



Research, Applied Analytics,
and Statistics



TAX POLICY CENTER
URBAN INSTITUTE & BROOKINGS INSTITUTION

8th Annual IRS/TPC Joint Research Conference on Tax Administration

June 20, 2018



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and Statistics



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Welcome

Eric Toder

Institute Fellow, Urban Institute, and
Codirector, Urban-Brookings Tax Policy Center

Ben Herndon

Chief Research and Analytics Officer, IRS



Session 1. Factors That Contribute to Voluntary Compliance

Moderator:

Jessica D'Itri
IRS, RAAS

Does Employer Withholding Affect Tax Compliance, and Why?

William Boning
University of Michigan

Accountants are not tax advisors: Using random audit data to test the effect of certified accountancy on SME's tax compliance in Denmark between 2008 and 2014

Johanne Søndergaard
Danish Customs and Tax Administration

Compliance Spillovers Across Taxes: Results from a Field Experiment

Carlos Scartascini
Inter-American Development Bank

Discussant:

Brian Erard
B. Erard & Associates

Does Withholding Change Taxpayer Behavior, and Why?

William C. Boning (wcboning@umich.edu)

IRS-TPC Conference

June 20th, 2018

Any opinions and conclusions expressed herein are those of the author(s) and do not necessarily represent the views of the Internal Revenue Service. All results have been reviewed to ensure that no confidential information is disclosed.

Why Withhold Income Tax?

- Even conditional on third-party income reporting, withholding correlates with compliance (IRS Tax Gap Estimates for 2010-2012; Kleven et al. 2011)
- Taxpayers who are underwithheld per a random audit underreport more income than those who are overwithheld (Chang and Schultz, 1990)
- States' introductions of withholding apparently increased revenues by 20% (Dusek and Bagchi 2017)

A Need for More Than Information?

- If tax authorities had perfect information, underreporting would always be detected
- But people wouldn't always turn in the tax
- Noncompliance: underreporting, nonfiling, and underpayment (Mazur and Plumley 2007)
- 14 million taxpayers owe \$138 billion in unpaid tax, penalties, and interest as of 2016 (IRS data book 2016)
- Collection is costly

Research Question

How and why does withholding, tax remittance by employers as income is earned, affect taxpayer behavior: filing, reporting, and remittance?

Sample and Treatment

- Sample panel of primary filers, tax years 2000-2013
- Restrict to households with 2009 wages of \$4,000-\$83,000 (if single) or \$4,000-\$178,000 (if filing jointly)
- Treatment group:
 - received a \$250 Economic Recovery Payment check in May 2009
 - had to repay it with their 2009 tax return
 - Equivalent to a \$250 reduction in withholding
- Control group:
 - did not receive a check
- [The wage restriction is needed to have \$250+ of Making Work Pay tax credit, as the check repayment reduced the credit amount]

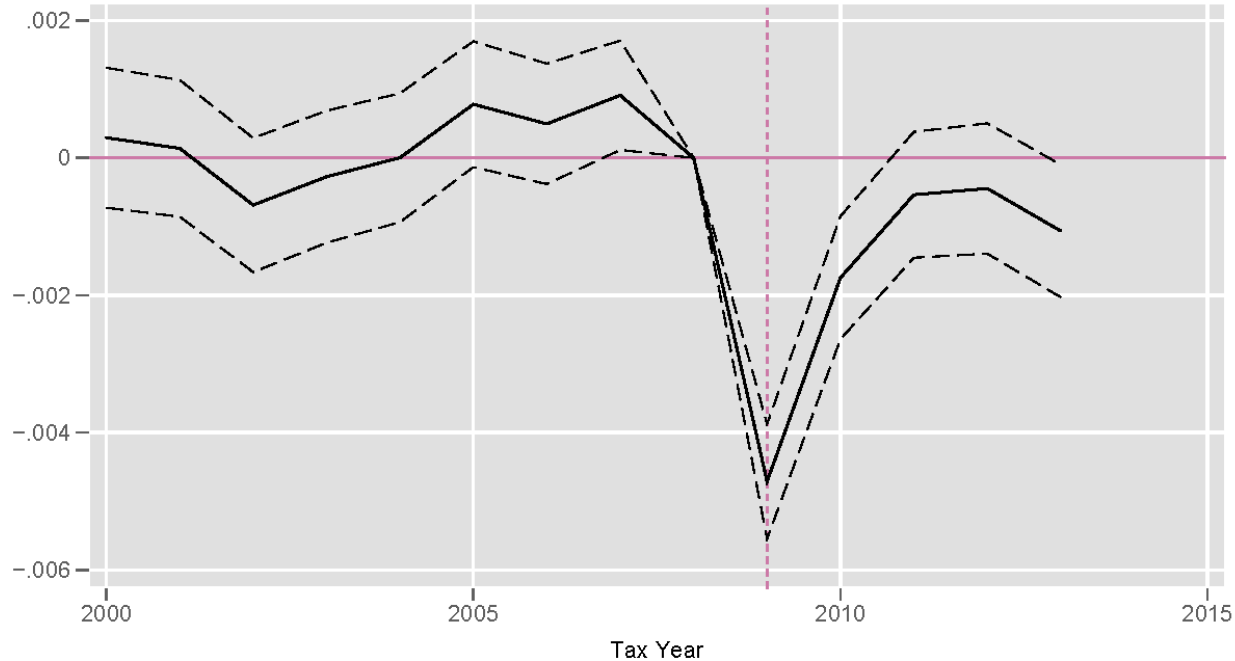
Specification

- Event Study with time-varying controls:

$$Y_{it} = \beta_t * treated * tax\ year_t + \delta_t + \eta_i + \sum_j \gamma_{tj} X_{0ij} + \epsilon_{it}$$

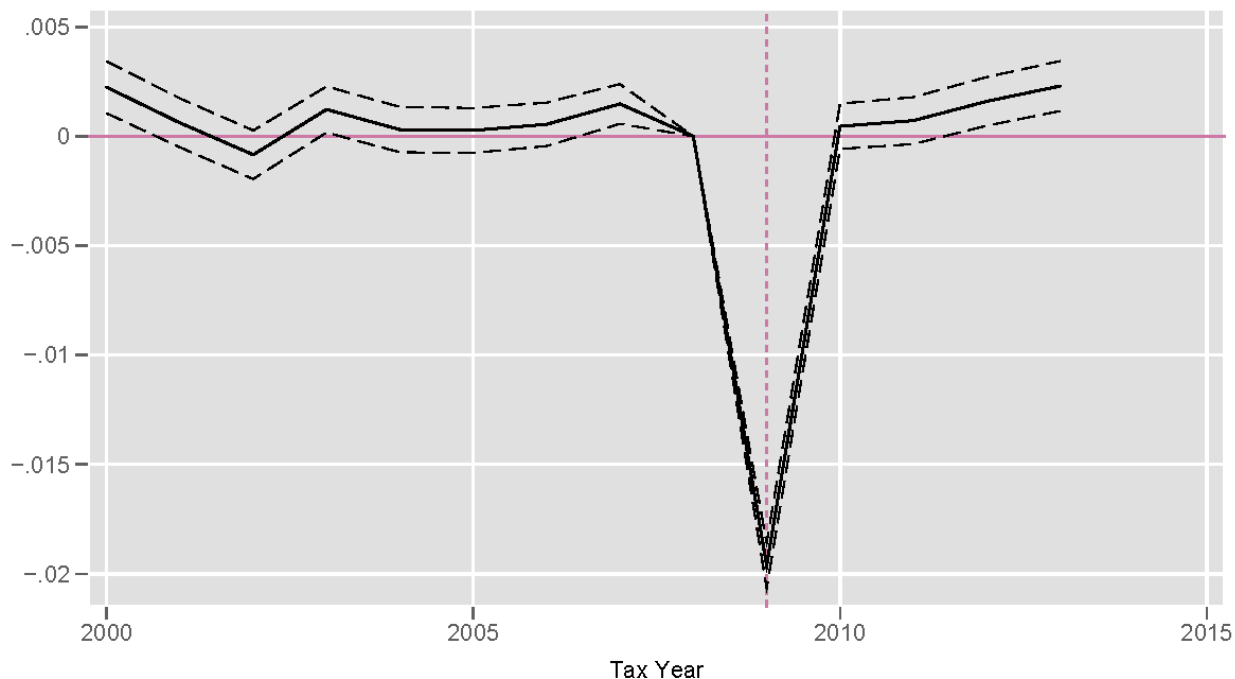
- Time-varying controls for pre-treatment demographics, wages, and balances due
- Conditional on age, one determinant of treatment is whether a taxpayer had chosen to claim social security retirement benefits before February 2009

