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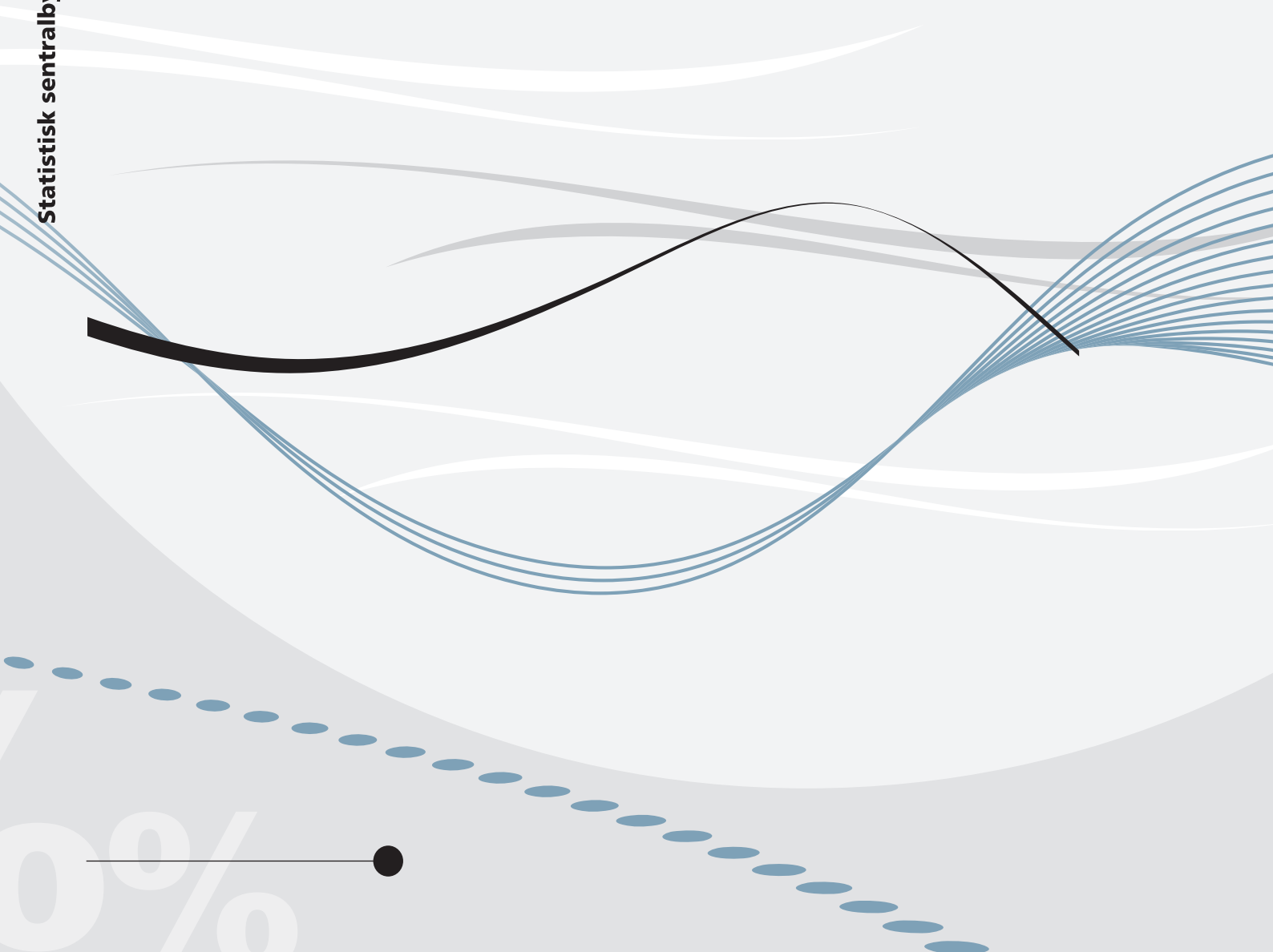
Deterrence effects of public disclosure



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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 disseminated. We observe income changes that are consistent with public disclosure deterring tax evasion: an approximately 3 percent higher 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.

Keywords: Tax Evasion, Income reporting, Quasi-experiments

JEL classification: H24, H26, H30

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Sammendrag

Det finnes ingen tidligere empiriske undersøkelser av om skatteundragelse påvirkes av offentlige skattelister. Våre resultater tyder på at elektronisk søkbare skattelister på internett øker rapportert inntekt. Den antatte hovedmekanismen er at det ikke er allment akseptert å unndra skatt. Vissheten om at naboer og bekjente observerer innkekt gjør det mer kostbart å jukse. Dersom det er et stort sprik mellom observert levestandard og inntekt, kan en risikere en å bli mistenkt for å unndra inntekt fra beskatning.

I likhet med mange andre analyser av skatteunndragelser, antar vi at det primært er selvstendig næringsdrivende og andre småbedriftseiere som kan bestemme hvor mye inntekt som rapporteres inn. For lønnstakere er inntekter og fradrag i stor grad rapportert inn av tredjepart, og vi forventer derfor ikke at denne gruppen er like påvirket av om det er åpenhet eller ikke.

Vi bruker overgangen til internettpublisering høsten 2001 som et tidsskille. Før dette kunne folk oppsøke det lokale skattekontoret og finne ønsket informasjon på papir. Men det er en hovedantakelse at overgangen til internettpublisering øker eksponeringen og den potensielle ”tapt anseelse”-effekten. For å få sikrere identifikasjon utnytter vi at det i noen norske kommuner var nær fullskala distribusjon av informasjon også før 2001. Vi har funnet frem til 31 norske kommuner hvor det lokale idrettslaget eller korpset solgte kataloger med skatteinformasjon om alle innbyggerne i kommunen på dør til dør-basis. Personene i disse kommunene fikk en vesentlig mindre endring i eksponering ved overgangen til internettpublisering i 2001 og er dermed kontrollgruppen i vår analyse. Småbedriftseiere i disse kommunene sammenliknes med eksperimentgruppen; småbedriftseiere i kommuner uten salg av kataloger med informasjon om alle innbyggerne.

Etter å ha kontrollert for en rekke egenskaper ved både personer og kommuner som kan tenkes å påvirke inntektsvekst, finner vi at småbedriftseierne i eksperimentgruppen har en gjennomsnittlig inntektsvekst i de fire årene etter internettpubliseringen som er 3 prosent høyere enn det vi ser i kontrollgruppen. Omregnet i kroner finner vi at rapportert inntekt øker med om lag 10 000 kroner i året i gjennomsnitt ved at skattelistene legges ut på nettet og anslår en provenyøkning på om lag en halv milliard kroner. Vi sensitivitetstester resultatene, og ser på heterogene effekter, som støtter opp under antakelsen at det er frykt for tapt anseelse som ligger bak inntektsøkningene.

