Taxes on the Internet: Deterrence Effects of Public Disclosure*

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Abstract

Supporters of public disclosure of personal tax information points 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 find 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, utilizing the fact that some local areas, prior to the shift to the Internet in 2001, 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 distributed. We observe a 2.7 percent average increase in incomes after 2001 among business owners living in areas where the switch to Internet disclosure represented a large change in access, which is consistent with public disclosure deterring tax evasion.

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

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that neighbors observe income details, taxpayers may be reluctant to underreport income, because a lack of correspondence between consumption of durables, such as house and car, and reported income, may induce reactions (from the neighbors) or represent a reputational loss. However, to our knowledge, effects of public disclosure on individual income reporting have never been systematically explored. One main 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. It has been traced 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: net income, tax payment, and net 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 most wealthy, or incomes of sports and entertainment celebrities.

However, the advent of 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 main 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 hassle 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 a new annual volume.

The practice of public disclosure was controversial even in the days of paper lists, but Internet access generated substantial resistance towards the system. Openness was challenged by arguments referring to invasion of privacy, generated 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 on a raid 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

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

can still search 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. 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 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 pre-2001 full scale tax-return information diffusion or not.

With respect to econometric identification, one would reasonably assume that business owners are randomly assigned to the two different categories of municipalities, municipalities with no pre-2001 special information distribution arrangements and municipalities with spread 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 given reporting effects, whereas no such effects are expected in the former group. Applying the difference-in-differences estimator to compute differences in mean income changes between the two groups after 2001 holds the promise of identifying the effect of Internet public disclosure on the income reporting of business owners.

Thus, the sample of individuals used in this study consists of persons from the 137 municipalities[3], observed before and after 2001 (from 1997 to 2004), and

[2]Even though digital search has been made more complicated from 2011, 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).

[3]From a total of near 430 municipalities in Norway.

categorized according to two different systems of information spread prior to 2001. As the income data we have available for this study are register-based and cover the whole population, that data set consists of approximately 345,000 individuals in 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). If Internet public disclosure is perceived as a more wide-ranging type of public disclosure, the treatment group is also influenced. However, the implication of the treatment group also being affected by the intervention is that the estimate is downward biased, measuring a minimum average response. This and other measurement problems will be elaborated upon through several robustness tests.

The plan of the paper is as follows. In Section 2 we briefly discuss the background for public disclosure, whereas the empirical strategy is described in Section 3. 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,[4] 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 penalty).[5] 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 (2012), provide support for public disclosure of non-compliance acting as an additional penalty mechanism.

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

[5] 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.

For instance, Corricelli et al. (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 attracting 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 on 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 (as reputational harm); see, for instance, Manski (1993) and Slemrod and Weber (2012).[6]

Accordingly, the empirical evidence is sparse on public disclosure in the income tax context. Slemrod, Hasegawa, Hoopes, and Ishida (SHHI, 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, SHHI uncover no evidence that disclosure increased reported business taxable income generally.

[6] 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.[7] 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[8] 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 at the insistence of data protectors.[9]

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 lists are 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 are 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,

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

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

[9] 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.

business associates and friends."

3 Empirical strategies

3.1 Internet exposure marks a difference

Since the middle of the nineteenth century there has been public disclosure of tax information in Norway. 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: net income, net 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 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 take 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 is sufficient to consider the move to the Internet a dramatic change in exposure among taxpayers.

From a rather general viewpoint let 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 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 over to the Internet 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.[10] 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. In so far as many interesting studies of the treatment literature focus on various effects of social interactions, 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 can be considered as independent.

A standard assumption of the difference-in-differences method is the assumption that time effects or trends are the same in both groups in the absence of the event (Internet exposure).[11] Thus, if we define non-treatment outcomes by y_{it}^n and observed covariates by q_{it}', and let period 0 and period 1 define periods before and after the Internet exposure, respectively, the equal trend assumption states that

$$E\left(\log y_{i1}^n | D_i = 1, Q_{i1}^{'} = q_{i1}'\right) - E\left(\log y_{i0}^n | D_i = 1, Q_{i0}^{'} = q_{i0}'\right) =$$
$$E\left(\log y_{i1}^n | D_i = 0, Q_{i1}^{'} = q_{i1}'\right) - E\left(\log y_{i0}^n | D_i = 0, Q_{i0}^{'} = q_{i0}'\right) \quad (2)$$

In other words, without any intervention (Internet), the growth in income is equal in the two groups, conditional on other characteristics, $Q_{it}^{'}$. It follows that it becomes important to find a mechanism for group assignment that mimics randomization. Next, we discuss which type of information that 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),

[10] 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.