Timely Filing Indicator Response to \$250 Predicted Increase in Balance Due



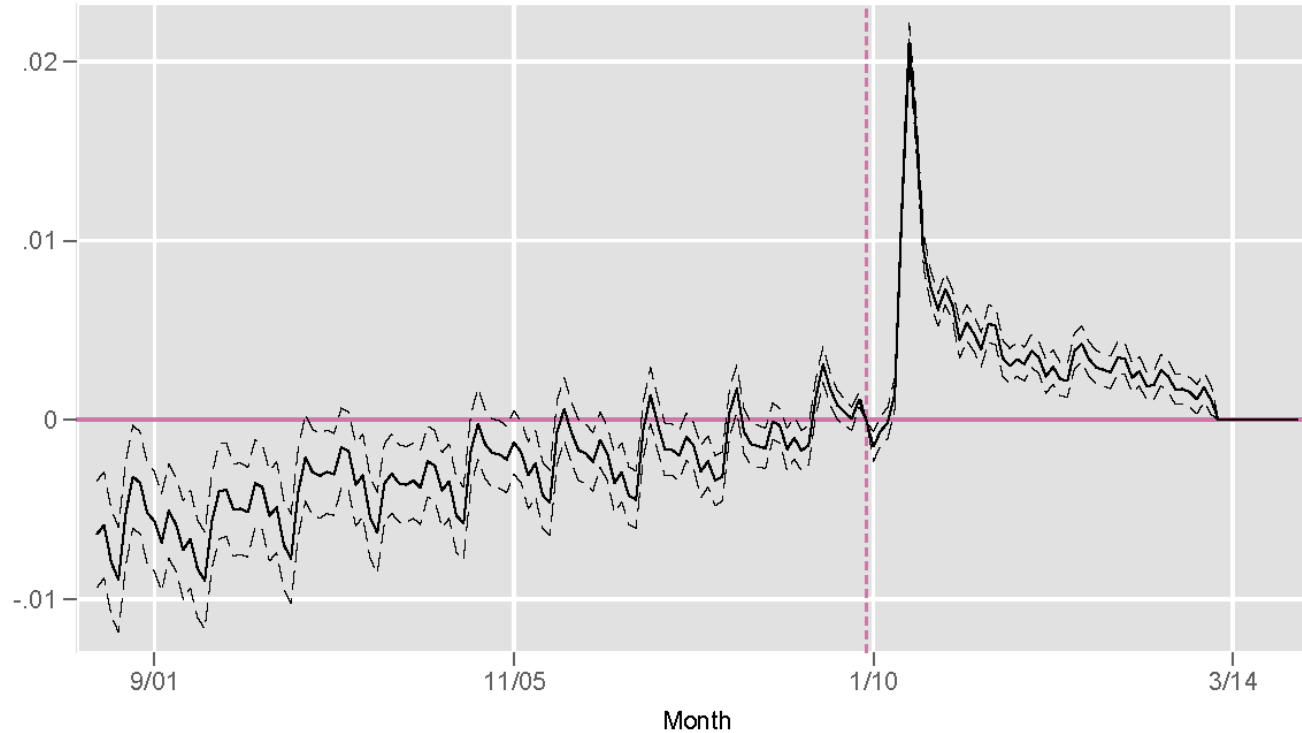
Notes: Dashed lines indicate 95% confidence intervals. Standard errors clustered by taxpayer. Sample is primary earners with substantial wage income. Coefficients are from a regression with taxpayer and year fixed effects and time-varying controls.

Fully Paid Indicator Response to \$250 Predicted Increase in Balance Due



Notes: Dashed lines indicate 95% confidence intervals. Standard errors clustered by taxpayer. Sample is primary earners with substantial wage income. Coefficients are from a regression with taxpayer and year fixed effects and time-varying controls.

Month-by-Month Response of Tax Debt Indicator to \$250 Predicted Increase in Balance Due: Tax debt spikes when 2009 balances are due



Notes: Dashed lines indicate 95% confidence intervals. Controls for linear time trend * treated and month-of-year * treated

Does withholding change reporting?

- Using raw NRP (random audit) data from 2006-2012, I test whether treatment alters underreported income and credits/deductions
- Outcomes:
 - dollar adjustment in audit
 - Net Misreporting Percentage (dollar adjustment/value determined by audit)
- In very early results, I find no statistically significant effect of 2009*\$250 Withholding Reduction on Total Tax or Taxable Income

Mechanisms: Liquidity

- Taxpayers may underpay because they lack liquid assets to smooth consumption and pay in full
- I test if results differ by subgroups:
 - With/without interest income in past year or all four most recent years (i.e. bank account assets)
 - With/without dividend income in past year (i.e. equity assets)
 - Whether household owns a home
- I find no differences in effects across these subgroups

Mechanisms: Mistakes

- Taxpayers made lots of mistakes:
 - 5% of the control group and 6.2% of the treatment group didn't file Schedule M to claim the Making Work Pay credit at all
 - 3.4% of the treatment group did not tick the relevant box to repay the \$250, an additional 1% used the wrong check amount

Mistakes Correlate with Underpayment

Correlations Between Mistakes in Computing Credit and Whether Fully Paid in 2009, by Treatment Group

	Control Group	Treatment Group
Any mistake related to credit	-0.0408	-0.213
Did not file credit, IRS filed using return information	-0.0374	-0.0181
Amount changed because did not use ERP check in computation	-0.00654	-0.245
Amount changed because used incorrect ERP check amount	-0.00961	-0.0224

Conclusion

- A quasi-experimental \$250 increase in balance due at tax filing led to:
- 0.5 percentage points more late filing
 - a 10% increase vs. control group in same year
- 1.9 percentage points more incomplete payment
 - a 52% increase vs. control group in same year
 - half fully repaid in 6 months
- Effects do not depend on measures of liquidity
- Underpayment correlates with mistakes
- No identified detected effects on reporting

Danish Accountants Are Not Tax Auditors

Using random audit data to test the effect of certified accountancy on SME's tax compliance in Denmark between 2008 and 2014

Dr Johanne Søndergaard
IT and Development Agency of the Danish
Ministry of Taxation
June 20, 2018
IRS- TPC conference
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SAMFUND

Brian Mikkelsen efter afsløringer og massiv kritik: Det har været en normal proces



17. maj 2018, 19:15
of Emil Ellesee Ditzel

MEST SETE PÅ TV2.DK



Ministerium lovede ingen negative konsekvenser – nu kritiseres lov f at koste formue i skat



For meget fusk og skattefifleri? Små virksomheders bløde regnskabsregler bør kulegraves

Det bør undersøges, om de gentagne lempelser af revisionspligten giver mere fusk og skattefifleri. Det siger to revisionseksperter. Den svenske Riksrevision anbefaler, at revisionspligten af samme grund genindføres.

ERHVERV | 12.01.2018 KL. 06:00



15.11.2017

Præsentation af lovforslaget om lempelse af

112.000 virksomheder har fravalgt revision

Pressemeddelelse | Det har konsekvenser, når virksomhederne fravalger revision og andre revisorerklæringer med sikkerhed. I sidste ende kan det gå ud over virksomhedernes mulighed for at tiltrække investeringer og kapital, vurderer Tom Vile Jensen, erhvervspolitisk direktør i FSR – danske revisorer.

Revision

På kun 10 år er antallet af virksomheder, der har fravalgt revision og andre revisorerklæringer med sikkerhed vokset fra bare 12 virksomheder i 2006 til over 112.000 virksomheder i 2016. Virksomhedernes fravalg af revisionspligten kommer i kølvandet på hele tre lempelser i revisionspligten på 10 år. Første gang i 2006, hvor virksomheder med en omsætning på op til tre mio. kroner fik lov at fravælge revisoren. Siden er revisionspligten lempet yderligere to gange, så alle virksomheder med en omsætning på op til otte millioner kroner i dag helt kan fravælge. Yderligere kan virksomheder med en omsætning på mellem otte mio. kroner og 89 mio. kroner i dag vælge om de vil have revision eller det mindre omfattende produkt 'udvidet gennemgang' af regnskabet.

Det viser en ny analyse "Anmærkninger fra revisor og mistede oplysninger ved fravalg af revision", som FSR – danske revisorer har udarbejdet på baggrund af over 252.000 årsregnskaber for 2016.

