Taxation of short-term rentals:
Evidence from the introduction of the "Airbnb tax" in Norway

Marcel Garz, Andrea Schneider
Jönköping International Business School

Abstract

This research note investigates the impact of a rental-income tax on hosts using Airbnb in Norway. We find that the cost increase implied by the tax did not induce hosts to exit the platform, nor did it lead to an increase in rental prices. These findings support the conjecture that the tax was insufficiently enforced, as it relied on taxpayers to self-report their rental income.

Corresponding author: Marcel Garz, Jönköping International Business School, P.O. Box 1026, 551 11 Jönköping, Sweden. Email: marcel.garz@ju.se. Declarations of interest: none. We gratefully acknowledge funding by the Jan-Wallander-och-Tom-Hedelius Foundation.

1. Introduction

Home-sharing platforms have substantially grown over the past decade. For instance, the global market leader, Airbnb, managed to multiply the number of guests finding accommodation through its platform from 50 million in 2015 to 800 million in 2022 (Airbnb, 2022). Airbnb's popularity has particularly increased in Norway, a country where income from short-term rentals was not taxed until recently. The tax exemption has received fierce criticism from the hotel industry, as well as from residents complaining about the loss of affordable housing and disturbances by tourists.

In response to the criticism, the Norwegian government introduced a tax on short-term rentals, colloquially called "Airbnb tax" (Solberg, 2017). As of 2018, 85% of the yearly income from short-term rentals above a threshold of 10,000 NOK (ca. 1,200 USD) is taxed at a rate of 22% (Skatteetaten, 2023). The tax should theoretically result in the exit of hosts for whom the tax liability makes renting unprofitable, induce price increases as hosts share part of the tax burden with the customer, or both (e.g., Bibler et al., 2021; Garz and Schneider, 2023) – at least if hosts comply with the tax. However, taxation relied on hosts' self-reports of relevant income, which is why the Norwegian Tax Agency questioned whether the tax could be enforced (Skatteetaten, 2022).[1]

We test whether and to what extent the tax affected hosts on Airbnb. To tackle endogeneity issues, we use a difference-in-differences approach with hosts in Sweden as a control group. While Norway and Sweden share many similarities regarding social norms, tourism, and housing markets, short-term rental regulations in Sweden remained unchanged, which makes the country an ideal counterfactual. Using individual-level data on the population of hosts between 2015 and 2019, we do not find evidence that hosts in Norway exited Airbnb or increased their prices. The confidence intervals of these null effects allow us to rule out economically meaningful effects, which supports the conjecture of poor tax enforcement.

We contribute to the literature on the regulation of short-term rentals (e.g., Koster et al., 2021; Valentin, 2021; Chen et al., 2022; Gauss et al., 2022). Our evidence is novel in that it refers to a situation where taxes are not collected by the platform (Bibler et al., 2021) nor enforced via data sharing (Garz and Schneider, 2023), showing that self-reporting by taxpayers might not be an optimal design choice when it comes to digital platforms.

[1] As of 2020, platforms are required to report hosts' income directly to the Tax Administration (Skatteetaten, 2022). We cannot investigate this policy change due to the lack of a valid control group after the outbreak of the COVID-19 pandemic.

2. Data

We use daily information about Airbnb hosts, rentals, and prices provided by AirDNA. The data cover all 111,238 hosts from Norway and Sweden between 2015 and 2019. For the analysis, we collapse the data to a panel of quarterly observations on individual hosts, which are nested in 641 municipalities.

We calculate the *number of properties* per host listed in a given quarter. Considering that many hosts own just one property, we also construct a binary variable (*listing propensity*) that takes the value 1 if any property is listed and 0 if not. The variable *listing price* captures the average fee (in USD) per night requested by the host. For hosts offering more than one property, we take the average over all listed objects. Finally, we create a binary variable (*above income threshold*) taking the value 1 for hosts earning more than 10,000 NOK / 1,200 USD in 2016, the last income year before the announcement of the tax.

