Using a natural experiment in the taxicab industry to analyze the effects of third-party income reporting ☆

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A R T I C L E   I N F O

A B S T R A C T

This paper uses confidential tax returns data from sole proprietor businesses to estimate behavioral responses to the introduction of Form 1099-K, a third-party income reporting law that requires credit and debit card companies to report to the Internal Revenue Service gross of payment transactions that businesses receive through their payment systems. We estimate the causal impact of Form 1099-K on business reporting by exploiting a natural experiment in which some cities in the U.S. passed ordinances requiring taxicabs to install credit card readers in their vehicles, while other cities did not pass such ordinances, creating plausibly exogenous variation in the share of receipts reported on Form 1099-K. We find that taxpayers respond to third-party information reporting in offsetting ways. In particular, we find that businesses from cities with mandatory credit card in taxicab ordinances reported more *receipts* after the introduction of Form 1099-K compared to similar businesses from cities without mandatory credit card in taxicab ordinances, but they also reported an increase in *expenses* of similar magnitude. On net, third-party information reporting led to small and statistically insignificant changes in *taxable income, profit,* and *tax liability*. These results are robust to a variety of alternative specifications and placebo tests.

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1. Introduction

According to Internal Revenue Service (IRS) estimates, the tax gap—the difference between what taxpayers should pay if they fully complied with the tax laws and what they actually pay in a timely manner—averaged over $450 billion annually for the period 2011-2013, with an associated voluntary compliance rate of 83.6 percent (IRS, 2019).[1] The overall voluntary compliance rate of 83.6 percent, however, masks substantial heterogeneity in compliance for different types of income. While over 99 percent of the wage and salary income and 83 percent of the partnership, capital gains, and alimony income is correctly reported to the IRS, only 45 percent of the farm income, rents and royalties, and nonfarm proprietor income is

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[1] The voluntary compliance rate is defined as the amount of taxes paid voluntarily and timely relative to total taxes legally due (or "total true tax"), expressed as a percentage.

correctly reported.[2] Indeed, there is emerging evidence that attributes most of the differences in the voluntary compliance rate across different sources of income to the presence – or the absence – of third-party information reporting, in which third-party sources such as employers and banks report taxable income earned by individuals directly to the tax administration (Kopczuk and Slemrod, 2006; Kleven et al., 2011; Phillips, 2014; Pomeranz, 2015).[3] Because third-party information reporting allows the tax administration to verify reported tax liability against income sources easily, information reporting is widely perceived as a policy tool to improve voluntary tax compliance.

The belief that third-party income reporting is effective in increasing voluntary tax compliance has led the U.S. Congress to expand the scope of information reporting over time to cover more sources of income, such as wage and salary (via Form W-2), interest (1099-INT), investment (1099-DIV), and non-employee incomes (1099-MISC). However, subjecting business income to information reporting is challenging because businesses earn income by transacting with thousands, even millions, of consumers, and there is no easy way to use consumers as third-party reporters.[4] To address the lack of third-party reporting of business income, Congress introduced a change in Internal Revenue Code § 6050W as part of the Housing and Economic Recovery Act of 2008. This change requires processors of credit and debit cards such as American Express, electronic payment systems such as PayPal, and online platforms such as Uber to report to the IRS the gross receipts of businesses accepting these forms of payment. A new information report called Form 1099-K, *Payment Card and Third-party Network Transactions*, was introduced to facilitate this reporting.[5]

Third-party reporting like Form 1099-K is likely to both immediately improve voluntary compliance and become more effective over time, given the rising trend towards the use of electronic payment methods. Another advantage of Form 1099-K is that it likely has low compliance costs, especially compared to a Value-added Tax (VAT), since it relies on a small number of large financial firms for information reporting. Thus, Form 1099-K can be useful even in countries with a VAT as the typical VAT excludes almost all small and medium firms due to its high compliance costs (Keen and Mintz, 2004). However, there are two significant omissions in the information reported on Form 1099-K that may reduce its compliance impact: income received in cash is not reported on Form 1099-K, and expenses are still not reported by third parties. These two limitations leave room for continued non-compliance through under-reporting of cash income or over-reporting of expenses. Thus, the actual impact of Form 1099-K on voluntary tax compliance is an empirical question.

However, estimating the causal impact of Form 1099-K is more challenging than it might appear. The implementation of Form 1099-K was not randomized, and, as a federal law, it affects all businesses in all states.[6] It is, therefore, not immediately apparent how one can determine control and treatment groups to identify the impact of Form 1099-K on voluntary compliance.

In this paper, we evaluate the impact of the introduction of Form 1099-K on voluntary compliance. To do so, we take advantage of a unique natural experiment in one important sector of local economies, the taxicab industry. Since 2000, more than 40 cities (or counties) have passed ordinances mandating that taxicabs install credit card readers in their vehicles. Taxi services operating in cities that require credit card readers will be affected more by Form 1099-K than taxi services operating in cities that do not have such ordinances since the presence of credit card readers discontinuously increases the receipts from credit cards that are reported on Form 1099-K. We are therefore able to estimate the causal impact of Form-1099-K on tax compliance by exploiting the natural experiment created by the interaction of the introduction of Form 1099-K in 2011 and the introduction at various times of local ordinances requiring taxicabs to install credit card readers across various U.S. cities.

We hand-collect the date of credit card in taxicab ordinance implementation from various legal databases, local newspapers, and government resources, and combine this information with confidential IRS administrative tax return data for the taxicab industry extracted from Form 1040 (U.S. Individual Income Tax Return), Schedule C (Profit or Loss from Business - Sole Proprietorship), and Form 1099-K (Payment Card and Third-party Network Transactions). We then use a difference-in-differences (DID) research design in which we compare the trends in the receipts, expenses, taxable income, profits, and

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[2] See IRS (1996, 2006, 2012, 2016) for earlier tax gap estimates. Variation by income type in the voluntary compliance rate has typically been found in other advanced economies (Kleven et al., 2011; Kleven, 2014; Kleven et al., 2016), in developing economies (Pomeranz, 2015; Naritomi, 2016; Brockmeyer and Hernandez, 2019), and even in laboratory experiments (Alm et al., 2009).

[3] Third-party information reporting is often combined with employer source-withholding, in which employers withhold taxes on employee income (i.e., "employer source-withholding") and also send information to the tax authorities on the withheld taxes (i.e., "third-party information reporting"). However, although these practices are clearly related, they are nonetheless distinct and separate practices. Note that the voluntary compliance rates for information reporting without withholding typically range from 83 percent to 93 percent, depending on the completeness of the information reporting. See IRS (1996, 2006, 2016).

[4] Some countries, in the context of a value-added tax, have adopted policies to reward final consumers if they report sales receipt to the tax administration. Naritomi (2019) provides evidence of one such initiative in Sao Paulo, Brazil.

[5] The Form 1099-K contains following information: the gross value of transactions, the value of transactions for each month of the tax year, the gross number of payment transactions as well as any federal or state income tax withheld. Form 1099-K reporting replaced Form 1099-MISC reporting for some payments, most notably payments by a business to an independent contractor exceeding $600 and transacted through payment card transactions or third-party-settlement entities.

[6] One exception is third-party settlement organizations such as PayPal, who are required to issue a Form 1099-K to a business only after 200 transactions and after the business makes more than $20,000 in a tax year. However, payment card companies such as American Express are always required to file Form 1099-K for a business making positive revenue through their payment company. Since taxi services are required to use traditional credit card readers that do not rely on third-party settlement entities, these thresholds do not apply to taxicabs. All of their receipts via credit cards are reported in Form 1099-K.

tax liability of taxi services that operate in cities with mandatory credit card ordinances (i.e., the treated group) to the taxi services that operate in cities without mandatory credit card ordinances (i.e., the control group), before and after the treatment.

Our baseline DID results indicate that taxpayers respond to third-party information reporting. However, their responses reflect the differential reporting requirements for *receipts* (which are subject to third-party reporting requirements) versus *expenses* (which are not subject to these requirements). Firms reported an increase in *receipts* after the introduction of Form 1099-K, but they also reported an increase in *expenses*. For example, taxicabs in cities with ordinances that require credit card readers in taxicabs reported 6.7 percent more *receipts* after the treatment compared to taxicabs in cities without such ordinances, an estimate that is significant at the one percent level. However, we find even larger and statistically significant responses for *expenses*, with expenses by taxicabs in cities with credit card readers increasing by eight percent after the treatment compared to taxicabs in cities without such ordinances. Overall, the offsetting impact on expenses led to small and statistically insignificant changes in business profit, taxable income, and tax liability. Moreover, these estimates are likely to be a lower bound for the effects of Form 1099-K because some taxicabs in cities without credit card in taxicab ordinances may still have credit card readers in their vehicles and thus are affected by Form 1099-K.

We explore several potential mechanisms to understand how taxpayers respond to third-party information reporting. First, to see precisely where taxpayers increased reported expenses to offset increased receipts, we look at the 14 individual line items for expenses from Schedule C. We do not find significant increases in any of the expenses except for the specific categories of depreciation and other expenses, both of which increase by about eight percent and are significant at the one percent level. This suggests that taxpayers tend to choose cost adjustments that are more difficult to verify, which is especially true for the other expenses categories. Second, we explore whether the compliance effects are driven by the third-party reporting provided by Form 1099-K or by the electronic paper trail provided by credit card readers, and we find that an electronic paper trail by itself may not be sufficient to increase voluntary tax compliance. Finally, we examine whether taxpayers who install credit card readers in their taxicabs voluntarily have higher compliance than those who do not install credit card readers voluntarily, and we find that this is, in fact, the case.

In addition, we estimate an event study DID specification, which corroborates the baseline DID results. All the estimates for both receipts and expenses are positive and statistically significant from the year of the treatment and forward. The event study DID estimates also provide supportive evidence for the validity of our difference-in-differences research design because all estimates before the treatment are close to zero and statistically insignificant at the conventional level. These results strongly suggest that the underlying identifying assumption of our research design is satisfied; that is, the outcomes between the treated group and the control group would have trended similarly in the absence of the treatment. Finally, these results are robust to a variety of alternative specifications and placebo tests.

We contribute to a nascent but rapidly growing literature on increasing voluntary tax compliance among small businesses, which are typically characterized by a very high rate of tax evasion. First, we are among the first studies to analyze the compliance effects of a novel third-party income reporting (e.g., using credit and debit card companies as third-party reporters). To the best of our knowledge, Slemrod et al. (2017) and Adhikari et al. (2021) are the only two studies examining the impact of credit card reporting in the U.S., and Brockmeyer and Hernandez (2019) is the only study examining the impact of credit card reporting law outside of the U.S.

Second, we use sharp and plausibly exogenous variation in the intensity of information reporting generated by the clean natural experiment in the taxicab industry, which provides a more credible identification strategy than existing studies.[7] For example, Slemrod et al. (2017) use a difference-in-differences analysis using Form 1099-K recipients who were already under partial third-party reporting via Form 1099-MISC before the introduction of Form 1099-K as a control. However, as 1099-MISC is designed to cover miscellaneous sources of income, it does not cover significant sources of business revenue, so the change in the intensity of exposure to information reporting is likely very low between their treatment firms and control firms.[8] Slemrod et al. (2017) also compare the outcomes between a small subset of firms reporting almost all of their receipts through Form 1099-K, which are more affected by Form 1099-K, to firms reporting less than half of their receipts through Form 1099-K, which are less affected by Form 1099-K. Since this measure uses self-reported total receipts to create the exposure to Form 1099-K, it is likely to be endogenous. Our paper is also related to Carrillo et al. (2017), who compare changes in reported receipts, expenses, and other outcomes between original and amended tax returns of firms receiving "notice of discrepancies" letters from Ecuador's tax authorities. They exploit variation generated by a policy intervention in 2011 and 2012 when Ecuador started comparing information reports filed by third parties to the firm's tax return and notified firms about detected revenue discrepancies. These notifications represent the first time that third-party information was systematically used in Ecuador for tax enforcement. However, these notifications were non-random, so Carillo et al. (2017) estimate the effects without an explicit control group.[9] In contrast, our variation comes from the

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[7] In addition, we provide a long-term impact of Form 1099-K (up to six years after Form 1099-K) as opposed to short term impact (up to two years after Form 1099-K) provided in Slemrod et al. (2017), analyze several new outcomes of interest like taxable income, tax liability, and profit, as well as analyze several new potential mechanisms.

