The international platform of Ludwig-Maximilians University's Center for Economic Studies and the Ifo Institute

Working Papers

www.cesifo.org/wp

Taxes on the Internet: Deterrence Effects of Public Disclosure

Joel Slemrod
Thor O. Thoresen
Erlend E. Bø

CESIFO WORKING PAPER NO. 4107
CATEGORY 1: PUBLIC FINANCE
FEBRUARY 2013

CESifo
Center for Economic Studies & Ifo Institute

CESifo *Working Paper No. 4107*

Taxes on the Internet: Deterrence Effects of Public Disclosure

Abstract

Supporters of public disclosure of personal tax information point to its deterrent effect on tax evasion, but this effect has not been empirically explored. Although Norway has a long tradition of public disclosure of tax filings, it took a new direction in 2001 when anyone with access to the Internet could obtain individual information on income, wealth, and income and wealth taxes paid. We exploit this change in the degree of exposure to identify the effects of public disclosure on income reporting. Identification of the deterrence effects of public disclosure is facilitated by the fact that, prior to the shift to the Internet in 2001, some municipalities had exposure which was close to the Internet type of public disclosure, as tax information was distributed widely through paper catalogues that were locally produced and disseminated. We observe income changes that are consistent with public disclosure deterring tax evasion: an approximately 3 percent average increase in reported income is found among business owners living in areas where the switch to Internet disclosure represented a large change in access.

JEL-Code: H300, H250.

Keywords: tax evasion, income reporting, quasi-experiments.

Joel Slemrod
Department of Economics
University of Michigan
Ann Arbor / Michigan / USA
jslemrod@umich.edu

Thor O. Thoresen
Research Department
Statistics Norway
Oslo / Norway
tot@ssb.no

Erlend E. Bø
Research Department
Statistics Norway
Oslo / Norway
eeb@ssb.no

January 22, 2013
We thank the interview corps of Statistics Norway and Bjørn Are Holth, Gunnar Claus and Inge Aukrust for help with the data collection, and acknowledge the helpful comments from seminar participants at Skatteforum 2012 (Larvik, Norway), the opening conference of Oslo Fiscal Studies (University of Oslo) and graduate students in the University of Michigan public finance summer reading group.

1 Introduction

Although not often explicitly stated, an important reason for a system of public disclosure is that it arguably deters people from tax evasion. For instance, given that neighbors observe income and expenditure details, taxpayers may be reluctant to underreport income, because a lack of correspondence between consumption of durables, such as a house and car, and reported income, may induce reactions (from the neighbors) or represent a reputational loss. However, to our knowledge, the effects of public disclosure on individual income reporting have never been systematically explored. One reason is that very few countries practice public disclosure of tax information at the individual level. As far as we know, only Finland, Sweden, Iceland and Norway have some sort of public disclosure at the personal level,[1] but Norway is exceptional in that (according to the present system) individual income tax return information can be addressed through electronic search.

Norway has a long history of public disclosure of information from income tax returns, going back at least to the middle of the nineteenth century (NOU, 2009:1). Citizens could visit the local tax office or the city hall and look through a book that contained information about each taxpayer in the local area. Persons were listed by name and address, along with key measures from the income tax return: income, tax payment, and wealth. The information was available for three weeks after the tax statement was made public. As the media had access to the same type of information, local newspapers would often communicate highlights from the lists, such as rankings of the richest and the most wealthy citizens, or incomes of sports and entertainment celebrities.

However, the advent of the Internet changed the form of the public disclosure of tax information rather dramatically. In the fall of 2001, a national newspaper offered online access to tax information for the whole population through the web version of the newspaper, and soon all of the major national newspapers followed. Now, one could simply sit at home by the computer and obtain information about relatives, friends, neighbors, or celebrities. Whereas not many people took the trouble to visit the local tax office for manual searches, obtaining the same information by computerized searches from home reduced the information access hurdle substantially. The web pages offering search engines for tax information have been among the most popular websites in Norway, especially shortly after the release of new annual information.

The practice of public disclosure was controversial even in the days of paper lists, but Internet access generated substantial resistance. Openness was chal-

[1] We are aware some examples of public tax disclosure from other countries in earlier times, such as France, Italy and the United States.

lenged by arguments referring to invasion of privacy, spurred by idle curiosity or more nefarious motivation. Examples of the latter included alleged tax-list-based bullying among school kids and tax lists found on criminals in the act of burglary. These examples may have influenced the decision to revise the system. Beginning in 2011, with respect to the tax statement for 2010, one can still click into the tax lists, but now one only gets access through a personalised log-in system for accessing online public services, which involves a pin-code and a password.[2]

The objective of the present analysis is, by the use of micro-unit income tax return data, to assess to what extent people react to public disclosure by reporting a different level of income than they otherwise would do. We treat the move from books in local offices to the Internet as a fundamental shift in accessibility, which can be exploited in an identification strategy based on evaluations of before and after outcomes. Given that wage earners have rather limited scope for tax evasion (third-party reporting is a standard procedure), compared to the self-employed and other owners of businesses, one may use observations of incomes of wage earners and owners of businesses before and after 2001 to obtain estimates of the public disclosure effect.

Because there are several other reasons for wage income and business income to move separately over time, we further refine the identification strategy by exploiting the fact that in a number of municipalities, prior to 2001, tax information about local residents was widely distributed through sales of paper copies of the tax lists. We consider the information level of these paper catalogues to be closer to Internet access, which implies that we can categorize our income data observations according to belonging to a municipality with substantial pre-2001 tax-return information diffusion, or not.

With respect to econometric identification, one would reasonably argue that business owners are effectively randomly assigned to the two different categories of municipalities: municipalities with no pre-2001 special information distribution arrangements, and municipalities with availability of paper catalogues prior to 2001. A survey, tracking areas with and without pre-2001 special arrangements, identified 31 municipalities where there were sales of books of tax return transcripts, and 106 municipalities with no such arrangements. It follows that the business owners in the latter group experienced a completely different information diffusion system after 2001, when the nationwide full-scale electronic version was in place, which may have had reporting effects, whereas no such effects are assumed in the former group. Applying the difference-in-differences estimator to compute differences in mean income changes between the two groups after 2001

[2]Despite the fact that the digital search is now more complicated, the tax authorities reported that as many as 709,000 unique users (from a total population of approximately 5 million people) carried out 13 million searches in 2011 (Norwegian Tax Administration, 2012).

holds the promise of identifying the effect of Internet public disclosure on the income reporting of business owners.

The sample of individuals used in this study consists of persons from 137 municipalities (out of a total of near 430 municipalities in Norway), observed before and after 2001 (from 1997 to 2004), and categorized according to two different systems of information availability prior to 2001. As the income data we have available for this study are register-based and cover the whole population, this data set consists of approximately 370,000 individuals of working age, observed over eight years. Several individual and municipality characteristics are accounted for in the empirical analyses.

Although the analysis utilizes a large number of control variables, there may still be unobserved differences between individuals in municipalities where there were no availability of paper catalogues prior to 2001 (treatment group) and individuals in municipalities which had distribution of paper catalogues before 2001 (control group). The omitted variable problem and other measurement issues are explored through several robustness tests, after presenting the main estimates.

The plan of the paper is as follows. In Section 2 we briefly discuss the background for public disclosure. The empirical strategy is described in Section 3, and Section 4 presents the results, including a number of sensitivity tests. Section 5 concludes the paper.

2 Deterrence effects of public disclosure

2.1 The deterrence mechanism

Public disclosure is designed to reduce the attractiveness of tax noncompliance as well as aggressive, but arguably legal, tax avoidance. Disclosure may complement deterrence by encouraging others with relevant information about true tax liability to come forward,[3] and the fear of that and subsequent tax noncompliance penalties—explicit and shaming—dampens such behavior. The first models of tax evasion, Allingham and Sandmo (1972) and Yitzhaki (1974), focused on the trade-off between pecuniary quantities (lower tax burden versus the risk of penalty).[4] These models have been extended in several directions, including frameworks which have accounted for moral sentiments of guilt and shame (Erard and Feinstein, 1994) and social conformity effects (Myles and Naylor, 1996; Fortin, Lacroix and Villeval, 2007). Laboratory experiments, as reviewed in Alm

[3] In Norway, the National Authority for Investigation and Prosecution of Economic and Environmental Crime (ØKOKRIM) has a designated phone number for whistle-blowing.

[4] However, Allingham and Sandmo (1972) mention that tax evasion may be limited if individuals fear loss of reputation, without including such considerations in their model.

(2012), provide support for public disclosure of non-compliance acting as an additional penalty mechanism. For instance, Corricelli, Joffily, Montmarquette and Villeval (2010) find a strong physiological impact of public display of evaders' pictures on the emotional arousal of tax evasion among evaders. Moreover, Laury and Wallace (2005) use experimental methods to analyze the relationship between the perception of confidentiality and taxpayer compliance, and find some evidence suggesting that when individuals perceive a breach in confidentiality (disclosure), they increase their level of compliance.

