	(4) Sum of booked days
Announcement of data sharing × Denmark	-0.028***	4.168	9.898*	1.913
	(0.006)	(4.943)	(5.914)	(4.984)
Start of data sharing × Denmark	-0.038**	0.518	12.904**	-6.795
	(0.016)	(9.534)	(6.522)	(6.698)
Mean of dep. var.	0.269	72.694	116.381	29.820
Adj. R^2	0.266	0.861	0.347	0.644
Observations	2961280	770883	761485	770883
Number of clusters	388	388	388	388

Notes: Difference-in-differences estimates (OLS), using quarterly data on Airbnb hosts between 2015q1 and 2019q4. The sample in Column (1) includes all hosts in Denmark and Sweden that listed a property for rent at least once during the sample period. The sample in Columns (2) to (4) excludes observations where hosts did not list any property. The column headers denote the dependent variable. All models include host fixed effects, time fixed effects, and a quadratic trend difference. Standard errors (in parentheses) are clustered by municipality.
* p<0.10, ** p<0.05, *** p<0.01

Table 3: Effect of data sharing on listings and bookings, by hosts' number of properties

	(1) Listing propensity	(2) Sum of listing days	(3) Listing price (USD)	(4) Sum of booked days
Announcement of data sharing × Denmark	-0.026***	-0.132	5.710***	-2.263
	(0.007)	(2.805)	(1.859)	(1.382)
Announcement × Denmark × 2 properties	-0.026**	-0.088	27.694	0.634
	(0.013)	(2.339)	(29.053)	(1.278)
Announcement × Denmark × 3 or more prop.	-0.016	96.196***	5.425**	108.402*
	(0.034)	(36.180)	(2.262)	(61.601)
Start of data sharing × Denmark	-0.038**	-3.696	8.232***	-10.084**
	(0.016)	(5.659)	(2.018)	(3.516)
Start × Denmark × 2 properties	0.002	2.060	35.972	0.120
	(0.018)	(2.039)	(39.440)	(1.533)
Start × Denmark × 3 or more prop.	0.037	101.355	6.201*	92.969
	(0.029)	(83.076)	(3.279)	(71.774)
Mean of dep. var.	0.269	72.694	116.381	29.820
Adj. R^2	0.268	0.862	0.347	0.648
Observations	2961280	770883	761485	770883
Number of clusters	388	388	388	388

Notes: Difference-in-differences estimates (OLS), using quarterly data on Airbnb hosts between 2015q1 and 2019q4. The sample in Column (1) includes all hosts in Denmark and Sweden that listed a property for rent at least once during the sample period. The sample in Columns (2) to (4) excludes observations where hosts did not list any property. The column headers denote the dependent variable. Hosts are categorized by their maximum number of properties listed in any quarter prior to treatment (1, 2, and 3 or more). The reference category is 1 property. All models include host fixed effects, time fixed effects, a quadratic trend difference, and the constituent terms of the interactions. Standard errors (in parentheses) are clustered by municipality.
* p<0.10, ** p<0.05, *** p<0.01

Table 4: Effect of data sharing on listings and bookings, by local Airbnb penetration

	(1) Listing propensity	(2) Sum of listing days	(3) Listing price (USD)	(4) Sum of booked days
Announcement of data sharing × Denmark	-0.029	9.822***	19.994	10.988**
	(0.052)	(3.670)	(19.479)	(4.935)
Announcement × Denmark × medium	0.072	-0.243	-15.066	-4.502
penetration	(0.057)	(5.827)	(18.692)	(8.682)
Announcement × Denmark × high penetration	-0.079	-18.926***	-11.875	-19.585**
	(0.089)	(5.377)	(18.927)	(7.941)
Start of data sharing × Denmark	-0.048	13.133*	23.491	6.546
	(0.082)	(6.974)	(22.423)	(5.565)
Start × Denmark × medium penetration	0.102	-6.315	-17.176	-6.786
	(0.080)	(11.479)	(22.058)	(11.542)
Start × Denmark × high penetration	-0.090	-30.646***	-11.990	-25.223***
	(0.126)	(9.970)	(22.345)	(8.801)
Mean of dep. var.	0.269	72.694	116.381	29.820
Adj. R^2	0.272	0.861	0.347	0.645
Observations	2961280	770883	761485	770883
Number of clusters	388	388	388	388

Notes: Difference-in-differences estimates (OLS), using quarterly data on Airbnb hosts between 2015q1 and 2019q4. The sample in Column (1) includes all hosts in Denmark and Sweden that listed a property for rent at least once during the sample period. The sample in Columns (2) to (4) excludes observations where hosts did not list any property. The column headers denote the dependent variable. Hosts are categorized in low (< p25), medium (>= p25 < p75), and high (>= p75) Airbnb penetration according to the average pre-treatment number of listed properties in their municipality, relative to the population size. The reference category is low penetration. Hosts with properties in multiple municipalities are assigned to the municipality where they generate most rental revenue. All models include host fixed effects, time fixed effects, a quadratic trend difference, and the constituent terms of the interactions. Standard errors (in parentheses) are clustered by municipality.
* p<0.10, ** p<0.05, *** p<0.01