[8] For instance, 1099-MISC is not required to be filed by businesses purchasing goods, it is only required to be filed by businesses purchasing services that also from unincorporated businesses. Importantly, it is not required to be filed by consumers who purchase goods and services for final consumption.

[9] Our paper is also related to two contemporaneous papers: Adhikari et al. (2021) and Brockmeyer and Hernandez (2019). Adhikari et al. (2021) use the pre-Form 1099-K geographical variation in the use of credit cards by consumers to estimate the impact of Form 1099-K on tax compliance. However, given

interaction of federal Form 1099-K implementation and local ordinances mandating the introduction of credit card readers in taxicabs. The passage of local ordinances is plausibly exogenous to the introduction of Form 1099-K because local ordinances were motivated by a desire to improve the public's rider experience rather than to improve industry tax compliance.[10] In fact, most of the cities do not levy either sales tax on taxi services or any income taxes, and the few cities that levy income taxes generate less than three percent of local government revenues from these taxes (Tax Policy Center, 2015).[11]

Third, since most of the third-party information reporting laws were adopted in the first half of the twentieth century, the introduction of Form 1099-K represents one of the major expansions of information reporting in recent decades. Thus, another contribution of our paper is the use of quasi-experimental variation created by an actual reform of an information reporting regime to study its impact on a large scale. We add to the existing studies on the effects of information reporting that mostly rely on variation created by randomized "threat of audit" letters (Kleven et al., 2011; Pomeranz, 2015), small-scale random audits (Kleven et al., 2011; Phillips, 2014), non-random "notice of discrepancies" letters arising from cross-checking of tax returns against information reports (Carrillo et al., 2017), or selection of firms into existing information reporting (Brockmeyer and Hernandez, 2019).

Fourth, our estimates are generated using a subset of taxpayers (sole proprietors providing taxi services) that are likely representative of the population that Form 1099-K was intended to target: small businesses in cash-intensive industries with unreported gross income that is difficult to audit at scale. The existing literature on the compliance effects of information reporting mostly focuses on large incorporated firms due to lack of data or policy changes that only affect sole proprietors (Pomeranz, 2015; Carrillo et al., 2017; Naritomi, 2019). However, sole proprietors constitute more than 70 percent of all U.S. businesses and generate more than ten percent of total business income. Thus, along with Slemrod et al. (2017) and Adhikari et al. (2021), we extend this literature by studying sole proprietors.

2. Institutional details of the taxi industry

The taxi industry is mostly organized in one of two ways: fleets of company-owned cabs or independent owner-drivers. The fleets are usually owned by comparatively large companies with significant market share. The company hires drivers, buys and maintains cars, buys access to a radio frequency for dispatching calls, and hires dispatchers. If local regulation requires a medallion license to operate the taxi, the fleets own these licenses. Before the 1970s, fleets hired drivers as employees. However, in the 1970s, they started leasing vehicles and related services to independent drivers. Today, almost all fleets lease vehicles to independent drivers, whereby the drivers pay a fixed amount to the company each period but then keep all fares and tips. In contrast, owner-drivers own their own vehicle and the medallion license as well as drive the vehicle. The owner-drivers still need to subscribe to dispatching services (Gilbert and Samuels, 2011; Schaller, 2016).

Both independent contractors and owner-drivers usually structure their businesses as sole proprietors, which makes sole proprietorship the most common organization form in the taxi industry. For instance, in 2010, a year before the implementation of Form 1099-K, 86 percent of the business tax returns for NAICS code 4530 filed Form 1040 Schedule C (i.e., sole proprietors), eight percent filed Form 1120-S (i.e., S-corporations), five percent filed Form 1120 (i.e., C-corporations), and one percent filed Form 1065 (i.e., partnerships).

The taxi industry is one of the most extensively regulated industries in the U.S. In most cases, the city government has full jurisdiction to regulate taxicabs. However, in a few instances, counties or metropolitan areas also have non-exclusive jurisdiction over taxicab regulation.[12] The industry remains highly regulated even today, despite some deregulation during the 1970s when many other industries such as trucking, airlines, and telecommunications were deregulated. However, by the 1990s, most of the cities that had deregulated taxi services had already re-introduced regulations. Local regulators govern nearly every aspect of taxi service: from the occupational license for drivers to the medallion license for vehicles to zoning

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the lack of credit card use data at the local level before Form 1099-K, they rely on 1099-K data to proxy for pre-1099-K credit card use. We have direct information on taxpayers (e.g., taxicabs) who are or are not subject to third-party information reporting. Brockmeyer and Hernandez (2019) use an event study exploiting the variation created in the coverage of third-party reporting by the voluntary adoption of credit card readers by firms in Costa Rica. Since firms can choose whether and when to install credit card readers, this approach cannot fully control for the biases caused by endogenous firm selection. In our setting, biases from firm selection are not of concern because it is municipal ordinances that create the variation in third-party income reporting, not firm choices. Thus, our paper provides highly credible estimates of the causal impact of third-party income reporting on tax compliance.

[10] As we read through newspaper articles and meeting minutes surrounding the passage of such local ordinances in our data collection process, we found many instances where the local legislatures explicitly motivated credit card in taxi ordinances as a way to improve taxi services; we did not find any instance where the motivation was tax-related. For instance, New Orleans implemented credit card in taxi ordinance in the year that they hosted the Super Bowl, in order to improve the quality of the city's taxi services. When New York passed such legislation, it was part of the "passengers' bill of rights", indicating the right of a passenger to be able to pay with cash or card. In Boston, the credit-card ordinance was part of legislation passed "in an effort to drastically improve the quality of taxi service in the City of Boston." Finally, Sacramento's credit-card ordinance was part of legislation requiring drivers to know how to speak English, have basic math skills, follow a dress code, and accept credit cards; indeed, a council member told a local newspaper after the passage of the law that "The quality of our taxis is a problem, and there are great taxis, there are people who do a phenomenal job, but we're always judged by the least among us."

[11] Only eight states currently apply sales or gross receipts taxes to taxi fares: Georgia, Hawaii, New Mexico, Ohio, Rhode Island, South Dakota, Washington, and Wyoming (Davis and Schiller, 2017). The cities that levy income tax are Baltimore, Cleveland, Columbus, Dayton, Kansas City, New York, Philadelphia, St. Louis, and Washington DC. The local income tax rates are usually between one percent to three percent.

[12] An example of non-exclusive jurisdiction is that a local government and the state government both can regulate the taxicab industry, but the state regulation supersedes the local regulation. There are some states, mostly smaller states like Rhode Island, Connecticut, and Delaware, that in theory have jurisdiction over taxicab regulation. However, in practice almost all such states give the jurisdiction to larger cities in their states.

of the routes to setting of the fares to methods of payment that must be accepted. This paper exploits variations in the local regulations mandating taxicabs to install credit card and debit card readers in their vehicles to study the impact of third-party reporting on tax compliance (Gilbert and Samuels, 2011; Schaller, 2015).

3. Data

We combine data from three basic sources: the implementation dates of municipal ordinances requiring the use of credit cards in taxicabs, the roll out dates of Uber and Lyft in metropolitan areas, and IRS administrative tax return data for the taxicab industry. The first two datasets – the dates of rideshare entry and the dates of municipal credit card mandates – were hand-collected from various legal databases and local newspapers. The IRS administrative tax data for taxpayers in the taxicab industry come from the third-party information reports (Form 1099-K, "Payment Card and Third-party Network Transactions") and individual tax returns (Form 1040 Schedule C, "Profit or loss from business (sole proprietorship)). We discuss each of these data sources next. A detailed discussion of the many steps in our data construction is given in the Online Appendix.

3.1. Ordinances for credit card readers in taxicabs

Table 1 presents information on the implementation date of municipal ordinances that mandated taxicabs to accept credit and debit cards for their services. To collect this information, we first examined the city ordinances of all cities in the U.S. with more than 100,000 population (315 cities), and we collected the information on the introduction of mandatory credit card in taxicabs ordinances.[13] In most cases, the jurisdiction that regulates taxicabs is the city government. There are a few states where the counties or metropolitan areas have non-exclusive jurisdiction over taxicab regulation, and in such cases, we examined the ordinances of all the counties and independent cities with more than 100,000 population.[14] For brevity, we refer to these ordinances as "city-level ordinances." Finally, we ran extensive generic searches in various law databases that contain municipal codes, in newspaper databases, and on Google.com, and we found only a few cities outside of the cities that we manually examined that had passed such ordinances (e.g., Coachella Valley, CA and Lawrenceburg, IN).

These databases did not always provide information on the specific date upon which a credit card ordinance was implemented. In such cases, we searched the meeting agendas and minutes of the city councils, city ordinances, and local newspaper articles. When necessary, we also requested relevant information from the local government through the Freedom of Information Act. We then cross-validated the implementation dates obtained from each of the sources with the other sources to check the consistency of the data. We are thus confident that the dates we use in the analysis accurately capture both the presence of such regulations and, when present, the date on which the regulation went into effect for almost all municipalities and counties in the U.S.[15]

3.2. Uber and Lyft roll out data

We hand-collected data on Uber and Lyft roll outs across municipalities from multiple sources on the internet. Both ridesharing services announced their major roll outs on their respective websites. We also searched local newspapers and Google.com for all the cities on our list for the roll out dates. In the case of Uber, we used the roll out of UberX since it is the most relevant and direct competition for traditional taxicab services. The roll outs of Uber and Lyft are at the metropolitan level, and not the county or city levels. Thus, we manually mapped the metropolitan areas to the relevant city or county. The roll out dates for the treated cities are reported in Table 1.

3.3. Tax return and information reporting data

We combine data from individual tax returns and information reports to create an unbalanced panel that covers the years 2002 through 2016. These data come from Form 1040, Form 1040 Schedule C, and Form 1099-K filed with the IRS, all accessed via the Compliance Data Warehouse (CDW), the IRS research repository of tax return data. The information reports from credit card and other payment processing companies were matched to the income details from 1040 Schedule C using

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[13] We compiled the data from multiple different sources because the municipal codes are not standardized. We proceeded in three steps. First, we consulted three major municipal law databases: Municode (https://www.municode.com/library/), eLaws (http://www.elaws.us/), and Amlegal (http://www.amlegal.com/code-library/). Second, for the cities that did not publish their code in this database, we searched various other law databases such as Code Publishing Co (https://www.codebook.com/listing), ClerkBase (http://clerkshq.com/default.ashx), General Code (http://www.generalcode.com/codification/ecode/library), Q Codes (http://www.qcode.us/codes.htm), eCode360 (http://ecode360.com), enCode Plus (http://www.encodeplus.com/), Franklin Legal Publishing (http://www.franklinlegal.net/search-codes), and Sterling Codifiers (http://www.sterlingcodifiers.com/). Finally, we supplemented the information from these databases with articles published in local newspapers, which we found using newspapers.com, LexisNexis Academic, and google.com.

[14] Colorado is the only state that has jurisdiction to regulate taxicabs and that also has a state-wide mandatory credit card requirement ordinance. Thus, the jurisdiction that regulates taxicabs is almost always at the local level.

[15] Note that regulations on the credit card reader in taxicabs only apply to taxicabs registered in the jurisdiction, and they do not prevent taxicabs from other jurisdictions that do not have such regulations from dropping off passengers in the city with credit card in taxicab regulations.

Table 1

The implementation data for credit card readers in taxicab ordinances and the data on the launch of UberX and Lyft across the U.S.