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

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 less scope for tax evasion, compared to people running their own businesses.[12] Third-party reporting of employees' income, which is a standard procedure in Norway, curbs the possibilities for underreporting among wage earners (Slemrod, 2007), and 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, bus_j, which takes the value 1 if individual i 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_i'), and unobservable individual effects (ε_{ijt}):

$$\log y_{ijt} = \alpha_0 + X_{ijt}^{'}\beta + \delta_1 bus_j + \delta_2 int_t + \delta_3 \left(bus_j \times int_t\right) + \varepsilon_{ijt}, \tag{3}$$

where α_0, β, δ_1, δ_2 and δ_3 are parameters. Error terms are assumed to have the same distribution over time. The principal hypothesis we are investigating is that $\delta_3 > 0$, but we also expect that $\delta_2 > 0$.

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 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 for 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, that often communicated 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

[12] 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) analyzed 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.

this type of information to finance their activities. In some, but not all areas, a local organization, such as the football club or the community band, would offer copies of the tax transcript for sale, door-to-door. The main assumption behind the exploitation of this characteristic for identification is that taxpayers in the treated localities, persons in areas without widespread income tax return information prior to 2001, respond differently to the changes in disclosure brought about by the information becoming available on the Internet.

To ascertain which municipalities were treated and which were not, we conducted a separate survey, tracking local areas with and without the pre-2001 special arrangements. We have found 31 municipalities where there were pre-2001 sales of books of tax return transcripts, and have identified 106 municipalities in which no such arrangements existed; Figure A1 in the Appendix shows the locations of the two different categories of municipalities.[13] In the latter group of municipalities, inhabitants experienced a fundamental change in the information diffusion system after 2001, when the nationwide full-scale electronic version was in place.[14] Now the sample of owners are further differentiated with respect to a dichotomous characteristic, a dummy variable denoted $nocat_k$, which takes the value 1 when the individual (over the whole time period) 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:

$$\begin{aligned} \log y_{ijkt} &= \alpha_0 + X_{ijkt}^{'}\beta + \delta_1 bus_j + \delta_2 int_t + \delta_3 nocat_k + \delta_4 (bus_j \times int_t) + \\ &\quad \delta_5 (bus_j \times nocat_k) + \delta_6 (int_t \times nocat_k) + \delta_7 (bus_j \times int_t \times nocat_k) \\ &\quad + Z_k^{'}\gamma + \varepsilon_{ijkt}. \end{aligned} \tag{4}$$

The main parameter of interest is δ_7, and 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 is seen as a stronger type of display than paper lists and the income growth of business owners in the

[13] As the data collection was based on personal contact between interviewers and chief officers in the municipalities and therefore rather 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.

[14] 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$ per piece) and sales helped the financing of leisure activities for children.

control group are affected by the new disclosure regime too, 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.

The model specified in Equation (4) can be characterized as saturated in the main regressors of the model, 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 (4) is not restrictive, see Angrist and Pischke (2009); we will return to the functional form dependency below. Equation (4) 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 (3), but is particularly relevant in Equation (4) as 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. Accounting for covariates, as municipality characteristics, $Z_k^{'}$ and individual characteristics, $X_{ijkt}^{'}$, is helpful for the precision of the estimate of the public disclosure effect.

Note that the wage earners, in contrast to their role in Equation (3), enter into Equation (4) as an additional control for the time trend; see Gruber (1994) for a similar approach.[15] If there for instance are omitted variables, as local idiosyncratic economic shocks not picked up the explanatory variables, it may useful 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 are representative for the responses of business owners. However, if wage earners who were shocked by the Internet exposure in 2001 (non-catalogue area) also adjusts their income to the new regime, the estimate of δ_7, as representing an effect for business owners, is biased downward.[16]

By letting the difference in income before and after the Internet exposure be symbolized by Δ, Equation (4) 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:

$$E\left(\Delta \log y_{ik} | nocat_k = 1, bus_j = 1\right) - E\left(\Delta \log y_{ik} | nocat_k = 0, bus_j = 1\right) - E\left(\Delta \log y_{ik} | nocat_k = 1, bus_j = 0\right) - E\left(\Delta \log y_{ik} | nocat_k = 0, bus_j = 0\right) = \delta_7. \quad (5)$$

We also show results when restricting the data set to business owners alone, which means that the dimension representing occupation is removed from Equa-

[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 in a catalogue area, have responded to the change in exposure too.