Figur: Udviklingen i antal selskaber, der har udvidet gennemgang af regnskabet

Pressemeddelelser 2018

Pressemeddelelser 2017

Pressemeddelelser 2016

Pressemeddelelser 2015

Pressemeddelelser 2014

Pressemeddelelser 2013

Pressemeddelelser 2012

Pressemeddelelser 2011

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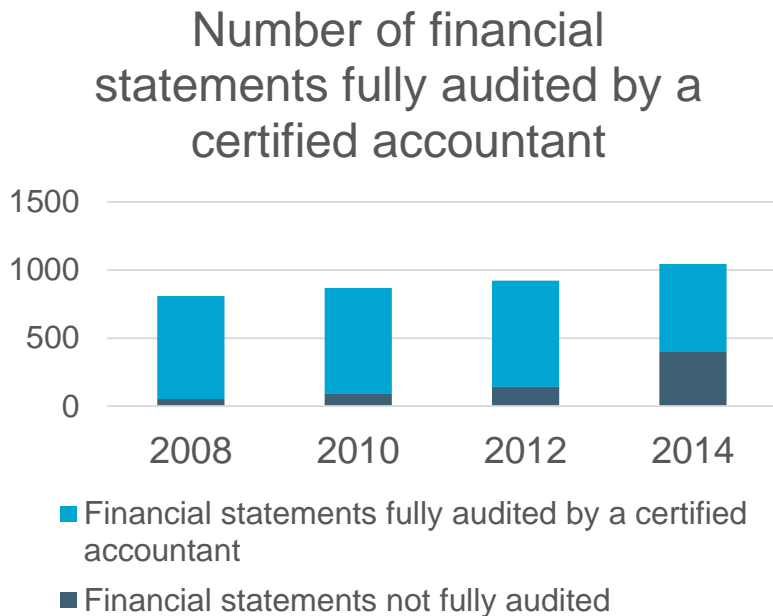
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3369 1047
4193 3147

Nyheder om revision

23.05.2018
Faglig Hotline og de tunge spørgsmål II: Håndtering af mangelfuldt bevis og urent trav

Relaxing the audit requirement

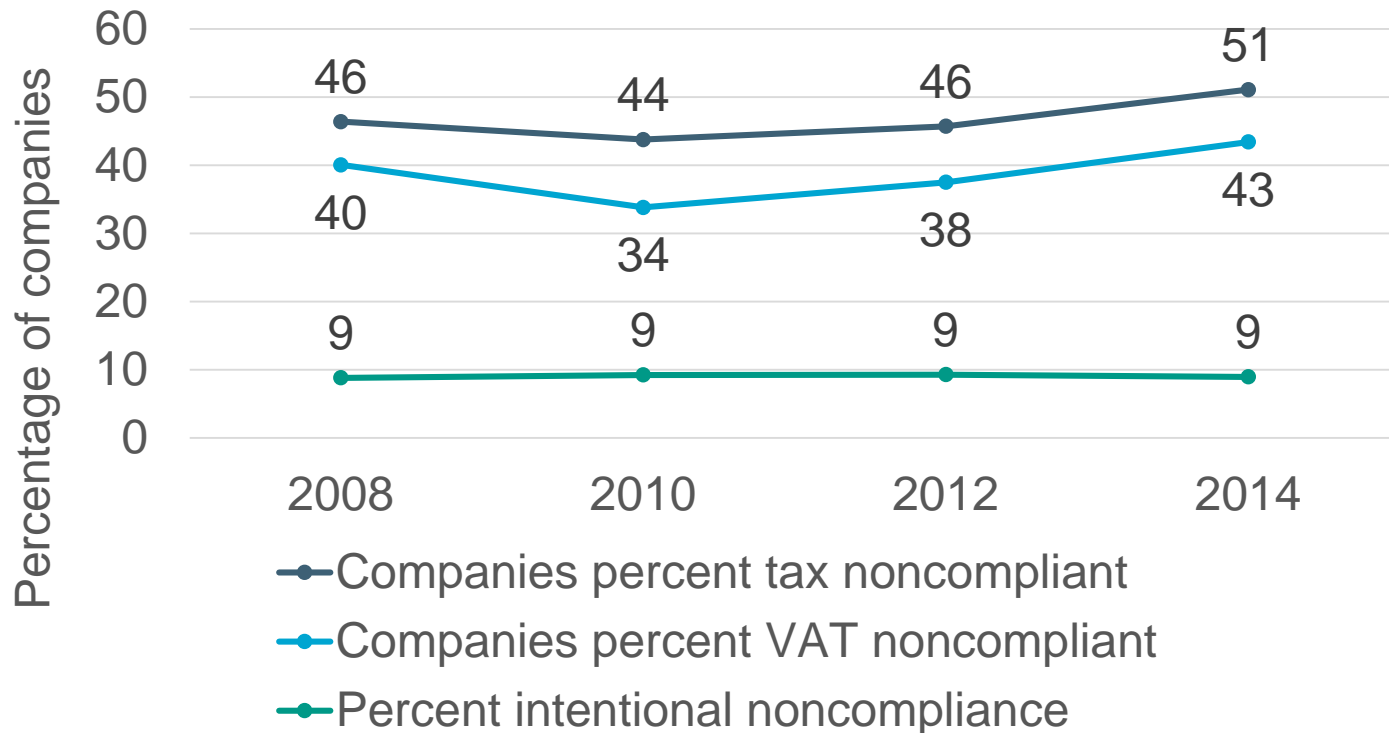


- Several rounds of the easing the audit requirement.
- Most encompassing change from 2012-2014
- Now limited companies and limited liability companies can opt out if they meet two of the following requirements:
 - a balance sheet total of less than 4 million DKK (640,000 USD)
 - a net turnover of less than 8 million DKK. (1,300,000 USD)
 - an average of fewer than 12 full-time employees.
- Even if it is a holding company
- In tax year 2014, 38% of companies opted out – not all of these could opt out. 44% of those that could opt out, did.

The role of third party agents in noncompliance

- “**Enforcer/ambiguity-exploiter**” model (Klepper et al 1991: 218)
- **Unambiguous** incomes: legal deterministic, e.g. salary and most interest incomes.
- **Ambiguous** incomes on the other hand or those where the amount is not unequivocally prescribed by law, e.g. self-employment incomes and capital gains incomes (Klepper et al 1991:211)
- Theory: the role of exploiter when tax laws are ambiguous, but as enforcers when tax laws are unambiguous.
- **Maximize after-tax income** in areas of ambiguous income using their “unique knowledge of reporting strategies” (1991:228).
- Where tax preparers can make a reasonable argument for the legality of a possibly noncompliant approach.
- Assist taxpayers in structuring their return in such a way as to increase the after-tax income, but it is **neither in the taxpayer nor the preparer’s interest to be penalized for noncompliance**

Rates of tax and VAT noncompliance and intentional noncompliance of Danish companies with 0-250 employees



Research question

What is the effect of the use of certified accountants on small and medium-sized enterprises' tax and VAT noncompliance in Denmark?



Data from enterprise compliance study

Enterprises with 0-250 employees including

- Companies
- Self-employed

Population: 579,000 enterprises.

Random sample every 2 (now 3) years

Use of data just for companies
from 2008-2014

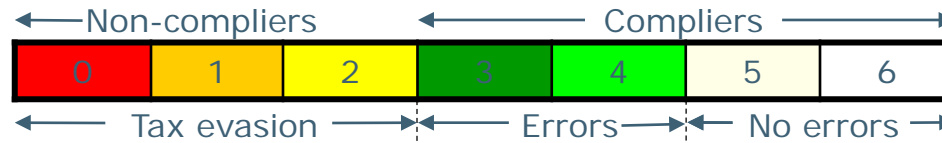
E.g. tax year 2014:

- **2,828** enterprises
 - 1,081 companies
 - 1,747 self-employed



Compliance rating

- Evaluation of tax auditor of **VAT and tax errors** based on a decision tree



- Compliance rating – measuring prevalence and intentionality of VAT and tax errors
- Rating 0-2: intentional noncompliance – large errors in the business' favor.

Hypotheses



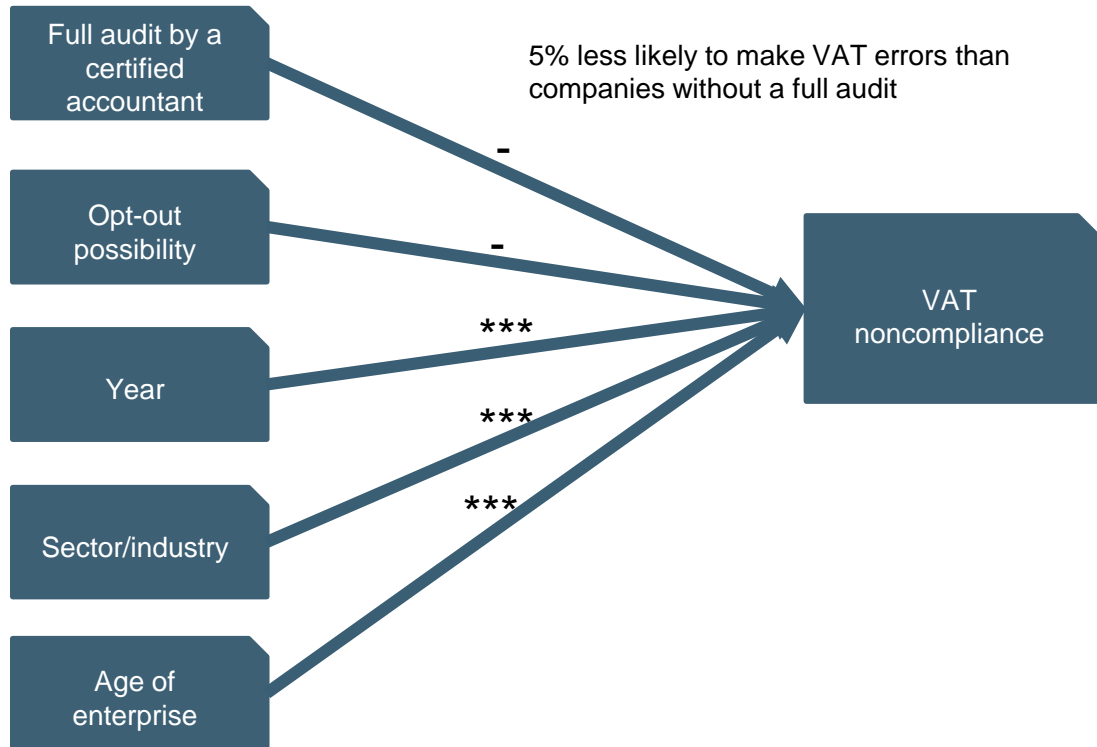
H1: A full audit by a certified accountant is associated with lower VAT noncompliance.