City	State	Who Regulates Taxicabs	Credit Card Effective Year	UberX Entry	Lyft Entry
Panel A: Credit Card Ordinances Before or With Form 1099-K Implementation					
Los Angeles	California	City	2001	2013	2013
Indianapolis	Indiana	City	2002	2013	2013
Dallas	Texas	City	2003	2013	2013
St. Louis	Missouri	Metropolitan	2004	2015	2017
Chicago	Illinois	City	2004	2013	2013
Seattle	Washington	City	2005	2013	2013
Philadelphia	Pennsylvania	City	2006	2014	2015
San Antonio	Texas	City	2007	2014	2014
Arlington	Texas	City	2007	2013	2013
Fort Worth	Texas	City	2007	2013	2014
Houston	Texas	City	2008	2014	2017
Denver	Colorado	State	2008	2013	2013
New York	New York	City	2008	2012	2014
Coachella	California	Metropolitan	2009	2014	2014
Kansas City	Missouri	City	2009	2014	2017
Boston	Massachusetts	City	2009	2013	2013
Anaheim	California	County	2010	2013	2013
San Francisco	California	City	2011	2012	2012
Panel B: Credit Card Ordinances After Form 1099-K Implementation					
Boise	Idaho	City	2012	2014	2016
Naperville	Illinois	City	2012	2013	2013
Minneapolis	Minnesota	City	2012	2013	2014
Lubbock	Texas	City	2012	2014	2017
New Orleans	Louisiana	City	2012	2015	2016
Arlington County	Virginia	County	2012	2013	2013
Charlotte	North Carolina	City	2012	2013	2013
Washington DC	District of Columbia	City	2013	2013	2013
South Bend	Indiana	City	2013	2014	2017
Alexandria	Virginia	City	2013	2013	2013
Cleveland	Ohio	City	2013	2014	2014
Sacramento	California	City	2014	2013	2013
Tuscaloosa	Alabama	City	2014	2014	2014
Columbus	Ohio	City	2014	2013	2014
Milwaukee	Wisconsin	City	2014	2014	2014
Cambridge	Massachusetts	City	2015	2013	2013
Baltimore	Maryland	City	2015	2013	2013
San Diego	California	Metropolitan	2015	2013	2013
Dayton	Ohio	City	2015	2014	2016
West Palm Beach	Florida	County	2016	2014	2014
Montgomery County	Maryland	County	2016	2013	2013
Miami	Florida	County	2016	2014	2014
Huntsville	Alabama	City	2016	2016	2017
Savannah	Georgia	City	2016	2015	2016
Athens	Georgia	City	2016	2014	2016

Notes: This table lists all the cities that have implemented credit card reader in taxicab ordinances by the end of 2016 and the year such ordinances became effective. It also lists the launch date of UberX and Lyft in these cities. In most cases, the jurisdiction to regulate taxicabs is with the city government. However, there are few states where the county, metropolitan area, or the state have jurisdiction over taxicab regulation. In such cases, we only list the largest city from that jurisdiction in the table.

anonymized Taxpayer Identification Numbers (TINs). The taxicab industry was identified using the principal business activity code 4853 from the North American Industry Classification System (NAICS), found on the Schedule C.

3.4. Sample restrictions

Although the tax return data from the IRS are of very high quality, there are still some errors in the data. Some errors are caused by mistakes in tax filing, and others are caused by unsuccessful matching of the TINs when combining Form 1099-K and 1040 Schedule C. We clean these data of outliers and data errors that create implausible numbers. For example, the total amount of receipts reported on Form 1099-K should be lower than the total receipts reported on Schedule C for most taxpayers because Form 1099-K reports only the amount received through credit card and electronic payment systems. Accordingly, we remove individual taxpayers for whom the ratio of receipts from Form 1099-K to Schedule C is greater than
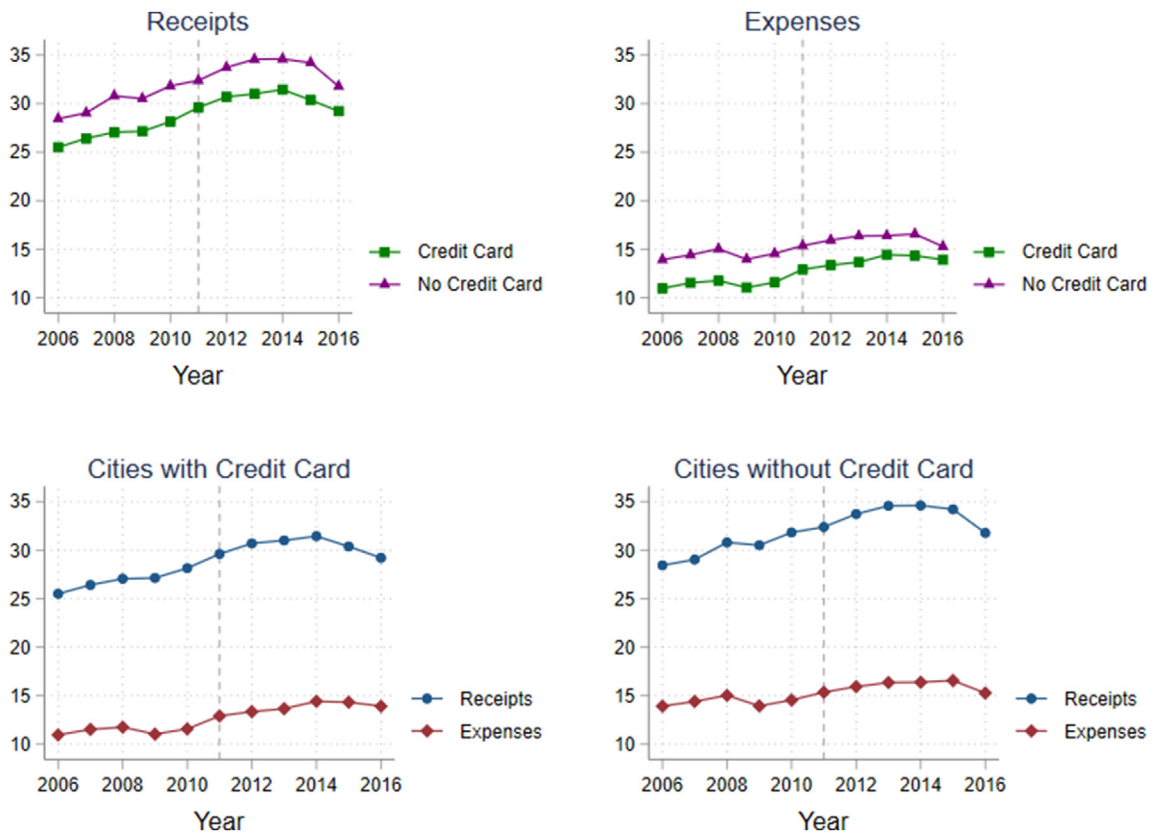
Fig. 1. The trend in reported receipts and expenses (in $1,000s) around Form 1099-K implementation in 2011

Notes: These figures show the trend in average receipts and average expenses around the federal Form 1099-K implementation in 2011. The figure in the top left plots the average receipts for the cities that implemented credit card ordinance by the end of the sample period versus cities that never implemented credit card ordinance. The figure in the top right plots average expenses for these two groups of cities. The figure in the bottom left plots the average receipts and average expenses for the cities that implemented credit card ordinance by the end of the sample period. The figure in the bottom right plots the average receipts and average expenses for the cities that never implemented credit card ordinance.

1.1.[16] We then trim the dataset by removing the top one percent of the total receipts and total expenses on a yearly basis. If a taxpayer is a top one percent outlier erroneously in any of these variables, all the variables we analyzed for that tax year are removed because the error in one variable raises concern about the underlying tax return in addition to the specific variable in question. We also require receipts and expenses to be greater than zero on a yearly basis. Finally, we remove from the sample taxpayers whose primary source of income is through Uber or Lyft because they are not usually affected by taxicab regulations. More importantly, all the income by Uber and Lyft is reported on Form 1099-K, and all this income is third-party reported irrespective of the city in which they operate. As a result, these taxpayers are not affected by our natural experiment.

3.5. Descriptive statistics

Fig. 1 plots various time-series graphs of total receipts and total expenses around 2011, the year in which Form 1099-K was implemented. The figure in the top left plots the average receipts for the cities that implemented the credit card ordinance by the end of the sample period versus cities that did not implement the credit card ordinance by the end of the sample period. The figure in the top right plots the average expenses for these two groups of cities.[17] These are raw and

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[16] There are several reasons why the Form 1099-K amount could exceed reported receipts. For example, consider merchandise returns or cash back services on card purchases provided by merchants. The full amounts of these transactions would be included in the payment processor and thus in the Form 1099-K amount, but these amounts would not all add to actual receipts for the merchants. Since we are focusing on the taxicab industry where cash use is still common, we do not expect the ratio of receipts from Form 1099-K to Schedule C to be close to 1, so this restriction removes outliers without being overly restrictive.

[17] Note that these groups are part of the treatment and control groups in our baseline analysis, but our baseline treatment indicator does not always begin in 2011. In fact, 2011 is the first year of treatment for only those cities that already implemented credit card in taxicabs ordinances by 2011. For the rest of the cities, the first year of treatment is the year credit card in taxicabs ordinances are implemented.

unadjusted figures. Even so, they allow us to inspect the parallel trend assumption underlying our baseline difference-in-differences model. We find suggestive evidence that the average receipts and expenses have parallel trends before 2011. The figure shows that the average receipts, as well as the average expenses, are slightly higher in the cities without credit card in taxicab ordinances than in the cities with such ordinances. For instance, average receipts in 2010, a year before 1099-K implementation, are \$26,582 in the cities with ordinances and \$31, 820 in the cities without the ordinances and the average expenses in 2010 are \$11,370 in the cities with ordinances and \$14,551 in the cities without ordinances.

The figure in the bottom left of Fig. 1 plots the average receipts and expenses for the cities with credit card in taxicab ordinances and the figure in the bottom right plots the average receipts and expenses for the cities without credit card in taxicab ordinances. These figures suggest that total receipts are about twice the total expenses. The ratio of receipts to expenses is similar for both the cities with credit card in taxicab ordinances and the cities without credit card in taxicab ordinances.

These simple trends do not suggest that Form 1099-K implementation was associated with higher reported receipts or higher reported expenses. However, we need to account for omitted variables that might be correlated with the Form 1099-K introduction and taxpayer filing behavior. We also need to account for any national trends in tax filing behavior or credit card use that may confound the results. The next section presents our empirical framework for disentangling the impact of Form 1099-K on taxpayer behavior.

4. Empirical strategy

Our methodology exploits the natural experiment created by the interaction of the introduction of Form 1099-K in 2011 and the introduction of city ordinances requiring taxicabs to install credit card readers. We use a difference-in-differences research design where we compare the trends in the receipts and the expenses of the taxi services that operate in cities with mandatory credit card in taxi ordinances (i.e., the treated group) versus these trends for taxi services that operate in cities without mandatory credit card ordinances (i.e., the control group) before and after the treatment. We argue that the introduction of credit card readers in taxicabs discontinuously increases the share of the revenue from credit cards reported on Form 1099-K, so that taxi services operating in cities with such ordinances will be affected by the third-party reporting more than those operating in cities without such ordinances.

Since we do not have a direct measure of tax under-reporting, we infer the effects on under-reporting from the data on taxpayer reports, using a "traces-of-evasion" approach (Slemrod and Weber, 2012). To this end, we examine the changes in total receipts and total expenses reported to the IRS around the treatment.

Our baseline difference-in-differences (DID) specification is given by:

$$Y_{ict} = \alpha + \beta_0 Treatment_{ct} + \sum_{j=1}^{J} \beta_j X_{ict} + \beta_i FE_i + \beta_c FE_c + \beta_t FE_t + \varepsilon_{ict} \tag{1}$$

where the dependent variables (Y_{ict}) are the logs of receipts and expenses for taxi service i in city c in year t. We are interested in estimating the coefficient on the indicator variable $Treatment_{ct}$, which equals one only in years when both of the conditions are satisfied: the city has implemented mandatory credit card ordinances and the IRS has implemented Form 1099-K (i.e., Tax Year $\geq$2011).