Internasjonalt sett er dette den første analysen som måler effekter av åpenhet om inntekts- og skatteinformasjon på personers rapportering av inntekter til skattemyndighetene. At norske data er de første som brukes til dette er ikke rart siden Norge er temmelig alene om å ha et slikt system. For mange land vil det være utenkelig å tillate en slik åpenhet, siden informasjon om skatt, inntekt og formue anses som privat.

1 Introduction

Although not often explicitly stated, an important reason for a system of public disclosure of tax and income information is that it arguably deters people from tax evasion. For instance, given that neighbors and acquaintances 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 and acquaintances) 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 accessed 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 generally 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 citizens with highest wealth and income, 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 substantially reduced the information access hurdle. 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 children 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 public disclosure intensity, 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 107 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 began, 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 holds the

[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).

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 138 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). Omitted variable problems and other measurement issues are explored through several robustness tests, discussed after presenting the main estimates. Here we also discuss the heterogeneity of responses.

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 tradeoff between pecuniary quantities (lower tax burden versus the risk of penalty).[4] These models have been extended in several directions, including frameworks that have accounted for moral sentiments of guilt and shame (Erard and Feinstein, 1994) and social conformity effects (Myles and Naylor, 1996; Fortin, Lacroix and Ville-

[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.

val, 2007). Laboratory experiments, as reviewed in Alm (2012), provide support for public disclosure of noncompliance 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 also 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. Hasegawa, Hoopes, Ishida and Slemrod (2013) 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, Hasegawa et al. uncover no evidence that disclosure increased reported business taxable income generally.[6]

[5] See also Andreoni, Erard and Feinstein (1998) and Slemrod (2007) for surveys of the tax compliance and the tax evasion literature, respectively, and Brock and Durlauf (2001) on social interaction models.

[6] See also Pomeranz (2013) on the effectiveness of the value added tax in facilitating tax

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 following widespread protest.[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 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

enforcement, providing micro-empirical evidence for the self-enforcing power of the paper trail in the VAT.

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

[8] However, Denmark has recently (June, 2012) begun 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.

list is "more likely to be spotted by suppliers, customers, business associates and friends."

3 Empirical strategy

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 to 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.[10] The fall of 2001 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 (for the year 2000) 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.

Given that the post-2001 version of public disclosure both involves wider information range (not restricted to local areas) and a change of mode of public disclosure (electronic search possibilities), we expect that some people may react by reporting higher income to the tax authorities in spring 2002 (for the year 2001). Treating the year 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 Norwegian population used the Internet in an average week, and 45 percent used it for private purposes. Hence, we believe 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.

[10] The wealth measure is net wealth, and taxes paid is the total of all personal tax paid. There are no self-reported tax items that are not reflected in the disclosed measures.

Given that the sudden change to Internet disclosure can be seen as a quasi-experiment, we employ the difference-in-differences estimator in the following, and define Internet disclosure as a binary treatment variable, switching on for a particular group after the change.[11] 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). In other words, without any intervention, the growth in reported income would have been equal in the groups, conditional on other characteristics. As the common trend assumption is not testable, the choice of empirical specification is guided by plausibility, and it follows that it is preferable to find a mechanism for group assignment that mimics randomization.

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 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; Kleven et al., 2011), so the same type of categorization may be applied in the present analysis. An empirical strategy based on identification of public disclosure effects from comparison of wage earners and business owners before and after Internet exposure is, however, subject to several possible confounding factors, or time-dependent unobservables. For instance, the business cycle may have a different effect on income growth of employees and business owners, so that the common time trend assumption may be violated. Moreover, a change in the definition of business income in 2003, see Figure A.2 in Appendix A, conceal the measurement of incomes after the change. Accordingly, we see no signs of public disclosure effects (see Appendix B) when estimating a regression model based on this reasoning.

To facilitate sharper identification, we utilize that the sample can be further

[11] 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). Athey and Imbens (2006), Blundell and Dias (2009), Angrist and Pischke (2009), Imbens and Wooldridge (2009) and Lechner (2011) provide overviews and more details about this identification method.

[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) analyze tax noncompliance by using unaudited income tax return data (and charitable contributions instead of spending on food). See also Hurst, Li and Pugsley (2013), who argue that there is substantial underreporting of income among self-employed even in survey data.

divided into treated and control groups by exploiting a rather peculiar arrangement prior to Internet access in 2001. Before 2001, the tax authorities, as a service to local communities, sent the income tax information to local newspapers, which often published highlights from the lists, such as rankings of the richest, or incomes of 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 taxpayers in the treated localities, persons in areas without widespread income tax return information prior to 2001, responded to 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 107 municipalities in which no such arrangements existed; Figure A1 in the Appendix shows the locations of the two different categories of municipalities.[13] We argue that in the latter group of municipalities, inhabitants experienced a fundamental change in the intensity of the information diffusion system after 2001, when nationwide full-scale electronic diffusion emerged.[14]

Thus, we employ a triple difference set-up in our main specification,[15] where log reported income for individual i at time t, $\log y_{ijkt}$, is explained by dummy variables and combinations of dummy variables, individual characteristics, $X_{it}^{'}$, municipality characteristics (including municipality fixed effects), $Z_{k}^{'}$ (k indexes municipalities), and unobservable individual effects (ε_{ijkt}). The dummy variable bus_j (j indexes occupations) takes the value 1 if the individual is a business owner (with scope for underreporting), and 0 if the person is a wage earner, the dummy variable denoted $nocat_k$ takes the value 1 when the individual resides in a municipality with no availability of paper catalogues prior to 2001 and the time dummy variable, int_t, takes the value 1 if the year is a year with Internet exposure. When we also include

[13] As the data collection was based on personal contact between interviewers and chief officers in the municipalities and therefore was quite resource-intensive. Due to resource constraints, 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 107 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 is 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.