H2: A full audit by a certified accountant is associated with higher tax noncompliance.

H3: A full audit by a certified accountant is associated with lower intentional noncompliance.

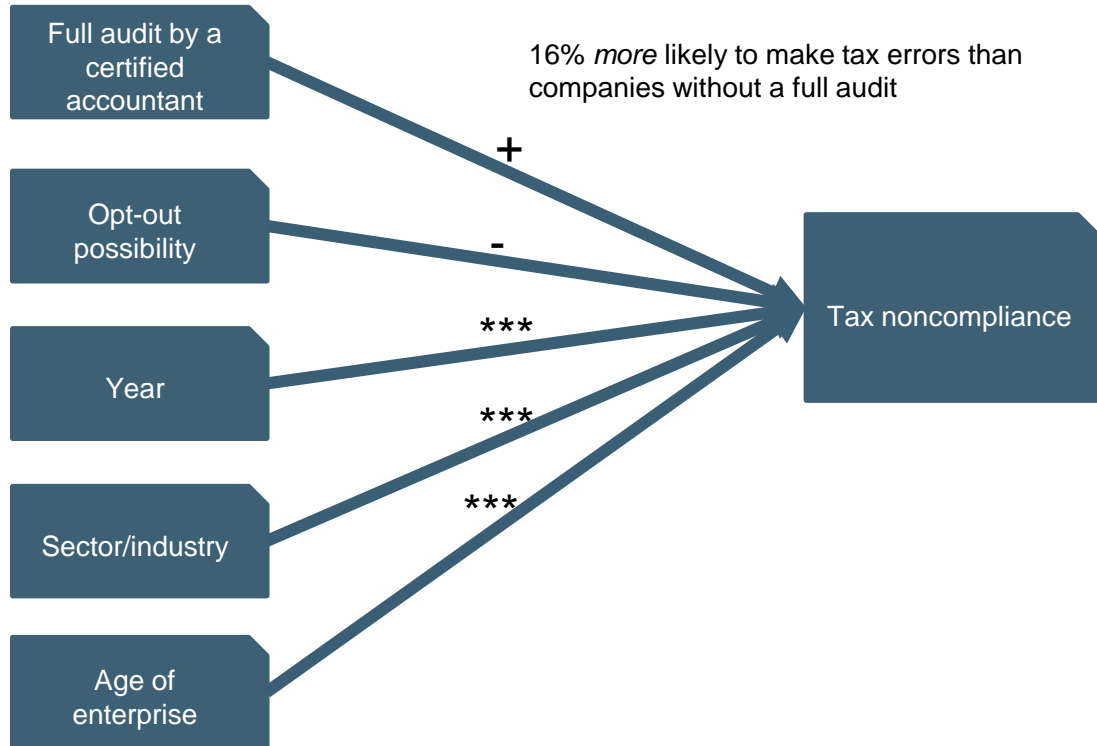
Results of logistic regression analysis

– VAT noncompliance



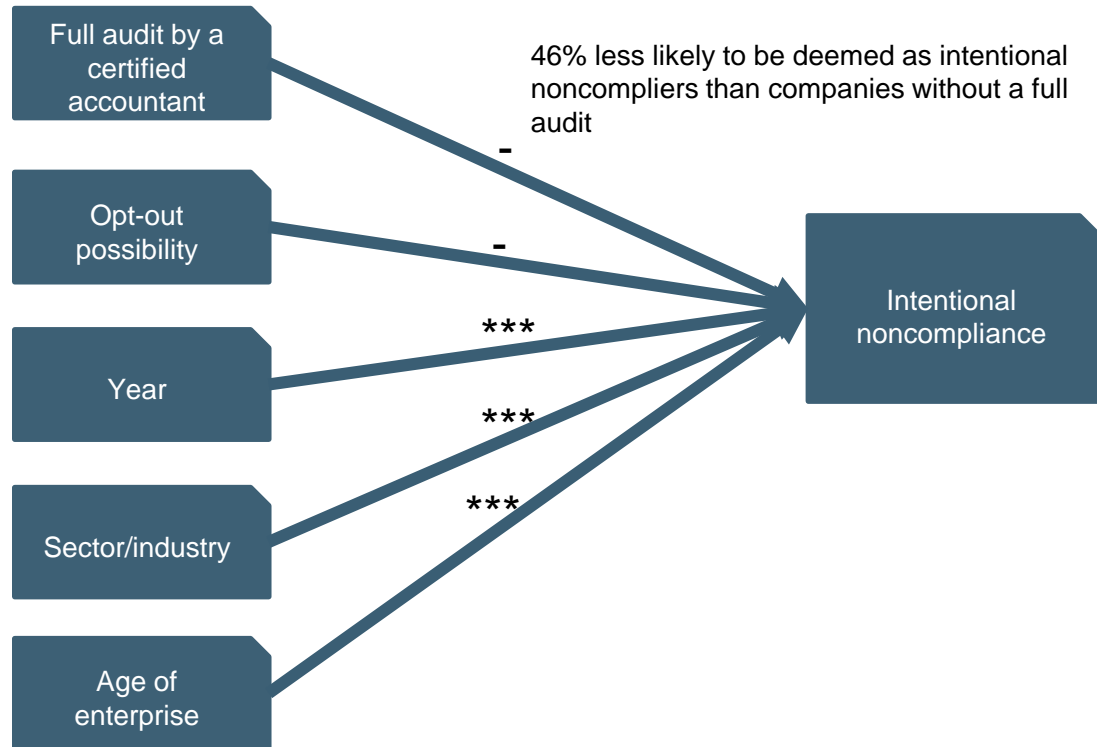
Results of logistic regression analysis

– tax noncompliance



Results of logistic regression analysis

– intentional noncompliance



Conclusions

Commissioner Chris Jordan of the Australian Tax Authority said this year: ‘For years I’ve heard how tax agents were **guardians of the system** – these random enquiry results tell me this is not the case for some agents’

Results from Denmark seem to indicate that Danish certified accountants could be considered ‘some agents’

The paper finds support for the theory of Klepper et al. (1991), namely that in more ambiguous tax legislation, accountants can act **as ambiguity-exploiters**, but within the more limiting confines of VAT legislation, they act as **VAT-enforcers**.

The results support the claim that accountants work mainly **in the interests of their clients**. They aid in income maximization where there is room to do so, but they do so in a way that does not appear as intentional noncompliance.



Compliance Spillovers Across Taxes. Results From a Field Experiment

Carlos Scartascini* Andrea Lopez-Luzuriaga*[†]

*Inter-American Development Bank

[†]George Washington University

IRS - TPC
June 20, 2018

Motivation

- Taxpayers who receive a deterrence message from the tax authority tend to increase tax compliance.
- Most of this literature has focused almost exclusively on the direct effect of the interventions.
- However, most taxpayers are liable for several taxes.
- What happens when the tax authority increases enforcement in one tax with compliance in other taxes?
 - Does enforcement in one tax creates positive spillovers (higher compliance in all taxes) or
 - Do taxpayers compensate across taxes to keep their total bill unchanged?

We evaluate the effect of increasing the salience of deterrence (penalties and enforcement) in the property tax on the tax declaration of the gross-sales tax.

Literature

- Deterrence message from the tax authority promotes tax compliance (Slemrod, Blumenthal, and Christian, [2001](#); Kleven et al., [2011](#); Chirico et al., [2015](#); Brockmeyer, Kettle, and Smith, [2016](#); Doerrenberg and Schmitz, [2017](#); Meiselman, [2018](#)).
- An increase in monitoring has a positive effect on compliance (LaLumia and Sallee, [2013](#); Almunia, Rodriguez, and David, [2015](#); Naritomi, [2016](#)).
- When taxpayers have limited attention, messages that raise the salience of fines and legal action could increase compliance (Bernheim and Rangel, [2007](#); Bernheim and Rangel, [2009](#); Castro and Scartascini, [2015](#); Chirico et al., [2017](#)).
- There is mix evidence of spillover effect.