Figure 1: Timeline of changes in short-term rental conditions in Denmark

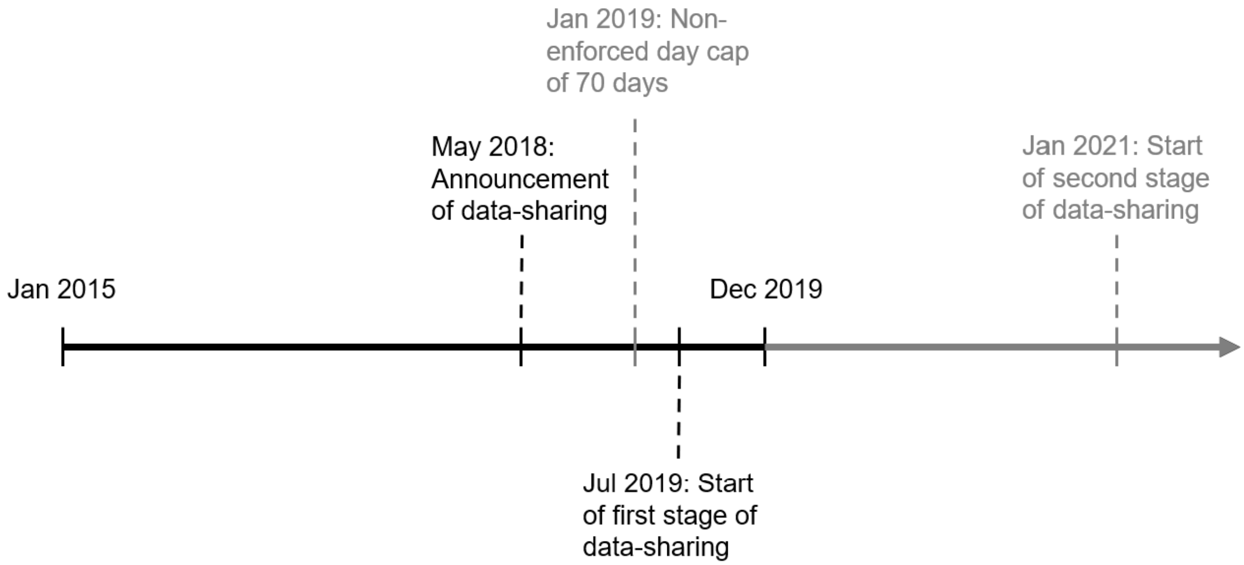
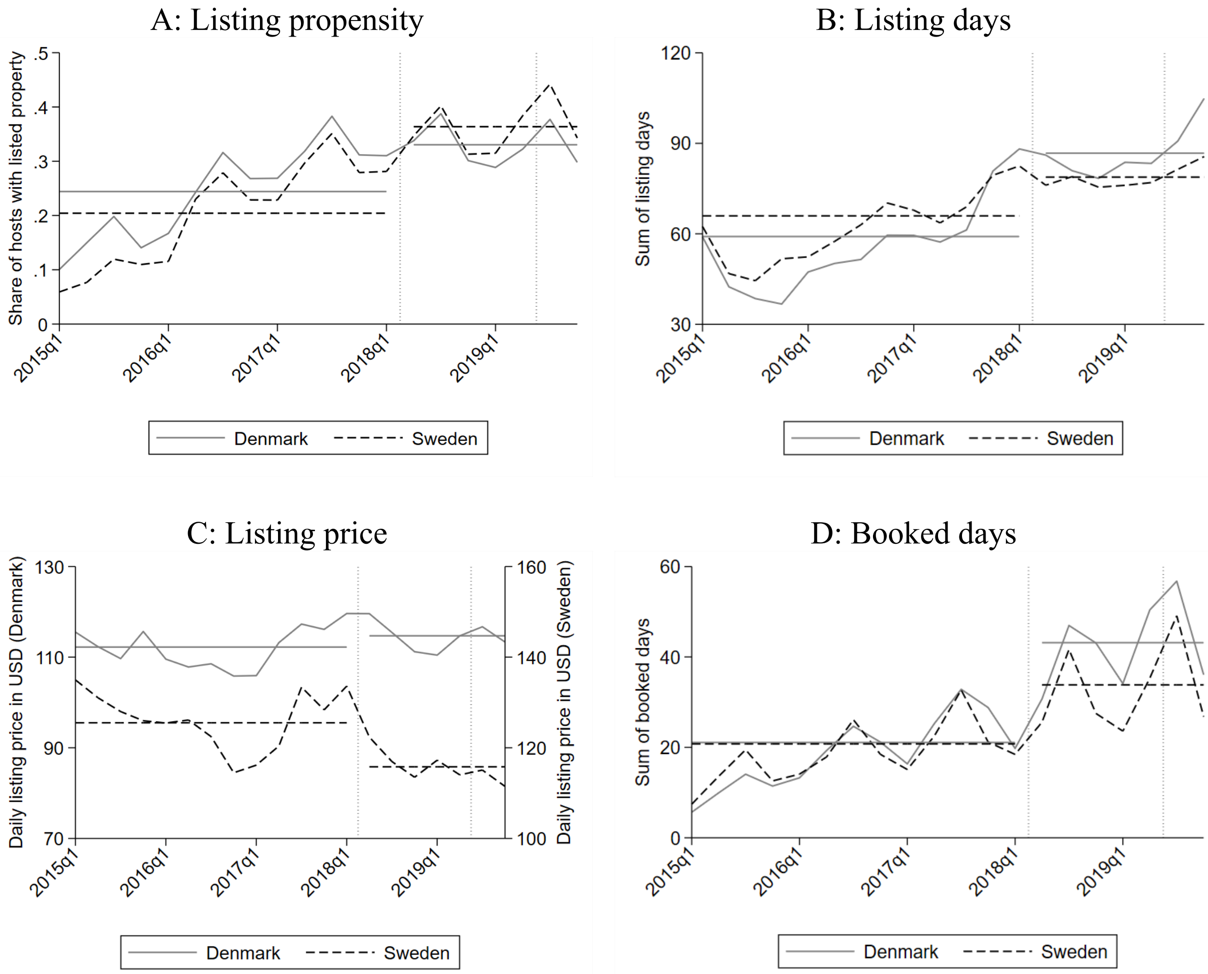
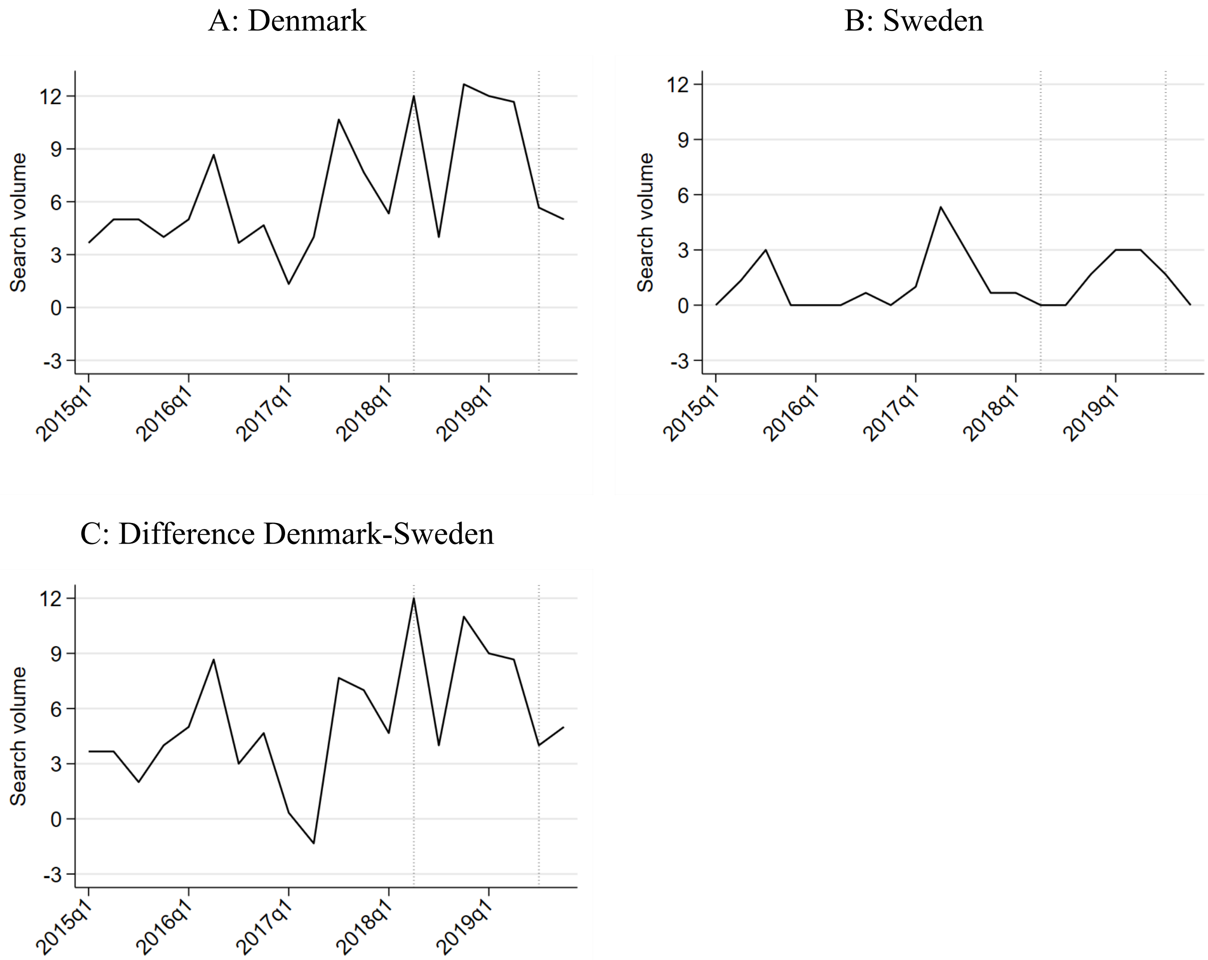