[15] See Gruber (1994) for a similar approach, where the procedure is referred to as "differences-in-differences-in-differences".

year fixed effects, symbolized by λ_t, we have

$$\log y_{ijkt} = \alpha_0 + \lambda_t + X_{it}^{'}\beta + \delta_1 bus_j + \delta_2 (bus_j \times int_t) + \delta_3 (bus_j \times nocat_k) + \delta_4 (int_t \times nocat_k) + \delta_5 (bus_j \times int_t \times nocat_k) + Z_k^{'}\gamma + \varepsilon_{ijkt}, \quad (1)$$

where α_0, β, γ, δ_1, δ_2, δ_3, δ_4, and δ_5 are parameters. Municipality fixed effects, $Z_k^{'}$, are particularly relevant 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 that we believe that people are essentially randomly assigned 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.

The main parameter of interest is δ_5. Under the hypothesis that public disclosure deters taxpayers from underreporting, reported income increases among business owners whose informational exposure is more affected by the Internet access, and δ_5 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,[16] the estimate of δ_5 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 (1) can be characterized as flexible in the main regressors (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 (1) is not restrictive (see Angrist and Pischke, 2009); we will return to the functional form dependency below.

Note that the wage earners enter into Equation (1) as an additional control for the time trend. 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 local macroeconomic developments are representative of the responses of business owners. However, if wage earners who were surprised by the Internet exposure in 2001 (i.e., who lived in a non-catalogue area) also are able to adjust their income to the new regime, the estimate of δ_5 is biased downward as a measure of the effect on business owners.[17] By letting the difference in income before and after the Internet exposure be denoted by Δ,

[16] For instance, this might occur because Internet disclosure has national scope, whereas the paper catalogue information was limited to the municipality.

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

Equation (1) 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:

$$E\left(\Delta \log y_{ik} | nocat_k = 1, bus_j = 1\right) - E\left(\Delta \log y_{ik} | nocat_k = 0, bus_j = 1\right) - E\left(\Delta \log y_{ik} | nocat_k = 1, bus_j = 0\right) - E\left(\Delta \log y_{ik} | nocat_k = 0, bus_j = 0\right) = \delta_5. \quad (2)$$

In Appendix B we also show estimation results when the sample is restricted to business owners alone, which means that the dimension representing occupation is removed from Equation (1).

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 over the whole period,[18] and lived in the same municipality in the period 1997-2000.[19] 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 living in the 138 municipalities[20] in the treatment and control groups prior to 2001. 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

[18] Persons with zero or negative income in one or more periods are excluded, which reduces the sample by approximately 20 percent. We have established that further sample restrictions, for example to taxpayers with more than NOK10,000 in income each year, do not influence the main empirical findings.

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

[20] The identification of the municipalities in the treatment and control groups were described in Section 3

wage income and earnings from self-employment (and other organizational forms which require that individuals report business income and there is no third-party information reporting). 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 the presentation of main results. Further, we categorize individuals as being business owners or wage earners. 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: business income or wage income.[21] Moreover, Table 1 sheds light on 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 reported income among business owners in the "non-catalogue" areas is higher than in the "catalogue" areas: 18.5 percent and 16.1 percent, respectively. This is further illustrated 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)[22] for wage earners and business owners, respectively. The vertical line marks that the change in disclosure happened between the reporting of incomes for 2000 and 2001, and the figure clearly depicts an abrupt

[21] 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 the 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 a 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.

[22] Given the data restrictions (persons should be observed in all years, 1993-2004), the extra years mean that the number of observations (each year) underlying Figure 1 is somewhat smaller (around six percent) than in the sample used in the regressions.

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]	295,125	349,789	275,964	320,467
Wage income (NOK)[a]	38,204	40,987	31,758	38,190
Business income (NOK)[a]	256,920	308,802	244,207	282,277
High school education	.57	.57	.61	.61
University education	.14	.14	.13	.13
Married	.69	.69	.69	.70
Number of children	.87	.72	.87	.72
Age (first period)	44.4		44.7	
Male	.76		.75	
Immigrant	.028		.022	
Number of individuals	21,493		8,091	
	Wage Earners			
	Non-catalogue		Catalogue	
	Before	After	Before	After
Income (NOK)[a]	255,035	305,820	234,469	281,485
Wage income (NOK)[a]	250,501	301,524	229,128	276,843
Business income (NOK)[a]	4,535	4,296	5,341	4,642
High school education	.51	.52	.51	.52
University education	.26	.27	.24	.25
Married	.64	.65	.62	.63
Number of children	.89	.78	.86	.74
Age (first period)	42.3		42.4	
Male	.52		.51	
Immigrant	.031		.022	
Number of individuals	266,345		78,111	

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

Table 2: Averages for municipality-level characteristics

	Non-catalogue	Catalogue	Others
Population (2000)	8,416	8,601	10,467
Increase in number of inhabitants, 2000-2004	168	128	260
Births per 1000 inhabitants (2001)	11.7	10.6	11.6
Share in high population density area (2001)	49.0	44.0	47.7
Unemployment rate (2000)	2.75	2.39	2.67
Increase in unemployment rate, 2000-2004	.98	.63	.98
Main economic activity[a]			
Share in construction	.26	.23	.23
Share in fisheries	.09	.03	.08
Share in manufacturing	.55	.68	.62
Share in farming	.29	.42	.27
Share in service sector	.61	.52	.59
Number of municipalities	107	31	297

[a] Each municipality is described by either one or a combination of two main activities

change beginning in 2001, as the difference between average income for business owners moves above the similar measure for wage earners.[23] Figure A2 in Appendix A shows the income developments behind Figure 1, that is, the development in reported 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.[24] However, we have no reason to expect that the variation in the definition of income affects the measurement of income for the two groups of business owners differently.[25] Also in Appendix A, Figure A3 shows a log version of Figure 1, which depicts the same general pattern.