Literature

- Across margins of the same tax: Carrillo, Pomeranz, and Singhal [\(2017\)](#) and Slemrod et al. [\(2017\)](#) show a **negative** spillover effect across margins of the same tax.
- Across individuals: Pomeranz [\(2015\)](#) **positive** spillover effects up the value-added-tax chain. Drago, Mengel, and Traxler [\(2015\)](#) show a **positive** spillover effect from treated to 'untreated' neighbors from a field experiment which varied the content of mailings sent to potential evaders of TV license fees. Boning et al. [\(2018\)](#) show **positive** spillover transmitted through tax-preparer networks. Carrillo, Castro, and Scartascini [\(2017\)](#) find evidence of spillovers across neighbors in a setting of positive incentives instead of deterrence.
- Across related taxes: Ortega and Scartascini [\(2015\)](#) show a **positive** spillover but in the context of tax delinquencies.

Literature

- Deterrence messages that increase the salience of penalties and the stringency of enforcement in one tax increase compliance with that tax
- Spillovers could be positive or negative.
 - Under some conditions, there seems to be positive spillover effects.
 - Or, taxpayers try to maintain the overall tax bill constant and adjust their tax declarations accordingly.

► [Analytical Model](#)

Background

- Castro and Scartascini [\(2015\)](#): Large field experiment designed to test the determinants of compliance with the property tax (“Public Space Conservation Tax”- CVP) in the municipality of Junín in Argentina. A message was included on the property tax bill.
- Approximately 23,000 taxpayers were randomly divided into three treatment groups and one control group.
 - Deterrence: beliefs about enforcement and fines).
 - Peer effects: beliefs about other taxpayers’ behavior.
 - Reciprocity: beliefs about the use of resources by the government.
- The deterrence message increased compliance rates in 12%.
- Message was sent in August 2011. Treatment period Sept-Oct 2011 (bim 5)
- Subsample: sole proprietors that are liable for a gross-sales tax (“Safety and Hygiene Inspection Tax” - SEH)
 - Control group: 608 individuals
 - Deterrence treatment: 115 individuals

Tax Characteristics - Differences

- Property Tax - CVP:
Tax is billed by the city - No informational asymmetries between the government and the taxpayer.
Important margin: whether to pay or not.
- Gross-sales Tax - SEH:
The tax is calculated based on reported margins - Informational asymmetries between the government and the taxpayer.
 - The gross monthly sales - reported once a month
 - The number of employees and the size of the establishment where the economic activity is developed - reported once a year.

Within a calendar year, the variable component of the tax is a function of the sales.
Important margin: reported sales.

Tax Characteristics - Similarities

- Most taxpayers pay every two months, and there are two due days for each tax.
- The CVP is paid in the first month of each calendar bimester, and the SEH is paid in the second month of the bimester.
- Fine: A cumulative compound monthly interest rate of 2% is applied to the outstanding liabilities.

Mechanisms - Deterrence Message

JUNÍN
GOBIERNO LOCAL

C.V.P. (Bimestral)

RECIBO: [REDACTED] PERIODO: 05/2011

PAGO ELECTRONICO: 010-00000572600000

DETALLE:

¿Sabía Usted que si no paga a tiempo el CVP, para una deuda de, por ejemplo, 1.000 pesos deberá pagar 268 pesos adicionales a fin de año, y el municipio puede llegar a intimarlo administrativa y hasta judicialmente?



Mantener la ciudad limpia, iluminada y en condiciones es un deber y un derecho de todos. Solo con el pago de sus tasas es posible.
Donar sangre y órganos es dar vida.

Contribuyente con deuda

Próximo Vencimiento 10/10/2011

PAGO CUOTA 5/2011
Mts. 9.5-Recor/2 Barrio Mecán-2 Lum.
Reparación Urbana

50,00	14,40
-------	-------

PAGO RESTO AÑO 2011 desde cuota 5
Mts. 9.5-Recor/2 Barrio Mecán-2 Lum.
Reparación Urbana

101,40	29,20	-3,43
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CUOTA ACTUAL

	RECIBO	PERIODO	
12/09/2011	65.40	12/09/2011	127.39
19/09/2011	65.71	19/09/2011	127.99

RESTO CUOTAS

	RECIBO	PERIODO	
12/09/2011	65.40	12/09/2011	127.39
19/09/2011	65.71	19/09/2011	127.99

JUNÍN
GOBIERNO LOCAL

C.V.P. (Bimestral)

RECIBO: 29161 PERIODO: 05/2011

CUOTA
05/2011

RESTO CUOTAS

JUNÍN
GOBIERNO LOCAL

C.V.P. (Bimestral)

RECIBO: 29161 PERIODO: 06/2011

RECIBO / RESTO DEL AÑO

	RECIBO	PERIODO	
12/09/2011	127.39	12/09/2011	65.40
19/09/2011	127.99	19/09/2011	65.71

CUOTA ACTUAL

	RECIBO	PERIODO	
12/09/2011	65.40	12/09/2011	127.39
19/09/2011	65.71	19/09/2011	127.99

Mechanism:

Did you know that if you do not pay the CVP on time for a debt of AR\$ 1,000 you will have to disburse AR\$ 268 in arrears at the end of the year and the Municipality can take administrative and legal action?.

Mechanisms - Deterrence Message

JUNÍN
GOBIERNO LOCAL

C.V.P. (Bimestral)

AL SEÑORADO DE: [REDACTED]
Calle: [REDACTED] No. [REDACTED]
Tel: [REDACTED] [REDACTED]
Fax: [REDACTED] [REDACTED]
E-mail: [REDACTED]
www.junin.gub.ar

PERIODO: 05/2011

PAISO ELECTRONICO: [REDACTED]

010-00000572600000

Detalle:

¿Sabía Usted que si no paga a tiempo el CVP, para una deuda de, por ejemplo, 1.000 pesos deberá pagar 268 pesos adicionales a fin de año, y el municipio puede llegar a intimarlo administrativa y hasta judicialmente?



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Contribuyente con deuda

Próximo Vencimiento 10/10/2011

PAGO CUOTA 5/2011
Mts. 9,5-Recorrid. 2 Barrio Mecán-2 Lum.
Reparación Urbana 50,80
14,40

Pago Resto año 2011 desde cuota 5
Mts. 9,5-Recorrid. 2 Barrio Mecán-2 Lum.
Reparación Urbana 191,40
29,20
Descuento pago resto año (3,36%) -3,43

	CUOTA ACTUAL	RESTO CUOTA
1º	12/09/2011 65.40	12/09/2011 127.39
2º	19/09/2011 65.71	19/09/2011 127.99

JUNÍN
GOBIERNO LOCAL

C.V.P. (Bimestral)

RECIBO: 29151 PERIODO: 05/2011

CUOTA 05/2011 [REDACTED]

RESTO CUOTAS [REDACTED]

	CUOTA ACTUAL
1º	12/09/2011 65.40
2º	19/09/2011 65.71

	ANUAL - RESTO DEL AÑO
1º	12/09/2011 127.39
2º	19/09/2011 127.99

JUNÍN
GOBIERNO LOCAL

C.V.P. (Bimestral)

RECIBO: 29161 PERIODO: 06/2011

CONTRIBUYENTE / PARTIDO: [REDACTED]

	ANUAL - RESTO DEL AÑO
1º	12/09/2011 127.39
2º	19/09/2011 127.99

	CUOTA ACTUAL
1º	12/09/2011 65.40
2º	19/09/2011 65.71

Mechanism: Fine

Did you know that if you do not pay the CVP on time for a debt of AR\$ 1,000 you will have to disburse AR\$ 268 in arrears at the end of the year and the Municipality can take administrative and legal action?.

Mechanisms - Deterrence Message

JUNÍN
GOBIERNO LOCAL

C.V.P. (Bimestral)

AL SEÑORADO DE: [REDACTED]
Calle: [REDACTED] [REDACTED]
Tel: [REDACTED] [REDACTED]
Fax: [REDACTED] [REDACTED]
E-mail: [REDACTED]
www.junin.gub.ar

PERIODO: 05/2011

PAISO ELECTRONICO: 010-00000572600000

Detalle:

¿Sabía Usted que si no paga a tiempo el CVP, para una deuda de, por ejemplo, 1.000 pesos deberá pagar 268 pesos adicionales a fin de año, y el municipio puede llegar a intimarlo administrativa y hasta judicialmente?



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Contribuyente con deuda

Próximo Vencimiento 10/10/2011

JUNÍN
GOBIERNO LOCAL

C.V.P. (Bimestral)

PERIODO: 05/2011

PAISO: [REDACTED]

CUOTA ACTUAL

	12/09/2011	05/10/2011
1º	65.40	127.39
2º	65.71	127.99

RESTO CUOTAS

	12/09/2011	05/10/2011
1º	65.40	65.71
2º	127.39	127.99

JUNÍN
GOBIERNO LOCAL

C.V.P. (Bimestral)

PERIODO: 06/2011

PAISO: [REDACTED]

ANUAL / RESTO DEL AÑO

	12/09/2011	10/10/2011
1º	127.39	127.99
2º	127.99	127.99

CUOTA ACTUAL

	12/09/2011	10/10/2011
1º	65.40	65.71
2º	127.39	127.99

Mechanism: Probability of Enforcement

Did you know that if you do not pay the CVP on time for a debt of AR\$ 1,000 you will have to disburse AR\$ 268 in arrears at the end of the year and the Municipality can take administrative and legal action?.