tion (4). As just discussed, a common trend specification which does not include the wage earners implies that a potential omitted variable problem (idiosyncratic shocks) is not controlled for, if we believe that the development for wage earners represent a valid description of the counterfactual. Moreover, this simplification may also remove a potential bias introduced by Equation (4) in measuring the effect on business owners, stemming from wage earners in the non-catalogue areas reacting to the new regime of disclosure, and wage earners in the catologue areas not reacting. Obviously, it is hard to discriminate between the two specifications; we 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 source of data for this study is the Income Statistics on Persons and Families (Statistics Norway, 2006). These statistics hold detailed micro panel information on the 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 in all eight years, and who lived in the same municipality for all eight years. 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 experiment and control groups. This means that we exploit data for approximately 345,000 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". Further, following from the empirical strategy, we categorize individuals into business owner or wage earner. This is done with respect to accumulated 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. Under the Norwegian dual income tax,[17] self-employed and owners of closely held firms report business income, and the so-called "split model" describes how this

[17] 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).

income is divided into capital return and return to the labor effort of the active owner.[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 being born outside Norway.

We see the that 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 more interesting 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 described in Figure 1, where the average income differences between the non-catalogue municipalities and catalogue municipalities are shown for each year of the period 1997-2004, for wage earners and business owners, respectively. The figure clearly depicts that there is an abrupt change beginning in 2001, as the difference between average income for business owners moves above the similar measure for wage earners. Figure A2 in the Appendix shows the income developments behind Figure 1, that is, the developments in income for wage earners and business owners in the catalogue and non-catalogue areas, respectively. It shows a marked reduction in income for owners of businesses in 2003, which is due to a change in the definition of income.[19] However, we have no reason to expect that this change in income definition affects business owners of the two groups differently.[20]

Even though there is no reason, a priori, to expect that there are systematic differences between the non-catalogue and the catalogue municipalities, it is important to control for observed characteristics of the local areas. Municipality characteristics hold the promise of accounting for differences in the economic environment which may influence income growth. We have therefore linked the individual income data to characteristics derived from the KOSTRA database,

[18] 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 results, as any such incentives would be identical as between catalogue and non-catalogue municipalities.

[19] The dependency of "accounting rules" is drawback of data from administrative registers.

[20] We have also estimated Equation (4) without the years 2003 and 2004. The results are similar (though the standard errors are somewhat smaller), and available on request

Table 1: Averages for individual characteristics, 1997-2000 and 2001-2004

	Business owners			
	Non-catalogue		Catalogue	
	Before	After	Before	After
Income (NOK)[a]	294,889	349,199	275,467	319,713
Wage income (NOK)[a]	37,055	40,087	31,161	37,250
Business income (NOK)[a]	257,834	309,112	244,306	282,462
High school education	.57	.57	.61	.61
University education	.14	.14	.13	.13
Married	.69	.70	.70	.71
No of Children	.89	.74	.88	.73
Age (first period)	44.6		44.8	
Male	.76		.75	
Immigrant	.027		.021	
No of obs	163,408		62,800	
	Wage Earners			
	Non-catalogue		Catalogue	
	Before	After	Before	After
Income (NOK)[a]	255,345	305674	234418	280731
Wage income (NOK)[a]	250,761	301,355	229,035	276,081
Business income (NOK)[a]	4,583	4,319	5,382	4,650
High school education	.52	.52	.51	.52
University education	.26	.27	.23	.24
Married	.66	.67	.64	.65
No of Children	.90	.79	.87	.75
Age (first period)	42.6		42.6	
Male	.52		.51	
Immigrant	.029		.021	
No of obs	1,941,072		587,040	

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

Figure 1: Average income differences between catalogue and non-catalogue municipalities, 1997-2004, wage earners and business owners. Thousand Norwegian kroner