Disclosure may affect tax reporting through other avenues. Taxpayers may reduce reported taxable income in order to minimize the attention of the press and of unsavory characters wishing to take advantage of their economic situation. On the other hand, some people might get satisfaction—bragging rights, if you will—from public appreciation of their level of affluence, and may be willing to pay for it in the form of a higher tax liability.

Defenders of tax privacy argue that taxpayers might feel vulnerable to embarrassment or harassment if others have access to their information (Blank, 2011). However, whereas in Norway there have been alleged examples of bullying of school children and burglaries based on information from income tax returns, possible positive effects in terms of the effects on income reporting have been more difficult to obtain. Both the literature on tax evasion and the literature on social interactions and tax evasions attest to the identification problems in such studies, stemming from severe empirical challenges when measuring illegal activities (evasion) and social interactions (such as reputational harm); see, for instance, Manski (1993) and Slemrod and Weber (2012).[5]

Accordingly, the empirical evidence is sparse on public disclosure in the income tax context. Slemrod, Hasegawa, Hoopes, and Ishida (2011) study the effect of the Japanese income tax disclosure system that was abolished in 2004/2005 on tax reports of individuals and businesses. They take advantage of the abolition and the fact that disclosure applied only to taxable incomes above 40,000,000 yen (about $400,000). They find strong evidence based on bunching of observations right below the disclosure threshold that, on average, individuals and businesses prefer to avoid disclosure; for the latter, this is consistent with the local characterization of "39 companies", whose reported taxable income is kept below the disclosure threshold so as not to provide evidence about their profitability, which might affect the deals they can make with other companies. However, Slemrod et al. uncover no evidence that disclosure increased reported business taxable income generally.

[5] See also Andreoni, Erard and Feinstein (1998) and Slemrod (2007) for surveys of the tax compliance and the tax evasion literature, respectively.

2.2 Worldwide experience

Historically, there have been shorter spells of public disclosure in some other countries, such as the U.S. and France.[6] Public access to corporate tax information is permitted in Japan, Finland, Sweden in addition to Norway (Lenter, Slemrod and Shackelford, 2003), whereas personal level public disclosure is associated with the Nordic countries. However, the other Nordic countries have far less openness, as there is no mass distribution in any of them. Denmark[7] has no public disclosure, whereas Sweden, Finland and Iceland have systems where one can apply to the tax authorities for information about individuals, in Iceland for only a very limited time period (Ministry of Finance, 2011). Nevertheless, the issue continues to be on the policy agenda in several countries. For example, in Italy in 2008 the tax authorities put all 38.5 million tax returns for 2005 up on the Internet, before being blacked out following widespread protest.[8]

2.3 Disclosure of tax evaders

In certain countries, there is public disclosure of information about tax evaders. For example, under Greek law, the presentation of a new budget is accompanied by the names of tax evaders in the previous year compiled by the finance ministry. In New Zealand the Commissioner of Inland Revenue regularly releases a document entitled "Tax Evaders Gazette" that lists those taxpayers who have been prosecuted or had penal tax imposed for evading their taxation obligations; as of April 1997 the Commissioner is able to also publish the names of those taxpayers involved with "abusive tax avoidance." The Canadian Customs and Revenue agency compliance strategy includes publicizing court convictions for tax fraud. In Ireland, a list of tax defaulters was formerly published on annual basis in the Revenue Commissioner's Annual Report, but recently the list is published on a quarterly basis in Iris Oifigiuil (the official newspaper of record in Ireland in which several legal notices, including insolvency notices, are required by law to be published) and reported in the national and local newspapers. According to the tax agency, this measure "aims to raise the profile of compliance and provide a continuous deterrent to other potential tax evaders. Frequently, taxpayers make a full disclosure of irregularities to auditors at the commencement of an audit to avoid the possibility of being published for tax offences." Moreover, the well-publicized quarterly list is "more likely to be spotted by suppliers, customers, business associates and

[6] See IRS (2011) for an overview over the history of public disclosure in the U.S.

[7] However, Denmark has recently (June, 2012) begun public disclosure of tax payments in the corporate sector, in order to encourage correct income reporting.

[8] The Economist, May 8th, 2008. Before being blacked out, vast amount of data were downloaded and transferred to other sites or burned in to disks and sold.

friends."

3 Empirical strategy

3.1 Internet exposure marks a difference

Since the middle of the nineteenth century there has been public disclosure of tax information in Norway (NOU, 2009:1). In recent decades an interested citizen could visit the local tax office, where he/she could get access to a book containing a list of each taxpayer in the local area (name, year of birth, postcode) and three variables from the income tax return: income, wealth, and taxes paid. Since the tax reform of 1992 the income measure reported is "ordinary income": gross income after the standard deduction and deductions for debt interest payments. The year 2001 (tax year 2000) represents a demarcation line in our empirical strategy because, for the first time, the national newspapers transferred the tax return information they received from the tax authorities to web pages. This implies that anyone with access to a computer and the Internet had access to the same measures, on a national rather than local scale, that were available prior to 2001 by physically making a trip to the local tax office.

Treating 2001 as a cut-off point in the empirical analysis rests upon two assertions. Firstly, under the public disclosure system prior to 2001, very few people actually visited the local tax offices for manual searches. We do not have any hard statistical evidence to justify this claim, but one can easily understand that for most citizens the costs of physically taking a trip to the location of the tax information represented a substantial barrier. Only persons with very low opportunity costs, and/or persons who have a strong desire for acquiring such information, would have consulted the printed lists. Second, the choice of using 2001 as a critical point in time is founded on electronically available information being widely spread. Even though Internet coverage has increased substantially since 2001, Vaage (2001) reports that in 2001 as much as 50 percent of the population used the Internet in an average week, and 45 percent used it for private purposes. Hence, we trust that limited information spread before 2001 and the high level of accessibility after 2001 are sufficient conditions for considering the move to the Internet a dramatic change in exposure among taxpayers.

From a rather general viewpoint, let reported income for individual i at time t depend on an individual fixed effect, α_i, a time trend, λ_t, a vector of individual-specific, time-varying covariates, $Q_{it}^{'}$, public disclosure, D_{it}, and an independently and identically distributed (i.i.d.) error term, ε_{it}:

$$\log y_{it} = \alpha_i + \lambda_t + Q_{it}^{'}\beta + \delta D_{it} + \varepsilon_{it}. \tag{1}$$

Given that the sudden change to the Internet disclosure can be seen as a quasi-experiment, we employ the difference-in-differences estimator in the following, and define D_{it} as a binary treatment variable, switching on for a particular group after the change.[9] The individual effect is then reduced to a time-invariant group effect, which is removed by differencing. This identification strategy rests upon several identifying assumptions, which we will return to, in particular when exploring alternative explanations for the empirical findings (in Section 4). Several of these assumptions are shared by other econometric techniques, such as the independence of outcomes, i.e. that treatment of one individual do not influence others. Many interesting studies of the treatment literature focus on various effects of social interactions, such as peer and neighborhood effects, see Manski (1993) and Brock and Durlauf (2001); the effects discussed here are related, as they stem from social interactions, but outcomes are considered as independent.

A standard assumption of the difference-in-differences method is the assumption that time effects or trends are the same in the absence of the event (Internet exposure), or, in other words, without any intervention, the growth in reported income is equal in the groups, conditional on other characteristics.[10] As the common trend assumption is not testable, the choice of empirical specification is guided by plausibility. It follows that it is important to find a mechanism for group assignment that mimics randomization. Next, we discuss which type of information can be used to resemble an experiment, given the empirical question of the present study. First, we discuss categorization by employment status – wage earners and self-employed – and then we introduce assignment based on residence in a municipality that had distribution of paper catalogues prior to Internet exposure.

3.2 Differential response of employees and business owners

A first approach to group assignment is a categorization based on contrasting outcomes for taxpayers who have the possibility to adjust their income with others who do not have this option. This is reminiscent of Pissarides and Weber (1989), who initiated an empirical strategy for tax compliance analysis based on dividing the sample into self-employed and wage earners, under the assumption that the employees have little or no scope for tax evasion, compared to people running

[9] Following different groups over time, before and after a major change for one of them, corresponds to a classical empirical design; see applications in, for instance, Card (1990), Card and Krueger (1994), and Abadie and Gardeazabal (2003). Blundell and Dias (2009), Angrist and Pischke (2009), Imbens and Wooldridge (2009) and Lechner (2011) provide overviews and more details about this identification method.