The vector $\sum X_{ict}$ includes dummy variables such as the presence of UberX and Lyft in the cities in the given year. We control for the introduction of UberX and Lyft in the cities because they can potentially affect the receipts and expenses of taxicabs. For example, entry of ridesharing services may lead to a loss of customers for the taxi industry, it may require taxis to spend more time driving around looking for a customer, and entry may encourage taxis to increase their services by increasing air conditioning or cleaning taxis more frequently. Taxi service fixed effects, city fixed effects, and year fixed effects control for unobserved time-invariant taxi service characteristics, time-invariant city characteristics, and national time trends, respectively. The standard errors are clustered at the jurisdiction that regulates taxicabs, which is a city or a county in most cases and a metropolitan area or a state in a few cases. For brevity, we refer to "clustering at the city level."

We also evaluate the dynamic nature of the treatment effects by running an event study DID specification, given by:

$$Y_{ict} = \alpha + \sum_{y=-6}^{y=5} \beta_y \, Years \, around \, Treatment_{cy} + \sum_{j=1}^{J} \beta_j X_{ict} + \beta_i FE_i + \beta_c FE_c + \beta_t FE_t + \varepsilon_{ict} \tag{2}$$

where $\sum_{y=-6}^{y=5} Years \, around \, Treatment_{cy}$ denotes six years before the treatment and six years after the treatment, omitting the year immediately before the treatment from the regression. This event study specification amounts to an annual difference-in-differences estimator where we compare the difference in outcomes between treatment and control counties in each year relative to the baseline difference in a year before treatment. This event study DID specification also allows us to test for the existence of parallel pre-treatment trends in the outcome variables between treated and control cities. If the coefficients before the treatment are zero, then this result increases our confidence that endogeneity issues do not pose a significant threat to the research design. However, if the coefficients before treatment are statistically significant, then this implies that our estimates of the treatment effect may be biased due to endogeneity issues. Also, if coefficients before the

Table 2
The difference-in-differences estimates of the impact of third-party income reporting on the log of reported receipts and expenses

	Log Total Receipts	Log Total Expenses
Credit Card X 1099-K	0.067***	0.080***
	(0.020)	(0.023)
Credit Card	-0.026	-0.013
	(0.040)	(0.037)
Lyft Present	-0.001	-0.004
	(0.014)	(0.015)
Uber Present	-0.009	-0.004
	(0.013)	(0.012)
Number of Observations	1,563,189	1,331,184

Notes: This table shows the impact of third-party income reporting on the log of reported receipts and expenses using difference-in-differences. The treatment is defined as the interaction of the introductions of local ordinances mandating credit card reader in taxicabs with federal Form 1099-K implementation in 2011. All specifications include city, year, and taxpayer fixed effects. The standard errors, in parenthesis, are clustered at the city level. * $p < .1$, ** $p < .05$, and *** $p < .01$.

treatment are significantly different from zero, then this might indicate the failure of the difference-in-differences approach to create a comparison group with counterfactual trends similar to the treatment group. The coefficients after the treatment describe the dynamic treatment effects of Form 1099-K in years following its implementation relative to the effect before the implementation. Standard errors are again clustered at the city level.

Finally, as discussed in detail later, we also estimate a wide range of robustness tests.

5. Results

5.1. Baseline DID estimates

Table 2 presents our baseline DID estimates of the impact of Form 1099-K on the log of receipts and expenses from our baseline difference-in-differences specification in equation (1), controlling for taxpayer fixed effects, city fixed effects, year fixed effects, and the presence of Uber and Lyft. We find an economically meaningful and statistically significant impact of third-party reporting on both receipts and expenses. Taxpayers in cities with credit card in taxicab ordinances reported 6.7 percent higher receipts after the treatment compared to taxpayers from cities without such ordinances, and the estimate is significant at the one percent level. However, we find an even larger and statistically significant response for expenses; that is, reported expenses by taxpayers in cities with credit card in taxicab ordinances increased by eight percent after the treatment compared to taxpayers from cities without such ordinances.

It is important to discuss how the launch of Uber and Lyft around the same time as the introduction of Form 1099-K could affect our estimates. One might expect the launch of Uber and Lyft to affect the income of the taxi industry negatively (Berger et al., 2018). In this case, failure to control adequately for the roll out of Uber and Lyft would underestimate the true impact of Form 1099-K. However, in most cases, we do not find any significant impact of the roll out of Uber or Lyft on the outcomes of the taxi industry. This result could be because these services were rolled out very aggressively, or in a period of months, while our data are only available with annual frequency. Also, within a couple of years of their first introduction in 2011, either Uber or Lyft was present in almost all the metropolitan and micropolitan areas. Consequently, the launch of Uber and Lyft may have affected the outcomes of taxi services, but it seems unlikely that they differentially affected the outcomes for our treated and control cities.[18]

Another potential concern is that if cities introduced credit card ordinances in response to the roll out of Uber and Lyft, then this action could bias our estimates. However, we do not find such evidence. Table 1 presents information on the implementation date of municipal ordinances that mandated taxicabs to accept credit and debit cards for their services and the roll out of Uber and Lyft across municipalities. We do not find any pattern of increased local regulation mandating credit card readers in taxicabs after 2011 when Uber and Lyft started rolling out aggressively across the U.S. In fact, slightly less than half of our treated cities introduced credit card in taxi ordinances before the launch of Uber and Lyft in 2011. When we look at the launch of these services in individual cities, we find that credit card in taxi regulations were passed after the launch of either Uber or Lyft in the relevant city only in the eleven cities out of forty-three treated cities reported in Table 1. Finally, as discussed later, we also conduct various robustness tests to examine further the sensitivity of our results to the presence of Uber and Lyft, and we do not find any significant impacts.

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[18] Indeed, the results when we do not control for the presence of Uber and Lyft are presented in the robustness test section's Fig. 7 Row 11, and they are very similar to our baseline DID results.

Table 3

The difference-in-differences estimates of the impact of third-party income reporting on the log of profit, taxable income, and tax liability

	Log Profit	Log Taxable Income	Log Tax Liability
Credit Card	0.015	0.019	0.019
X 1099-K	(0.019)	(0.019)	(0.022)
Credit Card	0.027	-0.031	-0.030
	(0.024)	(0.025)	(0.033)
Lyft Present	-0.00099	0.0072	0.013
	(0.014)	(0.016)	(0.022)
Uber Present	0.0059	0.0027	-0.0046
	(0.016)	(0.015)	(0.019)
Number of Observations	1,374,096	551,720	504,195

Notes: This table shows the impact of third-party income reporting on the log of profit, taxable income, and tax liability using difference-in-differences. The sample is the same as the baseline sample. The treatment is defined as the interaction of the introductions of local ordinances mandating credit card reader in taxicabs with federal Form 1099-K implementation in 2011. All specifications include city, year, and taxpayer fixed effects. The standard errors, in parenthesis, are clustered at the city level. $^{*}p <$ .1, $^{**}p <$ .05, and $^{***}p <$ .01.

5.2. DID estimates of additional outcomes

Although expenses increased by a greater percent than receipts, this does not necessarily imply that Form 1099-K was unsuccessful in improving overall tax compliance in the taxicab industry. Expenses are usually lower than receipts for a profitable business, so even if they grow more than receipts in percent terms, the level increase may be smaller, leading to higher reported taxable income, profits, and ultimately tax liabilities. Thus, we also directly evaluate the impact of Form 1099-K on these outcomes. The results are presented in Table 3. We find that third-party reporting led to a small but statistically insignificant increase in profit, taxable income, and tax liability, all of which are consistent with our baseline findings because the increase in reported receipts was offset by the increase in reported expenses. Since we are studying sole proprietors, their taxable income and tax liability can be affected by various factors outside of Form 1099-K such as spouses' earnings if married filing jointly, as well as by various exemptions, deductions, and tax credits that vary based on household circumstances. Nevertheless, since we are analyzing within-taxpayer responses, the estimates provide suggestive evidence that Form 1099-K did not increase reported taxable income or tax liability.[19]

In Fig. 2, we analyze several other potential impacts of Form 1099-K. First, there are many zero values in profit and taxable income, which are dropped from the sample when we express our outcome variable using a logarithmic scale. We follow Pomeranz (2015) and run a difference-in-differences specification that uses an indicator for positive profit and positive taxable income as dependent variables. This specification provides a relative measure indicating the likelihood of an increase in reported profit or taxable income after treatment compared to the firm's own history. Moreover, this specification is applicable to variables that include many zero values, and it is also robust against outliers. The results are reported in Fig. 2 Row 1 and Row 2, and they indicate that Form 1099-K did not affect the likelihood of firms reporting positive profit or positive taxable income.

Second, we analyze whether Form 1099-K affected the entry rate or exit rate of taxicabs. We consider that a firm entered the taxicab business in year X if the firm is observed in our dataset in year *X,* but it is not observed in our dataset in year *X-1.* Similarly, we call a firm exited the taxicab business in year *X* if it is observed in our dataset in year *X,* but it is not observed in our dataset in year *X+1.* The results are presented in Row 3 and 4 of Fig. 2. We do not find a statistically or economically meaningful impact of Form 1099-K on exit rate or entry rate.

Third, we analyze how local credit card ordinances requiring the installation of credit card readers in taxicabs affect the adoption of credit card readers by taxi cabs. Unfortunately, due to a lack of data on credit card use before the implementation of Form 1099-K in 2011, we can only analyze this on the sample of firms from cities that adopted credit card reader ordinances in 2011 or later.[20] Therefore, we remove from our analysis any cities that adopted local ordinances before 2011. We also restrict our sample to 2011 to 2016 (i.e., the years for which we have data on 1099-K). We then created a dummy variable that equals 1 if the taxicab received 1099-K (which proxies for having a credit card reader) and 0 otherwise. The difference-in-differences results are presented in Row 5 of Fig. 2. We find that local credit card ordinances increased credit card adoption by about 7.8 percentage points on average compared to the cities without such ordinances. The estimate rep-

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[19] A shift from receipts under-reporting to expense over-reporting may result in more detection of noncompliance on audit since the burden of proof for suspected expense misreporting is on the taxpayer, who must typically provide receipts justifying expenses in case of an audit. Thus, Form 1099-K can still be useful even in the case of completely offsetting responses by taxpayers.

[20] The estimates for "early adopters" are likely to be higher since UberX and Lyft were just starting to expand in a few cities in 2011, but they rapidly launched in many cities within the next few years. Thus, in the later years we are likely to find a faster increase in voluntary adoption of credit card readers by taxicabs even when they were not required to do so by local ordinances.
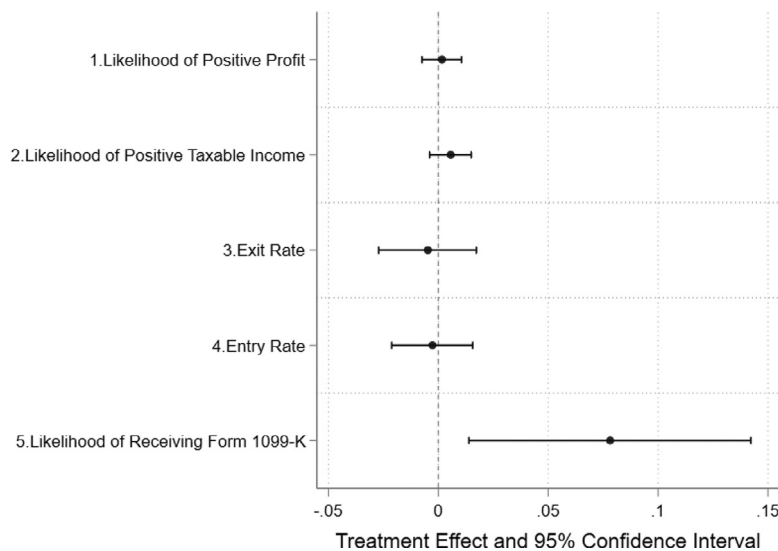
Fig. 2. The difference-in-differences estimates of the impact of third-party reporting on several binary outcomes
Notes: The figure plots the point estimates and 95 percent confidence intervals for the impact of treatment on the likelihood of firms exiting the market, entering the market, reporting positive profit, reporting positive taxable income, and receiving Form 1099-K, respectively. The Y-axis lists regression specifications and the X-axis denotes the range of the point estimates and their confidence intervals. The treatment is defined as the interaction of the introductions of local ordinances mandating credit card reader in taxicabs with federal Form 1099-K implementation in 2011. All specifications include city, year, and taxpayer fixed effects and the standard errors are clustered at the city level.

resents both statistically and economically significant increase in credit card adoption by firms. However, we note the caveat that the analysis is limited both in the number of cities covered and the number of years covered.[21]

5.3. Event study estimates

Fig. 3 presents our event study DID estimates of the impact of Form 1099-K on the log of receipts and expenses given in Eq. (2). The event study can be interpreted as an annual difference-in-differences estimator where we compare the difference in outcomes between the treatment group and the control group in each year relative to the baseline difference in a year before treatment. In addition to dummy variables indicating six years leading to treatment and six years following the treatment, we also include city fixed effects, year fixed effects, taxpayer fixed effects, and dummy variables indicating the presence of Uber and Lyft. The year immediately before the treatment is omitted from the regression. Standard errors are clustered at the city level.