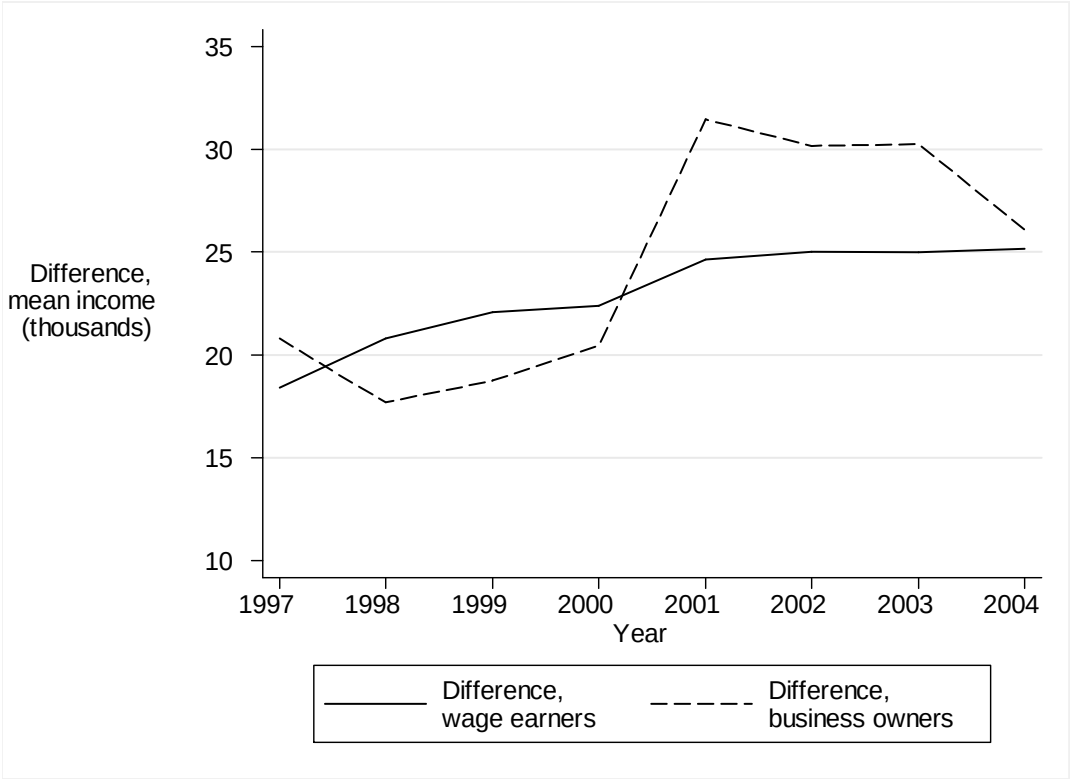


Table 2: Averages for municipality level characteristics

	Non-catalogue	Catalogue
Population (2001)	20,500	16,169
Population growth, 2004-2000	657	327
Births per 1000 inhabitants (2001)	12.1	10.9
Share in high population density area (2001)	70.9	56.9
Unemployment rate (2001)	2.7	2.4
Difference in unemployment, 2004-2000	1.1	0.8
Number of municipalities	106	31

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 which may be interpreted as indicators of economic prosperity, 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. The latter variable may also influence the deterrence effect of the Internet exposure, although it is not obvious in what direction. Finally, we also include a description of the economic basis of the municipalities in terms of an industry classification system, which was developed by Statistics Norway in the mid 90s (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 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 (3) by ordinary least squares (OLS).[21] Errors may be serially correlated in panel data and there may be other sources of clustering, which means that error terms are not independently and identically distributed. Following recommendations by Cameron, Gelbach and Miller (2006),[22] 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.

Table 3 shows that there is a large average increase in reported income in general after 2001, and that the growth rate of business owners does not deviate

[21] As we use register data and thus no stratification is involved, weights are not used in the regression. We will return to the weight issue when discussing results of matching procedures in the sensitivity tests to follow.

[22] Extending the suggestions by Bertrand, Duflo and Mullainathan (2004). See also the discussion of inference when exploiting the difference-in-differences estimator in 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.		Est.	S.E.	Est.	S.E.	Est.	S.E.
Business owner	δ_1	.066***	(.017)	-.011*	(.016)	.030*	(.016)
Post-2001	δ_2	.155***	(.040)	.154***	(.037)	.160***	(.039)
Business owner/ post-2001	δ_3	-.041	(.032)	-.017	(.032)	-.017	(.033)
Indiv. control var.		No		Yes		Yes	
Munic. control var.		No		No		Yes	
Observations		2,754,320		2,754,320		2,754,320	
R-squared		.011		.195		.205	
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$							

much from that. The main parameter of interest in Table 3 is δ_3, which shows the average treatment effect (introduction of Internet) 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 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 (3). Instead of exploring effects of improvements in Equation (3), we go on to discuss results when using the distribution of paper catalogues prior to 2001 for group assignments.

As an introduction to identification of public disclosure through estimation of Equation (4), 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 alreday noted) is 2.3 percentage points, and 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 (4), the effect of public disclosure is slightly larger, 2.4 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.[23]

[23]Even though it cannot be ruled out that it has had effect, as the counterfactual in this case is not descibed.