Figure 2: Trends in outcome variables in Denmark and Sweden



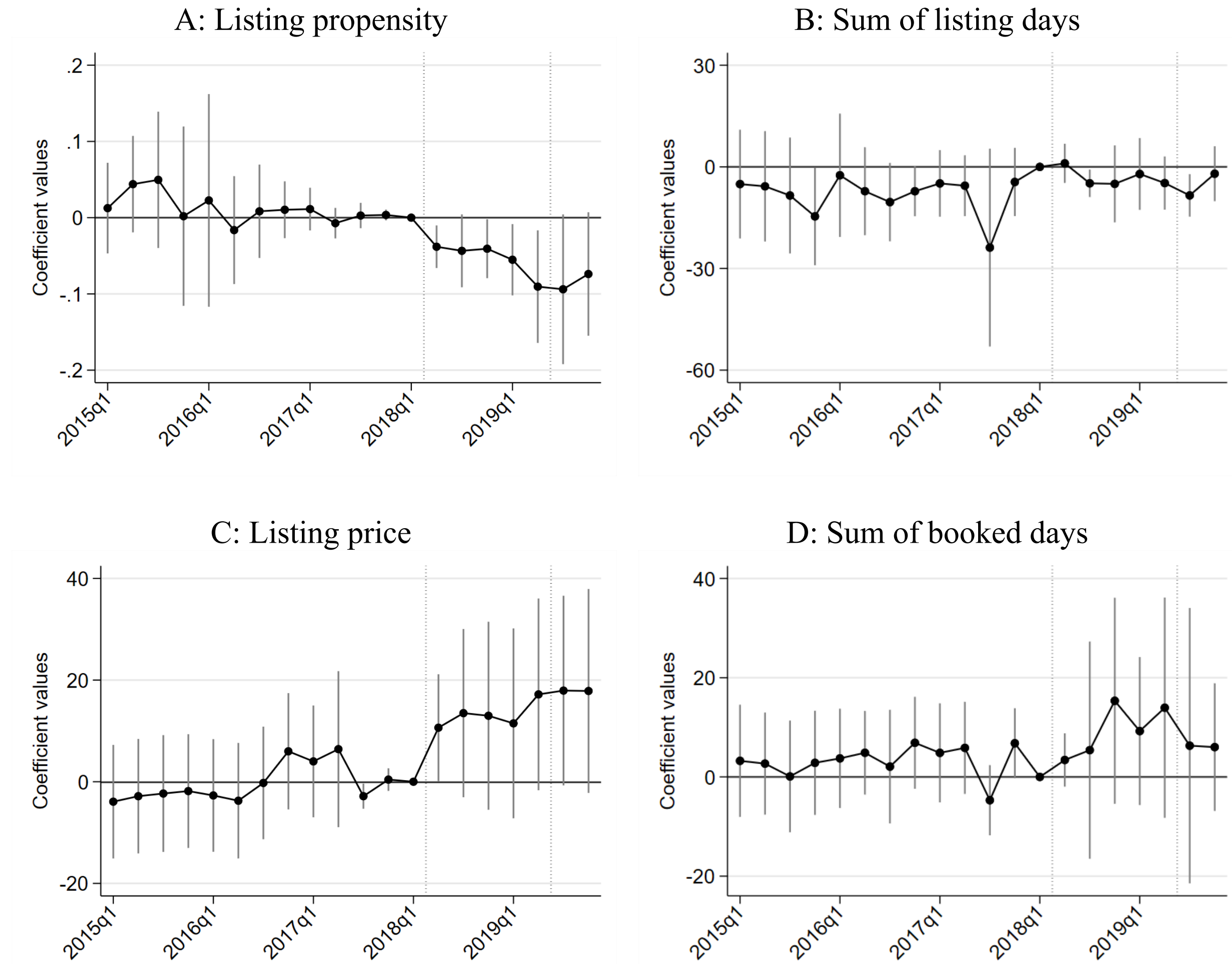
Notes: The dotted vertical lines denote the signing of the data-sharing agreement between Airbnb and Denmark and its implementation, respectively. The horizontal lines are country-specific sample means before and after the initial treatment.

Figure 3: Google searches for Airbnb tax



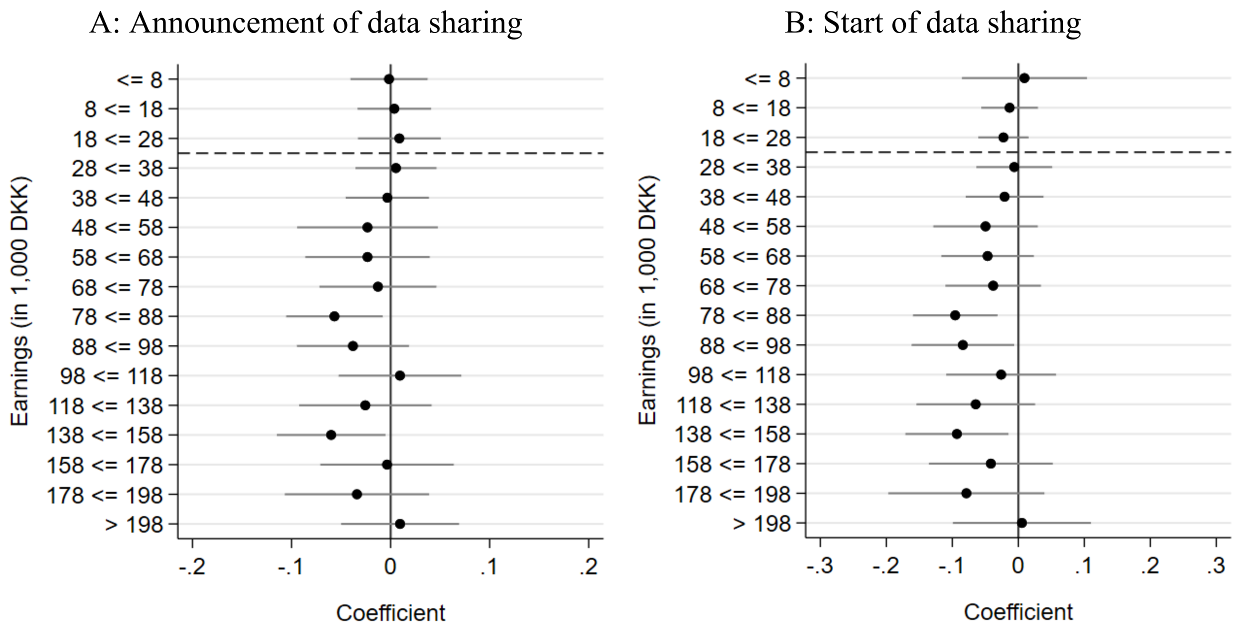
Notes: Panel A shows the volume of searches for "airbnb skat" from Denmark, while Panel B plots the volume of searches for "airbnb skatt" from Sweden. Panel C shows the difference in search volume between both countries. All data are obtained from Google Trends (all web searches) for the query period 2004-2022. The dotted vertical lines denote the signing of the data-sharing agreement between Airbnb and Denmark and its implementation, respectively.