We again find that Form 1099-K affected both the reported receipts and reported expenses. The estimates before the treatment (i.e., Year < zero) are all close to zero, and they are statistically insignificant at the five percent level, which supports the parallel trend assumption required for the validity of the difference-in-differences models. All estimates from year zero and forward are positive and statistically significant for both receipts and expenses. The estimates for the log of total receipts range from 2.3 percent higher in the treated cities versus the control cities in the year of treatment to 9.7 percent higher four years after the treatment. All estimates are significant at the conventional level. The estimates for the log of expense increases immediately after the treatment are in general modestly larger, ranging from 4.3 percent at the treatment year to 11.8 percent higher six years after the treatment. These estimates are also significant at the conventional level.

6. Potential mechanisms

In this section, we explore several potential mechanisms to understand how taxpayers respond to third-party information reporting. First, we look at all expense line items from Schedule C to see where taxpayers increased reported expenses to

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[21] The analysis using Form 1099-K as a proxy for having a credit card reader in taxicabs likely underestimates the true impact for a few reasons. There are several issues with matching Form 1099-K to individual taxpayers in general, a problem that is especially challenging for both sole proprietors in the taxi industry because they consist of many small firms. Importantly, as discussed earlier, taxi drivers usually lease or rent the vehicle from some other companies. The original owner of the vehicle (and not the individual drivers of the vehicle) receives Form 1099-K, and then the original owner sends via new Form 1099Ks the information to each driver leasing the vehicle. This institutional feature creates twice the hurdle in the matching process compared to most other businesses where the business owners do not usually lease the credit card reader from somebody else. Also, the city in which the vehicle is registered determines whether taxi drivers need to follow local ordinances, but this can be different from the city in which the taxi driver resides, especially in areas where multiple cities or counties make up a single metropolitan area or commuting zone.
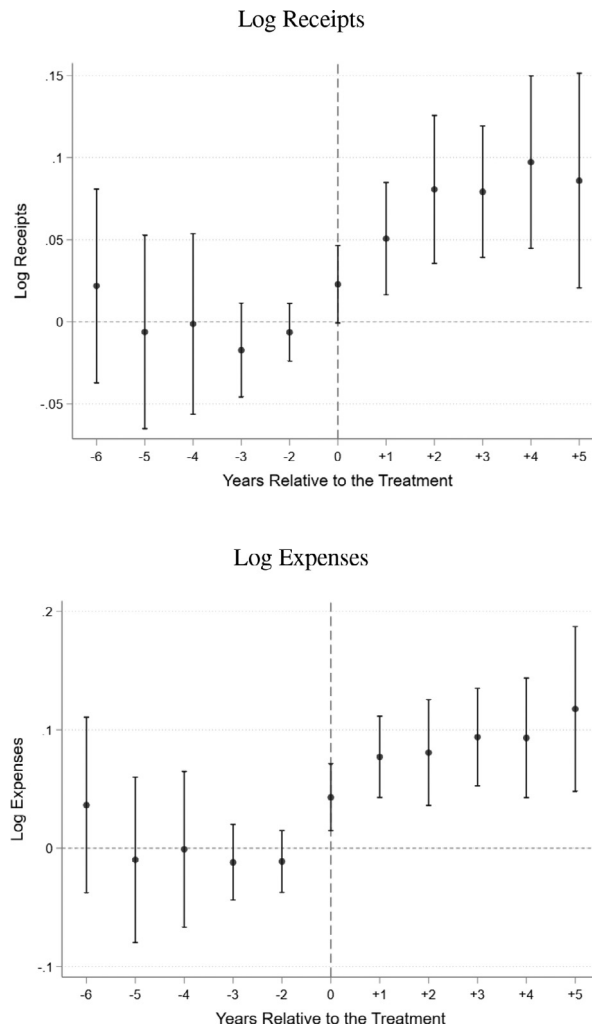
Fig. 3. The event study estimates of the impact of third-party reporting on the log of total receipts and total expenses
Notes: The figure shows the impact of third-party reporting on the log of total receipts and total expenses and the 95 percent confidence interval for the 12-year window around the treatment. The treatment is defined as the interaction of the introductions of local ordinances mandating credit card reader in taxicabs with federal Form 1099-K implementation in 2011. The year immediately before the treatment is omitted. The specification includes city, year, and taxpayer fixed effects as well as dummy variables indicating the presence of Uber and Lyft. The standard errors are clustered at the city level.

offset increased receipts. Second, we explore whether the compliance effects are driven by the third-party reporting provided by Form 1099-K or by the electronic paper trail provided by credit card readers. Third, we examine whether taxpayers who install credit card readers voluntarily have higher compliance than those who do not install credit card readers voluntarily.

6.1. Identifying the source of increased expenses

To see precisely where taxpayers increased reported expenses to offset increased receipts, we look at all expenses from Schedule C that are available to us, such as expenses for office supplies, car and truck, insurance, legal and professional services, meals and entertainment, business mortgage, home mortgage, repairs and maintenance, travel, utilities, cost of goods, wages, depreciation, and the total of all other expenses not individually reported (i.e., line 27a of Schedule C called other expenses). The results for individual expenses are presented in Fig. 4. We do not find significant increases in any of the line items for expenses except for the depreciation and other expenses categories, both of which increase by about eight percent (significant at the one percent level). Because almost all the increases in reported expenses are driven by increases in these two categories, this suggests that taxpayers tend to choose cost adjustments that are more difficult to verify during audits, which is especially true for the other expenses categories.

Furthermore, when we compare our estimates with the audit data of a random sample of nationally representative sole proprietors in the years before Form 1099-K implementation (i.e., 2006-2010), we find additional evidence that the increases in expenses reflect non-compliance due to over-reporting expenses. For example, only about 23 percent of sole proprietors
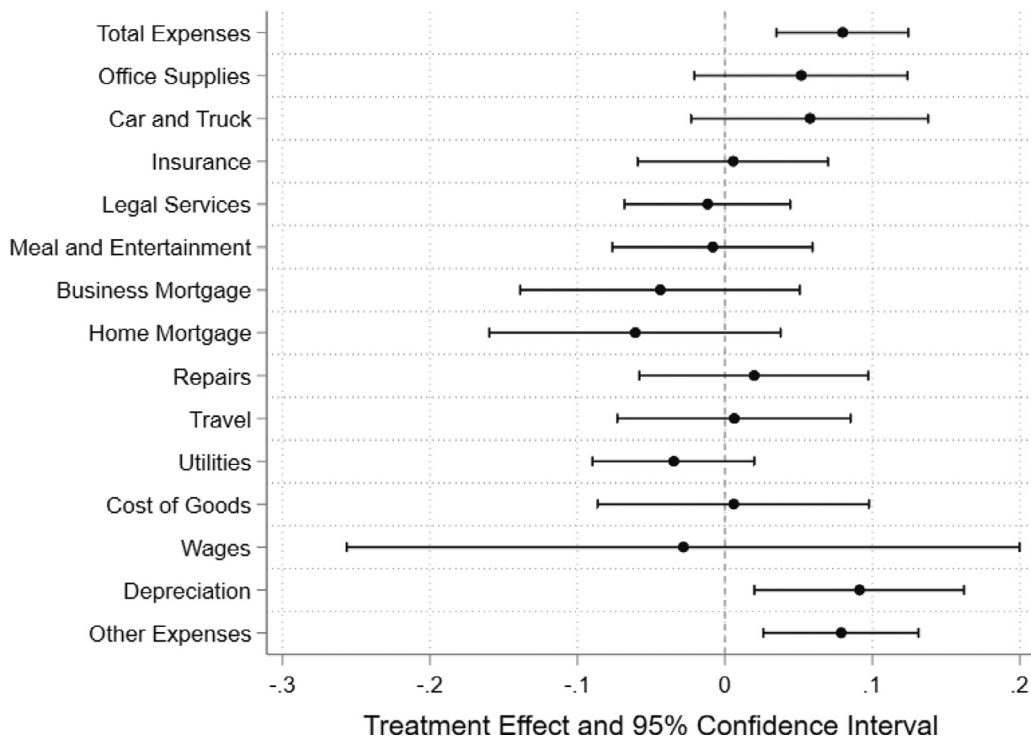
Fig. 4. Potential mechanisms: The difference-in-differences estimates of the impact of third-party reporting on individual expense items
Notes: The figure plots the point estimates and 95 percent confidence intervals for the impact of treatment on the log of individual expense items. It also shows the estimate for total expenses from the baseline specification to facilitate comparison. The Y-axis lists individual expense items and the X-axis denotes the range of the point estimates and their confidence intervals. The sample is identical to the baseline. The treatment is defined as the interaction of the introductions of local ordinances mandating credit card reader in taxicabs with federal Form 1099-K implementation in 2011. All specifications include city, year, and taxpayer fixed effects and the standard errors are clustered at the city level.

in the nationally representative audit sample adjusted their expenses upward when their receipts were adjusted upward by the auditors during the randomized audit. Thus, the corresponding increase in expenses that we consistently find is unusual compared to the randomized audit adjustment. Similarly, the median sole proprietor who was found to be under-reporting income in the nationally representative audit sample was also found to be over-reporting expenses, but only by 6.8 percent. In contrast, we find a very large increase in expenses in response to third-party information reporting, which is unusual compared to the randomized audit.

6.2. Isolating the impact of third-party reporting using early adopting cities

To understand whether our estimates are driven by the third-party reporting via Form 1099-K or via the installation of credit card readers in taxicabs, we analyze the impact of Form 1099-K only on treated cities that implemented credit card in taxi ordinances before 1099-K implementation in 2011. Even though we lose about half the treated cities by this restriction, there are various advantages of doing so.