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

	Non-catalogue	Catalogue	Difference
Income growth of business owners	18.4	16.1	2.3
Income growth of wage earners	19.7	19.8	-.1

Moreover, as denoted in Section 3, given that we show results for both a specification which accounts for different income developments among wage earners in the catalogue and non-catalogue municipalities when establishing the counterfactual and for a specification which focuses on effects among business owners alone, the small difference in income growth between the two groups, reported in Table 4, is reassuring, as it does not give any support for the results depending on the chosen technique.

Next, in Table 5 we turn to OLS estimation results for Equation (4). When the distribution of paper catalogues in some municipalities prior to the Internet exposure is used for identification of the effect of public disclosure, a positive effect of public disclosure clearly stands out, as signified by the parameter estimates of δ_7. The estimate for specification (3) suggests that on average approximately 2.7 percent of the income growth among business owners in the non-catalogue areas can be attributed to the substantially increased Internet exposure from 2001 and onwards.[24] In terms of the average income measures of Table 1, this means that without the Internet public disclosure, average income among business owners after 2001 would have been approximately NOK340,000 instead of approximately NOK349,000. This result is basically invariant with respect to the extent to which other observable characteristics are controlled for. Correspondingly, results are also very close to the tabular results of Table 4 (accounting for the table version showing results for differences in percentage points and not applying the log transformation).

To illustrate the economic influence of this effect, we have carried out some very simplified calculations. When multiplying the estimated income growth of 2.7 percent with the number of self-employed in 2001, and by using the average tax rate for the group, the tax revenue increases by approximately NOK1.3 billion. This corresponds to less than 0.2 percent of the total tax revenue from inland Norway in 2001.

As mentioned above, the inclusion of wage earners to depict the trend in incomes without the effect of Internet exposure, as in Equation (4), can be questioned. There may, for instance, be confounding factors that generate differential wage growth among business owners and not among wage earners. One cannot

[24] The percentage change is computed by using the formulas of van Garderen and Shah (2002).

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.		Est.	S.E.	Est.	S.E.	Est.	S.E.
Business owner	δ_1	.094***	(.022)	.001	(.019)	0.024	(.019)
Post-2001	δ_2	.155***	(.039)	.153***	(.037)	.154***	(.037)
Non-catalogue	δ_3	.073***	(.018)	.056***	(.013)	.025***	(.006)
Business owner/ post-2001	δ_4	-.064*	(.034)	-.039	(.035)	-.039	(.036)
Business owner/ non-catalogue	δ_5	-.033	(.028)	-.014	(.023)	-.007	(.021)
Post-2001/ non-catalogue	δ_6	9.5×10^{-5}	(.007)	8.2×10^{-5}	(.006)	6.7×10^{-5}	(.004)
Public disclosure[a]	δ_7	.033***	(.009)	.030***	(.008)	.030***	(.007)
Indiv. control var.		No		Yes		Yes	
Munic. control var.		No		No		Yes	
Observations		2,754,320		2,754,320		2,754,320	
R-squared		.012		.196		.205	

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

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

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.		Est.	S.E.	Est.	S.E.	Est.	S.E.
Business owner	δ_1	.091***	(.045)	.125***	(.047)	.030*	(.016)
Post-2001	δ_2	.039	(.028)	.035***	(.023)	-.023	(.021)
Public disclosure[a]	δ_3	.032***	(.012)	.031***	(.010)	.031***	(.007)
Indiv. control var.		No		Yes		Yes	
Munic. control var.		No		No		Yes	
Observations		226,208		226,208		226,208	
R-squared		.006		.125		.144	

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

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

rule out that public disclosure may affect wage earners too, and that the effect is 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 (4) is estimated, where the occupational group dimension is removed and we restrict the sample to individual 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 approximately the same size as seen in Table 5; 2.8 percent average income growth attributed to Internet public disclosure.

Of course, even though it is reassuring that the two different specifications point to the same response magnitude, the identification rests upon several questionable assumptions, some which will be addressed in the next subsection. However, the key assumption that the business owners in the catalogue area are not influenced by the Internet exposure cannot be further investigated. Yet, this particular source of uncertainty contributes to main estimates of Table 5 and Table 6 describing effects which are downward biased.

4.3 Results from alternative methods

In this section we assess the dependence of the main results with respect to some alternative methodological choices. To reduce the likelihood of producing results which falsely are interpreted as public disclosure effects and address potential deficiencies of the empirical design, we discuss some of the main methodological challenges. We will address several issues, some which are closely related to each

other, letting the discussion being framed under the following headings: inference, placebo estimation, functional form dependence, matching, panel data estimation, and results for more specific groups of business owners.