[10] See Athey and Imbens (2006) for a framework to allow for arbitrary differences in the composition of treatment and control groups.

their own businesses.[11] Third-party reporting of employees' income, which is a standard procedure in Norway, curbs the possibilities for underreporting among wage earners (Slemrod, 2007), so the same type of categorization may be applied in the present analysis. When emphasizing the differentiation into business owners and wage earners in the income process, income is explained by a dummy variable (when j indexes occupations), bus_j, which takes the value 1 if the individual is a business owner (with scope for underreporting), and 0 if the person is a wage earner, and a time dummy variable, int_t, which takes the value 1 if the year is a year with Internet exposure, in addition to other individual characteristics ($X_{it}^{'}$), and unobservable individual effects (ε_{ijt}):

$$\log y_{ijt} = \alpha_0 + \lambda_t + X_{it}^{'}\beta + \delta_1 bus_j + \delta_2 int_t + \delta_3 \left(bus_j \times int_t\right) + \varepsilon_{ijt}, \qquad (2)$$

where α_0, β, δ_1, δ_2 and δ_3 are parameters; $\delta_3 > 0$ indicating a public disclosure effect. We will return to error term assumptions in Section 4.

This identification strategy is subject to several possible confounding factors, or time-dependent unobservables, that may generate dissimilar growth in income for wage earners and business owner. For instance, the business cycle may have a different effect on incomes of employees and business owners, so that the common time trend assumption may be violated.

3.3 Differential response by pre-2001 access to taxpayer information

To facilitate sharper identification, we utilize that the sample can be further divided into treated and control groups by exploiting a rather peculiar arrangement prior to the Internet revolution in 2001. Before 2001, the tax authorities, as a service to the local community, sent the income tax information to local newspapers, which often published highlights from the lists, such as rankings of the richest and most wealthy, or incomes of sports and entertainment celebrities. But others could apply for a list too, and some local organizations exploited the attraction of this type of information to finance their activities. In some, but not all areas, members of the football club or the community band would go from door-to-door and offer copies of the entire tax transcript of that area for sale. The main assumption behind the exploitation of this institutional characteristic for identification is that

[11] Pissarides and Weber (1989) obtain identification of evasion by comparing the ratio of reported income to food consumption in the two groups, based on the assumption that preferences for food are similarly distributed. While Pissarides and Weber examined survey data, Feldman and Slemrod (2007) analyze tax noncompliance by using unaudited income tax return data. See also Hurst, Li and Pugsley (2010), who argue that there is substantial underreporting of income among self-employed even in survey data.

taxpayers in the treated localities, persons in areas without widespread income tax return information prior to 2001, respond in a greater degree to the changes in disclosure brought about by the information becoming available on the Internet compared to those who had this arrangement.

To ascertain which municipalities were treated and which were not, we conducted a survey, tracking local areas with and without the pre-2001 special arrangements. We found 31 municipalities where there were pre-2001 sales of books of tax return transcripts, and 106 municipalities in which no such arrangements existed; Figure A1 in the Appendix shows the locations of the two different categories of municipalities.[12] We argue that in the latter group of municipalities, inhabitants experienced a fundamental change in the information diffusion system after 2001, when nationwide full-scale electronic diffusion emerged.[13] Now the sample of owners are further differentiated with respect to a dichotomous characteristic, a dummy variable denoted (when k indexes municipalities) $nocat_k$, which takes the value 1 when the individual resides in a municipality with no availability of paper catalogues prior to 2001, and the value 0 when the individual belongs to a municipality where there was distribution of catalogues before 2001:

$$\begin{aligned} \log y_{ijkt} &= \alpha_0 + \lambda_t + X_{it}^{'}\beta + \delta_1 bus_j + \delta_2 int_t + \delta_3 nocat_k + \delta_4 \left(bus_j \times int_t\right) + \\ &\quad \delta_5 \left(bus_j \times nocat_k\right) + \delta_6 \left(int_t \times nocat_k\right) + \delta_7 \left(bus_j \times int_t \times nocat_k\right) \\ &\quad + Z_k^{'}\gamma + \varepsilon_{ijkt}. \end{aligned} \tag{3}$$

The main parameter of interest is δ_7. Under the hypothesis that public disclosure deters taxpayers from underreporting, reported income moves higher among business owners whose informational exposure is more affected by the Internet access, and δ_7 is therefore expected to be positive. If Internet disclosure is a stronger type of display than paper lists and the income growth of business owners in the control group are affected by the new disclosure regime too,[14] the estimate of δ_7 is biased downward, and in this sense represents a lower bound of the public disclosure effects on business owners' income reporting.

[12] As the data collection was based on personal contact between interviewers and chief officers in the municipalities and therefore was quite resource-intensive, we stopped the data collection once we had found more than 30 municipalities with pre-2001 sales of books. At that stage we had identified 106 municipalities with no such arrangements.

[13] We do not have information about the spread of paper catalogues in the control group prior to 2001, but assume that the institution itself had effect. The price of the catalogues are not expected to represent an impediment, as prices were relatively low. For example, in the municipality of Eidskog in 1999 and 2000, the catalogues were sold for 50 Norwegian kroner (or approximately $6 each) and sales helped the financing of leisure activities for children.

[14] For instance, this might occur because the Internet version represents national spread, whereas the paper catalogue distribution was limited to the municipality.

The model specified in Equation (3) can be characterized as saturated in the main regressors of the model (without the control covariates), as it contains a parameter for every combination of the main explanatory variables observed in the data, which implies that the additive linear form of Equation (3) is not restrictive (see Angrist and Pischke, 2009); we will return to the functional form dependency below.

Equation (3) includes controls for municipality characteristics, Z_k', such as the unemployment rate, population size, etc. Of course, such controls could have been introduced in Equation (2), but are particularly relevant in Equation (3) because the key regressor is defined by a municipality-level attribute. These control variables hold the promise of picking up contemporaneous shocks that may affect outcomes. Given the random assignment into groups, we do not expect individual characteristics to be a source of omitted-variables bias in the measurement of the effect of public disclosure, but including X_{it}' is helpful for the precision of the regression estimates.

Note that the wage earners, in contrast to their role in Equation (2), enter into Equation (3) as an additional control for the time trend; see Gruber (1994) for a similar approach.[15] If for instance there are omitted variables, such as local idiosyncratic economic shocks, that are not picked up the explanatory variables, it may be advantageous to use relative income developments for wage earners, in the catalogue and non-catalogue municipalities, as a control. Of course, this rests on the assumption that wage earners' reactions to the economic business cycle and other macroeconomic developments are representative of the responses of business owners. However, if wage earners who were shocked by the Internet exposure in 2001 (i.e., who lived in a non-catalogue area) also adjust their income to the new regime, the estimate of δ_7 is biased downward as a measure of the effect on business owners.[16] By letting the difference in income before and after the Internet exposure be symbolized by Δ, Equation (3) can be seen as using the income growth for three groups to define the counterfactual outcome; the difference between wage earners in the catalogue and non-catalogue groups, in addition to business owners in a catalogue area:

$$E\left(\Delta \log y_{ik} | nocat_k = 1, bus_j = 1\right) - E\left(\Delta \log y_{ik} | nocat_k = 0, bus_j = 1\right) - E\left(\Delta \log y_{ik} | nocat_k = 1, bus_j = 0\right) - E\left(\Delta \log y_{ik} | nocat_k = 0, bus_j = 0\right) = \delta_7. \quad (4)$$

We also show estimation results when restricting to business owners alone, which means that the dimension representing occupation is removed from Equation

[15] In Gruber (1994) this procedure is referred to as "differences-in-differences-in-differences".

[16] We cannot rule out that the fourth group, wage earners living in a catalogue area, responded to the change in exposure as well.

(3). As just discussed, a common-trend specification that does not include the wage earners implies that a potential omitted variable problem (idiosyncratic shocks at municipality level) is not controlled for, if we believe that the development for wage earners represent a valid description of the counterfactual. However, this simplification may remove a potential bias introduced by Equation (3) in measuring the effect on business owners, stemming from responses of wage earners: wage earners in the non-catalogue may react to the new regime of disclosure, whereas wage earners in the catalogue areas are less likely to react. We find it difficult to discriminate between the two specifications; we will find it reassuring if they both point to the same response magnitudes.

4 Results and sensitivity tests

4.1 Data and descriptive statistics

The primary sources of data for this study are the Income Statistics on Persons and Families (Statistics Norway, 2006). These statistics hold detailed micro panel information on the whole Norwegian population derived from several public registers, including a full coverage of data from income tax returns. We utilize data for eight years, from 1997 to 2004, which means that we have data for four years before the Internet exposure, 1997-2000, and for four years after, 2001-2004. We restrict our analysis to persons of working age (25-59 in 1997) who had positive income and lived in the same municipality in the period 1997-2000.[17] Given that the assignment into groups with and without paper catalogues prior to the Internet disclosure in 2001 is a key characteristic of the identification strategy, we restrict the sample to individuals in the 137 municipalities (from a total of near 430 municipalities in Norway) in the treatment and control groups. This means that we exploit data for approximately 370,000 individuals.