First, the installation of credit card readers may increase reported receipts through improved tax compliance, as demonstrated earlier. However, installation may also increase receipts through an increase in the demand for taxi services as people might hail cabs more often if they know they can use a credit card. When we use cities that implemented credit card rules after 2011, we cannot distinguish between these two impacts. However, if we assume that the increase in revenues after credit card installation is caused by a one-time shift in demand, using cities that passed credit card ordinance before 2011 will have already accounted for the impact of credit card installation on our outcomes. Second, the installation of a credit card can increase tax compliance through third-party income reporting or through the electronic paper trail that credit card transactions provide.[22] Again, using pre-2011 credit card cities, we can attribute the difference when Form 1099-K was implemented in 2011 to third-party reporting only because there was already an electronic paper trail presence for cities that implemented credit card ordinances before 2011. Third, if expenses related to credit card installation are incurred in the first year or two after installation, then using pre-2011 cities implies that the increase in reported expenses after Form

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[22] Credit card installation may also increase tax compliance by reducing the compliance cost of filing taxes because it can make the amount of receipts visible, salient, and tractable to taxpayers. This effect would be even larger in the case of third-party reporting.
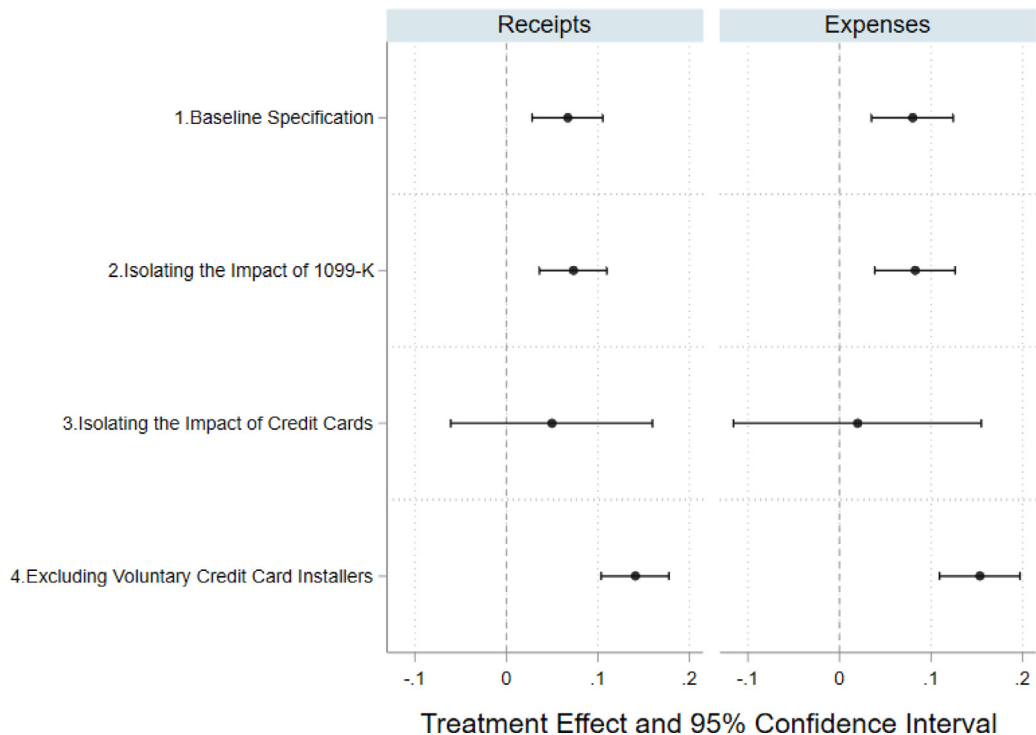
Fig. 5. Potential mechanisms: The difference-in-differences estimates on the log of total receipts and total expenses
Notes: The figure shows the point estimate and 95 percent confidence interval for the impact of treatment on the log of total receipts and total expenses. The treatment is defined as the interaction of the introductions of local ordinances mandating credit card reader in taxicabs with federal Form 1099-K implementation in 2011 for all specifications except for *3. Isolating the Impact of Credit Cards* where the treatment is defined as the introductions of local ordinances mandating credit card reader in taxicabs in the pre-1099-K period. All specification includes city, year, and taxpayer fixed effects. The standard errors are clustered at the city level.

1099-K implementation does not include a one-time increase in expenses related to credit card installation. Fourth and finally, credit card regulation after 2011 might be endogenous, especially if some post-2011 cities implemented credit card regulation with the knowledge that, if taxicabs use credit cards, then the recently implemented Form 1099-K would make more of their income visible. Thus, we can avoid any biases caused by endogeneity by restricting our analysis to cities that implemented the ordinance before 2011.

The results are presented in Row 2 of Fig. 5. We find that the removal of late implementing cities from the analysis slightly increases the impact of Form 1099-K on receipts, increasing the impact from 6.7 percent to 7.3 percent while also increasing the impact on expenses (from 8.0 percent to 8.2 percent). The results suggest no significant compliance impact from the electronic paper trail alone, a result that indicates that the increasing use of electronic payment systems that leave a paper trail in and of itself may not be sufficient to increase voluntary tax compliance.

6.3. Isolating the impact of credit card readers using pre-1099-K data

Here we attempt to estimate directly the net impact of credit card readers on reported receipts and expenses through all channels except for third-party reporting. We do so by limiting our analysis period to pre-Form 1099-K years. Thus, the treatment is now defined as taxicabs installing credit card readers due to cities implementing credit card in taxicab ordinances in the absence of Form 1099-K. The two plausible channels through which credit card readers may increase business reporting in the absence of Form 1099-K are by providing the paper trail of transactions and by increasing the demand for taxi services. Thus, if we find a significant impact on periods before 2011, then we can adjust our estimates of the impact of third-party reporting to purge all other non-third-party reporting effects.

The results are presented in Row 3 of Fig. 5. We do not find any significant increase in total receipts or expenses. This result suggests that the presence of an electronic paper trail alone is not enough to encourage tax compliance. It also suggests that credit card readers do not significantly increase the demand for taxi services.

6.4. Exploring if voluntary credit card installers respond differently

Here we examine whether taxpayers who installed credit card readers voluntarily have higher compliance than those who did not install credit card readers voluntarily. To do so, we remove from our sample any taxpayers from the cities

without credit card in taxicab ordinances who still received Form 1099-K. Thus, our control group consists of only those firms that have not installed credit card readers in their vehicles. Doing so also provides us a "cleaner" control group because the income of taxicabs that voluntarily installed credit card readers would be reported to the IRS via Form 1099-K, even when these taxicabs operated in control cities, which could attenuate our estimates toward zero. However, there is a disadvantage of removing such taxpayers from the sample because doing so can introduce selection bias as well as change the composition of the control group. For instance, we are likely to remove most taxpayers working for big taxicab companies such as Yellow Cabs because taxicabs from these companies accept credit cards in most cities.

The results are presented in Row 4 of Fig. 5. We find that reported revenues, as well as reported expenses, are twice the size of the baseline estimates when we remove voluntary credit card installers from the control group. This result suggests that taxicabs that did not install credit card readers voluntarily had a higher non-compliance rate compared to the full sample of taxicabs.

7. Placebo experiments and robustness tests

7.1. Placebo experiments

We also use placebo experiments to test whether the estimated impact of third-party reporting could be driven entirely by chance. We use the same difference-in-differences specifications as in our baseline DID specifications, but our estimates focus on sole proprietors from various other industries that should not be affected by the mandatory credit card in taxicab ordinances. If most of these placebo treatment effects are close to zero, we have further assurance that our research design is not producing spurious results. We run placebo experiments on the list of industries that are designated as cash-intensive for audit purposes by the IRS (2010) because our treatment industry (i.e., Taxi and Limousine services) is also one of the industries designated as cash-intensive for audit purposes. These 14 industries and their 4-digit NAICS codes are: 4451 (Grocery Stores), 4452 (Specialty Food Stores), 4471 (Gas Stations), 4533 (Used Merchandise Stores), 4542 (Vending Machine Operators), 7211 (Traveler Accommodation), 7225 (Restaurants and Other Eating Places), 8111 (Automotive Repair and Maintenance), 8112 (Electronic and Precision Equipment Repair and Maintenance), 8114 (Personal and Household Goods Repair and Maintenance), 8121 (Personal Care Services), 8123 (Drycleaning and Laundry Services), 8129 (Other Personal Services), and 8141 (Private Household Services).

The results are presented in Fig. 6. Twenty-five out of twenty-eight of the placebo estimates are statistically indistinguishable from zero at the five percent level. However, we find two industries (Electronic Repair and Personal Care Services) that show an increase in receipts that is significant at the five percent or better level. We also find one industry (Automotive Repair) that shows an increase in expenses that is significant at the five percent or better level. Thus, we conclude that our research design identifies the true impact of Form 1099-K and does not produce spurious results.

7.2. Robustness tests

We also conduct a series of robustness tests to test the sensitivity of our baseline specification. The results are presented in Fig. 7. Overall, these robustness tests confirm our baseline DID results. We discuss each of them below.

Restricting the sample to large cities. As noted earlier, we collected data on credit card ordinances for all cities and counties with a population greater than 100,000 by browsing their municipal codes manually and then conducting an extensive search for the rest of the cities. Thus, in our baseline DID specification, we assumed that the rest of the cities in the U.S. do not have such ordinances. We are confident that our sample is complete. Nevertheless, we test whether our results are robust if we restrict our samples to large cities only. To do so, we run three analyses restricting our analysis to the largest 300 cities, the largest 200 cities, and the largest 100 cities, respectively.

The results are presented in Rows 2-4 in Fig. 7. The estimates are very similar to our baseline DID results, regardless of the specific sample restriction.

Testing robustness to outliers. Although the tax return data from the IRS are of high quality, there are still some errors in the data. Some errors are caused by errors in filing and others by unsuccessful matching of the TINs when combining Form 1099-K and 1040 Schedule C. Thus, in our baseline DID regressions, we trimmed the sample to remove the top one percent of reported values for total receipts and total expenses on a yearly basis.

Here we test whether our results are sensitive to the threshold we chose to flag an observation as an outlier by trimming the sample to remove the top five and top ten percent of the reported values in the same manner. We also test whether our results change when we restrict our sample even further by removing taxpayers from the entire sample if they belong to the top one percent in any year. The results are presented in Rows 5-7 in Fig. 7. Again, our estimates are very similar to the baseline DID estimates.

Using a balanced panel. We test whether our results are sensitive to our choice of an unbalanced panel in the baseline estimates by constructing an 11-year balanced panel from 2006 to 2016. This restriction severely reduces the sample size since there is a high turnover in the taxi industry. Nevertheless, we find that the results, presented in Row 8 of Fig. 7, are very similar to the baseline results. The size of the estimates is only slightly larger for both receipts and expenses compared to the baseline results, and both are significant at the conventional level.
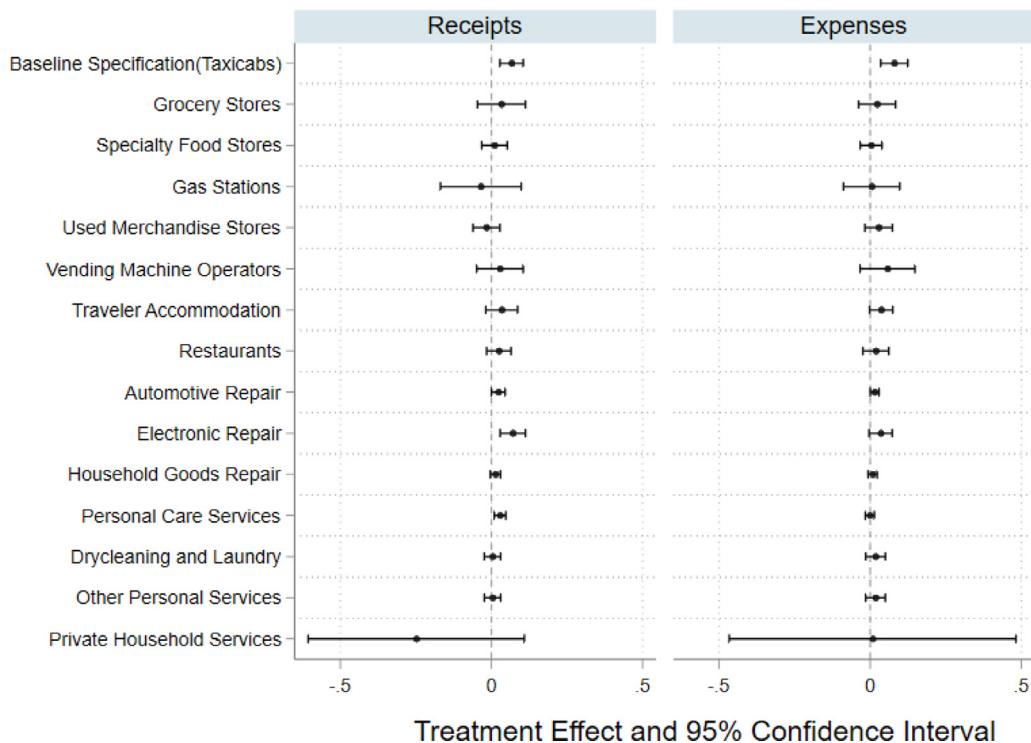
Fig. 6. Placebo experiment: The difference-in-differences estimates of the impact of third-party reporting on the log of total receipts and total expenses for non-treated cash-intensive industries

Notes: The figure shows the point estimate and 95 percent confidence interval for the impact of treatment on the log of total receipts and total expenses for other cash-intensive industries that should not be affected by the local credit card in taxicab ordinances. It also shows the estimate from the baseline specification (i.e., taxicabs) to facilitate comparison. The Y-axis lists other cash-intensive industries and the X-axis denotes the point estimates and their confidence intervals. The treatment is defined as the interaction of the introductions of local ordinances mandating credit card reader in taxicabs with federal Form 1099-K implementation in 2011. All specification includes city, year, and taxpayer fixed effects. The standard errors are clustered at the city level.