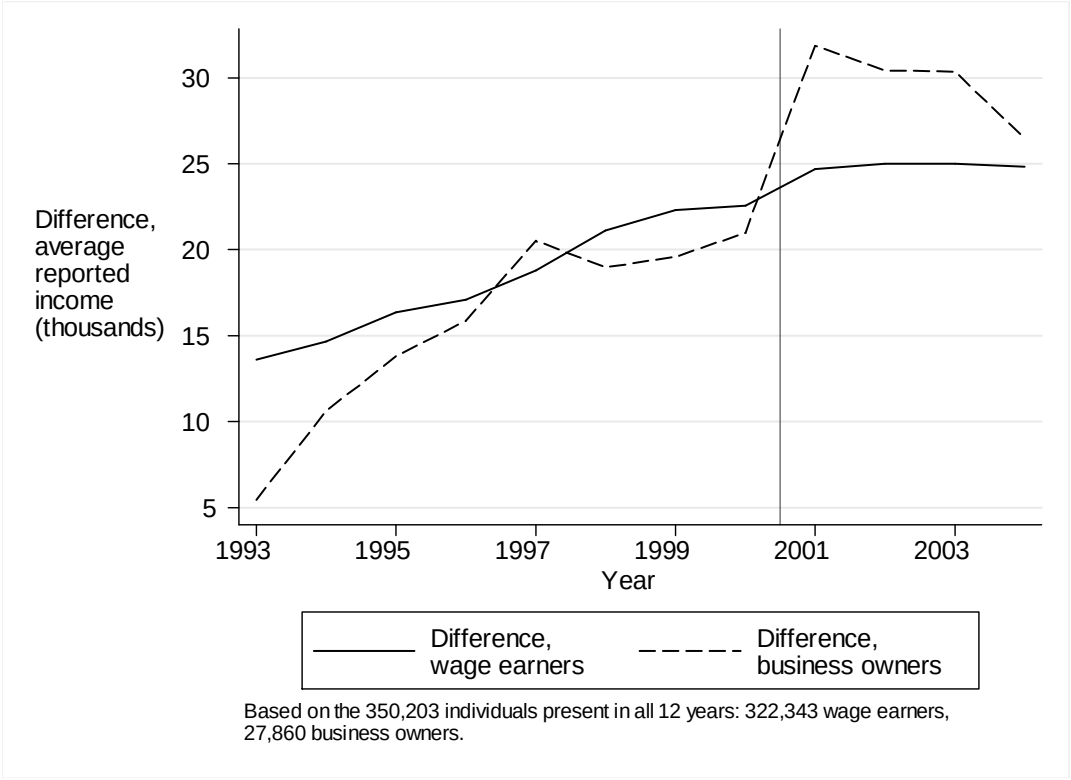
As the treatment is at the municipality level, possible differences between treated and non-treated municipalities are of interest. In the regressions to come, we use fixed effects to control for observed or unobserved differences between municipalities. Still, if there are systematic differences between the municipalities

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

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

[25] We have also estimated Equation (1) 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.

Figure 1: Differences in average reported income (thousand Norwegian kroner) between residents of non-catalogue and catalogue municipalities, 1993-2004. Wage earners and business owners.



that affect both the treatment and the outcome variable, the identification may be in doubt. Thus, there are reasons to explore to what extent the municipalities in the two groups differ. This can be done as the individual income data are linked to information on municipality characteristics 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 presented in Table 2 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 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 (including the main categories from the economic classification system), given the categorization into the non-catalogue and the catalogue groups. We also present information from the remaining municipalities, with unknown treatment status. 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. The shares of municipalities in different main economic activities are not fundamentally different in the two groups. Moreover, the municipalities both in the treatment and control groups seem to be quite similar to the average unclassified municipality. We will anyway use municipality fixed effects in the regressions; in one specification we also let income growth depend on the industry classification.

4.2 Main results

As an introduction to identification of public disclosure through estimation of Equation (1), Table 3 presents a simple tabular version of the results based on the income estimates of Table 1; thus, income is measured in levels rather than logs. The table shows that the difference in growth of reported income (measured as the difference in average income growth between 1997-2000 and 2001-2004) between business owners in non-catalogue and catalogue municipalities (as already noted) is 2.39 percentage points. If we adjust the benchmark for differences between in-

Table 3: Growth in average reported income, between 1997-2000 and 2001-2004. Business owners and wage earners in non-catalogue and catalogue municipalities

	Non-catalogue	Catalogue	Difference
Income growth of business owners	18.52	16.13	2.39
Income growth of wage earners	19.91	20.05	-.14

come growth for wage earners of the two groups of municipalities (-.14), which is the case under the specification presented in Equation (1), the effect of public disclosure is slightly larger, 2.53 percentage points. The small difference in income growth between wage earners in catalogue and non-catalogue areas, reported in Table 3, suggests that results are less influenced by either employing a triple difference specification (including wage earners) or a double difference specification (excluding wage earners). Correspondingly, the public disclosure effect from an estimation of a specification that focuses on effects among business owners alone (see Appendix B) is very similar to results for the triple difference specification (see below).

Next, in Table 4, we turn to ordinary least squares (OLS) estimation results for Equation (1). 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),[26] we use two-way clustering. Standard errors are clustered 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 or municipality level industry specific growth, regression (2) accounts for individual characteristics, whereas specification (3) controls for both individual characteristics and municipality level industry specific growth (all specifications have fixed municipality effects).

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_5. The estimate for specification (3) (controlling for both individual characteristics and industry specific growth) suggests that on average approximately 3.1 percent of the growth in the reported income among business owners in the non-catalogue areas can be attributed to the substantially increased Internet exposure affecting incomes for 2001 and after. The standard error is 0.66, 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, the average reported income among business owners after 2001 would have been less than NOK340,000, instead of approximately NOK350,000. Note, though, that Figure 1 may suggest that the

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

effect may be fading by the end of the period under investigation (in 2004).

This baseline result is largely invariant with respect to the extent to which other observable characteristics are controlled for. Correspondingly, the regression results are close to the results of Table 3 (the table version shows results for differences in percentage points and does not apply the log transformation). We interpret this as corroborative evidence for exploiting a group assignment procedure that is minimally subject to omitted variables bias. In Table B1 in Appendix B we show estimation results for the public disclosure effect given a more restricted sample, consisting of business owners only.

To illustrate the economic influence of this effect, we have carried out some simplified calculations. As already noted, an income growth of 3.1 percent corresponds to an income increase of approximately NOK10,000. When multiplying this figure by the average number self-employed in 2001-2004, and using the average tax rate for the group (0.27), we calculate that the effect of Internet public disclosure on revenue nationwide would be approximately NOK0.5 billion. For 2001 this corresponds to approximately 0.2 percent of the total tax revenue from the individual income tax. If the level of underreporting among business owners in Norway is similar to what recent studies have found in the neighboring countries (see Engström and Holmlund (2009), Kleven et al. (2011) and Johansson (2005), for estimates Sweden, Denmark and Finland, respectively), there is still (after Internet public disclosure) substantial tax evasion among business owners. If we use the estimate of Kleven et al. (2011) for Denmark as an indicator of the magnitude, suggesting that 15 percent of self-employment income is evaded,[27] it follows that the estimated effect of Internet public disclosure of 3.1 percent has cut tax evasion in this group by approximately one-fifth. In the appraisal of this back-of-the-envelope estimate, one should remember that there was a system for public disclosure before the Internet exposure, which most likely moderates the effect, and that there is no public disclosure effect in the estimate for Denmark.