Figure 4: Event study plots



Notes: The graphs show the coefficients from OLS regressions of the variables stated above the graphs on interactions of the Denmark dummy with time dummies, conditional on host and time fixed effects, as described in Equation (12). The reference period is the first quarter of 2018. The dotted vertical lines denote the signing of the data-sharing agreement between Airbnb and Denmark and its implementation, respectively. The grey solid spikes denote the 95% confidence interval, based on standard errors clustered by the host's primary municipality.

Figure 5: Treatment effects on listing propensity, by 2017 earnings



Notes: The figure shows regression coefficients from estimating a version of Equation (11) where we interact $DK_c \times t_q^{2018q2}$ (Panel A) and $DK_c \times t_q^{2019q3}$ (Panel B) with income class dummies. The regression uses quarterly data on all Airbnb hosts between 2015q1 and 2019q4 that listed a property for rent at least once in 2017 and includes host fixed effects, time fixed effects, a quadratic trend difference, and the constituent terms of the interactions. The spikes denote the 95% confidence interval, based on standard errors clustered by municipality.

Online Appendix

Table A1: Model comparison for treatment effects in Denmark

	(1) Linear trend difference	(2) Quadratic trend difference [change from (1) relative to effect size]	(3) Cubic trend difference [change from (2) relative to effect size]
Listing propensity			
Announcement of data sharing	-0.037	-0.028	-0.030
		[0.321]	[0.067]
Start of data sharing	-0.056	-0.038	-0.022
		[0.474]	[0.727]
Sum of listing days			
Announcement of data sharing	5.090	4.168	3.985
		[0.221]	[0.046]
Start of data sharing	2.746	0.518	0.837
		[**4.301**]	[0.381]
Listing price			
Announcement of data sharing	9.900	9.898	10.956
		[0.000]	[0.097]
Start of data sharing	12.909	12.904	11.159
		[0.000]	[0.156]
Sum of booked days			
Announcement of data sharing	5.076	1.913	3.213
		[**1.653**]	[0.405]
Start of data sharing	0.847	-6.795	-9.063
		[**1.125**]	[0.250]

Notes: The table compares the treatment effect sizes from difference-in-differences models with differential linear, quadratic, and cubic time trends as described in Equation (11). All models include host fixed effects and time fixed effects. Values in brackets show the absolute difference between coefficients – divided by the absolute treatment effect – when comparing Models (1) and (2) and Models (2) and (3), respectively. Ratios ≥ 1.000 (in bold font) indicate a difference in the magnitude of the treatment effect or more.

Table A2: Differences between municipalities, by level of Airbnb penetration

	Level of Airbnb penetration		
	Low	Medium	High
Airbnb penetration (# of listed properties per 1,000 inhabitants)	4.51	14.56	47.56
Number of properties per host	0.39	0.36	0.32
Listing propensity (binary)	0.26	0.27	0.28
Sum of listing days	23.86	20.17	14.59
Average daily listing price (USD)	102.20	117.33	123.87
Sum of booked days	8.60	7.90	7.24
Average age in municipality	41.55	40.43	36.50
Average income in municipality (USD)	41409.05	41692.78	45348.05
Population density (inhabitants per square km)	1360.07	3556.52	7329.68
Share of employed people	0.47	0.50	0.54
Share with higher educ. degree	0.10	0.20	0.23
Share with immigration background	0.16	0.24	0.24
Percent of hosts with 2 properties	2.37	2.14	1.74
Percent of hosts with 3 or more properties	1.03	0.76	0.35
Number of municipalities	261	116	11
Observations	727,419	1,222,014	1,011,847

Notes: *Low* penetration refers to the first quartile of the distribution of the average pre-treatment number of listed properties per 1,000 inhabitants. *Medium* penetration comprises municipalities in the second and third quartiles, whereas *high* penetration relates to the fourth quartile.

Table A3: Effect of data sharing on listings and bookings (conditional on year fixed effects)

	(1) Listing propensity	(2) Sum of listing days	(3) Listing price (USD)	(4) Sum of booked days
Panel A: With country-specific quadratic trend variables				
Announcement of data sharing × Denmark	-0.028***	3.990	9.635	1.851
	(0.006)	(5.155)	(5.862)	(5.292)
Start of data sharing × Denmark	-0.038**	0.111	12.089*	-6.871
	(0.016)	(10.151)	(6.341)	(7.150)
Adj. R^2	0.261	0.861	0.347	0.643
Panel B: Without country-specific trend variables				
Announcement of data sharing × Denmark	-0.065	4.986	12.581**	6.344
	(0.050)	(3.661)	(4.880)	(4.791)
Start of data sharing × Denmark	-0.095	2.668	16.944***	2.941
	(0.072)	(6.284)	(6.119)	(6.819)
Adj. R^2	0.258	0.861	0.347	0.643
Mean of dep. var.	0.269	72.694	116.381	29.820
Observations	2961280	770883	761485	770883
Number of clusters	388	388	388	388

Notes: Difference-in-differences estimates (OLS), using quarterly data on Airbnb hosts between 2015q1 and 2019q4. The sample in Column (1) includes all hosts in Denmark and Sweden that listed a property for rent at least once during the sample period. The sample in Columns (2) to (4) excludes observations where hosts did not list any property. The column headers denote the dependent variable. All models include host fixed effects and year fixed effects. Standard errors (in parentheses) are clustered by municipality.
* p<0.10, ** p<0.05, *** p<0.01

Table A4: Effect of data sharing on listings and bookings (conditional on covariates)

	(1) Listing propensity	(2) Sum of listing days	(3) Listing price (USD)	(4) Sum of booked days
Announcement of data sharing × Denmark	-0.040***	-0.275	10.991	4.660
	(0.011)	(1.796)	(8.632)	(7.145)
Start of data sharing × Denmark	-0.057***	-3.071	13.663*	-6.284
	(0.017)	(7.323)	(8.113)	(7.740)
Mean of dep. var.	0.269	72.666	116.368	29.861
Adj. R^2	0.269	0.861	0.346	0.644
Observations	2953599	768585	759187	768585
Number of clusters	386	386	386	386

