Taxes on the Internet: Deterrence Effects of Public Disclosure[†]

By ERLEND E. BØ, JOEL SLEMRÖD, AND THOR O. THORESEN*

Although Norway has a long tradition of public disclosure of tax filings, starting in 2001 anyone with Internet access could obtain individual information on income and income taxes paid. We examine the effect on income reporting of this change in the degree of public disclosure, making use of the fact that prior to 2001, in some municipalities, tax information was distributed widely through locally produced paper catalogs. We find an approximately 3 percent higher average increase in reported income among business owners living in areas where the switch to Internet disclosure represented a large change in access. (JEL H24, H25, H26)

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 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 individual income tax return information can be accessed through electronic search.

*Bø: Research Department, Statistics Norway, Postboks 8131 Dep. NO-0033 Oslo, Norway (e-mail: eeb@ssb.no); Slemrod: Stephen M. Ross School of Business and Department of Economics, University of Michigan, 701 Tappan Street, Ann Arbor, MI 48109 (e-mail: jslemrod@umich.edu); Thoresen: Research Department, Statistics Norway, Postboks 8131 Dep. NO-0033 Oslo, Norway, and Oslo Fiscal Studies, University of Oslo (e-mail: tor@ssb.no). We thank the interview corps of Statistics Norway and Bjørn Are Holth, Gunnar Claus, and Inge Aukrust for help with the data collection, and acknowledge the helpful comments from a referee of the journal, Austan Goolsbee, Edwin Leuven, Alari Paulus, and seminar participants at Skatteforum 2012 (June 2012 Larvik, Norway), the University of Michigan public finance summer reading group (2012), the opening conference of Oslo Fiscal Studies (December 2012, University of Oslo), the 18th Meeting of the Society of Labor Economics (May 2013, Boston), the 43rd Spring Symposium of the National Tax Association (May 2013, Washington, DC), the 69th Conference of the International Institute of Public Finance (August 2013, Sicily), the Department seminar, Department of Economics, the Norwegian University of Science and Technology (October 2013, Trondheim), and the 2014 Meeting of the American Economic Association (January 2014, Philadelphia).

[†]Go to <http://dx.doi.org/10.1257/pol.20130330> to visit the article page for additional materials and author disclosure statement(s) or to comment in the online discussion forum.

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

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 (Norges offentlige utredninger (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 challenged 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 personalized 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. Although we view this response as mostly reflecting a change in tax evasion, we address another motivation—social bragging. Given that income serves as a device for signaling, public disclosure also strengthens the incentives to use income to demonstrate personal success, resulting in the same responses as we interpret as resulting from less tax evasion.

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

²Despite 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).

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 catalogs 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 catalogs prior to 2001. A survey, tracking areas with and without pre-2001 special arrangements, identified 31 municipalities (out of a total of 430 municipalities in Norway) 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, while no such effects are assumed in the former group. The difference in the change in reported income between business owners in municipalities where there was no availability of paper catalogs prior to 2001 (treatment group) and individuals in municipalities which had distribution of paper catalogs before 2001 (control group), holds the promise of identifying the effect of Internet public disclosure on income reporting. 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 sample of individuals used in this study consists of persons from 138 municipalities, observed before and after 2001 (from 1997 to 2004), in municipalities characterized according to two different systems of information availability prior to 2001. As the income data we have available are register-based and cover the whole population, we establish a panel dataset, consisting of approximately 370,000 individuals of working age, each person observed over eight years.

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

I. Deterrence Effects of Public Disclosure

A. *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

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

and shaming—dampens such behavior. The first models of tax evasion, Allingham and Sandmo (1972) and Yitzhaki (1974), focused on the trade-off between pecuniary quantities (lower tax burden versus the risk of penalty).⁴ These models have been extended in several directions, including frameworks 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 Villeval 2007). Laboratory experiments, as reviewed in Alm (2012), provide support for public disclosure of noncompliance acting as an additional penalty mechanism. For instance, Coricelli 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 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 et al. (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. (2013) uncover no evidence that disclosure increased reported taxable business income generally.⁶

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

⁵See 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.

⁶See also Pomeranz (2013) on the effectiveness of the value-added tax in facilitating tax enforcement, providing micro-empirical evidence for the self-enforcing power of the paper trail in the VAT.