Our identification of the impact of public disclosure 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, Figure 1 does not provide any strong indication of the control group also changing behavior. Moreover, it is important to note that the implication of public disclosure also affecting members of the control groups is that the estimates of Table 4 are downward biased.

[27] Johansson (2005) estimates that self-employment income is underreported by 16–40 percent in Finland, whereas Engström and Holmlund (2009) find, using Swedish data, that households with at least one self-employed member underreport their total incomes by around 30 percent.

Table 4: 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	.0128***	(.020)	.028	(.018)	.027	(.018)
Business owner/ post-2001	δ_2	-.068*	(.037)	-.041	(.037)	-.038	(.035)
Business owner/ non-catalogue	δ_3	-.021	(.024)	-.006	(.020)	-.006	(.020)
Post-2001/ non-catalogue	δ_4	-.002	(.003)	-.002	(.003)	-.004	(.003)
Public disclosure[a]	δ_5	.034***	(.008)	.032***	(.008)	.031***	(.007)
Indiv. control var.[b]		No		Yes		Yes	
Industry spec. gr.[c]		No		No		Yes	
Observations		2,992,320		2,992,320		2,992,320	
R-squared		.035		.206		.207	

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

Standard errors clustered at municipality level, and by year.

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

[b] Age, age squared, education at high school or university level, marital status, number of children, gender and immigrant background.

[c] Flexible trend in industry specific growth; year interacted with municipality industry classification.

All specifications with fixed effects for years and municipalities.

4.3 Robustness checks and heterogeneous responses

In this section we assess the robustness of the main results with respect to some alternative methodological choices. We will address the definition of income, inference and functional form dependence. We also do robustness checks in the form of placebo tests, municipality matching, and panel data estimation. In order to explore which groups of business owners who have responded, we also look at response heterogeneity. For the purpose of comparison we use the public disclosure estimate from Table 4, column (3) (based on Equation (1), with controls for individual characteristics and municipality level industry specific growth) as the benchmark.

Definition of income So far we have used gross "earned income" as the income variable. In Table 5 we show results for an alternative income concept: "ordinary income" under the dual income tax system of Norway, which is the income concept actually reported by the tax authorities in the public disclosure. This measure of income takes taxable transfers, capital income and some income deductions into account in addition to wage and business income. We see a clearly significant response estimate when using this alternative income concept, too; interestingly, the measured response is higher than the main estimate (referred to as the "Base specification" in Table 5). One possible explanation for the higher estimate may be that business owners to a larger extent can use self-reported deductions to adjust their incomes and tax burdens (in contrast to wage earners, whose deductions are primarily third-party-reported). Then the higher estimate reflects both a higher reported income and a reduction in unwarranted deductions.

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.[28] 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 5 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 of statistical inference. To facilitate comparison, in Table 5 we report estimates in terms of percentage changes. The robust variance refers to the standard "sandwich" (or Eicker-Huber-White)

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

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 calculate 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 that 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 the reported income of businesses.

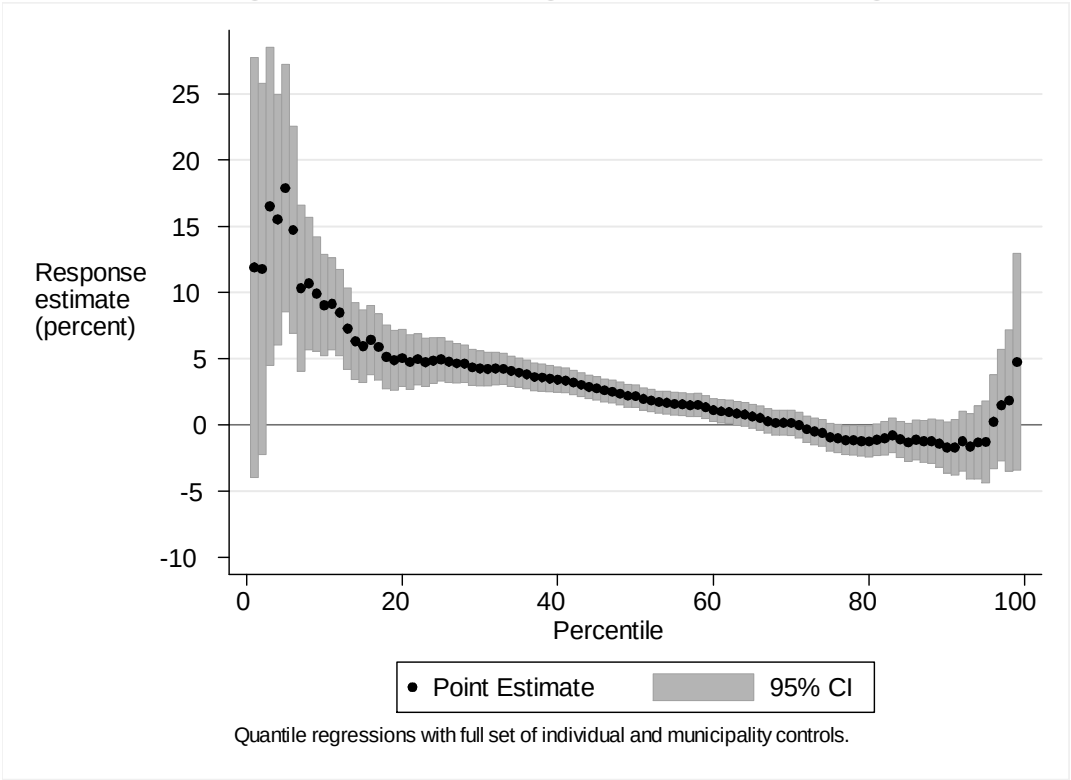
Functional form dependence Several authors have noted that the standard difference-in-differences estimator involves scale-dependent identifying assumptions; see Meyer, Viscusi and Durbin (1995), Heckman (1996) and Athey and Imbens (2006). In other words, the results of the analysis may be dependent on the functional form. For example, in our analysis 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 5 shows that results are altered by using a non-transformed dependent variable. When recalculating the estimate of a regression with a non-transformed variable to make it comparable to the base specification, the estimate is now 1.79, and it is only statistically significant different from zero at the 10% level.

Further, we have investigated results for an alternative specification to explore whether the public disclosure effect differs over the distribution of income. The usual method is quantile regression, 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). 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),[29] this method very straighforwardly provides information about how slope coefficients vary over the income distribution.[30] To get estimates of the unconditional quantile effect, we use the unconditional quantile regression method advised by Firpo, Fortin and Lemieux (2009). Thus, the unconditional quantile regressions provide predictions for the median or another point of the income distribution with

[29] 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.