Table 1: Summary statistics

	Norway		Sweden				
	Mean	SD	Mean	SD	Min.	Max.	Pre-treatment difference (*p*-value)
Number of properties	0.34	1.69	0.33	2.37	0.00	805.00	0.804
Listing propensity (binary)	0.26	0.44	0.26	0.44	0.00	1.00	0.545
Average daily listing price (USD)	119.31	158.94	120.70	376.63	0.00	105453.39	0.477
Above inc. thresh. in 2016 (binary)	0.19	0.39	0.23	0.42	0.00	1.00	0.175
Number of observations		1,091,360		1,133,400			
Number of quarters		20		20			
Number of hosts		54,568		56,670			
Number of municipalities		351		290			

Notes: The rightmost column reports *p*-values (adjusted for clustering by municipality) for the null hypothesis of no differences in means in the pre-treatment period (2015q1 – 2017q3). The number of observations is lower for the listing price because this variable is not observed for inactive hosts.

3. Estimation

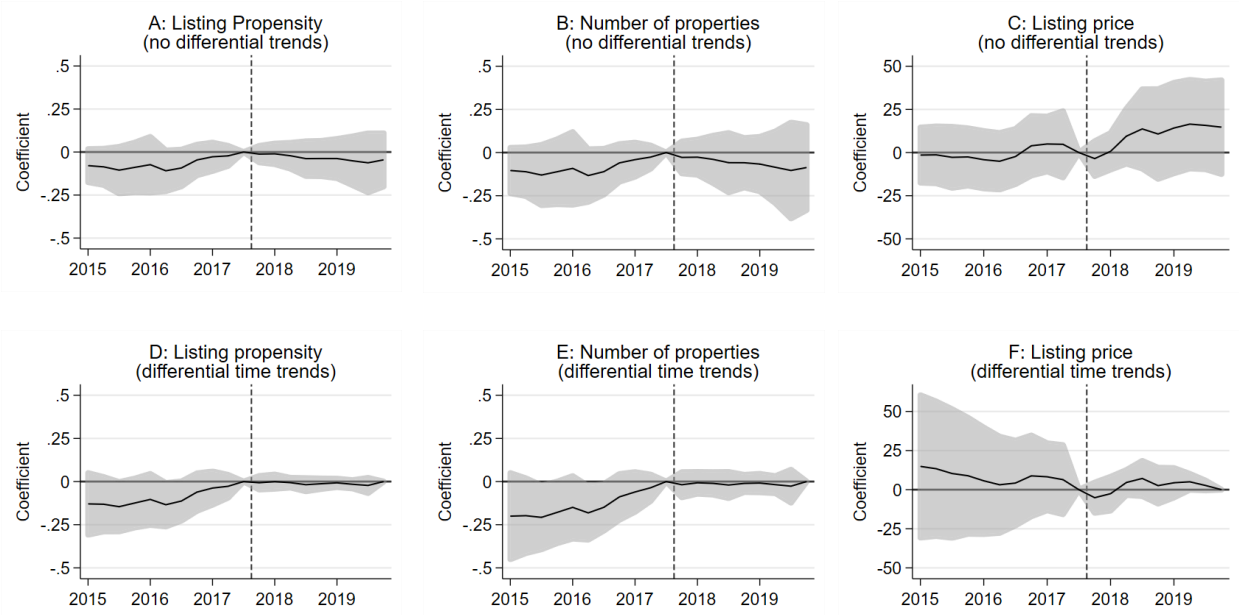
We first test for pre-existing trends by regressing outcome variables y on interactions between an NO dummy (1 for hosts in Norway, 0 for hosts in Sweden) and lags l and leads f of the treatment date:

$$y_{i,m,q,c} = \sum_{j=-l}^{f} \beta_j \rho_{q+j} \times NO_c + \mu_i + \rho_q + \theta NO_c \times q + \varepsilon_{i,m,q,c} \quad (1)$$

where observations refer to host i in municipality m and country c during quarter q. We include host fixed effects μ_i and the full set of time effects ρ_q. Equation (1) optionally includes differential linear time trends

($NO_c \times q$) to account for the possibility of diverging developments unrelated to the tax. To capture possible anticipation effects, we consider the governmental announcement of the tax in 2017q4 as the treatment date (Solberg, 2017), rather than its implementation in 2018q1. We cluster the standard errors by municipality, which accounts for error correlation within repeated observations on hosts as well as municipalities.