B. Worldwide Experience

Historically, there have been shorter spells of public disclosure in some other countries, such as the United States and France.⁷ Public access to corporate tax information is permitted in Japan, Finland, and 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. Denmark⁸ 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 (Norwegian 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

C. 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 a penal tax imposed for evading their taxation obligations; as of April 1997 the Commissioner is also able to 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 an annual basis in the Revenue Commissioner’s Annual Report, but recently the list is published on a quarterly basis in *Iris Oifigiúil* (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” (Government of Ireland 2011). Moreover, the well-publicized quarterly list is “more likely to be spotted by suppliers, customers, business associates and friends.”

⁷ See Internal Revenue Service (2011) for an overview of the history of public disclosure in the United States.

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 Before being blacked out, vast amounts of data were downloaded and transferred to other sites or burned onto disks and sold (Economist 2008).

II. 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 other expenses, such as debt interest payments, commuting costs, and contributions to charity.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.¹¹ 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 involves both a 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. First, 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 (2002) 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.

Given that the sudden change to Internet disclosure can be seen as a quasi-experiment, we employ a difference-in-differences individual fixed effects model. Internet disclosure is represented by a binary treatment variable, switching on for a particular group after the change. 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

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.

¹¹ Publication was well before the deadline of 2001 tax filings.

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 A2 in Appendix A, conceals 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 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 Appendix A 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

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 (1989) 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 (2014), who argue that there is substantial underreporting of income among self-employed even in survey data.

¹³ 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 catalogs in the control group prior to 2001, but assume that the institution itself had effect. The price of the catalogs is not expected to represent an impediment, as prices were relatively low. For example, in the municipality of Eidskog in 1999 and 2000, the catalogs were sold for 50 Norwegian kroner (NOK) (or approximately \$6 each) and sales helped the financing of leisure activities for children.

We employ the individual fixed effects estimator,¹⁵ letting log reported income for individual i at time t , $\log y_{it}$, be explained by combinations of group dummy variables and time, a vector of observed time-varying characteristics, X'_{it} , individual unobserved effects, μ_i , year fixed effects, λ_t , and an idiosyncratic error term, ε_{it} . The dummy variable bus_i takes the value 1 if the individual is a business owner (with scope for underreporting), and 0 if the person is a wage earner, whereas the dummy variable denoted $nocat_i$ takes the value 1 when the individual resides in a municipality with no availability of paper catalogs prior to 2001 and 0 if the individual were exposed for distribution of tax information through paper catalogs. When we denote the time period with Internet exposure by τ_t , we have

$$(1) \log y_{it} = \alpha + \delta_1(bus_i \times \tau_t) + \delta_2(nocat_i \times \tau_t) + \delta_3(bus_i \times nocat_i \times \tau_t) \\ + X'_{it}\beta + \mu_i + \lambda_t + \varepsilon_{it},$$

where α , β , δ_1 , δ_2 , and δ_3 are parameters.

The main parameter of interest is δ_3 , which under the main hypothesis that public disclosure deters taxpayers from underreporting is expected to be positive. If Internet disclosure is a stronger form of disclosure 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_3 is biased downward, and in this sense represents a lower bound of the public disclosure effects on business owners' income reporting.

Given that we believe that people are essentially randomly assigned into groups, and because a time-constant individual effect is included in the regression, we do not expect individual time-varying characteristics to substantially affect the public disclosure effect. In addition to standard (time-varying) covariates (age, education, marital status, and number of children), X'_{it} also includes a variable which describes the nature of the economic activity in the municipalities where the individuals reside interacted with time dummies, to allow for industry specific growth. In the sensitivity tests, the results of which are described after presenting the main results, we discuss other possible sources of heterogeneous trends, such as dependency on preexisting income growth.

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, it may be advantageous to use relative income developments for wage earners in the catalog and noncatalog 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 noncatalog area) were also able to adjust their income to the new regime, the

¹⁵In a previous version of the paper, (Bø, Slemrod, and Thoresen 2014) we estimated a group fixed effects version of this model, obtaining results that are (essentially) similar to those reported here. See also robustness checks in Section IVC.

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

estimate of δ_3 is biased downward as a measure of the effect on business owners.¹⁷ 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).