[30] 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.

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



respect to public disclosure.

Figure 2 presents results of a number of quantile regressions (one for each percentile). For the median, we find an estimate of 2.2 percent, which is somewhat lower than the 3.1 percent change according to OLS. We note that point estimates are mostly above the horizontal (i.e., zero effect) line, but see that for income levels above the 70th percentile point estimates are primarily negative, though not significant (according to the 95 percent confidence interval). People who already report high income may be less exposed to the shaming effect, but note also that local newspapers reported the income and tax information of inhabitants with very high income (such as the top 50-100 of the local area) even before 2001. Higher-income taxpayers may have behaved as if there was effective public disclosure of their tax information even before it was available on the Internet. The lower estimates at higher percentiles may also explain why the level specification (not log income), which implies that more weight is given to the top incomes, produces a smaller response estimate (compared to the base specification).

Table 5: Effect of public disclosure on reported incomes for alternative methodological approaches

	Estimate	Standard error[a]
Base specification[b]	3.11***	0.68
Other definition of income		
All taxable income minus deductions	4.73***	1.12
Alternative variance estimators		
Robust variance	3.11***	0.71
Clustering at the municipality level	3.11***	1.10
Collapsed income for two periods	3.11***	0.68
Alternative functional forms		
No log-transformation of dependent variable	1.79*	0.93
Median regression	2.17***	0.44
Placebo tests		
Internet exposure introduced in 1999	-0.12	0.91
Prop. score alloc. of control group municip.	-0.07	1.05
Matching		
Propensity score	3.09***	0.87
Panel data method		
Fixed effect[c]	3.11***	1.13
Specific groups		
Doctors, lawyers, engineers, etc.	4.24***	1.53
Taxi drivers, restaurant owners, etc.	-0.51	0.80
Above median municip. population density	2.72***	0.74
Above median municip. population	2.84***	0.67

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

[a] Standard errors clustered at municipality level, and by year, unless otherwise stated.

[b] Column (3) in table 5, specification with individual controls and industry specific growth.

[c] Standard errors clustered at the municipality level.

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 two different placebo tests. In the first test we proceed as if the intervention happened in 1999 instead of 2001, and measure incomes in the years before and after, 1997-1998 and 1999-2000.[31]

The second placebo test uses results of propensity score estimation (Rosenbaum and Rubin, 1983) to generate a placebo reform for half of the control group municipalities. 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. To test whether this is the case, we estimate the propensity of treatment (i.e. sales of tax lists) based on municipality characteristics and municipality level mean values of individual characteristics.[32] The municipalities in the control group are then allocated to (placebo) treatment and control groups, depending on their propensity score. 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 (1) is then estimated with the new data set.

As shown in Table 5, the two placebo tests reveal no signs of an effect of public disclosure on reported income. This is consistent with our belief that the assignment mechanism based on paper catalogues represents a convincing assignment procedure for maintaining a common trend assumption.

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 reform, we now use matching to make the control and treatment group more similar.[33] The average propensity scores[34] of the control and treatment group in our sample are, respectively, .65 and .81 before matching.

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

[32] The propensity score estimation is a probit estimation of the probability of treatment for a municipality, based on municipality characteristics and mean values of individual characteristics. The municipality characteristics used are population, births per 1000 inhabitants, the share of residents in high population density area, rate of unemployment and industry classification; all measured in the year 2000. We also use the following variables, for each municipality averaged in the years 1997-2000 over the individuals in our sample: wage, age, sex, education and share of business owners.

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

[34] This is estimated as described in the last subsection.

Municipalities are matched by pairwise (or nearest neighbor) matching.[35] We then have a data set consisting of 29 matched municipality pairs, which are as similar as possible based on observable characteristics.[36] The individuals in these two groups are then used as a new sample, on which we estimate equation (1). 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 repeated cross-sections. 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 (1). This allows us to control for possible unobserved, time-constant characteristics of individuals that are correlated with the independent variables. 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 3.11, exactly similar to the estimate of the base specification, though the standard error is somewhat larger.

Response heterogeneity There are reasons to expect that there are differences across industries with respect to underreporting. Given that we have added to the dataset detailed information about which industry the business owners belong to (Statistics Norway, 2005),[37] there is scope for further examination of differences across different sectors. In Table 5 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 bars, restaurants and other catering businesses. These groups of businesses have been subject to news reports of tax evasion, and have also 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. Without any further priors, we refer to empirical investigations by Erard and Ho (2003) and Artavanis,

[35] We use a caliper, i.e. the largest allowed difference of propensity score between matches, of .025. Two treated municipalities does not have any matches within the .025 caliper, and are thus dropped.

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

[37] The industry classification follows the structure of the NACE code used by the European Union. Where the code differs between years for an individual, the modal code is used.

Morse and Tsoutsoura (2012). The latter study shows empirical evidence of more tax evasion among doctors, engineers, private tutors, financial services agents, accountants, and lawyers.[38] Thus, we estimate our base specification on business owners only from these groups.[39]

Table 5 shows a stronger effect than average for a group that includes doctors, lawyers, engineers, and other professions. For taxi drivers and restaurant owners, 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 types of businesses. We take this as corroborative evidence of the deterrence effect of public disclosure working through a shaming effect. Whereas the effect of public disclosure is large for businesses where reputation is important, as for instance for lawyers, there is no effect in businesses where tax evasion is believed to be more common.

We may also explore if there are any other individual or municipality characteristics that mediate the magnitude of the public disclosure effect. For instance, it may be argued that the population density influences public disclosure, i.e. the shaming effect is more relevant in small, transparent communities. 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 densely populated, presumably rural, municipalities, although the difference is not large. Similarly, in municipalities with population above the median, the public disclosure effect is somewhat smaller.

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

[38] Artavanis et al. use the larger loans given to self-employed, compared to wage earners with similar incomes, as indication of tax evasion. Their explanation for the difference in tax evasion between business categories is a difference in paper trail. Erard and Ho (2004) also find more tax evasion among lawyers and doctors.

[39] The categories included are: building of complete constructions or parts thereof; civil engineering; financial intermediation and activities auxiliary to financial intermediation; legal, accounting, book-keeping and auditing activities; tax consultancy; market research and public opinion polling; business and management consultancy; holding; adult and other education; medical practice activities; activities of religious, political and other membership organisations.

income to Internet public disclosure. The main hypothesis of a shaming effect from public disclosure on tax evasion is also supported by finding somewhat larger effects in smaller, less densely populated areas, and for business categories which are believed to be "reputation sensitive".