Factors Against Estimation

- Small treatment group
- We cannot observe reported sales directly, but only the declared tax.
- The gross-sales tax is sum of a tax over sales, tax over number of employees and a tax over size. The declared sales is the only one that varies within a fiscal year.
- The declared sales affect only a fraction of the estimated tax (for the average taxpayer, a 10% change in declared sales implies a 6% change in declared tax).
- There is a minimum tax that applies to all taxpayers whose sales are below a certain threshold; that is $(T_{gs} = \max(T_{gs}^{min}, T_{gs}(\tilde{Y}, \dots)))$
- The minimum tax was updated according to inflation: Jan-Apr: AR\$89.25, May-Aug: AR\$92.82 and Sep-Dec: AR\$96.56.

All of these factors should work against finding positive results.

Difference-in-Difference Estimator

β_{MDE} with our sample size and data structure for a significance level of 5% and a power of 0.8 for an OLS estimation with $\ln(\text{tax})$ as outcome is 20 percentage points. To address our data limitations we use a difference-in-difference estimator.

$$y_{it} = a_0 + a_1 T_d + \gamma t_{bim5} + \delta D_{it} + X_{it}^t \beta + \varepsilon_{it}$$

- y_{it} is the variable of interest
- T_d is one if the taxpayer received the deterrence letter for the property tax.
- t_{bim5} is one for the bim five (Sep-Oct) and zero from the bim four (Jul-Aug).
- D_{it} is the difference-in-difference estimator.
- X_{it}^t is a vector of controls (annual sales of the previous year, economic sector, dummies for the bins of the number of employees and size, age of the firm, gender of the owner, lagged outcome variable, blocks fixed effect)
- Standard errors are clustered by blocks.

Main Results

- We do not find evidence suggesting the existence of a negative spillover.
- Increasing the salience of fines and enforcement probabilities for those who don't comply with the payment of the property tax does not decrease how much the same individual declares on the gross-sale tax.
- The group that received the deterrence message in their property tax bill increased their gross-sales tax payment on average by 2 percentage points more than the control group.
- For completeness we also look at the probability of paying the tax by the due date. In no case we find a negative effect.
- Our results may be underestimating:
 - The tax is computed according to the declared sales over a two-month period. Most of those in the treatment group could have received the message after the first month declaration.
 - The declared tax -the variable we observe- is only partially affected by the level of declared sales
 - Many of the taxpayers pay the minimum tax; if there is any effect in this group, we may be unable to observe their response.

Effect of the Deterrence Letter on the Reported Tax

Difference-in-Difference Estimator: $y = \ln(\text{tax})$

	(1)	(2)	(3)	(4)	(5)	(6)
Deterrence	-0.0155 (0.0104)	-0.0141 (0.00962)	-0.0122 (0.00881)	-0.0133 (0.0103)	-0.0139 (0.00959)	-0.0120 (0.00871)
After (bim 5)	0.0284*** (0.00139)	0.0310*** (0.00114)	0.0310*** (0.00112)	0.0310*** (0.00117)	0.0310*** (0.00113)	0.0309*** (0.00111)
D (Treatment x after)	0.0219 (0.0111)	0.0208* (0.0100)	0.0204* (0.00972)	0.0219 (0.0108)	0.0208* (0.00996)	0.0203* (0.00962)
N	1433	1326	1326	1326	1326	1326
Sector Dummies	No	No	Yes	No	No	Yes
Size Dummies	No	Yes	Yes	No	Yes	Yes
B. Characteristics	No	No	No	Yes	Yes	Yes
Period	4vs5	4vs5	4vs5	4vs5	4vs5	4vs5

Monetary amounts are in Argentine Pesos (AR\$). Standard errors cluster by block are in parentheses. In all specifications, we include the lag outcome and blocks fixed effects. In specifications from three onwards, we include dummies for the economic sector, and from four to six we include dummies for the bins of the tables of the number of employees and the size of the store in square meters. The business characteristics, we include as controls are an indicator for have paid the minimum tax in the previous period, the annual sales of 2010, the age of the firm in years and gender of the proprietor.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Concluding Remarks

- Those taxpayers that received the treatment in the property tax declared more and were more likely to pay their gross-sales taxes than those in the control group.
- Spillovers across taxes are possible.
 - Spillovers could be negative, which may reduce or completely compensate the impact of the intervention.
 - Spillovers could also be positive, which would enhance the impact of the intervention.
- Analytical model that predicts that the size and sign of the spillover depends on:
 - The effect of the deterrence message on the salience of the penalty;
 - The effect of the deterrence message on how people evaluate the ability of the government to enforce several taxes at the same time.

Model First Stage: Pay or not the CVP

- Notation:

W Wealth, T Tax, θ Fine.

$P_c(P)$ is the (perceived) probability that the city government enforces the penalties.

P Overall perception regarding the control capacity of the city government.

$$\frac{\partial P_c}{\partial P} > 0$$

- The utility when paying the tax is:

$$U(W - T).$$

- If she decides not to pay, her expected utility is:

$$P_c(P) U(W - \theta T) + (1 - P_c(P)) U(W)$$

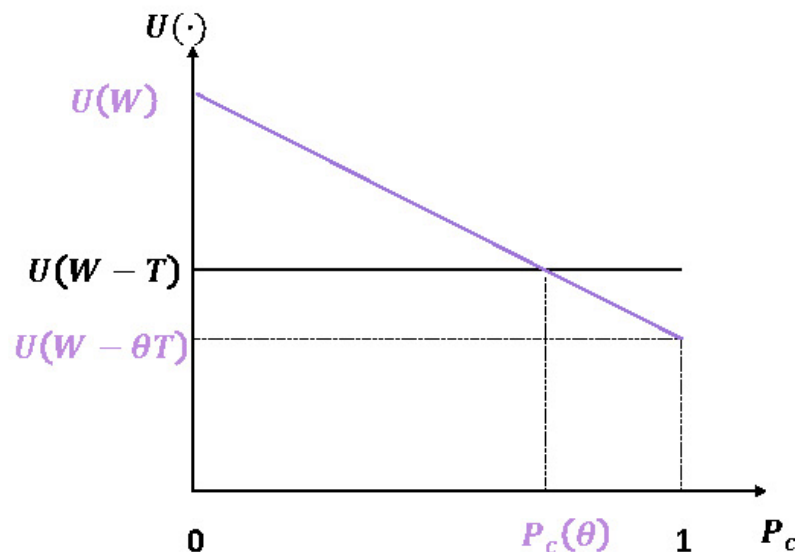


Figure: Pay or not the CVP

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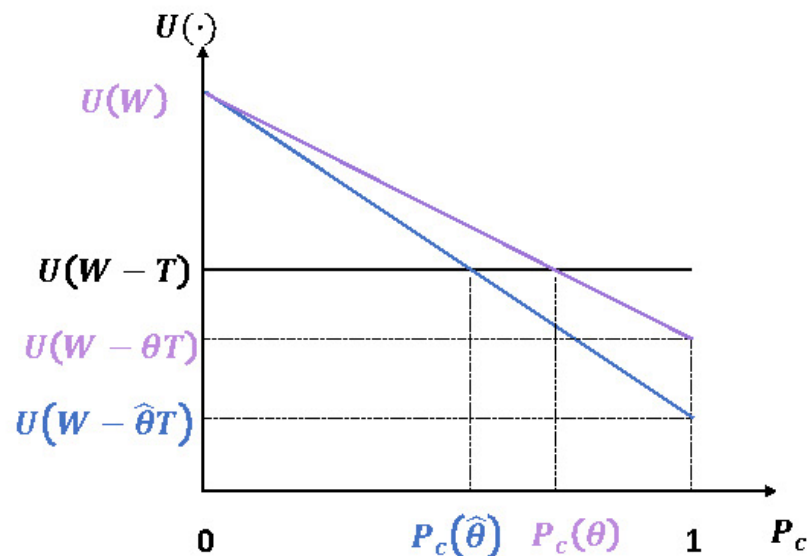


Figure: Change Perceived Fine $\hat{\theta} > \theta$

Model Second Stage: Declare Sales SEH

Traditional Allingham-Sandmo (A-S) model with a risk-averse individual with a increasing concave utility function.

Notation:

- y are the individual's true sales
- \tilde{y} are the reported sales
- t is the tax rate for reported sales.
- $P_s(P, P_c(P))$ is the probability of being caught under-reporting sales.

The individual maximization problem can be written as:

$$\max_{\tilde{y}} E(U) : (1 - P_s(P, P_c(P))) U(y - t\tilde{y}) + P_s(P, P_c(P)) U(y - t\tilde{y} - \theta t(y - \tilde{y}))$$

For notation convenience $X = y - t\tilde{y}$ and $\hat{X} = y - t\tilde{y} - \theta t(y - \tilde{y})$.