Notes: Difference-in-differences estimates (OLS), using quarterly data on Airbnb hosts between 2015q1 and 2019q4. The sample in Column (1) includes all hosts in Denmark and Sweden that listed a property for rent at least once during the sample period. The sample in Columns (2) to (4) excludes observations where hosts did not list any property. The column headers denote the dependent variable. All models include host fixed effects, time fixed effects, a quadratic trend difference, and the average age, average income, population density, share of employed citizens, share of citizens with higher education degree, and share of citizens with an immigration background in the host's primary municipality, as well as the USD/DKK and USD/SEK exchange rates. Standard errors (in parentheses) are clustered by municipality.
* p<0.10, ** p<0.05, *** p<0.01

Table A5: Data sharing and value of local currencies

	(1) Exchange rate	(2) Exchange rate
Announcement of data sharing × Denmark	0.003	0.003
	(0.010)	(0.004)
Start of data sharing × Denmark	-0.000	-0.000
	(0.017)	(0.006)
Time fixed effects	No	Yes
Adj. R^2	0.943	0.994
Observations	40	40

Notes: Difference-in-differences estimates (OLS), using quarterly data at the country level. The dependent variable is the USD/DKK and USD/SEK exchange rate. All models include country fixed effects and a quadratic trend difference. Robust standard errors in parentheses.
* p<0.10, ** p<0.05, *** p<0.01

Table A6: Effect of data sharing on listing days and booked days (Poisson estimates and trimmed dependent variables)

	Poisson		Trimmed (OLS)	
	(1)	(2)	(3)	(4)
	Sum of listing days	Sum of booked days	Sum of listing days	Sum of booked days
Announcement of data sharing × Denmark	1.039	0.981	3.932	3.037
	(0.038)	(0.083)	(4.547)	(3.951)
Start of data sharing × Denmark	0.985	0.739***	1.622	-2.097
	(0.068)	(0.086)	(8.339)	(6.312)
Mean of dep. var.	72.694	30.348	70.259	28.551
Observations	770883	757475	770793	770803
Number of clusters	388	388	388	388

Notes: Difference-in-differences estimates, using quarterly data on Airbnb hosts between 2015q1 and 2019q4. The sample excludes observations where hosts did not list any property. The column headers denote the dependent variable. Columns (1) and (2) show incidence-rate ratios. The estimates in Column (3) and (4) are obtained after trimming the top 0.01% of the dependent variables. All models include host fixed effects, time fixed effects, and a quadratic trend difference. Standard errors (in parentheses) are clustered by municipality.
* p<0.10, ** p<0.05, *** p<0.01

Table A7: Effect of data sharing on the number of listed properties

	(1)	(2)	(3)	(4)
	Full sample		Sample excluding observations without listing	
	OLS	Poisson	OLS	Poisson
Announcement of data sharing × Denmark	-0.044***	0.912***	0.001	0.996
	(0.010)	(0.030)	(0.031)	(0.015)
Start of data sharing × Denmark	-0.055**	0.851***	-0.053	0.955
	0.025	(0.050)	(0.062)	(0.029)
Mean of dep. var.	0.353	0.353	1.324	1.324
Adj. R^2	0.372		0.947	
Observations	2961280	2961280	770883	770883
Number of clusters	388	388	388	388

Notes: Difference-in-differences estimates, using quarterly data on Airbnb hosts between 2015q1 and 2019q4. The sample in Columns (1) and (2) includes all hosts in Denmark and Sweden that listed a property for rent at least once during the sample period. The sample in Columns (3) to (4) excludes observations where hosts did not list any property. The dependent variable is number of listed properties. Columns (1) and (3) show OLS coefficients. The estimates in Columns (2) and (4) are incidence-rate ratios. All models include host fixed effects, time fixed effects, and a quadratic trend difference. Standard errors (in parentheses) are clustered by municipality.
* p<0.10, ** p<0.05, *** p<0.01

Table A8: Effect of data sharing on listings and bookings in Denmark when modeling demand spillovers to Sweden

	(1) Listing propensity	(2) Sum of listing days	(3) Listing price (USD)	(4) Sum of booked days
Announcement of data sharing × Denmark	-0.028***	4.164	9.899*	1.908
	(0.006)	(4.944)	(5.914)	(4.986)
Start of data sharing × Denmark	-0.038**	0.523	12.903**	-6.802
	(0.016)	(9.533)	(6.521)	(6.699)
Distance of hosts in Sweden to Danish border (bins)				
0 < 200 km	0.034	-3.947	14.486	-3.894
	(0.052)	(13.547)	(13.009)	(6.506)
200 < 400 km	0.046	-2.067	8.710	15.892
	(0.062)	(28.993)	(12.595)	(15.421)
400 < 600 km	-0.008	-26.560**	18.550	-2.637
	(0.059)	(12.221)	(12.757)	(5.804)
600 < 800 km	0.094	-28.927	27.548*	-35.525
	(0.072)	(25.226)	(14.049)	(28.122)
Mean of dep. var.	0.269	72.694	116.381	29.820
Adj. R^2	0.266	0.861	0.347	0.644
Observations	2961280	770883	761485	770883
Number of clusters	388	388	388	388

Notes: Difference-in-differences estimates (OLS), using quarterly data on Airbnb hosts between 2015q1 and 2019q4. The sample in Column (1) includes all hosts in Denmark and Sweden that listed a property for rent at least once during the sample period. The sample in Columns (2) to (4) excludes observations where hosts did not list any property. The column headers denote the dependent variable. All models include host fixed effects, time fixed effects, and a quadratic trend difference. Standard errors (in parentheses) are clustered by municipality.
* p<0.10, ** p<0.05, *** p<0.01

Table A9: Evaluation of compliance with day cap in Denmark

	(1) Property with > 70 booking days p.a. [yes/no] (OLS)	(2) Number of properties with > 70 booking days p.a. (OLS)	(3) Number of properties with > 70 booking days p.a. (Poisson)
Start of day cap × Denmark	-0.010***	0.036	0.981
	(0.003)	(0.096)	(0.039)
Mean of dep. var.	0.083	1.380	1.380
Adj. R^2	0.334	0.852	
Observations	2961280	246069	246069
Number of clusters	388	384	384

Notes: Difference-in-differences estimates, using quarterly data on Airbnb hosts between 2015q1 and 2019q4. The sample in Column (1) includes all hosts in Denmark and Sweden that listed a property for rent at least once during the sample period. The sample in Columns (2) and (3) only includes hosts with property with more than 70 booking days per year. The column headers denote the dependent variable and estimation method. Columns (1) and (2) show OLS coefficients. The estimates in Column (3) are incidence-rate ratios. All models include host fixed effects, time fixed effects, and a quadratic trend difference. Standard errors (in parentheses) are clustered by municipality.
* p<0.10, ** p<0.05, *** p<0.01

Table A10: Effect of data sharing on listings and bookings, only hosts in low-penetration areas