In Table 1, which shows estimates of mean values for individual-level characteristics used in the regressions (Table 2 shows descriptive statistics for the municipality-level variables), the two different time periods are referred to as "before" and "after". The income concept used is "earned income", consisting of wage income and earnings from self-employment (and other organizational forms which require that individuals report business income). Thus, the measure of income used does not include capital income. However, we show results for an alternative definition of income (capital income included) in the sensitivity tests, following after the presentation of main results. Further, we categorize individuals as being business owners or wage earners. This is done with respect to accumulated

[17] We do this to ensure sure that modes of disclosure (before 2001) are fully absorbed by the individuals.

income over the whole eight-year time period, and individuals are allocated into one of the two groups depending on the most dominant income source; business income or wage income.[18] Moreover, Table 1 reflects the key identifying tool of the present paper, by showing separate figures for people belonging to municipalities with and without distribution of paper catalogues in the first time period. The table includes figures for a number of individual characteristics that are controlled for in the empirical analysis: education (dummies for having education at the high school level and at the university level, respectively), marital status, number of children, gender and immigrant background.

We see that the average first-period income level, both among among business owners and wage earners, is somewhat higher in the "non-catalogue" areas. Education may be an explanation for that difference, as we see that a higher share of the population has a university-level education in these municipalities. But of more interest and consistent with the main hypothesis of the paper, we observe that the average growth in income among business owners in the "non-catalogue" areas is higher than in the "catalogue" areas: 18.4 percent and 16.1 percent, respectively. This is further shown in Figure 1, where the average reported income differences between non-catalogue and catalogue municipalities are shown for each year of the period 1993-2004 (thus, we have added information for four years prior to 1997)[19] for wage earners and business owners, respectively. The figure clearly depicts an abrupt change beginning in 2001, as the difference between average income for business owners moves above the similar measure for wage earners.[20] Figure A2 in the Appendix shows the income developments behind Figure 1, that is, the development in income for wage earners and business owners in the catalogue and non-catalogue areas, respectively. Of note (in Figure A2) is a marked reduction in reported income for owners of businesses in 2003, which is due to a change in the definition of business income.[21] However, we have no reason to expect that

[18] The tax system in place in the time period under investigation here was a dual income tax, introduced by the tax reform of 1992, and replaced by a modified version of a dual income tax system in 2006; see, for example, Sørensen (2005) and Thoresen, Bø, Fjærli, and Halvorsen (2012). Self-employed and owners of closely held firms report business income, and the so-called "split model" of the Norwegian dual income tax describes how this income is divided into capital return and return to the labor effort of the active owner. Thoresen and Alstadsæter (2010) describe how the split model of the dual income tax motivated business owners to move to a widely held firm organization to lower their tax burden. However, given the empirical approach of the present paper, we do not expect such manoeuvres to affect our results, as any such incentives would be identical as between businesses located in catalogue and non-catalogue municipalities.

[19] This means that the number of observations behind Figure 1 (given the data restrictions) is somewhat lower than for the rest of the analysis.

[20] The time series stops in 2004 as the tax reform of 2006 (phased in during 2005) represents a break. Both schedules and tax bases were changed by the reform.

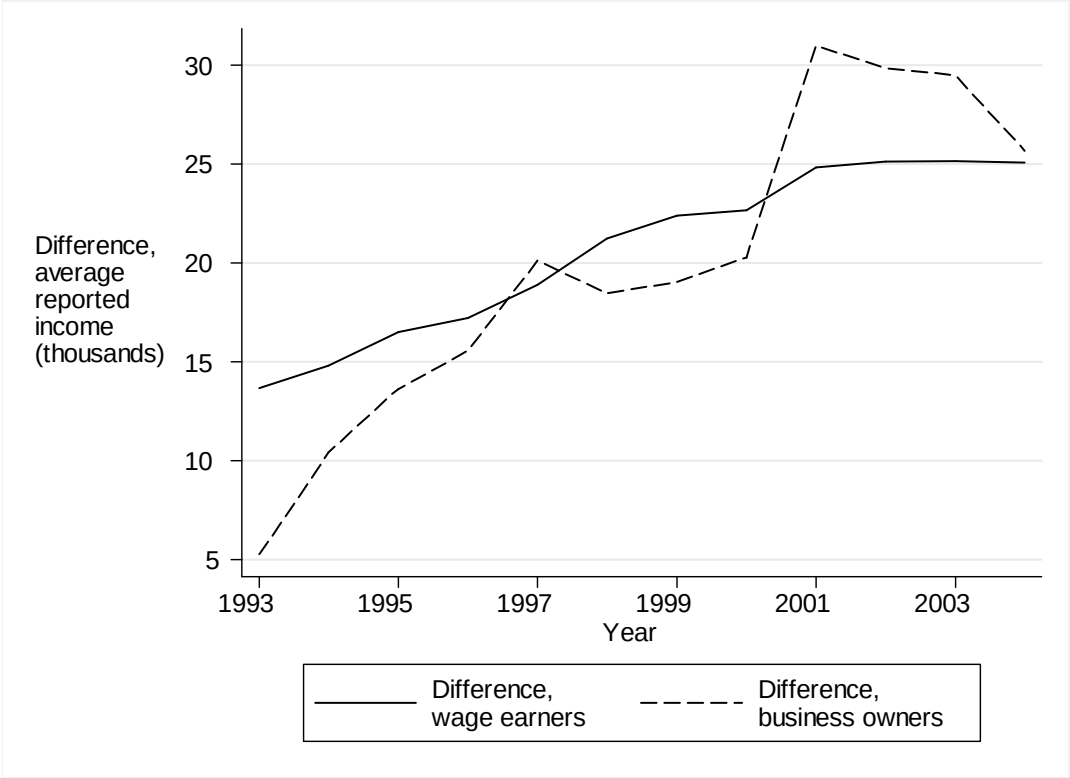
[21] The dependency on "accounting rules" is a drawback of data taken from administrative registers.

Table 1: Averages for individual characteristics, 1997-2000 (before) and 2001-2004 (after)

	Business owners			
	Non-catalogue		Catalogue	
	Before	After	Before	After
Income (NOK)[a]	294,651	349,035	275,964	320,467
Wage income (NOK)[a]	38,038	40,871	31,758	38,190
Business income (NOK)[a]	256,612	308,165	244,207	282,277
High school education	.57	.57	.61	.61
University education	.14	.14	.13	.13
Married	.69	.69	.69	.70
No of Children	.87	.73	.87	.72
Age (first period)	44.4		44.7	
Male	.76		.75	
Immigrant	.028		.022	
Number of individuals	21,258		8,091	
	Wage Earners			
	Non-catalogue		Catalogue	
	Before	After	Before	After
Income (NOK)[a]	255,155	305,985	234,469	281,485
Wage income (NOK)[a]	250,588	301,657	229,128	276,843
Business income (NOK)[a]	4,568	4,329	5,341	4,642
High school education	.51	.52	.51	.52
University education	.26	.27	.24	.25
Married	.64	.65	.62	.63
No of Children	.89	.78	.86	.74
Age (first period)	42.3		42.4	
Male	.52		.51	
Immigrant	.031		.022	
Number of individuals	261,338		78,111	

[a] Average exchange rate against USD, 1997-2004: 1$=7.75NOK

Figure 1: Average income differences between non-catalogue and catalogue municipalities, 1993-2004, wage earners and business owners. Thousand Norwegian kroner



the variation in the definition of income affects the measurement of income for the two groups of business owners differently.[22]

Municipality characteristics are linked to the individual income data based on information derived from the KOSTRA database, which is established by Statistics Norway for the comparison of municipalities. For example, the database includes population and employment statistics for the municipalities of Norway (Statistics Norway, 2012a; Statistics Norway, 2012b). Some of the variables we account for are characteristics that may be interpreted as indicators of economic prosperity, such as population growth, birth rates, unemployment and changes in local unemployment rates. We also include population size and the share of the population living in densely populated areas. Finally, we also include a description of the nature of the economic activity in the municipalities in terms of an industry

[22] We have also estimated Equation (3) without the years 2003 and 2004. The point estimates are similar to what we obtain when including all years in the regressions, while the standard errors are somewhat smaller.

Table 2: Averages for municipality-level characteristics

	Non-catalogue	Catalogue
Population (2001)	20,584	16,031
Population growth, 2000-2004	666	330
Births per 1000 inhabitants (2001)	12.2	10.9
Share in high population density area (2001)	71.0	56.8
Unemployment rate (2001)	2.6	2.4
Increase in unemployment, 2000-2004	1.1	0.8
Number of municipalities	106	31

classification system, which was developed by Statistics Norway in the mid 1990s (thus, some years before the data period). It consists of 16 different categories, characterizing the main economic activities of the municipality, such as farming, fisheries, manufacturing, service sectors, etc., which we code as dummy variables.

Table 2 presents mean values for the municipality level information (except the industry classification system), given the categorization into the non-catalogue and the catalogue groups. As with the individual characteristics, there are differences between the average measures, but the differences do not unambiguously give support to any conjectures regarding differential economic development in the two groups. We see that population growth and birth rates are higher on average in the non-catalogue areas, as are unemployment rates and unemployment growth.