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 probabilities for 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 our main estimate for effect of public disclosure (reported as the "Main estimate" in Table 7) is not particularly dependent on the precise method for statistical inference. To facilitate comparison, in Table 7 we report estimates in terms of percentage changes.[26] The robust variance refers to the standard "sandwich" (or Eicker-Huber-White) estimate of variance,[27] which account 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 show results for a method to account for correlated errors within the municipality (one-way clustering) as the second alternative, and finally present results when data are aggregated into two periods only, before and after the Internet exposure. Even though some variation in estimates and standard errors across techniques are observed, we see that all measures give support to public disclosure having a significant effect on income reporting.

Placebo estimation Robustness tests of results when using the difference-in-differences estimator often include so-called "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, one may construct false interventions and use different selection rules for allocating municipalities into treatment and control groups. We have carried out three different placebo tests. In the first test we let the intervention happen in 1999 instead of 2001, and measure incomes in two years before and after, 1997-1998 and 1999-2000, respectively. In the second test we let

[25] Recall that no specific measures have been taken to utilize the panel structure of the data, and hence repeated observations of the same units are also ignored when calculating errors. We will return to results of panel data estimation shortly.

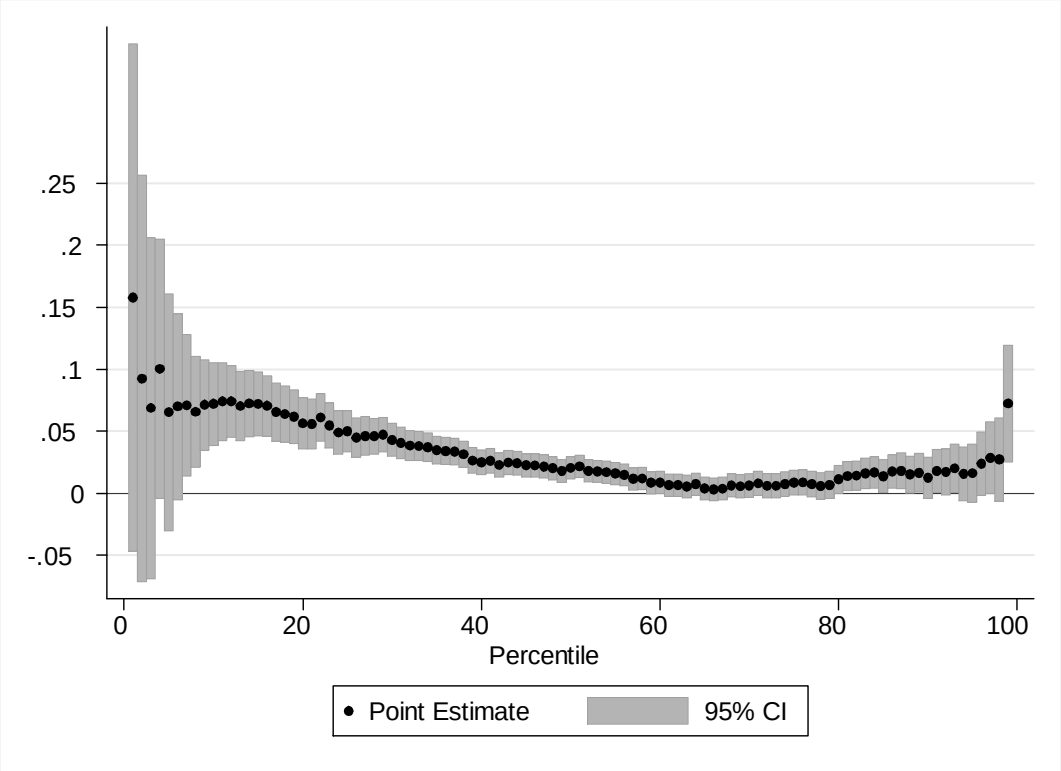
[26] Which means that for the log income spefication, the unbiased estimator of percentage change of van Garderen and Shah (2002) is used.

[27] As for instance reported by Stata.

Table 7: Effect of public disclosure for alternative methodological approaches. Percentage change

	Estimate	Standard error
Main estimate	2.66***	.77
Alternative variance estimators		
Robust variance	2.71***	.68
Clustering at the municipality level	2.46**	1.17
Collapsed income for two periods	2.74***	.63
Placebo estimations		
Internet exposure introduced in 1999	-.56	.87
Random assignment to treatment and control	-.73	.68
Placebo reform, control group	1.14	1.18
Alternative functional form		
No log-transformation of dependent variable		
Median regression	2.06***	.46
Matching		
Propensity score		
Median regression with matching		
Panel data method		
Fixed effect		
Specific groups		
Taxi drivers and restaurant owners		
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$		

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



the computer randomly assign municipalities to the treatment and control groups, using the same group sizes as before.