	(1) Listing propensity	(2) Sum of listing days	(3) Listing price (USD)	(4) Sum of booked days
Announcement of data sharing × Denmark	-0.016*	9.802**	27.670	8.909*
	(0.010)	(3.885)	(22.772)	(4.758)
Start of data sharing × Denmark	-0.029*	13.275*	30.403	-3.147
	(0.015)	(7.631)	(21.997)	(5.684)
Mean of dep. var.	0.259	94.492	102.314	34.151
Adj. R^2	0.320	0.861	0.254	0.649
Observations	727257	181241	180834	181241
Number of clusters	261	261	261	261

Notes: Difference-in-differences estimates (OLS), using quarterly data on Airbnb hosts between 2015q1 and 2019q4. The sample in Column (1) includes all hosts in low Airbnb penetration areas in Denmark and Sweden that listed a property for rent at least once during the sample period. The sample in Columns (2) to (4) excludes observations where hosts did not list any property. Low Airbnb penetration areas are those in the first quartile of the average pre-treatment number of listed properties per municipality, relative to the population size. The column headers denote the dependent variable. Standard errors (in parentheses) are clustered by municipality.
* p<0.10, ** p<0.05, *** p<0.01

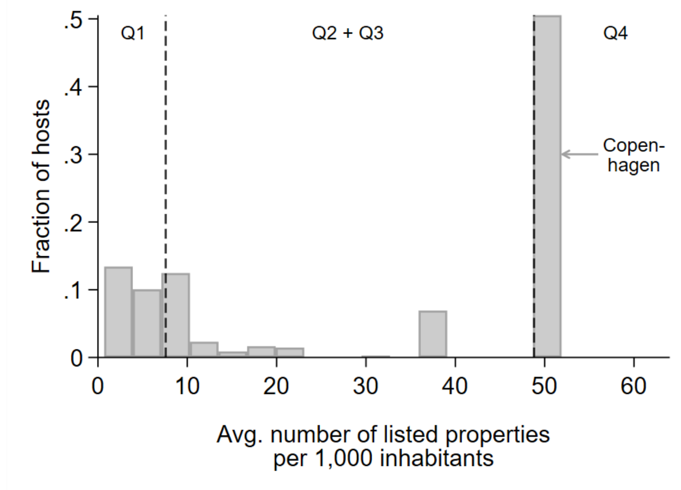
Table A11: Effect of data sharing on entry and exit probabilities

	(1) Entry (yes/no)	(2) Exit (yes/no)
Announcement of data sharing × Denmark	-0.010*	0.013***
	(0.006)	(0.005)
Start of data sharing × Denmark	0.009	0.013
	(0.008)	(0.012)
Mean of dep. var.	0.070	0.059
Adj. R^2	-0.009	0.015
Observations	2961280	2961280
Number of clusters	388	388

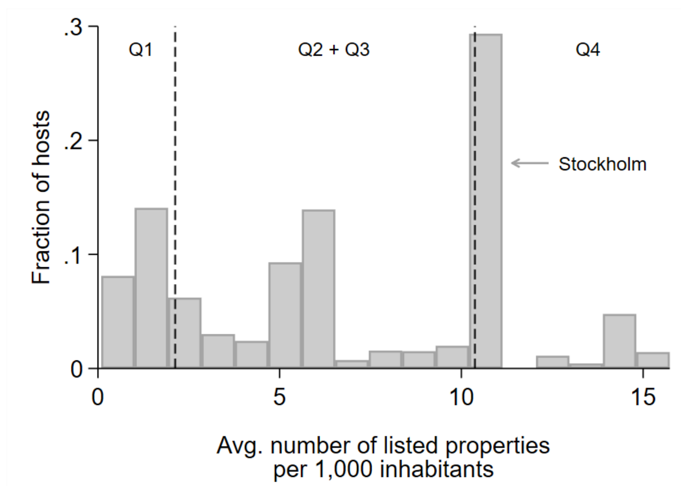
Notes: Difference-in-differences estimates (OLS), using quarterly data on all Airbnb hosts in Denmark and Sweden that listed a property for rent at least once during the sample period. The column headers denote the dependent variable. *Entry* is a binary variable that takes the value 1 if a host lists a property in the current quarter but not in the previous quarter. Similarly, *Exit* takes the value 1 if a host does not list a property in the current quarter but listed a property in the previous quarter. All models include host fixed effects, time fixed effects, and a quadratic trend difference. Standard errors (in parentheses) are clustered by municipality.
* p<0.10, ** p<0.05, *** p<0.01

Figure A1: Pre-treatment levels of local Airbnb penetration

A: Denmark

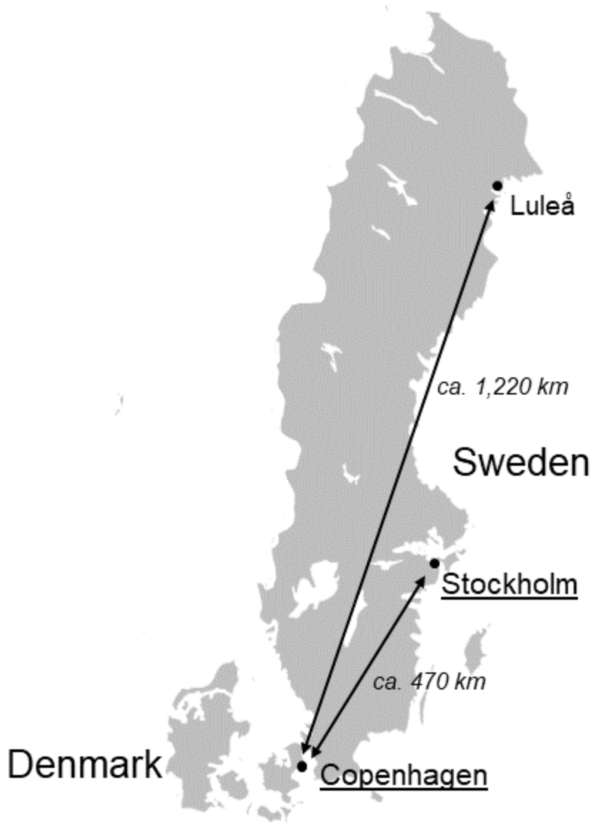


B: Sweden



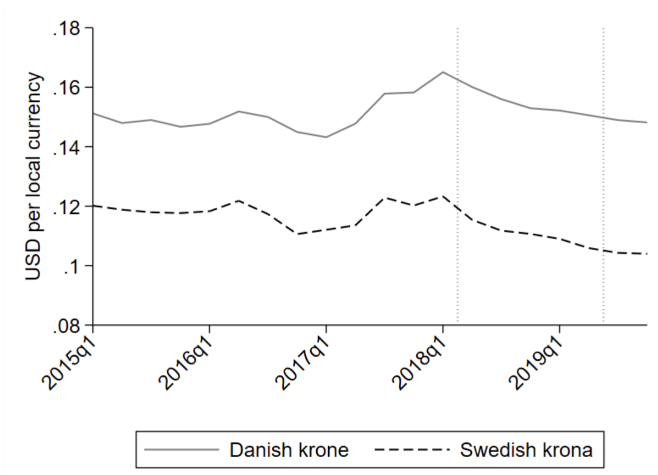
Notes: Airbnb penetration is measured as the number of listed properties per 1,000 inhabitants in a host's primary municipality.