4.2 Main results

In Table 3 we show the results of estimating Equation (2) by ordinary least squares (OLS). Errors may be serially correlated in panel data and there may be other sources of clustering, which means that error terms are not i.i.d. Following recommendations by Cameron, Gelbach and Miller (2006),[23] in Table 3 and in the following tables we cluster standard errors at the municipality level and by year. Results for three different specifications are presented: regression (1) does not include any controls for characteristics of the individuals and municipalities, regression (2) accounts for individual characteristics, whereas specification (3) controls for both.

The main parameter of interest in Table 3 is $\widehat{\delta_3}$, which shows the average treatment effect (introduction of Internet disclosure) on business owners in 2001. As signified by negative and insignificant parameter estimates, there are no signs of business owners increasing their reported income after 2001. Thus, we see no indication of an effect of public disclosure in Table 3. One possible confounding

[23] See also Bertrand, Duflo and Mullainathan (2004) and Donald and Lang (2007).

Table 3: Effect of public disclosure on income reporting. OLS-regressions based on wage earner/business owner group assignment

		(1)		(2)		(3)	
Explanatory var.	Coeff.	Est.	S.E.	Est.	S.E.	Est.	S.E.
Business owner	δ_1	.068***	(.017)	-.010	(.016)	.020	(.015)
Post-2001	δ_2	.027	(.057)	.026	(.048)	.026	(.051)
Business owner/ post-2001	δ_3	-.044	(.032)	-.018	(.032)	-.018	(.033)
Indiv. control var.		No		Yes		Yes	
Munic. control var.		No		No		Yes	
Observations		2,950,384		2,950,384		2,950,384	
R-squared		.013		.192		.202	

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

factor is that Norway went into a recession in 2001 (Statistics Norway, 2003), and even though this is expected to be captured by the municipality-level control variables, there may be systematic differences between wage earners and business owners not captured by Equation (2). Instead of exploring modifications of the empirical strategy employed in Equation (2), we turn to discussing results when using the distribution of paper catalogues prior to 2001 for group assignment.

As an introduction to identification of public disclosure through estimation of Equation (3), Table 4 presents a simple tabular version of the results based on the income estimates of Table 1. The table shows that the difference in income growth between business owners in non-catalogue and catalogue municipalities (as already noted) is 2.35 percentage points. If we adjust the benchmark for differences between income growth for wage earners of the two groups of municipalities, which is the case under the specification presented in Equation (3), the effect of public disclosure is slightly larger, 2.48 percentage points. Thus, the relatively lower income growth among wage earners in the treatment area does not provide any strong indications of public disclosure also affecting taxpayers in this group.[24] Moreover, as denoted in Section 3, given that we show results for both a specification that accounts for different income developments among wage earners in the catalogue and non-catalogue municipalities when establishing the counterfactual and for a specification that focuses on effects among business owners alone, the small difference in income growth between the two groups of wage earners,

[24]Even though it cannot be ruled out that it had an effect, as the counterfactual in this case is not described.

Table 4: Average growth in reported income, business owners and wage earners in non-catalogue and catalogue municipalities

	Non-catalogue	Catalogue	Difference
Income growth of business owners	18.41	16.06	2.35
Income growth of wage earners	19.92	20.05	-.13

reported in Table 4, is reassuring, as it does not indicate that the results depend on the chosen technique.

Next, in Table 5 we turn to OLS estimation results for Equation (3). When the distribution of paper catalogues (in some municipalities) prior to the Internet exposure is used for identification, a positive effect of public disclosure clearly stands out, as signified by the parameter estimates of δ_7. The estimate for specification (3) (control for both individual and municipality characteristics) suggests that on average approximately 3.1 percent of the growth in reported income among business owners in the non-catalogue areas can be attributed to the substantially increased Internet exposure from 2001 and onwards. The standard error is 0.66, which suggests a highly significant result (p-value below 0.01 percent). In terms of the average income measures of Table 1, this means that without public disclosure, average income among business owners after 2001 would have been approximately NOK331,000 instead of approximately NOK341,000.

This baseline result is largely invariant with respect to the extent to which other observable characteristics are controlled for. Correspondingly, the regression results are very close to the results of Table 4 (accounting for the table version showing results for differences in percentage points and not applying the log transformation). We interpret this as corroborative evidence for exploiting a group assignment procedure that is minimally subject to omitted variables bias.

To illustrate the economic influence of this effect, we have carried out some very simplified calculations. When multiplying the estimated income growth of 3.1 percent with the number of self-employed in 2001, and by using the average tax rate for the group, tax revenue increases by approximately NOK0.6 billion. This corresponds to approximately 0.1 percent of the total Norwegian tax revenue in 2001 (taxation of oil excluded).

As mentioned above, the inclusion of wage earners to depict the trend in incomes without the effect of Internet exposure, as in Equation (3), may be questioned. There may, for instance, be confounding factors that generate differential wage growth among business owners and not among wage earners. One cannot rule out that public disclosure may have affected wage earners too, and in that case the effect is most likely stronger for the wage earners of the non-catalogue area. Therefore we also derive estimates for an empirical specification restricted to business owners alone. A simplified version of Equation (3) is estimated, where

Table 5: Effect of public disclosure on income reporting. Estimation results for regressions based on pre-2001 catalogue group assignment

		(1)		(2)		(3)	
Explanatory var.	Coeff.	Est.	S.E.	Est.	S.E.	Est.	S.E.
Business owner	δ_1	.096***	(.022)	.003	(.019)	.025	(.018)
Post-2001	δ_2	.028	(.058)	.027	(.049)	.027	(.051)
Non-catalogue	δ_3	.072***	(.019)	.056***	(.015)	.025***	(.008)
Business owner/ post-2001	δ_4	-.068**	(.034)	-.040	(.035)	-.041	(.036)
Business owner/ non-catalogue	δ_5	-.034	(.027)	-.015	(.023)	-.007	(.021)
Post-2001/ non-catalogue	δ_6	-.002	(.008)	-.002	(.005)	-.002	(.003)
Public disclosure[a]	δ_7	.033***	(.008)	.031***	(.007)	.031***	(.007)
Indiv. control var.		No		Yes		Yes	
Munic. control var.		No		No		Yes	
Observations		2,950,384		2,950,384		2,950,384	
R-squared		.015		.193		.202	

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

[a] Business owners in the non-catalogue area after Internet exposure, wage earners incl.

Table 6: Effect of public disclosure on income reporting. Estimation results for regressions based on pre-2001 catalogue group assignment, business owners only

		(1)		(2)		(3)	
Explanatory var.	Coeff.	Est.	S.E.	Est.	S.E.	Est.	S.E.
Business owner	δ_1	.004	(.080)	.007	(.073)	.006	(.077)
Post-2001	δ_2	.038	(.028)	.035	(.023)	-.022	(.022)
Public disclosure[a]	δ_3	.031***	(.011)	.029***	(.009)	.029***	(.006)
Indiv. control var.		No		Yes		Yes	
Munic. control var.		No		No		Yes	
Observations		234,792		234,792		234,792	
R-squared		.007		.124		.142	

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

[a] Business owners in the non-catalogue area after Internet exposure

the occupational group dimension is removed and we restrict the sample to business owners only. Table 6 reveals, as expected given the very small difference in growth rates presented in Table 4, that the public disclosure effect in this more restricted sample is only slightly smaller than as seen in Table 5, with a 2.9 percent average growth in reported income attributed to Internet public disclosure. It is reassuring that the two different specifications point to approximately the same response magnitudes.

The identification rests upon several assumptions, some of which will be addressed in the next subsection. The key assumption that business owners in the catalogue area are not influenced by the Internet exposure - the common trend assumption - is not testable. However, it is important to note that the implication of public disclosure also affecting members of the control groups is that the estimates of Table 5 and Table 6 are downward biased.

4.3 Results from alternative methods

In this section we assess the robustness of the main results with respect to some alternative methodological choices. To reduce the likelihood of results that are falsely interpreted as public disclosure effects, and to address potential deficiencies of the empirical design, we discuss some of the main methodological challenges. We will address several issues: definition of income, inference, placebo tests, functional form dependence, matching, panel data estimation, and results for more specific groups of business owners. For the purpose of comparison we use the estimate from Table 5, column (3) (based on Equation (3), with controls for individual and

municipality level characteristics) as the benchmark.

Definition of income So far we have used gross "earned income" as the income variable. In Table 7 we show results for an alternative income concept: "ordinary income" under the dual income tax system of Norway, which is the income concept reported by the tax authorities in the public disclosure. This measure of income takes capital income and some income deductions into account. We see a clearly significant response estimate when using this alternative income concept too; indeed, the measured response is higher than the main estimate (referred to as the "Base specification" in Table 7)

Inference As already discussed, an important challenge of the empirical design is the possibility of correlations over time and between individuals of the same group, which may result in clustered or non-independent errors.[25] Ignoring such effects increases the probability of false rejections of the null hypothesis. Consequently, above we reported results for a procedure suggested by Cameron, Gelbach and Miller (2006), which adjusts measures of variance for two-way clustering, both municipality and year clustering.