Using MCC codes to validate NAICS codes. The NAICS code we use to identify taxicabs are self-reported by taxpayers in their Schedule C. To test the robustness of our results against error in filing the NAICS code, we use the merchant category code (MCC) reported in Form 1099-K to validate the sample, where MCC code of 4121 represents the taxi industry; that is, we restrict our sample to firms that fulfill two criteria: taxpayers that self-report their NAICS code as 4835, and taxpayers whose Form 1099-K reports the firm's MCC codes as 4121.[23] We only require that the MCC code is reported at least once for each taxpayer in the full sample window so that we are able to capture as many taxpayers as possible that have a credit card reader in their vehicle. Nevertheless, this analysis can significantly change the composition of the control group.

The results are presented in Row 9 of Fig. 7. The impact of third-party reporting is positive and significant at the 5 percent level for total receipts and total expenses. However, the size of the estimates decreases to 4.4 percent for receipts and 5.7 percent for expenses.

Using a five-digit NAICS code to identify the taxicab industry. In our baseline DID estimates, we use NAICS code 4853 to classify taxicabs because this is the level of detail provided in the instructions for filing Schedule C. Thus, we expect most taxpayers to report 4-digit NAICS code in their Schedule C. However, NAICS 4853 includes both taxicab and limousine services and, since limousines are not affected by credit card ordinances, using NAICS code 4853 could attenuate our estimates towards zero. In this robustness test, we examine whether our results are sensitive to the use of 5-digit NAICS code to classify taxicabs.

The results are presented in Row 10 of Fig. 7. As expected, the sample size drops significantly when moving from the 4-digit NAICS code to the 5-digit NACIS code (i.e., from 1.56 million observations to 34, 558 observations). Nevertheless, we still find a positive and significant impact of third-party reporting on total receipts, even a larger impact than in our baseline DID estimate. The estimate for total expenses is largely unchanged, but it is no longer significant at the conventional level.

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[23] There are several advantages of using the MCC code to validate our sample. First, the MCC code is provided by third-party reporters (i.e., banks and credit card companies) who are usually a large corporation, and so these reporters may be more careful than taxi drivers who fill out the NAIC codes. Second, credit card companies classify consumer spending by MCC codes when determining the cash-back or bonus points awarded, so they have a strong incentive to properly report MCC codes. However, there is one major drawback of using the information on MCC codes to classify taxicabs: MCC codes are available in Form 1099-K only and so using MCC codes would exclude all taxpayers who never received a Form 1099-K from the analysis.
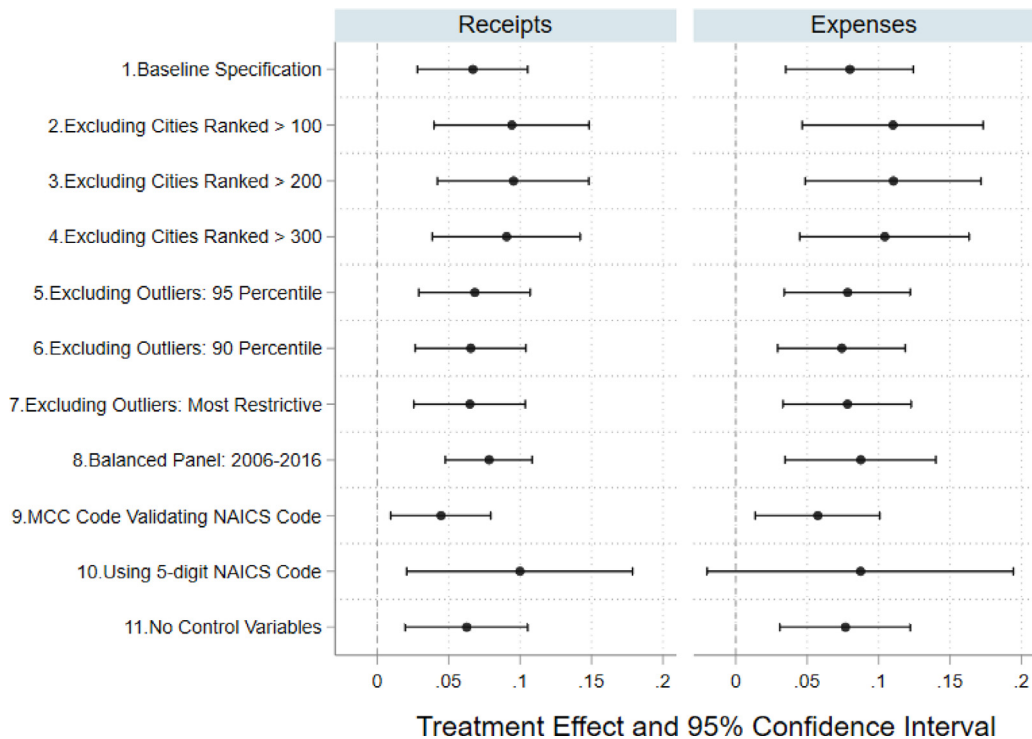
Fig. 7. Robustness tests: The difference-in-differences estimates of the impact of third-party reporting on the log of total receipts and total expenses under alternative specifications
Notes: The figure shows the point estimate and 95 percent confidence interval for the impact of treatment on the log of total receipts and total expenses obtained in various robustness tests. It also shows the estimate from the baseline specification to facilitate comparison. The Y-axis briefly describes the robustness tests and the X-axis denotes the range of the point estimates and their confidence intervals. The treatment is defined as the interaction of the introductions of local ordinances mandating credit card reader in taxicabs with federal Form 1099-K implementation in 2011 for all specifications. All specification includes city, year, and taxpayer fixed effects. The standard errors are clustered at the city level.

Excluding all control variables. In the baseline specification, we control for the launch of UberX and Lyft in all the cities in our sample. Here, we test if our results are sensitive to the presence of ridesharing services. The results are presented in Row 11 of Fig. 7, and they remain very similar to the baseline results.

Adding restrictions to make the sample size consistent. The number of observations in our baseline analysis changes across specifications for two reasons: there is a high turnover in the taxi industry, and some firms may not report positive receipts or positive expenses in all years. In this robustness test we keep the sample consistent by requiring that firms report both positive receipts and positive expenses during the analysis window. Doing so allows us to test whether the differences in the results are driven by the differences in the sample composition or not.

Panel A of Table 4 presents the results. As seen in column 1 and column 2, the number of observations is the same for receipts and expenses. The point estimates are also very similar to the baseline result. Thus, we conclude that the sample differences are not driving the main results. We also use this new sample restriction to re-estimate our event study estimates reported in Fig. 3 and other outcomes such as taxable income, tax liability, and profit reported in Table 3. The re-estimated event study results are presented in Fig. 8 and the re-estimated results for other additional outcomes are presented in Table 4. The event study is very similar to the baseline results. Similarly, all additional outcomes remain statistically insignificant and similar in magnitude. Note that we do not have a consistent sample size for these additional outcomes. As the number of households in the income range reporting zero tax liability is very high, doing so would significantly decrease our sample size, so the sample composition would drastically change if we try to keep the number of observations the same across these additional outcomes of interest. Therefore, in Panel B of Table 4, we restrict the sample to the firms belonging to households that always report positive taxable income and positive tax liability (i.e., specifications with the lowest number of observations). As seen in column 4 and column 5, the number of observations is the same for taxable income and tax liability. The main results are still qualitatively similar to our baseline estimates; that is, we find that both total receipts and total expenses increase. However, for this small subset of households, we find a slightly larger increase in total receipts and a slightly smaller increase in total expenses, which leads to a statistically and economically significant increase in profit. However, taxable income and tax liability still remain similar in magnitude and statistically insignificant. Thus, even in the sample that always reported positive taxable income and positive tax liability, the overall results remain similar to the baseline results.

Table 4

Robustness test: Impact of third-party income reporting on log of several outcome variables using a more consistent sample size across specifications

	Receipts	Expenses	Profit	Taxable Income	Tax Liability
Panel A: Restricted to Positive Receipts and Expenses					
Credit Card X 1099-K	0.059***	0.079***	0.016	0.014	0.012
	(0.021)	(0.022)	(0.022)	(0.022)	(0.025)
Credit Card	-0.019	-0.013	0.030	-0.030	-0.032
	(0.034)	(0.037)	(0.024)	(0.026)	(0.033)
Lyft Present	0.001	-0.004	0.005	0.011	0.016
	(0.013)	(0.015)	(0.017)	(0.017)	(0.023)
Uber Present	-0.002	-0.004	0.005	0.005	-0.003
	(0.011)	(0.012)	(0.019)	(0.016)	(0.020)
Number of Observations	1,303,221	1,303,221	1,115,753	454,021	414,740
Panel B: Restricted to Positive Taxable Income and Tax Liability					
Credit Card X 1099-K	0.076***	0.056***	0.053**	0.019	0.019
	(0.019)	(0.019)	(0.026)	(0.020)	(0.022)
Credit Card	-0.000	0.009	0.043	-0.030	-0.030
	(0.025)	(0.024)	(0.028)	(0.028)	(0.033)
Lyft Present	-0.006	0.012	-0.015	0.011	0.013
	(0.019)	(0.017)	(0.019)	(0.019)	(0.022)
Uber Present	0.001	0.001	0.005	-0.001	-0.005
	(0.017)	(0.016)	(0.019)	(0.017)	(0.019)
Number of Observations	487,118	429,425	387,736	504,195	504,195

Notes: This table shows the impact of third-party income reporting on the log of reported receipts, expenses, profit, taxable income, and tax liability using difference-in-differences. The sample in Panel A is restricted to firms with positive receipts and positive expenses throughout the years analyzed, while the sample in Panel B is restricted to firms with positive taxable income and positive tax liability throughout the years analyzed. The treatment is defined as the interaction of the introductions of local ordinances mandating credit card reader in taxicabs with federal Form 1099-K implementation in 2011. All specifications include city, year, and taxpayer fixed effects. The standard errors, in parenthesis, are clustered at the city level. * p < .1, ** p < .05, *** p < .01.
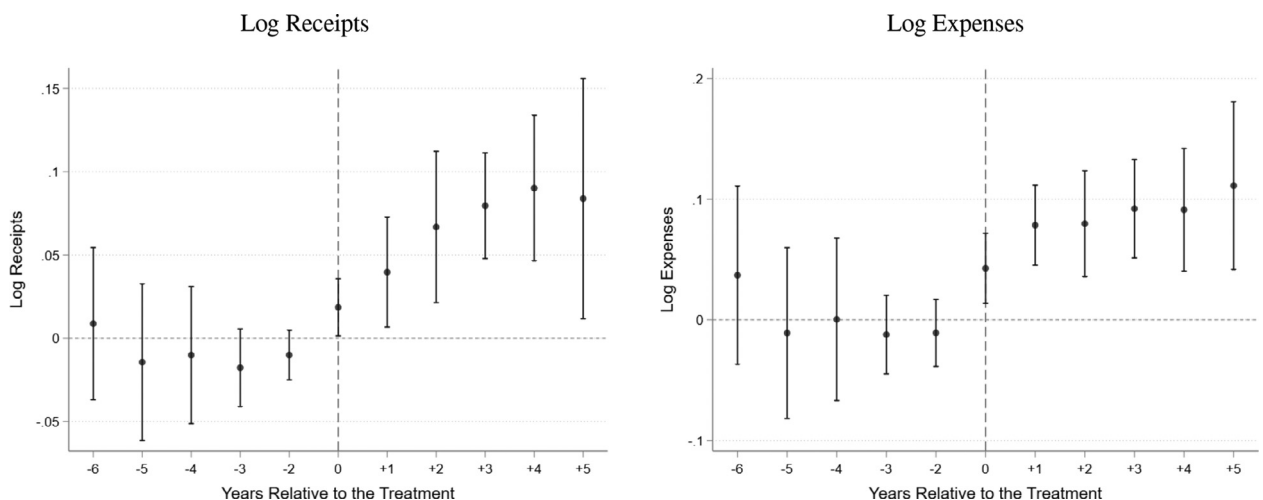

Fig. 8. The event study estimates of the impact of third-party reporting on the log of total receipts and total expenses using a more consistent sample size across specifications

Notes: The figure shows the impact of third-party reporting on the log of total receipts and total expenses and the 95 percent confidence interval for the 12-year window around the treatment. The sample is restricted to firms with positive receipts and positive expenses throughout the years analyzed. The treatment is defined as the interaction of the introductions of local ordinances mandating credit card reader in taxicabs with federal Form 1099-K implementation in 2011. The year immediately before the treatment is omitted. The specification includes city, year, and taxpayer fixed effects as well as dummy variables indicating the presence of Uber and Lyft. The standard errors are clustered at the city level.