The third placebo test exploits matching methods. A propensity score of treatment is estimated, based on municipality characteristics and mean values of individual characteristics for the different municipalities. The propensity score gives the probability of treatment, given covariates (Rosenbaum and Rubin, 1983). To check whether observable characteristics are correlated with treatment, and thus driving the results, the 15 municipalities with highest propensity score are given a placebo treatment, and we estimate Equation (4) with the 15 municipalities with the lowest propensity score as control group. The results of these tests, reported in Table 7, all suggest that the main estimate is hard to replicate.

Functional form dependence Several authors have denoted 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 become dependent on the functional form. For example, so far, income is measured by employing a log transformation, which puts a restriction on the common trend assumption which is different from what would be the case if we apply non-transformed income as the dependent variable; for instance Meyer et al. (1995) found results which were sensitive to this choice. As indicated by the results of Table 4 and confirmed by estimating Equation (4) with income (not log of income) as the dependent variable, results are not influenced by this particular choice, see Table 7.

More generally, as noted in Section 3, a saturated model (in the main variables) is less sensitive to the assumption that outcome is additive in observed and unobserved variables. We have however 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, see Koenker and Hallock (2001). Thus, quantile regressions provide predictions for the median or another point of the income distribution with respect to public disclosure, i.e. shows how income of an individual in the relevant quantile position change because of public disclosure through Internet exposure. In addition to being based on an alternative econometric specification,[28] for example in a median regression the coefficients will be estimated by minimizing the absolute deviations from the median, this method very straighforwardly provides information about how slope coefficients vary over the income distribution.[29]

[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.

Figure 2 presents results of a number of quantile regressions (one for each percentile). For the median, we find an estimate of 2.1 percent, which is somewhat lower than the percentage change according to OLS, of 2.7 percent. We see that all point estimates are above the horizontal line, but see that estimates are non-significant, according to the 95 percent confidence interval, for very low incomes and for many income levels above the 60th percentile. We will return to further descriptions of the heterogeneity in responses soon.

Matching [to be added]

Panel data estimation [to be added]

Results for specific groups of business owners [to be added]

5 Conclusion

We are not aware of any papers which have presented empirical estimate of individual income reporting effects of public disclosure of information from income tax returns. Norwegian tax-payers experienced a change in public disclosure in 2001 when anyone with access to the Internet could find individual information on income, wealth, and income and wealth taxes paid. We have used this fundamental change in the exposure and the fact that some local areas, prior to the shift to the Internet in 2001, distributed paper catalogues (which is assumed to be close to the Internet type of public disclosure), to identify effects of public disclosure. We attribute approximately 2.7 percent increase in income to reporting effects due to Internet public disclosure.

The version of public disclosure discussed in this paper is a rather excessive type of disclosure, which will be unfeasible in many countries. An alternative is the more moderated form of public disclosure as implied by the present system of Norway.