In Table 7 we show results for three alternative methods to derive standard errors, to show that the significance of our main estimate for the effect of public disclosure is not dependent on the precise method for statistical inference. To facilitate comparison, in Table 7 we report estimates in terms of percentage changes. The robust variance refers to the standard "sandwich" (or Eicker-Huber-White) estimate of variance, which accounts for heteroskedastic disturbances by using the empirical variance-covariance matrix; see Froot (1989) and Rogers (1993). Following recommendations by Bertrand, Duflo and Mullainathan (2004) to produce consistent standard errors, we also show results for two methods to account for serially correlated errors: one-way clustering at the municipality level and a specification which diminishes the effect of the panel dimension of the data by aggregating into two periods only, before and after the Internet exposure. Even though some variation in estimates and standard errors across techniques are observed, all measures give support to public disclosure having a statistically significant effect on reported income.

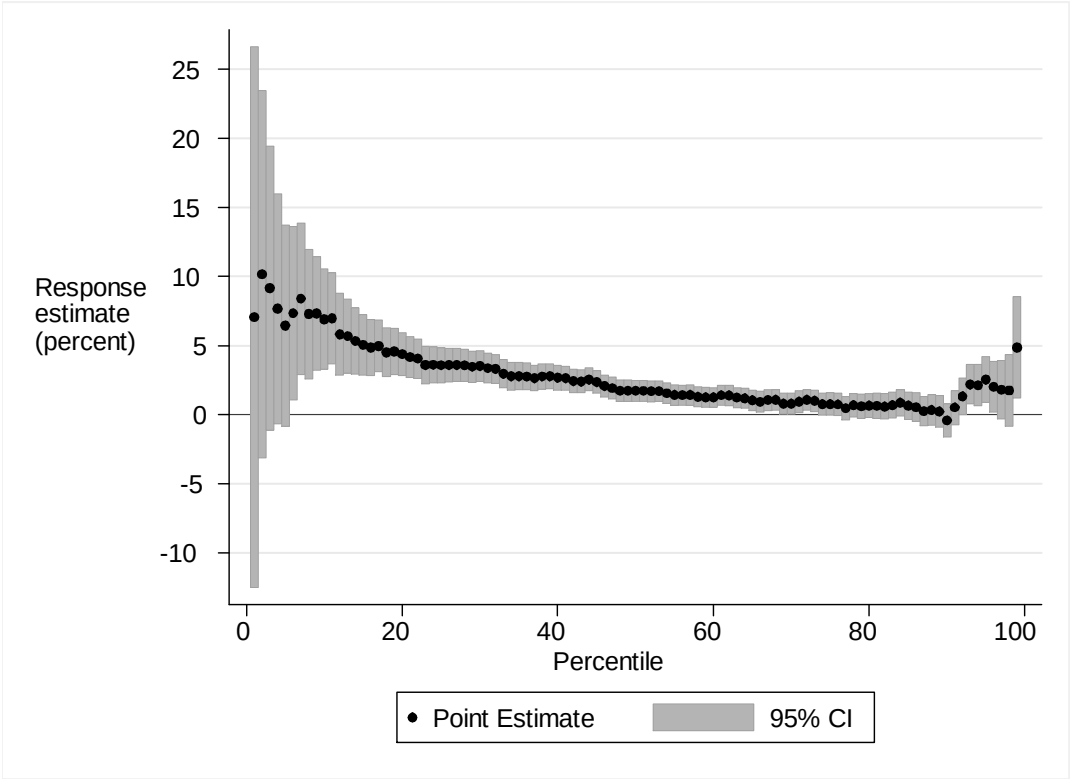
Placebo tests In order to assess to what extent the method is sensitive to picking up effects that are unrelated to the phenomenon in question, we have carried out three different placebo tests. In the first test we proceed as if the intervention happened in 1999 instead of 2001, and measure incomes in the two

[25] Recall that no specific measures have been taken to utilize the panel structure of the data. We discuss results of panel data estimation below.

Table 7: Effect of public disclosure on reported incomes for alternative methodological approaches

	Estimate	Standard error
Base specification	3.08***	0.66
Another definition of income		
All taxable income minus deductions	4.88***	1.51
Alternative variance estimators		
Robust variance	3.08***	0.71
Clustering at the municipality level	3.08***	1.13
Collapsed income for two periods	2.85***	0.58
Placebo tests		
Internet exposure introduced in 1999	-0.10	0.83
Random assignment to treatment and control	-0.18	0.96
Prop. score alloc. of control group municip.	-0.14	1.12
Alternative functional forms		
No log-transformation of dependent variable	2.05*	1.25
Median regression	1.73***	0.40
Matching		
Propensity score	3.16***	0.81
Panel data method		
Fixed effect	2.58***	0.69
Specific groups		
Taxi drivers and restaurant owners	-0.56	0.57
Above median municip. population density	2.58***	0.69
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$		

Figure 2: Effect of public disclosure on income reporting across percentiles. Quantile regressions for specification based on pre-2001 catalogue group assignment



years before and after, 1997-1998 and 1999-2000.[26] In the second test we let the computer randomly assign municipalities to the treatment and control groups, using the same group sizes as before. We have carried out 100 such random assignments and report the average estimates across the 100 simulations. The third placebo test uses results of propensity score estimation (Rosenbaum and Rubin, 1983) to generate a placebo reform for half of the control group. If it is possible to obtain significant results based on observable characteristics of the municipalities, it would indicate that there are observable characteristics correlated with the treatment and contributing to the significant effect of the main estimate. The municipalities in the control group are allocated to (placebo) treatment and control groups, depending on their propensity score,[27] such that the 15 municipalities with the highest propensity are allocated to the treatment group, and the 15 municipalities with the lowest propensity score are allocated to the control group. Equation (3) is then estimated with the new data set.

As shown in Table 7, the three placebo tests reveal no signs of effects of public disclosure. This is consistent with our belief that the assignment mechanism based on paper catalogues represents a convincing random assignment procedure. In the case where we randomly assign municipalities to the treatment and control groups, we observe a significant "effect" 11 times at the 5 percent level, which is more than expected (5), but still indicates a low probability for rejecting a true null hypothesis of no effect in this data set.

Functional form dependence Several authors have noted that the standard difference-in-differences estimator involves scale-dependent identifying assumptions; see Meyer, Viscusi and Durbin (1995), Heckman (1996) and Athey and Imbens (2006). In other words, the results of the analysis may be dependent on the functional form. For example, to this point we have employed a log transformation of the dependent variable, which puts a restriction on the common trend assumption that is different from what would be the case if we use non-transformed income as the dependent variable; for instance, Meyer et al. (1995) found results that were sensitive to this choice. Similarly, Table 7 shows that results are altered by using a non-transformed dependent variable. The point estimate is now 2.05, and it is only barely statistically significant different from 0 at the 10% level.

Further, we have investigated results for an alternative specification where the conditional median, or another quantile of the distribution, of the dependent variable is a linear function of the regressors, as in Koenker and Hallock (2001). Thus, quantile regressions provide predictions for the median or another point of the in-

[26] The years after 2000 are excluded, as they have been affected by the real reform.

[27] The propensity score estimation is based on a probit estimation of the probability of treatment based on municipality characteristics and mean values of individual characteristics.

come distribution with respect to public disclosure. In addition to being based on an alternative econometric specification (for example, in a median regression the coefficients will be estimated by minimizing the absolute deviations from the median),[28] this method very straighforwardly provides information about how slope coefficients vary over the income distribution.[29]

Figure 2 presents results of a number of quantile regressions (one for each percentile). For the median, we find an estimate of 1.7 percent, which is somewhat lower than the percentage change according to OLS of 3.1 percent. We note that all point estimates except one are above the horizontal (i.e., zero effect) line, but see that estimates are not significant (according to the 95 percent confidence interval) for very low incomes and for several income levels above the 70th percentile.

Matching Propensity score matching is usually used to enhance comparability between groups. Table 2 reveals some differences between the municipalities in our control and treatment group. One might worry that, even though we use controls for municipality characteristics, these differences may bias the results. In contrast to one of the sensitivity tests above, where we exploited propensity score matching to design a placebo estimation, we now use matching to make the control and treatment group more similar.[30] The average propensity score of the control and treatment group in our sample is respectively .64 and .81 before matching. Municipalities are then matched by pairwise (or nearest neighbor) matching.[31] We thus obtain a data set consisting of 29 matched municipality pairs, which are as similar as possible based on observable characteristics.[32] The individuals in these two groups are then used as a new sample, on which we estimate equation (3). Given the close correspondence between matching techniques and regressions (they are both control strategies) and the small effects of accounting for other explanatory variables on the estimate of the public disclosure effect, we do not expect the results to be sensitive to the use of a propensity score technique. Accordingly, we find that the estimate of the public disclosure effect is very similar to the estimate of the base specification.

Panel data estimation So far, the panel dimension of the data has not been utilized in the identification of effects, and we might as well have used data from

[28] There may also be other arguments for applying a quantile formulation, such as providing a more efficient estimator than OLS when the error term is non-normal.