Using the Goodman-Bacon (2021) decomposition. In an important recent paper, Goodman-Bacon (2021) proves that, when the treatment assignment is staggered over time across the cities, as in our case, the two-way fixed effects estimator (i.e., aggregate DID coefficient) represents a weighted average of all potential two-group and two-period comparisons (i.e., 2×2 DID comparisons), where the weights are based on group sizes and variance in treatment. The potential 2×2 DID estimates are based on what is being used as a control group, and some of the important potential DID estimates are: (i) a group treated later serves as the control group for an earlier treatment group; (ii) a group treated earlier serves as the control group for a later treatment group; (iii) a group that never receives the treatment serves as the control group; and (iv) a group treated prior to the start of the analysis serves as the control group. Goodman-Bacon (2021) demonstrates that it is important to decompose the aggregate DID estimates into these 2×2 DID estimates because the aggregate DID effect can be biased, especially if treatment effects are time-varying. The bias usually comes when a group treated earlier serves as the control group for a group treated later. However, if there are large groups of never-treated cities, like in our case, then this can significantly reduce any such bias. The Goodman-Bacon (2021) decomposition also allows us to inspect which group of 2×2 DID contribute most to the aggregate treatment effect. In our setting, it is likely that the group of cities that passed the local ordinances before 2011 contribute the most to the treatment effect for two reasons. First, this group of cities falls in the middle of the sample window, so it has the highest treatment variation, and it will contribute the most to the aggregate treatment effect. Second, 42 percent of our treated cities turns from untreated to treated in 2011, so these cities will also make up a larger share of the aggregate treatment effect.

The Goodman-Bacon (2021) decomposition requires a strictly balanced panel, and, in the current version of the software program, the specification with control variables does not produce the 2×2 DID estimates discussed above. Therefore, we use a strictly balanced panel from 2009 to 2016 without control variables to run the Goodman-Bacon (2021) decomposition.[24]

The results are presented in Fig. 9, which plots the aggregate DID estimate and three 2×2 sub-components from each of the ten iterations as well as the average of them. The X-axis plots the weights, and the Y-axis plots the 2×2 DID estimates for three groups: early treated as the treatment group versus later treated as the control group, later treated as the treatment group versus early treated as the control group, and ever treated as the treatment group versus never treated as the control group.

We find that our results are not affected by the issues highlighted by Goodman-Bacon (2021) because we find that the comparison of ever treated group versus never treated group contributes more than 70 percent of the aggregate DID effect, while the bias-inducing 2×2 DID (i.e., later treated versus earlier treated) only account for less than 15 percent of the aggregate DID effect. Thus, as seen in Fig. 9, our aggregate estimates are very similar to ever treated group versus never treated group as well as earlier treated versus later treated, but quite different than the bias-inducing later treated versus earlier treated. The estimates from later treated versus earlier treated are in general positive but smaller for both receipts and expenses. Thus, correcting for the bias generated by that comparison would likely slightly increase the size of our baseline estimates.[25]

8. Conclusions

In this paper, we analyze taxpayer responses to the introduction of Form 1099-K. We find that firms report more receipts after the introduction of Form 1099-K. However, we also find that the increase in reported revenues was accompanied by an offsetting increase in expenses, resulting in no significant increase in taxable income.

Our results provide new insights on the impact of Form 1099-K in the U.S. Even though our results appear qualitatively similar to Slemrod et al. (2017) and Adhikari et al. (2021), we are able to exploit a more robust identification strategy that allows us to find quantitatively larger, statistically significant, and more robust impacts than either of these studies. For instance, Slemrod et al. (2017) do not find any impacts on receipts or expenses on their full sample of firms; they only find an impact for around ten percent of the firms with the 1099-K receipts as a share of total receipts close to one, which they interpret as the sample of firms that are most likely to evade taxes; and in the small subset of the firms that are likely to evade taxes, they find that a 1 percent increase in reported receipts leads to a 0.82 to 0.96 percent increase in reported expense. In contrast, we find that a 1 percent increase in reported receipts leads to a much larger 1.2 percent increase in reported expenses in our full sample of taxicabs. Adhikari et al. (2021) find a significant but modest impact on

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[24] The program also gives error messages for our baseline sample due to the large size of the dataset and the large number of cities we analyze. Therefore, we restrict the analysis to a 25 percent random sample of balanced panel of firms from 2009 to 2016 belonging to the top 300 cities, and we repeat this procedure ten times to test the sensitivity of the results to the 25 percent random sampling. The aggregate DID estimate and three 2x2 Sub-components from each of the ten iterations as well as the average of them are reported in Fig. 9. Since the taxi industry has a very high turnover and our original sample window spans 15 years (i.e., 2002-2016), running the balanced panel analysis would substantially reduce our sample size. Therefore, we reduce the sample years to span 2009-2016 so that the sample starts at least 2 years before the first year of treatment for the early-adopting cities and covers all later-adopting cities. Note that, as discussed in the robustness tests above, our baseline results are similar to both the specification using a balanced panel and the specification without control variables.

[25] This issue is also highlighted in several recent papers by Cengiz et al. (2019), Callaway and Sant'Anna (2021), and Sun and Abraham (2021). These papers all provide new estimators that deal with the bias from staggered DID by estimating event-study DID specifications and modifying the set of effective control groups in the treatment effect estimation process, thereby ensuring that firms receiving treatment are not used as a control group. However, the proposed methods in these papers differ in various ways, especially regarding which observations are used as effective control groups and how the control variables are incorporated in the analysis. We believe that our aggregate results would likely be qualitatively and quantitively similar if we estimated them using these other new methods because we find that the bias-inducing 2x2 DID only account for less than 15 percent of the aggregate DID effect.
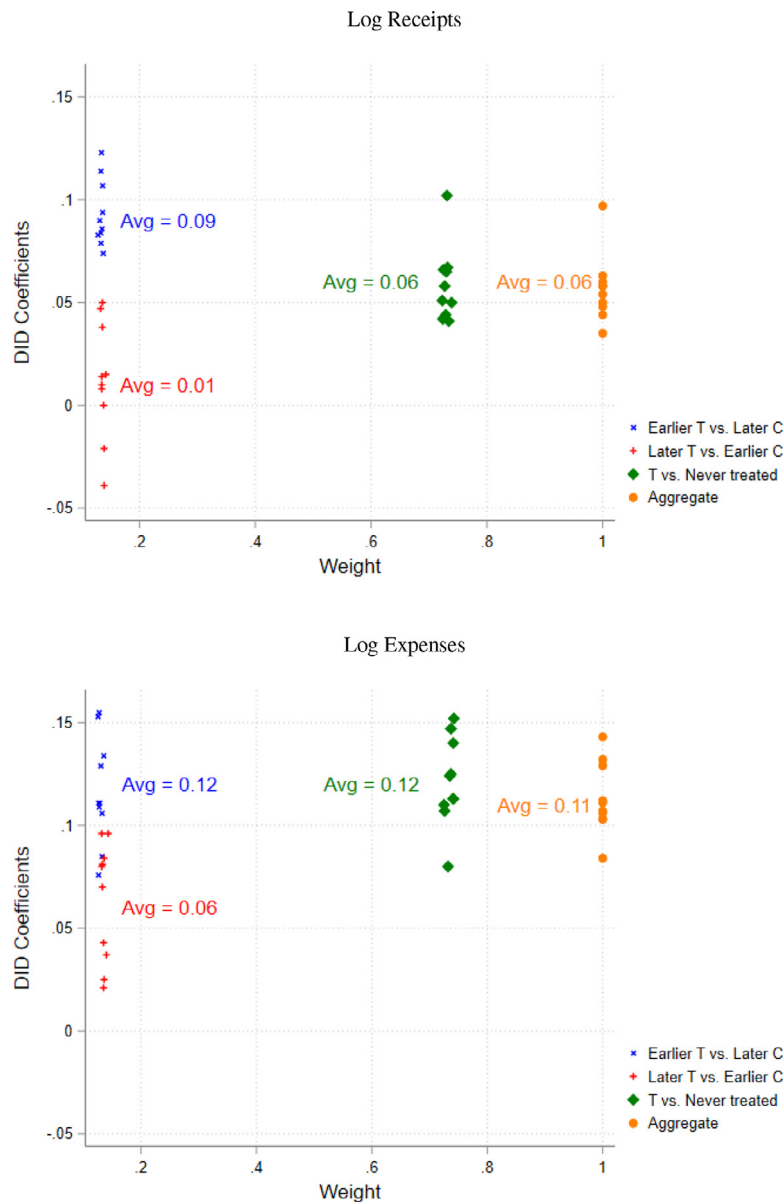
Fig. 9. The Goodman-Bacon decomposition of aggregate DID estimate into three 2×2 sub-components

Notes: The Figure plots ten iterations of the aggregate two-way fixed-effects DID estimate and its 2×2 DID sub-components using the 25 percent random sample of balanced panel of firms from 2009 to 2016 belonging to the top 300 cities. The X-axis plots the weights, and the Y-axis plots the 2×2 DID estimates for three groups: early treated as the treatment group vs. later treated as the control group, later treated as the treatment group vs. early treated as the control group, and ever treated as the treatment group vs. never treated as the control group. It also provides the average value of ten iterations for the aggregate DID and each sub-components.

receipts on their full sample of firms, and they also find substantial heterogeneity across business type, business size, and industry. However, they do not find a significant impact on expenses for their full sample or for many of their subgroups, implying a modest increase in tax compliance even among the offsetting firms. Our results are much closer to the findings of Carrillo et al. (2017), who find that a 1 percent increase in reported receipts in response to "notice of discrepancies" letters from Ecuador's tax authorities leads to 0.96 percent increase in reported expenses.

Overall, then, the results of Adhikari et al. (2021) suggest that Form 1099-K modestly increased tax compliance on average. However, the results of Slemrod et al. (2017) indicate that Form 1099-K was not very successful in increasing tax compliance among the subset of firms that are the most likely evaders, and our results suggest the same for the taxi industry. The taxi industry is typically characterized by a higher rate of tax evasion than other industries. It is also the industry that Form 1099-K was intended to target as taxis are typically small businesses in cash-intensive industries with unreported

gross income that is difficult to audit at scale. Thus, our results suggest that Form 1099-K was successful in increasing reported receipts among firms intended to be affected by 1099-K. Even so, our results indicate the Form 1099-K still did not result in much higher tax compliance due to offsetting behavior by these firms. The ability of a business to shift its reporting along different margins of behavior not covered by third-party reporting may therefore limit the overall compliance impact of the programs like Form 1099-K because it largely covers receipts, but it provides no additional information on expenses. Even companies that are required to accept credit cards may still transact some of their business in cash, and cash remains uncovered by any third-party information reporting, thereby providing an avenue by which income may be under-reported.

Our results suggest that Form 1099-K will lead a firm to report a larger share of true receipts to the IRS over time, given the rising trend towards the use of electronic payment methods that are covered by Form 1099-K. Moreover, they suggest that introducing tax incentives to encourage businesses and consumers to use credit cards, like in Greece and South Korea, could increase tax compliance by making Form 1099-K more effective. However, as long as businesses have some margins of behavior that are not covered by third-party information reporting, the ability of such reporting requirements may not achieve their intended outcomes. Thus, the introduction of third-party expense reporting, as has been proposed by the Biden administration recently, is an important step to increasing tax compliance by limiting the offsetting response that our results demonstrate (U.S. Treasury, 2021).

Declaration of Competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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