III. Results and Sensitivity Tests

A. 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 catalogs 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 municipalities²⁰ in the treatment and control groups prior to 2001. This means that we exploit data for approximately 370,000 individuals.

In Table 1, we present mean values for individual-level characteristics in the treatment group (no catalog) and control group (Table 2 shows descriptive statistics for the municipality-level variables). The two different time periods are referred to as “before” and “after.” The income concept used is “earned income,” consisting of wage income and earnings from self-employment (and other organizational forms that require that individuals report business income). Thus, the measure of income used does not include capital income. However, we show results for an alternative definition of income (capital income included) in the sensitivity tests. 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

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

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. The assignment to treatment depends only on the period before 2001.

²⁰ See Section II for a description of the selection of municipalities.

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 et al. (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,

TABLE 1—AVERAGES FOR INDIVIDUAL CHARACTERISTICS, 1997–2000 (BEFORE) AND 2001–2004 (AFTER)

	Business owners			
	Noncatalog		Catalog	
	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	0.57	0.57	0.61	0.61
University education	0.14	0.14	0.13	0.13
Married	0.69	0.69	0.69	0.70
Number of children	0.87	0.72	0.87	0.72
Age (first period)		44.4		44.7
Male		0.76		0.75
Immigrant		0.028		0.022
Number of individuals		21,493		8,091
Wage Earners				
	Noncatalog		Catalog	
	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	0.51	0.52	0.51	0.52
University education	0.26	0.27	0.24	0.25
Married	0.64	0.65	0.62	0.63
Number of children	0.89	0.78	0.86	0.74
Age (first period)		42.3		42.4
Male		0.52		0.51
Immigrant		0.031		0.022
Number of individuals		266,345		78,111

Note:

^a Average exchange rate against USD, 1997–2004; US\$1 = NOK 7.75

identifying tool of the present paper, by showing separate figures for people belonging to municipalities with and without distribution of paper catalogs in the first time period as well as the time-varying individual characteristics that are controlled for in the regression analysis: education (dummies for having education at the high school level and at the university level, respectively), marital status and number of children.

We see that the average first-period income level, both among business owners and wage earners, is somewhat higher in the “noncatalog” 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 “noncatalog” areas is higher than in the “catalog” areas: 18.5 percent and 16.1 percent, respectively. This is further illustrated in Figure 1, where the average reported income differences between noncatalog and catalog municipalities are shown for each year of the

given the empirical approach of the present paper, we do not expect such maneuvers to affect our results, as any such incentives would be identical as between businesses located in catalog and noncatalog municipalities.

TABLE 2—AVERAGES FOR MUNICIPALITY-LEVEL CHARACTERISTICS

	Noncatalog	Catalog	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	0.98	0.63	0.98
Main economic activity ^a			
Share in construction	0.26	0.23	0.23
Share in fisheries	0.09	0.03	0.08
Share in manufacturing	0.55	0.68	0.62
Share in farming	0.29	0.42	0.27
Share in service sector	0.61	0.52	0.59
Number of municipalities	107	31	297

Note:

^aEach municipality is described by either one or a combination of two main activities.

FIGURE 1. DIFFERENCES IN AVERAGE REPORTED INCOME (THOUSAND NORWEGIAN KRONER) BETWEEN RESIDENTS OF NONCATALOG AND CATALOG MUNICIPALITIES, 1993–2004. WAGE EARNERS AND BUSINESS OWNERS.

Note: Based on the 350,203 individuals present in all 12 years: 322,343 wage earners, 27,860 business owners.

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 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

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 6 percent) than in the sample used in the regressions.

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.

Figure 1, that is, the development in reported income for wage earners and business owners in the catalog and noncatalog 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 individual fixed effects, which control for observed or unobserved differences between individuals, and thus also between the two groups of municipalities. Still, if there are systematic differences between the municipalities 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 because 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,b). 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 noncatalog and the catalog 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 noncatalog 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. In any event, we will employ individual fixed effects (nesting municipal fixed effects) in the regressions, and in one specification we let income depend flexibly on the industry classification interacted with year.

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.