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A Appendix: Figure appendix

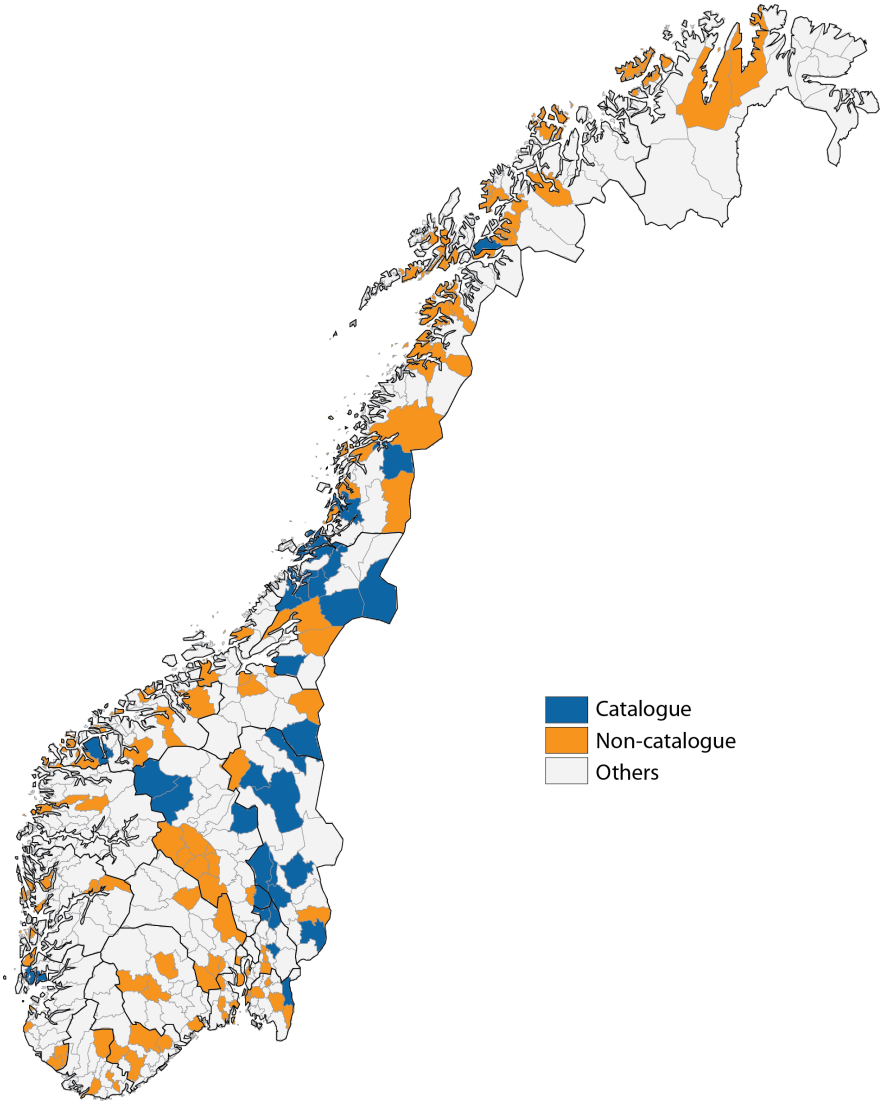

Figure A1. Spatial location of catalogue and non-catalogue municipalities

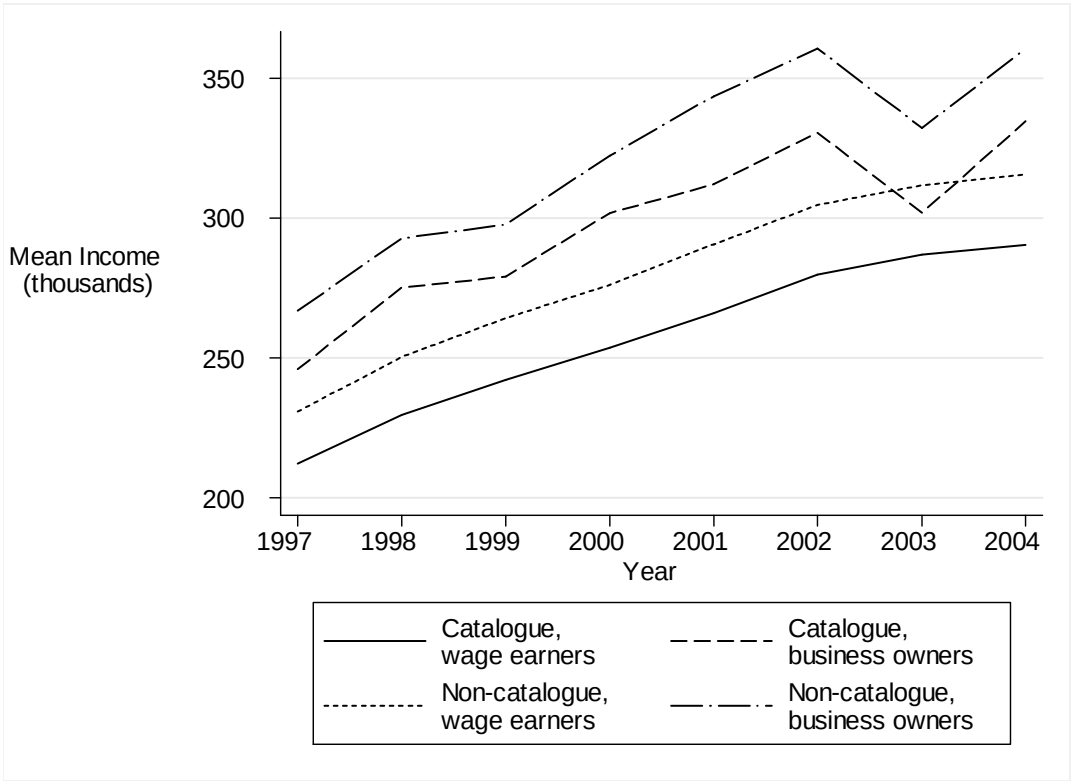

Figure A2. Average income 1997-2004, wage earners and business owners. Thousand Norwegian kroner