Model Second Stage: Declare Sales SEH

$$\max_{\tilde{y}} E(U) : (1 - P_s(P, P_c(P))) U(y - t\tilde{y}) + P_s(P, P_c(P)) U(y - t\tilde{y} - \theta t(y - \tilde{y}))$$

The first order conditions can be written as:

$$\begin{aligned} -(1 - P_s(P, P_c(P))) U'(X)t &+ P_s(P, P_c(P)) U'(\hat{X})(-t + \theta t) &= 0 \\ -t(1 - P_s(P, P_c(P))) U'(X) &+ tP_s(P, P_c(P)) U'(\hat{X})(\theta - 1) &= 0 \end{aligned}$$

Since the utility function is concave the second order conditions are satisfied:

$$D = t^2(1 - P_s(P, P_c(P))) U''(X) + t^2 P_s(P, P_c(P)) U''(\hat{X})(\theta - 1)^2 \leq 0$$

Comparative Statics - Increase in Penalties

Differentiating the first order conditions with respect to θ and solving for $\frac{\partial \tilde{y}}{\partial \theta}$:

$$\begin{aligned}\frac{\partial \tilde{y}}{\partial \theta} &= \frac{-t[P_s(P, P_c(P)) U'(\hat{X})]}{D} \\ \text{sign} \left[\frac{\partial \tilde{y}}{\partial \theta} \right] &= \text{sign} \left[t P_s(P, P_c(P)) U'(\hat{X}) \right] \\ \frac{\partial \tilde{y}}{\partial \theta} &> 0\end{aligned}$$

Comparative Statics - Increase in the Perceived Probability of Detection

The effect of an increase in the perceived probability depends on the assumptions about how taxpayers update their beliefs regarding overall enforcement.

Differentiating the first order conditions with respect to $P_c(P)$ and solving for $\frac{\partial \tilde{y}}{\partial P_c(P)}$:

$$\frac{\partial \tilde{y}}{\partial P_c(P)} = \frac{-1}{D} \left[tU'(X) + tU'(\hat{X})(\theta - 1) \right] \frac{\partial P_s(P, P_c(P))}{\partial P_c(P)}$$

$$\text{sign} \left[\frac{\partial \tilde{y}}{\partial P_c(P)} \right] = \text{sign} \left[\frac{\partial P_s(P, P_c(P))}{\partial P_c(P)} \right]$$

Assumption	$\frac{\partial P_s(P, P_c(P))}{\partial P_c(P)}$	$\text{sign} \left[\frac{\partial P_s(P, P_c(P))}{\partial P_c(P)} \right]$
$P_c(P) \perp P_s(P, P_c(P))$	0	0
$P = P_c = P_s$	1	positive
$P_s = P - P_c$	-1	negative

Balance Test Pre Treatment Period (Aug-Jul Bim 4)

	Difference: Deterrence		Control Group		N
Ln Tax SEH	0.106	(0.0891)	4.817***	(0.0263)	723
Ln Tax SEH exclude outliers (1%)	0.0360	(0.0350)	4.706***	(0.0153)	694
1 if retail sector	0.0140	(0.0510)	0.638***	(0.0243)	723
1 if industry	-0.0357***	(0.00924)	0.0444***	(0.00740)	723
Annual sales 2010 AR\$	36291.9	(53967.2)	220453.6***	(25439.4)	669
Num. of employees 2010	0.278	(0.165)	0.532***	(0.0577)	669
Num. of proprietors working 2010	0.0363	(0.0239)	1.002***	(0.00400)	669
Indoor space m2	22.52	(13.92)	91.08***	(6.762)	669
Outdoor space m2	3.010	(3.551)	4.666***	(1.189)	669
Paid SEH by 1st date	0.0339	(0.0422)	0.288***	(0.0267)	723
Paid SEH by 2nd date	-0.0122	(0.0290)	0.151***	(0.00900)	723
Paid SEH in Full	0.0275	(0.0343)	0.680***	(0.0222)	717
Paid CVP by 1st date	0.0139	(0.0418)	0.334***	(0.0323)	723
Paid CVP by 2nd date	-0.0192	(0.0307)	0.150***	(0.0181)	723
Paid CVP in Full	0.0551	(0.0540)	0.597***	(0.0392)	723
Manual swept	-0.0145	(0.0595)	0.414***	(0.0765)	723
Mechanical swept	-0.00789	(0.0665)	0.408***	(0.0662)	723
Num. lights	0.0175	(0.153)	2.955***	(0.101)	723
Ln front to street	0.00742	(0.0673)	2.555***	(0.0380)	723
1 if pay CVP monthly	-0.00493	(0.00280)	0.00493	(0.00280)	723

Each row shows a regression of the variable on the treatment. Monetary amounts are in Argentine Pesos (AR\$).

Standard errors are cluster at the block level and in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Placebo Test Effect of the Deterrence Letter on the Reported Tax

Difference-in-Difference Estimator: $y = \ln(\text{tax})$

	(1)	(2)	(3)	(4)	(5)	(6)
Deterrence	-0.00983 (0.0155)	-0.000930 (0.00248)	0.000227 (0.00308)	-0.0000611 (0.00230)	-0.000733 (0.00253)	0.000488 (0.00318)
After placebo (bim 4)	-0.0422*** (0.00558)	-0.0281*** (0.00160)	-0.0278*** (0.00162)	-0.0291*** (0.00155)	-0.0280*** (0.00159)	-0.0277*** (0.00163)
D (Placebo T. x after)	-0.00640 (0.0183)	-0.0138 (0.0114)	-0.0138 (0.0114)	-0.0140 (0.0115)	-0.0138 (0.0114)	-0.0137 (0.0113)
N	1431	1322	1322	1322	1322	1322
Sector Dummies	No	No	Yes	No	No	Yes
Size Dummies	No	Yes	Yes	No	Yes	Yes
B. Characteristics	No	No	No	Yes	Yes	Yes
Period	3vs4	3vs4	3vs4	3vs4	3vs4	3vs4

Monetary amounts are in Argentine Pesos (AR\$). Standard errors cluster by block are in parentheses. In all specifications, we include the lag outcome and blocks fixed effects. In specifications from three onwards, we include dummies for the economic sector, and from four to six we include dummies for the bins of the tables of the number of employees and the size of the store in square meters. The business characteristics, we include as controls are an indicator for have paid the minimum tax in the previous period, the annual sales of 2010, the age of the firm in years and gender of the proprietor.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Effect of the Deterrence Letter on Probability of Paying

Difference-in-Difference Estimator

	(1) Paid 1D	(2) Paid 2D	(3) Paid	(4) Paid 1D	(5) Paid 2D	(6) Paid
Deterrence	0.0240 (0.0391)	-0.0110 (0.0283)	0.0229 (0.0308)	-0.0133 (0.0471)	-0.0140 (0.0401)	0.00165 (0.0176)
After (bim 5)	-0.00616 (0.0177)	0.0303 (0.0155)	0.00119 (0.00761)	-0.0182 (0.0182)	0.0255 (0.0138)	-0.00332 (0.0127)
D (Treatment x after)	-0.0560 (0.0333)	0.0637* (0.0296)	0.0127 (0.0246)	-0.0388 (0.0403)	0.0812** (0.0277)	0.0133 (0.0278)
N	1445	1445	1435	1314	1314	1312
Blocks FE	Yes	Yes	Yes	Yes	Yes	Yes
Paid t-1	No	No	No	Yes	Yes	Yes
Controls	No	No	No	Yes	Yes	Yes
Period	4vs5	4vs5	4vs5	4vs5	4vs5	4vs5

Monetary amounts are in Argentine Pesos (AR\$). Standard errors cluster by block are in parentheses. Controls are dummies for the sector, indicator for have paid the minimal tax in the previous period, variables from the annual declaration of 2010 (annual sales, dummies for the bins of the tables of the number of employees and the size of the store in square meters), age of the firm in years and gender of the proprietor.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Placebo Test Effect of the Deterrence Letter on on Probability of Paying

Difference-in-Difference Estimator

	(1) Paid 1D	(2) Paid 2D	(3) Paid	(4) Paid 1D	(5) Paid 2D	(6) Paid
Deterrence	-0.00434 (0.0326)	0.0421 (0.0350)	0.0300 (0.0253)	-0.0332 (0.0446)	0.00678 (0.0257)	-0.0173 (0.0217)
After placebo (bim 4)	-0.0209 (0.0154)	0.00455 (0.0136)	-0.00000165 (0.00878)	-0.0201 (0.0170)	0.0103 (0.0136)	-0.00517 (0.0128)
D (Placebo T. x after)	0.0301 (0.0266)	-0.0482 (0.0335)	-0.00651 (0.0165)	0.0198 (0.0333)	-0.0243 (0.0368)	0.0214 (0.0327)
N	1445	1445	1433	1309	1309	1307
Blocks FE	Yes	Yes	Yes	Yes	Yes	Yes
Paid t-1	No	No	No	Yes	Yes	Yes
Controls	No	No	No	Yes	Yes	Yes
Period	3vs4	3vs4	3vs4	3vs4	3vs4	3vs4

Monetary amounts are in Argentine Pesos (AR\$). Standard errors cluster by block are in parentheses. Controls are dummies for the sector, indicator for have paid the minimal tax in the previous period, variables from the annual declaration of 2010 (annual sales, dummies for the bins of the tables of the number of employees and the size of the store in square meters), age of the firm in years and gender of the proprietor.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Discussant Comments: Factors that Contribute to Voluntary Compliance

Brian Erard

Common themes for papers

- Focus on relatively neglected factors that influence tax compliance
 - Withholding
 - Accountants
 - Spillover effects of enforcement
- Reliance on field or natural experiments
 - Withholding changes under Making Work Pay Credit
 - Elimination of requirement or some Danish SMEs to have a full financial audit by a certified financial accountant
 - Randomized controlled study of spillover effects of a enforcement message intervention

Overall strengths and weaknesses

Strengths

- Reliance on experimental or quasi-experimental methods lends a measure of credibility to results
- Transparent descriptions of methodology and findings

Weaknesses

- Reasons for the observed changes in behavior not altogether clear
- Generality of findings (external validity) uncertain

William's paper on withholding

- Clever approach for examining how an unrequested reduction in withholding without a compensating reduction in tax liability impacts filing and payment compliance
 - Relies on the fact that the size of the withholding reduction under MWP was common for all workers, but the actual value of the credit was smaller for those receiving social security benefits.
 - In the absence of a pro-active adjustment to their withholding levels, social security recipients risked being under-withheld at tax time.