Figure 1: Event study plots



Notes: The figure shows estimates of fictional treatment dates on the variables stated above the graphs (see Equation 1). The leave-out period is 2017q3. The shaded area denotes the 99% confidence interval.

We plot the values of the β_j's in Figure 1. According to the figure, there are no major pre-trends, which supports the claim that hosts in Sweden constitute a valid control group. To formally evaluate the treatment, we estimate the following two-way fixed effects specification:

$$y_{i,m,q,c} = \alpha_1 NO_c \times after_q^{2017q3} + \mu_i + \rho_q + \theta NO_c \times q + \varepsilon_{i,m,q,c} \quad (2)$$

where the binary variable $after^{2017q3}$ equals 1 once the tax was announced and 0 before. The coefficient α_1 on the interaction $NO_c \times after_q^{2017q3}$ measures the treatment effect on hosts in Norway relative to Sweden. We do not observe significant differences in pre-treatment means of the outcome variables (Table 1), which is why we neither include control variables nor use any matching procedure.

4. Results

As Table 2 indicates, specifications without differential time trends (Columns 1, 3, and 5) produce noisier estimates than specifications with trends (Columns 2, 4, and 6). For that reason, and because the trend variables help to reduce potential estimation bias due to minor violations of parallel trends, we focus on the specifications with differential trends when interpreting the results.

Table 2: Effects of the short-term rental tax

	(1)	(2)	(3)	(4)	(5)	(6)
	Listing propensity		Number of properties		Listing price (USD)	
Panel A: All hosts						
Norway × after	0.03	-0.00	0.02	-0.02	7.82***	-3.34
	[-0.08,0.14]	[-0.02,0.02]	[-0.16,0.20]	[-0.07,0.04]	[2.53,13.10]	[-11.66,4.98]
Differential trend	No	Yes	No	Yes	No	Yes
Mean of dep. var.	0.26	0.26	0.33	0.33	120.04	120.04
Adj. R^2	0.28	0.28	0.32	0.32	0.35	0.35
Observations	2224760	2224760	2224760	2224760	555361	555361
Number of clusters	641	641	641	641	641	641
Panel B: Only hosts above the 10,000 NOK income threshold in 2016						
Norway × after	0.05	-0.04	0.07	-0.09	8.94***	-0.63
	[-0.10,0.19]	[-0.10,0.01]	[-0.13,0.27]	[-0.19,0.02]	[5.37,12.51]	[-4.38,3.12]
Differential trend	No	Yes	No	Yes	No	Yes
Mean of dep. var.	0.46	0.46	0.60	0.60	121.86	121.86
Adj. R^2	0.40	0.40	0.39	0.39	0.66	0.66
Observations	467280	467280	467280	467280	213548	213548
Number of clusters	592	592	592	592	592	592

Notes: Difference-in-differences estimates (OLS) using quarterly data on Airbnb hosts between 2015q1 and 2019q4. In Panel A, the sample in Columns (1) to (4) includes all hosts in Norway and Sweden that listed a property for rent at least once during the sample period. The sample in Columns (5) to (6) excludes observations where hosts did not list any property. All models include host and time fixed effects. Values in brackets denote the 95% confidence interval, based on standard errors clustered by municipality.
* p<0.10, ** p<0.05, *** p<0.01