TABLE 3—GROWTH IN AVERAGE REPORTED INCOME, BETWEEN 1997–2000 AND 2001–2004.
BUSINESS OWNERS AND WAGE EARNERS IN NONCATALOG AND CATALOG MUNICIPALITIES

	Noncatalog	Catalog	Difference
Income growth of business owners	18.52	16.13	2.39
Income growth of wage earners	19.91	20.05	−0.14

TABLE 4—EFFECT OF PUBLIC DISCLOSURE ON INCOME REPORTING.
ESTIMATION RESULTS FOR REGRESSIONS BASED ON PRE-2001 CATALOG GROUP ASSIGNMENT

Explanatory var.	Coeff.	(1)		(2)		(3)	
		Est.	SE	Est.	SE	Est.	SE
Business owner/ post-2001	δ_1	−0.068***	(0.009)	−0.035***	(0.009)	−0.032***	(0.009)
Noncatalog/ post-2001	δ_2	−0.002	(0.003)	−0.002	(0.003)	−0.004	(0.003)
Public disclosure ^a	δ_3	0.034***	(0.012)	0.031***	(0.011)	0.031***	(0.011)
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 ²		0.048		0.080		0.080	

Notes: Standard errors clustered at municipality level. All specifications with individual fixed effects and year fixed effects.

^aBusiness owners in the noncatalog area after Internet exposure, wage earners included.

^bAge, age squared, education at high school or university level, marital status, number of children.

^cFlexible trend in industry-specific growth; year interacted with municipality-industry classification.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

B. 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 noncatalog and catalog municipalities (as already noted) is 2.39 percentage points. If we adjust the benchmark for differences between income growth for wage earners of the two groups of municipalities (−0.14), 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 catalog and noncatalog 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 the results for the specification detailed below.

Next, in Table 4, we turn to fixed effects estimation results for equation (1). Because errors may be serially correlated in panel data and there may be other

sources of clustering, the error terms may not be i.i.d. Following recommendations by Bertrand, Duflo, and Mullainathan (2004),²⁶ we cluster standard errors at the municipality level. Results for three different specifications are presented: regression 1 does not include any controls for observable 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.

A positive effect of public disclosure clearly stands out in Table 4, as signified by the parameter estimates of δ_3 . The estimate for specification 3 (controlling for both individual characteristics and industry-specific growth) suggests that on average a 3.1 percent growth in the reported income among business owners in the noncatalog areas can be attributed to the substantially increased Internet exposure affecting incomes for 2001 and after. As the standard error is 1.11, the estimate is clearly significant (p -value below 0.01). 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 suggests that the effect may have been 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-variable 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. The effects are somewhat smaller, but still significantly different from zero.

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; Johansson 2005, for estimates in 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

²⁶ See also Donald and Lang (2007). In one alternative specification, reported later, we use the two-way clustering recommended by Cameron, Gelbach, and Miller (2006).

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.

appraisal of this back-of-the-envelope estimate, one should remember that there was a system for public disclosure in Norway even before the Internet exposure, which most likely moderates the effect, and that there is no public disclosure in Denmark.

C. 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, matching techniques, and alternative econometric specifications. In order to explore which groups of business owners responded to the changes in disclosure regime, 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. Results of the alternative estimations are presented in Tables 5 and 6.

Individual-Specific Trends.—First, we explore the effects of introducing a control variable accounting for possible individual trends in income. Based on income for the years 1997–2000, a linear trend in log income is created for each individual, and the trend is extrapolated to the years after 2000. This does not have a large effect on the estimate of public disclosure, as shown in Table 5. The estimate is close to the main estimate, referred to as “Base specification” in Table 5.

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 as well; interestingly, the measured response is higher than the main estimate. 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 more often third-party-reported). Thus, the higher estimate may reflect 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. Ignoring such effects increases the probability of false rejections of the null hypothesis. Consequently, above we reported results for a procedure suggested by Bertrand, Duflo, and Mullainathan (2004), which adjusts measures of variance for one-way clustering at the municipality level.