Difference-in-differences

- William employs a difference-in-differences strategy to compare the pre- and post-policy trends in late filing and late payment for wage earners *who receive* (treatment group) and *who do not receive* (comparison group) social security payments
- A potential concern is that the treatment and comparison groups are dissimilar:
 - Control group is older, more likely to be married, has fewer kids, lower wage earnings, and higher interest income
 - Might expansion of Additional Child Tax Credit EITC for households with 3 children under ARRA contaminate findings a bit?
 - Likely differences in terms of some unobservables as well: education level, attitudes, experiences, etc.

Parallel trends?

- As a result of the differences between the treatment and comparison group, there is potential for the parallel trends assumption to be violated
 - Would be helpful to see a graph of the raw pre-treatment and post-treatment trends for the two groups
- To address the observed and time-invariant unobserved differences across groups, a regression framework is employed that includes relevant pre-determined characteristics:

$$y_{it} = \alpha_i + \sum_{t=1}^T \beta_t ERP_i + \sum_{t=1}^T \gamma_t X_{it} + \epsilon_{it}$$

Methodological issues

- Incorporating pre-determined variables in the specification helps to account for observed differences between the two groups that can explain differences in their respective trends
 - However, there still may be unobserved time-varying differences that influence the trend in the dependent variable
 - Also, one now is relying on the adequacy of the regression assumptions
- The placebo tests (estimated impacts in pre-treatment years) are helpful in assessing the adequacy of the specification
- I suggest estimating an LDV model as a sensitivity test:

$$y_{it} = X_{it}\gamma + \sum_{k=1}^{T_0} \delta_k y_{ik} + \beta ERP_i + \epsilon_{it}, \quad t = (T_0 + 1), \dots, T$$

Implications

- The results suggest that a perhaps unrecognized change in withholding status relative to tax liability led to an increase in late filing and unpaid tax.
- This seems unsurprising, although the more specific findings are rather surprising:
 - \$250 fall in withholding leads to 10% increase (0.5 percentage point) in late filing and 52% increase in late payments (1.9 percentage points)
 - Would these values have been lower if this was not the Great Recession?
 - The failure to pay all taxes due was apparently not driven by liquidity constraints.
- I would like to see more discussion of how to interpret these finding in the context of the theoretical models in the paper.
 - The models posit that taxpayers are forward-looking and anticipate the effects of a reduction in withholding.
 - An alternative perspective is that the treatment group members were caught off guard at tax time, leaving them with a more constrained set of options. Can we quantify how much better off they would have been if that had not been myopic?

Johanne's paper on accountants

- As with the previous paper, this one examines a “natural experiment”
 - This time it involves a removal of a requirement to have a full audit of a business' financial statements for certain SMEs in Denmark (beginning in 2006, with an expanding number of exemptions in 2011 and in 2013).
- Goal is to examine whether the lack of a full financial audit is associated with tax compliance
- Hypotheses:
 - Full financial audit leads to better VAT compliance
 - Full financial audit leads to worse income tax compliance
 - Full financial audit leads to less *apparent* intentional tax noncompliance

Some considerations

- Verification that the financial statements are accurate does not necessarily imply that the tax return is accurate
- Examples of differences between financial statement and tax return:
 - Choice of reporting period (e.g., FY vs. TY)
 - Selected accounting conventions:
 - Cash vs accrual
 - LIFO vs. FIFO
 - Differences between tax rules & GAAP, such as depreciation and expensing
- Even those not receiving a full financial audit frequently rely on accountants for some audit work (review of books; perhaps some tax assistance)
 - So, really comparing those receiving a full financial audit to those receiving more selective audit services

Empirical approach

- Logit analysis of likelihood of tax noncompliance using pooled random audit data, FY 2008, 2010, 2012, 2014
 - Key explanatory variable of focus: dummy for whether taxpayer had a full financial audit
 - Other controls:
 - Dummy for being eligible to forgo full financial audit
 - Industry dummies
 - Log of years in operation
 - Year dummies

Findings

Holding other measured factors constant,

- Those eligible to forgo a full financial audit are more likely to be compliant with both VAT and income tax.
 - Presumably because they are smaller and have less opportunity
- Among those eligible to forgo a full financial audit, those who do opt out are less likely to be compliant with VAT but more likely to be compliant with income tax
 - This is attributed to accountants serving as transparent rule enforcers of the relatively simpler VAT, but ambiguity exploiters of the relatively more complex income tax

Limitation of methodology

- Among those eligible to opt out of a full financial audit, the choice was entirely their own
 - So, there is a self-selection problem
- Consider the following model of the joint decision regarding whether to have a full financial audit and whether to cheat:

$$\begin{aligned}A^* &= X_A\beta_A + \epsilon_A \\ C^* &= X_C\beta_C + \gamma A + \epsilon_C\end{aligned}$$

- If we apply logit to the second equation, we are unable to distinguish the effect of the full financial audit from the selection effect

Other thoughts

- Interesting to think about comparing reporting behavior of the same taxpayers over a period spanning the old rules and the full phase-in of the new rules.
 - Might be possible to develop a difference-in-difference design to compare pre- and post-policy trends for those who opted in, those who opted out, and those with modestly larger size who had not opportunity to choose.

Carlos and Andrea's paper

- Randomized field experiment involving a treatment group of property owners who receive a deterrence message and a control group that does not.
 - Deterrence message is directed towards improving compliance with the property tax.
- Focus in this paper is on the subset of property owners in the experiment who also were subject to a gross sales tax on their business activity
 - The key question is if attempted deterrence of property tax noncompliance had any spillover impact on gross sales tax compliance
 - In the pre-test period, about 68% fully pay the gross sales tax

Estimation methodology

- The authors rely on the following regression specification:

$$y_{it} = \alpha_0 + \alpha_1 T_d + \gamma \text{ILLU} + \delta D_{it} + \vartheta y_{i,t-1} + X_{it} \beta + \epsilon_{it}$$

- I am not sure it is appropriate to estimate this model over the full period given the lagged dependent variable.
- Moreover, considering this is a subsample of sole proprietors from the randomized field experiment, this seems to be an unnecessarily complicated specification.
 - Shouldn't the treated and untreated taxpayers be similar both in terms of observed and unobserved characteristics owing to randomization?
 - I think a simple difference-in-differences specification would be more appropriate, at least as a starting point

Results

- Results hint at a weak positive impact of the deterrence message for the property tax on compliance with the gross sales tax
- However, a footnote in the paper indicates that the deterrence message had no significant impact on overall compliance with the property tax.
 - If the deterrence message did not affect behavior with respect to the specific tax the message was focused on, this lessens one's confidence that the tax would impact compliance on another unrelated tax.

An area ripe for more research

- As noted by Carlos and Andrea, much remains unknown about spillover effects of deterrence efforts.
- Some relevant factors for further research include:
 - The importance of the context of the enforcement effort.
 - The perceived link between this effort and the resources devoted to enforcing other taxes.
 - The quality of the opportunities for noncompliance on other taxes.



Session 1. Factors That Contribute to Voluntary Compliance

Moderator:

Jessica D'Itri
IRS, RAAS

Does Employer Withholding Affect Tax Compliance, and Why?

William Boning
University of Michigan

Accountants are not tax advisors: Using random audit data to test the effect of certified accountancy on SME's tax compliance in Denmark between 2008 and 2014

Johanne Søndergaard
Danish Customs and Tax Administration

Compliance Spillovers Across Taxes: Results from a Field Experiment

Carlos Scartascini
Inter-American Development Bank

Discussant:

Brian Erard
B. Erard & Associates



Research, Applied Analytics,
and Statistics



TAX POLICY CENTER
URBAN INSTITUTE & BROOKINGS INSTITUTION

8th Annual IRS/TPC Joint Research Conference on Tax Administration

Next session begins at 11:00