To our knowledge this is the first empirical estimate of the effect of public disclosure on overall individual tax compliance. As there was public disclosure of tax and income information before the change to the Internet exposure in Norway, the measured 3 percent effect is most likely limited by this, which means that larger effects of Internet public disclosure may be expected under other institutional conditions. We note, though, that the Norwegian version of public disclosure discussed in this paper is a rather specific and extreme type of disclosure that is arguably infeasible in many countries.

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6 Appendix

A Figure appendix

Figure A.1: Spatial location of catalogue and non-catalogue municipalities

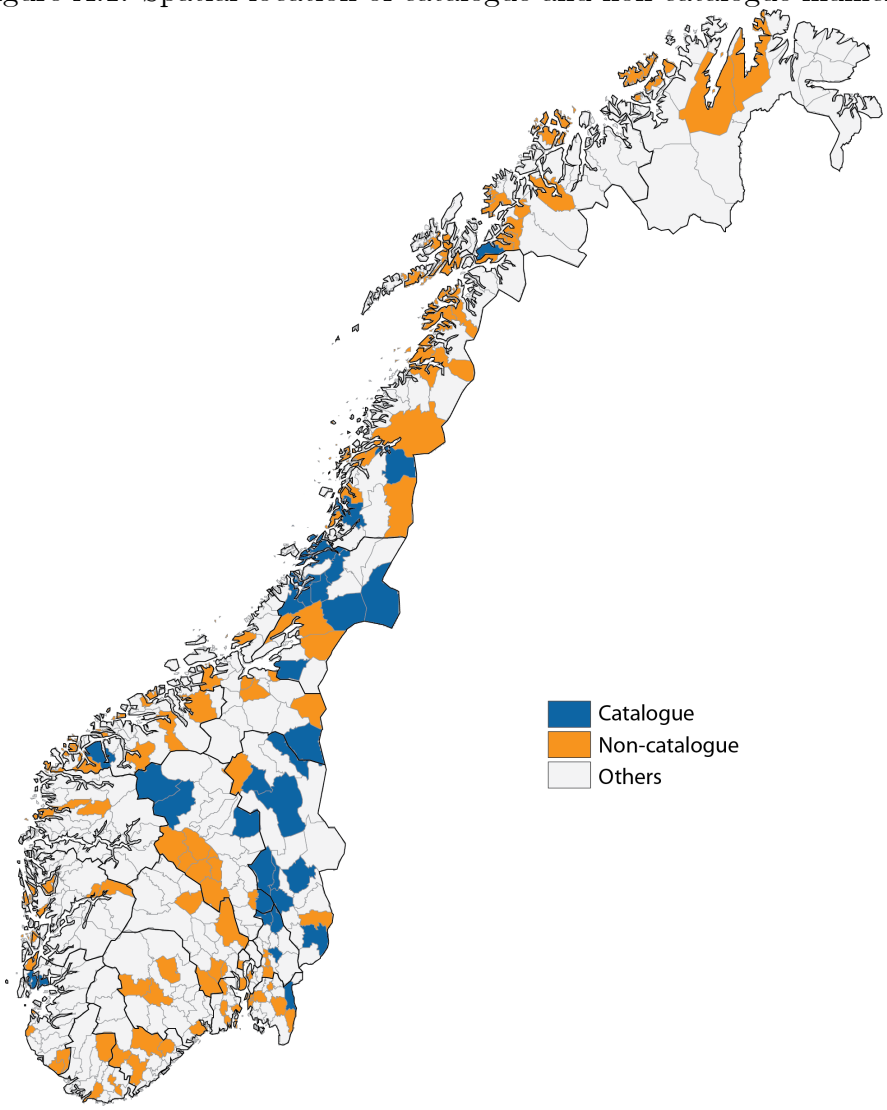


Figure A.2: Average reported income 1993-2004, wage earners and business owners. Thousand Norwegian kroner.