Figure A2: Map of Denmark and Sweden



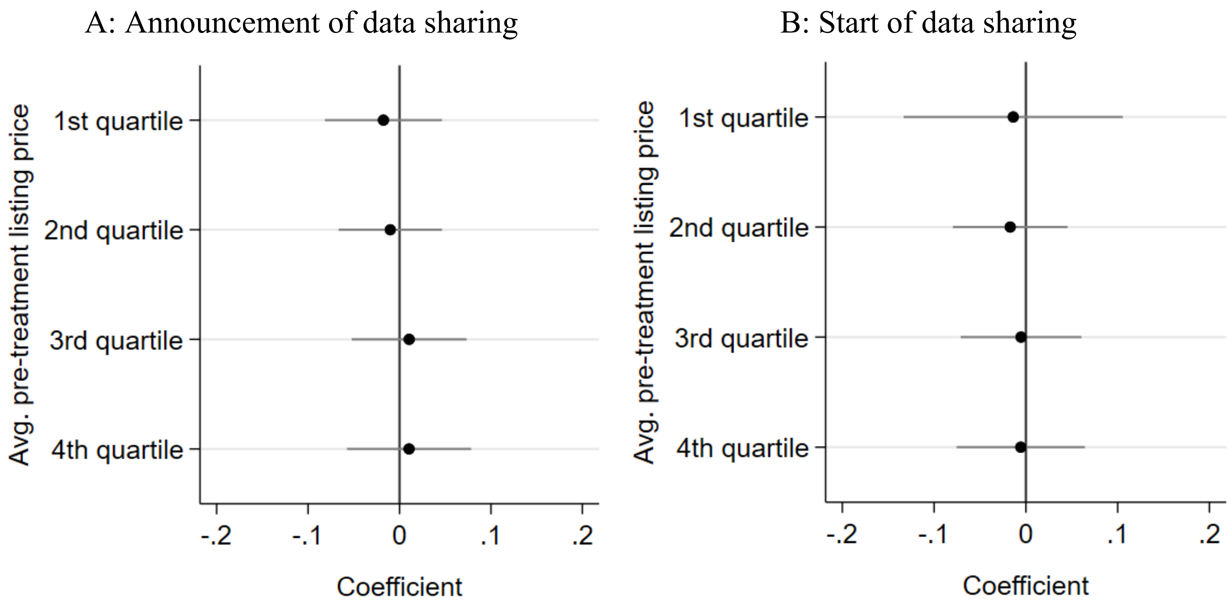
Notes: The distances are beelines.

Figure A3: Development of local currencies



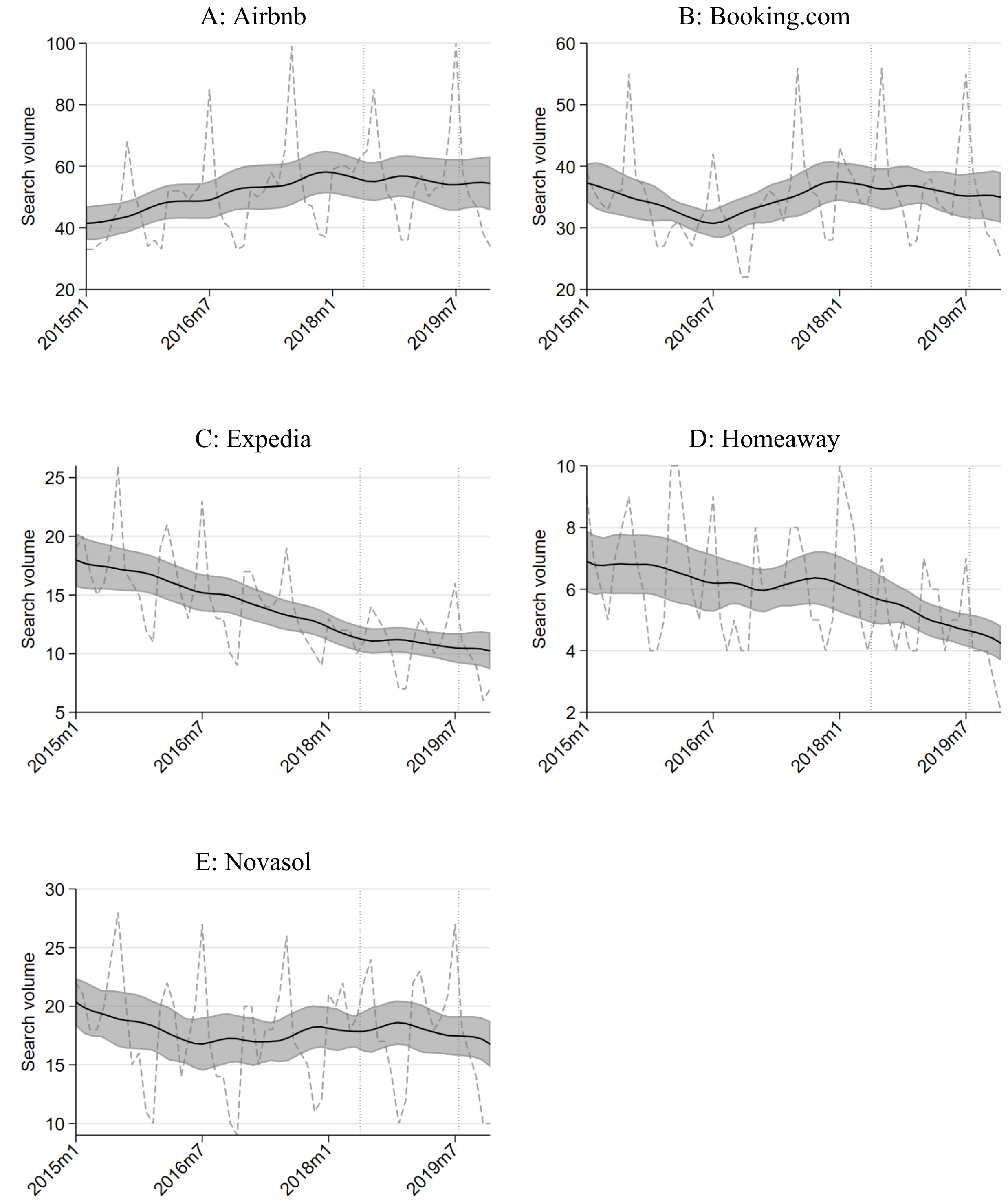
Notes: The dotted vertical lines denote the signing of the data-sharing agreement between Airbnb and Denmark and its implementation, respectively.