Results for all hosts are presented in Panel A. We do not find a significant effect of the tax on hosts' propensity to list property on Airbnb, with point estimates close to zero. According to the 95% confidence

interval in Column (2), we can rule out changes larger than +/- 2 percentage points.[2] Hosts did not significantly change their number of listed properties either (Columns 3 and 4). The specification without differential trends in Column (5) indicates a significant price increase but this effect disappears once we control for trend differences (Column 6).[3] Here, the confidence intervals indicate a lower bound of -11.66 USD (ca. 9.7% of the mean price) and an upper bound of 4.98 USD (ca. 4.1% of the mean price). Hence, any potential price increase was small at best, compared to the tax rate of 22%. As Panel B of Table 2 shows, we do not find any significant changes when we restrict the analysis to hosts that realized earnings above the tax-free threshold in the last income year before the tax was announced (in the specifications with differential trends). The confidence intervals are again reasonably narrow to rule out economically sizable effects.

5. Conclusion

Back-on-the-envelope calculations indicate that hosts on average earned 5,131 USD per year during 2018/19, which implies an average yearly tax liability of 735 USD. The size of this liability should arguably translate into a cost large enough to expect that renting becomes unprofitable for a noticeable fraction of hosts or that part of the cost is passed to the customer. However, our results imply that hosts neither left Airbnb nor increased their prices in response to the tax, with confidence intervals that allow us to rule out economically meaningful effect sizes. The findings support the conjecture of poor tax enforcement, as feared by the Norwegian Tax Administration, and emphasize the need for effective tax designs in the context of digital platforms.

[2] Accounting for clustering by municipality yields the most conservative confidence intervals. That is, the intervals would be smaller when clustering the standard errors by host or not clustering at all (results available on request).
[3] Hence, the estimate in Column (5) is likely biased due to minor pre-trends.

References

Airbnb (2022). Airbnb Annual Report 2022. February 25, 2022.

Bibler, A. J., Teltser, K. F., & Tremblay, M. J. (2021). Inferring Tax Compliance from Pass-Through: Evidence from Airbnb Tax Enforcement Agreements. *Review of Economics and Statistics*, 103, 636–651.

Chen, W., Wei, Z., & Xie K. (2022). The Battle for Homes: How Does Home Sharing Disrupt Local Residential Markets? *Management Science*, 68, 8515–9218.

Garz, M., & Schneider, A. (2023). Data sharing and tax enforcement: Evidence from short-term rentals in Denmark. Working Paper.

Gauss, P., Gensler, S., Kortenhaus, M., Riedel, N. & Schneider, A. (2022). Regulating the Sharing Economy: The Impact of Home-Sharing Ordinances on Commercial Airbnb Activity. Working Paper.

Koster, H. R. A., van Ommeren, J. & Volkhausen, N. (2021). Short-term rentals and the housing market: Quasi-experimental evidence from Airbnb in Los Angeles. *Journal of Urban Economics*, 124, 103356.

Skatteetaten (2022). Rapporteringsplikt for Airbnb og andre utleiere synliggjør milliardinntekter. Norwegian Tax Administration. Press release, April 29, 2022.

Skatteetaten (2023). Tax rules for short-term letting of homes and holiday homes. Norwegian Tax Administration. URL: https://www.skatteetaten.no/en/person/taxes/get-the-taxes-right/property-and-belongings/houses-property-and-plots-of-land/letting-of-houses-and-property/short-term-letting-of-dwellings-and-holiday-homes/tax-rules-for-short-term-letting-of-homes-and-holiday-homes/, last accessed: January 1, 2023.

Solberg, S. M. (2017). Dette betyr statsbudsjettet for din lommebok. Nettavisen, Nettavisen, October 12, 2017. URL: https://www.nettavisen.no/na24/dette-betyr-statsbudsjettet-for-din-lommebok/s/12-95-3423380193

Valentin, M. (2021). Regulating Short-term Rental Housing: Evidence from New Orleans. *Real Estate Economics*, *49*, 152–186.