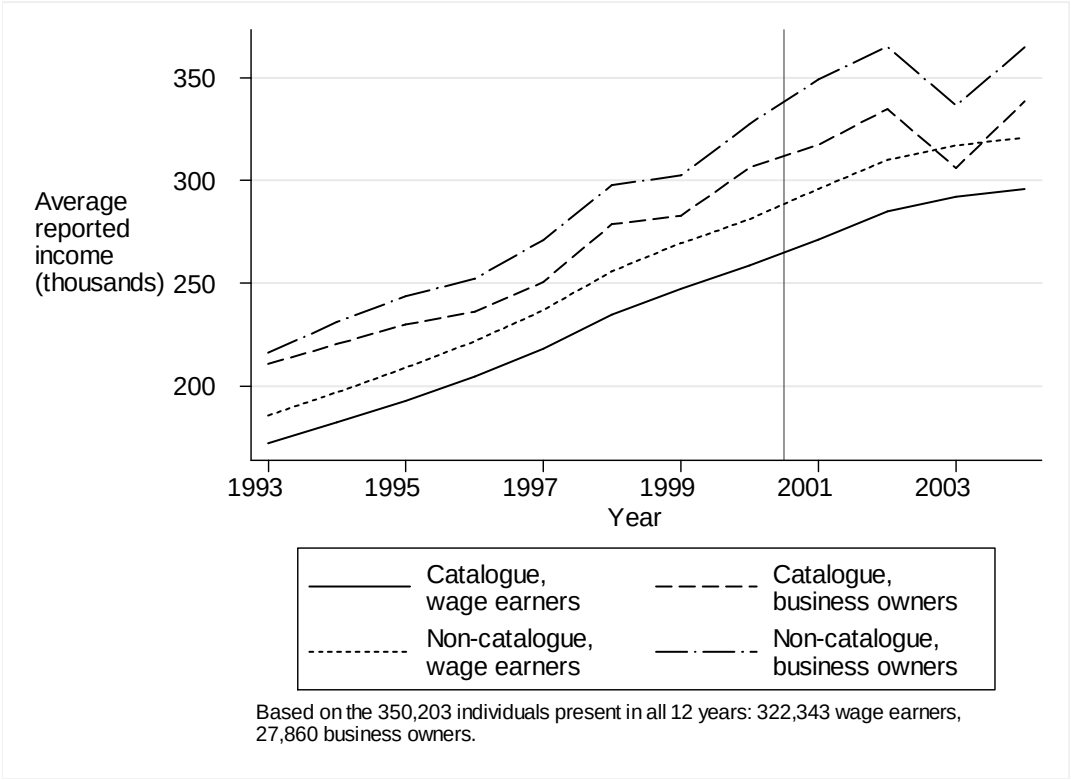
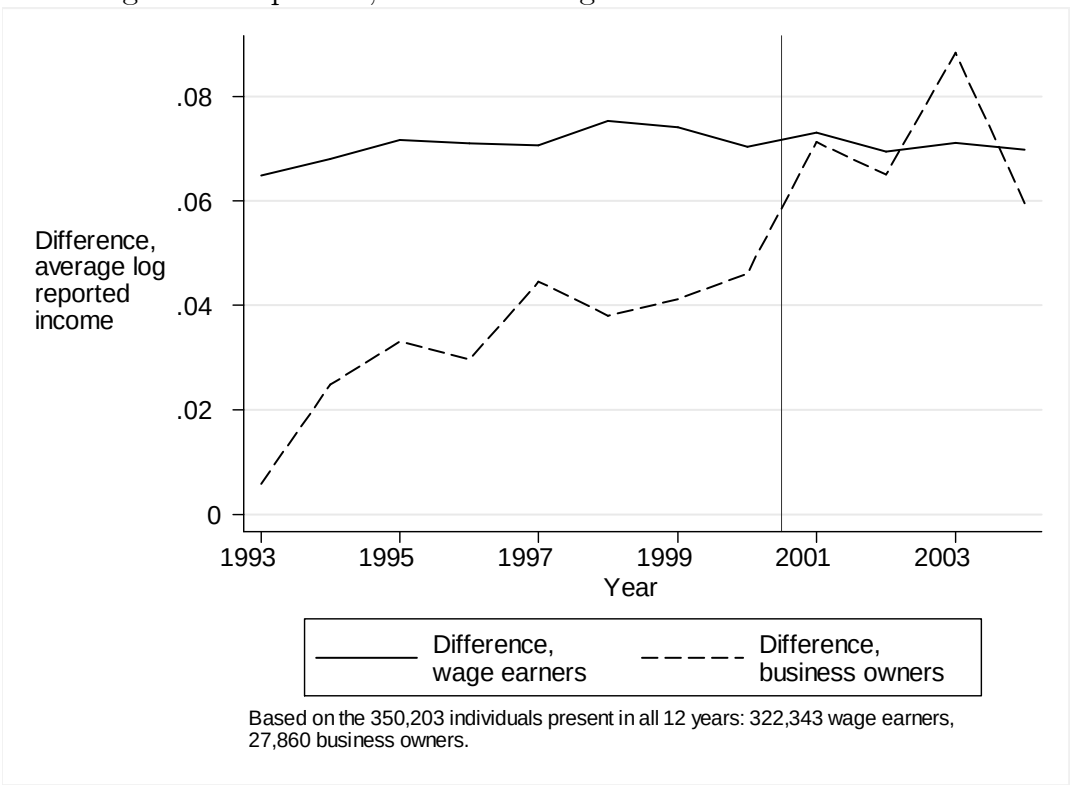


Figure A.3: Differences in log of average reported income between non-catalogue and catalogue municipalities,1993-2004. Wage earners and business owners.



B Estimation results for other specifications

As discussed in the main body of the text, alternative specifications to Equation (1) have also been estimated. Here we refer to estimation results for two spefications which each removes one dimension from Equation (1), the distinction between catalogue and non-catalogue areas and the occupational group dimension (restrict to business owners only), respectively. Thus, we present estimation results for $\log y_{ijt} = \alpha_0 + \lambda_t + X_{it}^{'}\beta + \kappa_1 bus_j + \kappa_2 \left(bus_j \times int_t\right) + Z_k^{'}\gamma + \varepsilon_{ijt}$, and for $\log y_{ijt} = \alpha_0 + \lambda_t + X_{it}^{'}\beta + \eta_1 \left(nocat_k \times int_t\right) + Z_k^{'}\gamma + \varepsilon_{ikt}$, respectively, where α_0, λ_t, κ_1, κ_2, and η are parameters. Variable explanations can be found in Section 3.

Table B1: Effect of public disclosure on income reporting. OLS estimation results for specification 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	κ_1	.113***	(.015)	.024	(.015)	.022	(.015)
Business owner/ post-2001	κ_2	-.043	(.034)	-.018	(.034)	-.015	(.032)
Indiv. control var.[a]		No		Yes		Yes	
Industry spec. gr.[b]		No		No		Yes	
Observations		2,992,320		2,992,320		2,992,320	
R-squared		.035		.206		.207	

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

Standard errors clustered at municipality level, and by year.

[a] Age, age squared, education at high school or university level, marital status, number of children, gender and immigrant background.

[b] Flexible trend in industry specific growth; year interacted with municipality industry classification.

All specifications with fixed effects for years and municipalities.

The parameter estimate for $\widehat{\kappa_2}$ in Table B1 shows that the identification of public disclosure in Equation (1) rests on the information on catalogue and non-catalogue areas, and is not captured by a simple double difference estimation strategy based on differences between business owners and wage earners before and after 2001. As signified by negative and insignificant parameter estimates, there are no signs of business owners as a group increasing their reported income after 2001, in comparison with wage earners. Such an empirical strategy is subject to several confounding factors. For instance may time-dependent unobservables, as the business cycle, blur the identification of responses among business owners

as a group. Moreover, the change in the measurement of business income in 2003, as seen in Figure A.2, influences the results.

Next, the inclusion of wage earners to depict the trend in incomes without the effect of Internet exposure, as in Equation (1), 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. Table B2 reveals, as expected given the very small difference in growth rates presented in Table 3, that the public disclosure effect in this more restricted sample is close to (and a little smaller) than the estimate seen in Table 4.

Table B2: Effect of public disclosure on income reporting. OLS estimation results specification based on 2001 pre-catalogue assignment, business owners only

		(1)		(2)		(3)	
Explanatory var.	Coeff.	Est.	S.E.	Est.	S.E.	Est.	S.E.
Public disclosure[a]	η	.032***	(.013)	.030**	(.013)	.025**	(.010)
Indiv. control var.[b]		No		Yes		Yes	
Industry spec. gr.[c]		No		No		Yes	
Observations		236,672		236,672		236,672	
R-squared		.051		.159		.160	

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

Standard errors clustered at municipality level, and by year.

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

[b] Age, age squared, education at high school or university level, marital status, number of children, gender and immigrant background.

[c] Flexible trend in industry specific growth; year interacted with municipality industry classification.

All specifications with fixed effects for years and municipalities.

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