[29] Of course, possible non-linear relationships can be investigated under OLS, too. However, quantile regression is a method where the distributional aspect is innate. See also Athey and Imbens (2006), who propose a nonlinear difference-in-differences method.

[30] See also Heckman, Ichimura and Todd (1997) and Abadie (2005) for approaches to matching.

[31] We use a caliper, i.e. largest allowed difference of propensity score between matches, of .025.

[32] The propensity score is now respectively .66 and .65 for the control and treatment group.

repeated cross-sections. As highlighted by Equation (1), the approach may also account for individual fixed effects. Thus, in order to take advantage of the panel dimension of the data and ascertain to what extent results are influenced by controlling for individual heterogeneity, we have estimated an individual fixed effects version of Equation (3) without the wage earners. As expected, given the close correspondence between difference-in-difference estimation and fixed effects estimation, this specification also gives a clearly significant effect of public disclosure: the point estimate is 2.58.

Results for specific groups of business owners There are reasons to expect that there are differences across industries with respect to underreporting. Given that we have linked information about which industry the business owners belong to (Statistics Norway, 2005) to the dataset, there is scope for further examination of differences across different sectors. We do not know of any theoretical guidelines as to which types of sectors would be expected to react to public disclosure. In Table 7 we report the result of an estimation that have been carried out when limiting the group of business owners to taxi drivers and owners of restaurants and other catering businesses. These two groups of businesses have received particular attention from Norwegian tax authorities; for example, in the budget proposal for 2013 (Ministry of Finance, 2012), new regulations have been proposed to reduce tax evasion among taxi drivers. However, the non-significant estimate for the public disclosure effect suggests that Internet exposure has no influence (on average) on the income reporting of individuals belonging to these two types of businesses. As we do not know the specific mechanisms driving our main results, it is hard to speculate why we do not find any results here.

We may also explore if there are any other individual or municipality characteristics that mediate the magnitude of the public disclosure effect. For instance, above we discussed the effect of population density on public disclosure. When restricting the data set to individuals belonging to municipalities with above median population density, we see that the point estimate is somewhat lower than the base specification estimate. Thus, this result suggests that the public disclosure effect is somewhat larger in less dense, presumably rural, municipalities.

5 Conclusion

As of 2001 any Norwegian taxpayer with access to the Internet could find individual information on income, wealth, and income and wealth taxes paid. Prior to 2001, in some local areas access to this information was widespread. We have used this fundamental change in access to disclosure to identify income reporting effects of public disclosure. We attribute an approximately 3 percent increase in reported

income to Internet public disclosure. To our knowledge this is the first empirical estimate of the effect of public disclosure on overall individual tax compliance. We note, though, that the Norwegian version of public disclosure discussed in this paper is a rather extreme type of disclosure that is infeasible in many countries. Thus, the estimate may be an upper bound of effects of other methods of public disclosure.

References

[1] Abadie, A. (2005). Semiparametric Difference-in-Differences Estimators. Review of Economic Studies, 72, 1–19.

[2] Abadie, A. and J. Gardeazabal (2003). The Economic Costs of Conflict: A Case Study of the Basque Country. American Economic Review, 93, 112–132.

[3] Allingham, M.G. and A. Sandmo (1972). Income Tax Evasion: A Theoretical Analysis. Journal of Public Economics, 1, 323–338.

[4] Alm, J. (2012). Measuring, Explaining, and Controlling Tax Evasion: Lessons from Theory, Experiments, and Field Studies. International Tax and Public Finance, 19, 54–77.

[5] Andreoni, J., B. Erard, and J. Feinstein (1998). Tax Compliance. Journal of Economic Literature, 36, 818–860.

[6] Angrist, J.D. and J.-S. Pischke (2009). Mostly Harmless Econometrics. An Empiricist Companion. Princeton University Press, Princeton and Oxford.

[7] Athey, S. and G.W. Imbens (2006). Identification and Inference in Nonlinear Difference-in-Differences Models. Econometrica, 74, 431–497.

[8] Bertrand, M., E. Duflo, and S. Mullainathan (2004). How Much Should We Trust Differences-in-Differences Estimates? Quarterly Journal of Economics, 119, 249–275.

[9] Blank, J.D. (2011). In Defense of Individual Tax Privacy. New York University Law and Economics Working Papers 263.

[10] Blundell, R. and M.C. Dias (2009). Alternative Approaches to Evaluation in Empirical Microeconomics. Journal of Human Resources, 44, 565–641.

[11] Brock, W. and Durlauf, S. (2001). "Interactions-Based Models". In J. Heckman and E. Leamer (eds.), Handbook of Econometrics, vol. 5, North Holland, Amsterdam, 3297–3380.

[12] Cameron, A.C., J.B. Gelbach, and D.L. Miller (2006). Robust Inference with Multi-Way Clustering, NBER Technical Working Paper No. 327, National Bureau of Economic Research, Cambridge, MA.

[13] Card, D. (1990). The Impact of the Mariel Boatlift on the Miami Labour Market, Industrial and Labor Relations Review, 44, 245–257.

[14] Card, D. and A.B. Krueger (1994). Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania. American Economic Review, 84, 772–93.

[15] Coricelli, G., M. Joffily, C. Montmarquette, and M.-C. Villeval (2010). Cheating, Emotions, and Rationality: An Experiment on Tax Evasion. Experimental Economics, 13, 226–247.

[16] Donald, S.G. and K. Lang (2007). Inference with Difference-in-Differences and Other Panel Data. Review of Economics and Statistics, 89, 221–233.

[17] Erard, B. and J.S. Feinstein (1994). The Role of Moral Sentiments and Audits Perceptions in Tax Compliance. Public Finance/Finances Publiques 49, 70–89 (Supplement).

[18] Feldman, N. and J. Slemrod (2007). Estimating Tax Compliance with Evidence from Unaudited Tax Returns. Economic Journal, 117, 327–352.

[19] Fortin, B., G. Lacroix, and M.-C. Villeval (2007). Tax evasion and social interactions. Journal of Public Economics, 91, 2089–2112.

[20] Froot, K.A. (1989). Consistent Covariance Matrix Estimation with Cross-sectional Dependence and Heteroskedasticity in Financial Data. Journal of Financial and Quantitative Analysis, 24, 333–355.

[21] Gruber, J. (1994). The Incidence of Mandated Maternity Benefits. American Economic Review, 84, 622–644.

[22] Heckman, J. (1996). "Comment on Eissa: Labor Supply and the Economic Recovery Act of 1981". In M. Feldstein and J. Poterba (eds.), Empirical Foundations of Household Taxation, The University of Chicago Press, Chicago, 5–38.

[23] Heckman, J., H. Ichimura, and P. Todd (1997). Matching as an Econometric Evaluation Estimator: Evidence from Evaluating a Job Training Programme. Review of Economic Studies, 64, 605–654.

[24] Hurst, E., G. Li, and B. Pugsley (2010). Are Household Surveys Like Tax Forms: Evidence from Income Underreporting of the Self-Employed. NBER Working Paper No. 16527 (November), National Bureau of Economic Research, Cambridge, MA.

[25] Imbens, G.W. and J.M. Wooldridge (2009). Recent Developments in the Econometrics of Program Evaluation. Journal of Economic Literature, 47, 5–86.

[26] IRS (2011). Disclosure and Privacy Law Reference Guide. Publication 4639 Catalog Number 50891P (Rev. 9-2011), Department of the Treasury Internal Revenue Service.

[27] Koenker, R. and K. Hallock (2001). Quantile Regression. Journal of Economic Perspectives, 15, 143–156.

[28] Laury, S. and S. Wallace (2005). Confidentiality and Taxpayer Compliance. National Tax Journal, 57, 427–438.

[29] Lechner, M. (2011). The Estimation of Causal Effects by Difference-in-Difference Methods. University of St. Gallen Department of Economics Discussion Paper no. 2010-28, University of St. Gallen. Revised October 2011.

[30] Lenter, D., D. Shackelford, and J. Slemrod (2003). Public Disclosure of Corporate Tax Return Information: Accounting, Economics, and Legal Perspectives. National Tax Journal, 56, 803–830.

[31] Manski, C. (1993). Identification of Endogenous Social Effects: The Reflection Problem, Review of Economic Studies, 60, 531–42.

[32] Meyer, B.D., W.K. Viscusi, and D.L. Durbin (1995). Workers' Compensation and Injury Duration: Evidence from a Natural Experiment. American Economic Review, 85, 322–40.

[33] Ministry of Finance (2011). Answer No. 33 to members of Parliament. 19 May, 2011 (in Norwegian).

[34] Ministry of Finance (2012). Prop. 1 LS (2012-2013) (in Norwegian).

[35] Myles, G.D. and R.A. Naylor (1996). A Model of Tax Evasion with Group Conformity and Social Customs. European Journal of Political Economy, 12, 49–66.

[36] Norwegian Tax Administration (2012). Press release about the tax lists for 2012, 18 October, 2012 (in Norwegian).