Figure A4: Treatment effects on listing propensity, by pre-treatment listing price



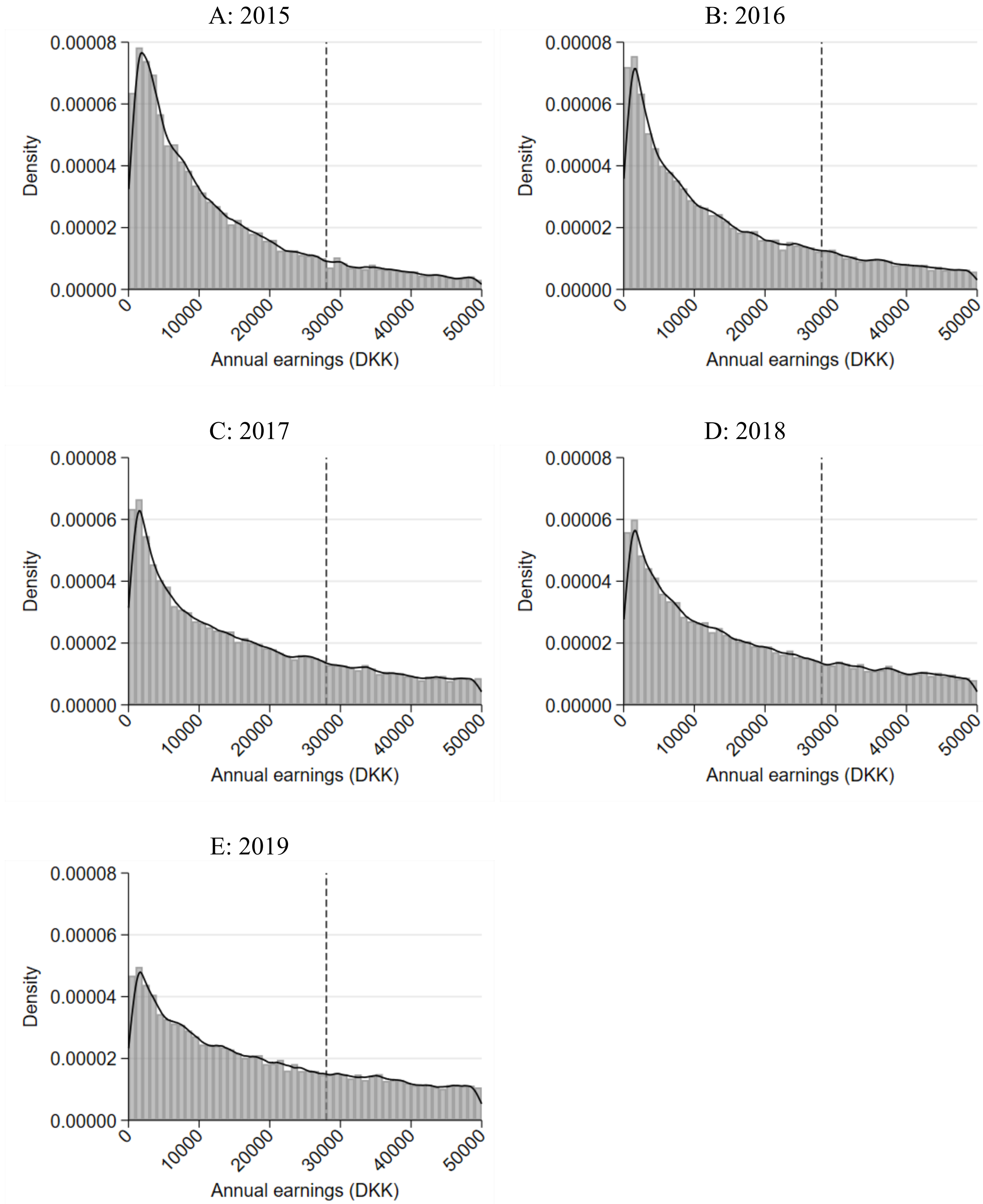
Notes: The figure shows regression coefficients from estimating a version of Equation (11) where we interact $DK_c \times t_q^{2018q2}$ (Panel A) and $DK_c \times t_q^{2019q3}$ (Panel B) with dummies for quartiles of hosts' average pre-treatment listing price. The regression uses quarterly data on all Airbnb hosts between 2015q1 and 2019q4 that listed a property for rent at least once during the investigation period and includes host fixed effects, time fixed effects, a quadratic trend difference, and the constituent terms of the interactions. The spikes denote the 95% confidence interval, based on standard errors clustered by municipality.

Figure A5: Google searches in Denmark for Airbnb and relevant alternative providers



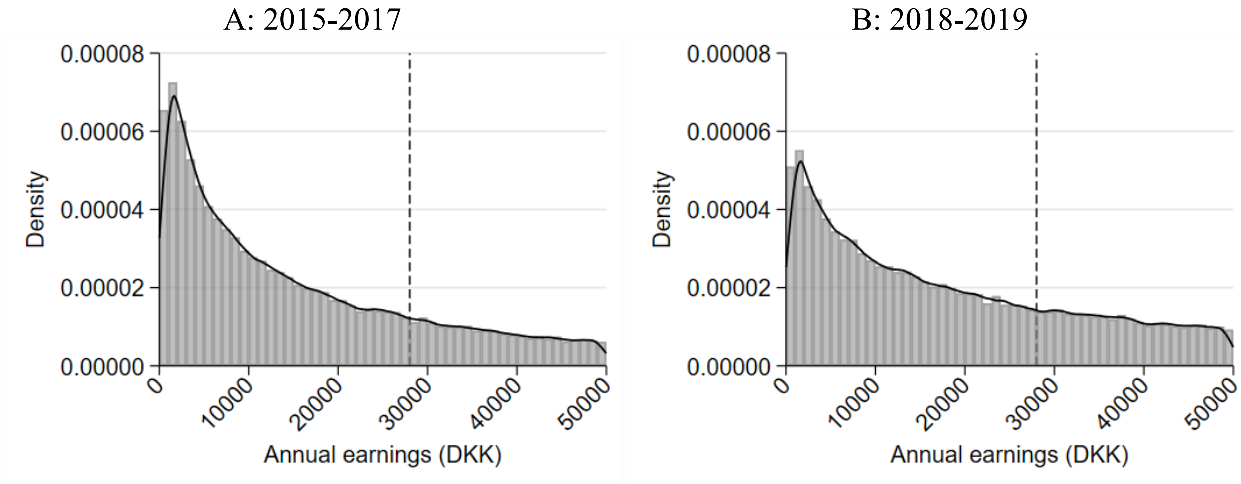
Notes: The figure shows data from Google Trends (all web searches, query period 2004-2022) for the companies stated above the panels. The data shown in the individual panels are on the same scale, enabling comparison of the relative interest in one company to others. The dashed line shows the raw data, whereas the solid black line is a polynomial fit with 95% confidence interval. The dotted vertical lines denote the signing of the data-sharing agreement between Airbnb and Denmark and its implementation, respectively.

Figure A6: Distribution of annual earnings on Airbnb Denmark, by year



Notes: The dotted vertical line denotes the 28,000 DKK income threshold.

Figure A7: Distribution of annual earnings on Airbnb Denmark, before and after the treatment



Notes: The dotted vertical line denotes the 28,000 DKK income threshold.