[37] NOU (2009:1). Individ og integritet. Personvern i det digitale samfunnet, Norges offentlige utredninger. Departementenes servicesenter, Oslo, Norway (in Norwegian).

[38] Pissarides, C.A. and G. Weber (1989). An Expenditure-Based Estimate of Britain's Black Economy. Journal of Public Economics, 39, 17–32.

[39] Rogers, W.H. (1993). Regression standard errors in clustered samples. Stata Technical Bulletin 13, 19–23. Reprinted in Stata Technical Bulletin Reprints, 3, 88–94.

[40] Rosenbaum, P.R. and D.B. Rubin (1983). The Central Role of the Propensity Score in Observational Studies for Causal Effect. Biometrika, 70, 41–55.

[41] Slemrod, J. (2007). Cheating Ourselves: The Economics of Tax Evasion. Journal of Economic Perspectives, 21, 25–48.

[42] Slemrod, J., M. Hasegawa, J. Hoopes, and R. Ishida (2012). The Effect of Public Disclosure on Reported Taxable Income: Evidence from Individuals and Corporations in Japan, University of Michigan Working Paper.

[43] Slemrod, J. and C. Weber (2012). Evidence of the Invisible: Toward a Credibility Revolution in the Empirical Analysis of Tax Evasion and the Informal Economy. International Tax and Public Finance, 19, 25–53.

[44] Statistics Norway (2003). Economic Survey 3/2003. http://www.ssb.no/vis/kt_en/arkiv/art-2003-09-11-01-en.htm.

[45] Statistics Norway (2005). Account Statistics. Annual Reports for Non-financial Limited Companies, http://www.ssb.no/english/subjects/10/regnaksje_en/.

[46] Statistics Norway (2006). Income Statistics for Persons and Families. http://www.ssb.no/english/subjects/05/01/inntpf_en/.

[47] Statistics Norway (2012a). Population Statistics. http://www.ssb.no/english/subjects/02/02/folkendrkv_en/.

[48] Statistics Norway (2012b). Register-based Employment Statistics. http://www.ssb.no/english/subjects/06/01/regsys_en/.

[49] Sørensen, P.B. (2005). Neutral Taxation and Shareholder Income. International Tax and Public Finance, 12, 777–801.

[50] Thoresen, T.O. and A. Alstadsæter (2010). Shifts in Organizational Form under a Dual Income Tax System. FinanzArchiv/Public Finance Analysis, 66, 384–418.

[51] Thoresen, T.O., E.E. Bø, E. Fjærli, and E. Halvorsen (2012). A Suggestion for Evaluating the Redistributional Effects of Tax Changes: With an Application to the 2006 Norwegian Tax Reform. Public Finance Review, 40, 303–338.

[52] Vaage, O.F. (2002). Norsk mediebarometer 2001. Statistical Analyses 53, Statistics Norway (in Norwegian).

[53] Yitzhaki, S. (1974). A Note on 'Income Tax Evasion: A Theoretical Analysis'. Journal of Public Economics, 3, 201–2.

A Appendix: Figure appendix

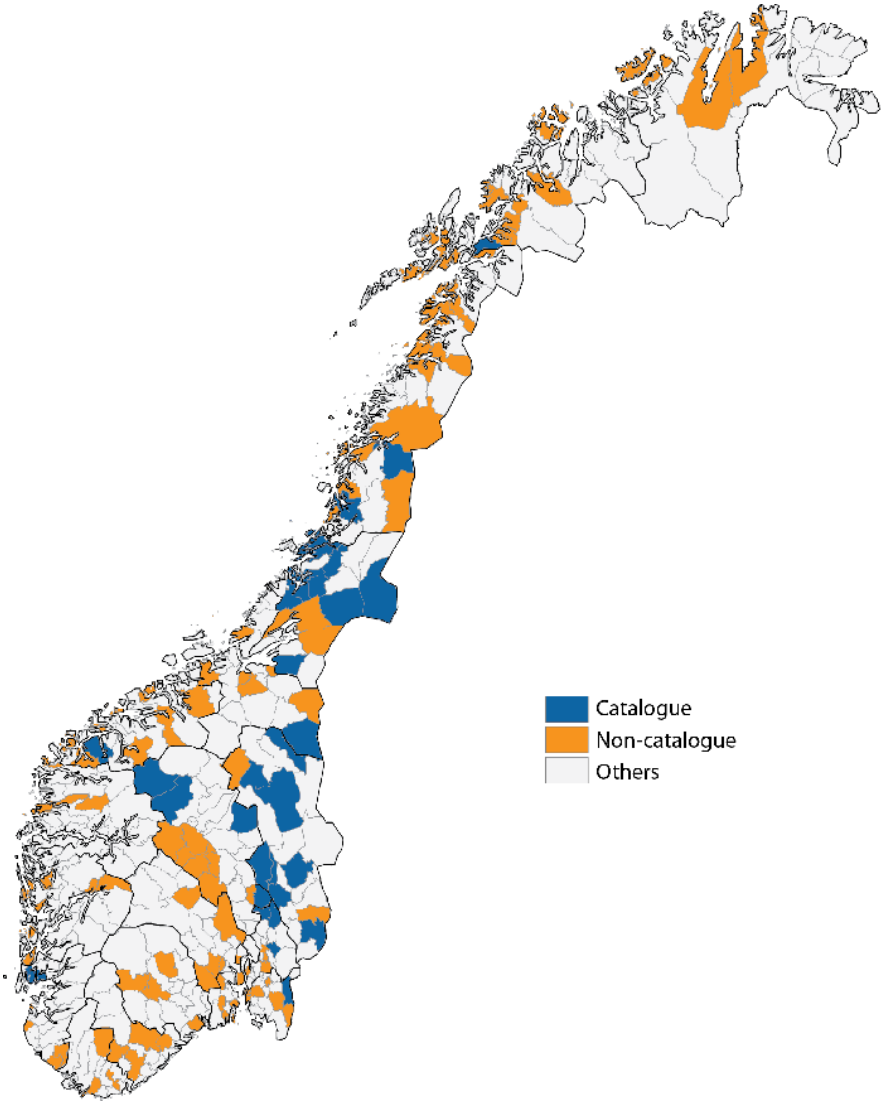


Figure A1. Spatial location of catalogue and non-catalogue municipalities

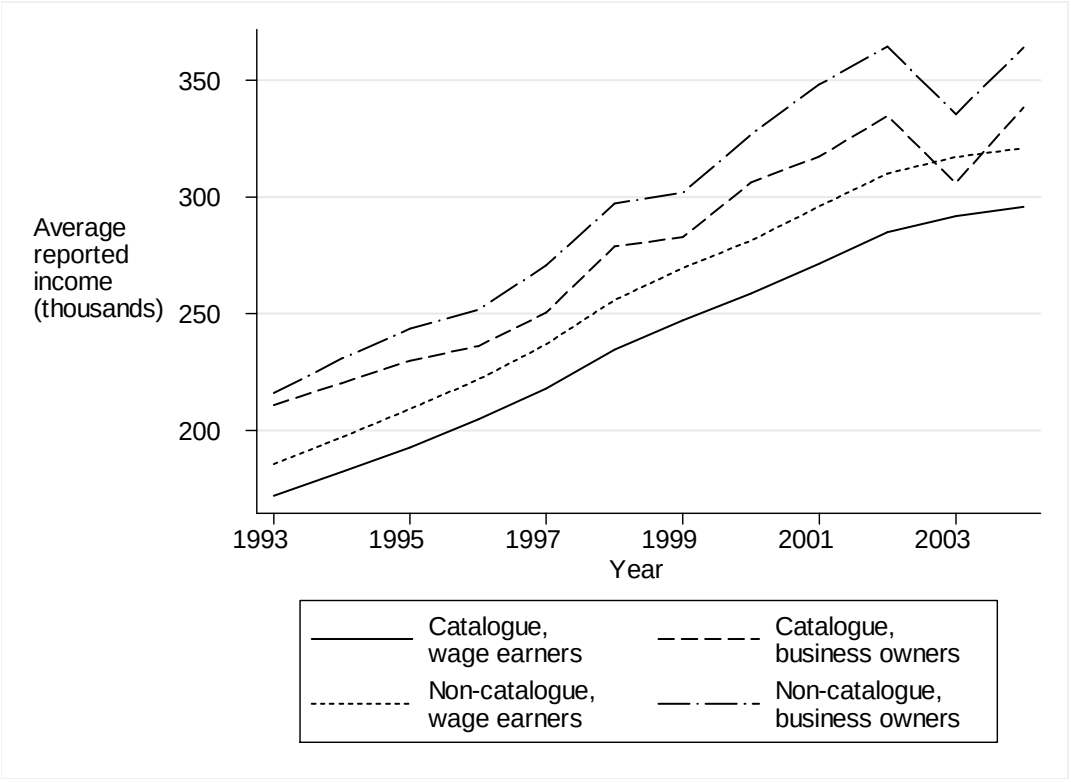


Figure A2. Average reported income 1993-2004, wage earners and business owners. Thousand Norwegian kroner