In Table 5 we show results for two 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. The robust

TABLE 5—EFFECT OF PUBLIC DISCLOSURE ON REPORTED INCOMES
FOR ALTERNATIVE METHODOLOGICAL APPROACHES

	Estimate	SE ^a
Base specification ^b	3.06***	1.11
Individual trends		
Individual-specific trend in income	2.80***	0.99
Other definition of income		
All taxable income minus deductions	4.72***	1.67
Alternative variance estimators		
Robust variance	3.06***	0.69
Collapsed income for two periods	3.11***	0.68
Municipality fixed effects		
Clustering at the municipality level	3.11***	1.10
Clustering at the municipality and year levels	3.11***	0.68
Functional form dependence		
No log transformation of dependent variable	1.76	1.16
Median regression	2.17***	0.44
Placebo tests		
Internet exposure introduced in 1997 ^c	1.54	1.65
Prop. score alloc. of control group municip.	−0.12	1.82
Matching		
Matching of municipalities	3.13**	1.52
Specific groups		
Doctors, lawyers, engineers, etc.	4.34	2.65
Taxi drivers, restaurant owners, etc.	−0.76	2.17
Above-median municip. population density	2.63**	1.22
Above-median municip. population	2.78**	1.11

Notes:^aStandard errors clustered at municipality level, unless otherwise stated.^bColumn 3 in Table 4, specification with individual controls and industry-specific growth.^cEstimated on a 6 percent smaller sample, see footnote 22.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

variance refers to the standard “sandwich” (or Eicker-Huber-White) estimate of variance, which accounts for heteroskedastic disturbances by using the empirical variance-covariance matrix; see Froot (1989) and Rogers (1993). Another specification, suggested by Bertrand, Duflo, and Mullainathan (2004), implies that data are aggregated into two periods only, before and after the Internet exposure. This diminishes the effect of the panel dimension on the standard errors. 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.

Municipality Fixed Effects.—We also show that results for a specification with municipality fixed effects are very close to the results with individual fixed effects. Following recommendations by Cameron, Gelbach, and Miller (2006) to calculate consistent standard errors, we show results for two-way clustering, municipality and year clustering. We also report results with clustering at the municipality level, as in the main specification.

Functional Form Dependence.—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, Viscusi, and Durbin (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 1.76, and it is not statistically significant at the usual standards ($t = 1.52$).

Furthermore, we explore whether the public disclosure effect differs over the distribution of income. A standard way to investigate heterogeneity in the distribution of effects is quantile regressions, 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),²⁸ this method very straightforwardly provides information about how estimated slope coefficients vary over the income distribution.²⁹ To get estimates of the unconditional quantile effect, we use the unconditional quantile regression method advised by Firpo, Fortin, and Lemieux (2009). Thus, unconditional quantile regressions provide predictions for the median or another point of the income distribution with respect to public disclosure.³⁰

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 seventieth 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 (i.e., without logging income), which implies that more weight is given to the top incomes, produces a smaller response estimate (compared to the base specification).

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

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

³⁰ For practical reasons we base our quantile regressions on specifications with municipality and year fixed effects, a full set of controls and two-way clustering.

FIGURE 2. EFFECT OF PUBLIC DISCLOSURE ON INCOME REPORTING ACROSS PERCENTILES.
QUANTILE REGRESSIONS FOR SPECIFICATION BASED ON PRE-2001 CATALOG GROUP ASSIGNMENT

Note: Quantile regressions with full set of individual and municipality controls.

two different placebo tests. In the first test we proceed as if the intervention happened in 1997 instead of 2001, and measure incomes in the years before and after, 1993–1996 and 1997–2000.³¹ The choice of 1997 as a placebo year is influenced by the pattern in Figure 1, where there seems to be a kink in 1997.³²

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.³³ 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 dataset.

³¹ The years after 2000 are excluded, as they have been affected by the actual change in disclosure regime.

³² The dataset used in the placebo test is around 6 percent smaller than the main dataset, as we need to add information on the years 1993–1996. This dataset is the same as used to construct Figure 1.

³³ 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 use the following individual variables (for each municipality taking averages over the years 1997–2000 and over the individuals): wage, age, sex, education, and share of business owners.

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 catalogs 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 the control and treatment groups. 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.³⁴ The average propensity scores³⁵ of the control and treatment group in our sample are, respectively, 0.65 and 0.81 before matching. Municipalities are matched by pairwise (or nearest neighbor) matching.³⁶ We then have a dataset consisting of 29 matched municipality pairs, which are as similar as possible based on observable characteristics.³⁷ 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 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.

Response Heterogeneity.—There are reasons to expect that there are differences across industries with respect to underreporting. As we have added to the dataset detailed information about which industry the business owners belong to (Statistics Norway 2005),³⁸ there is scope for further examination of differences across different sectors. In Table 5 we report the result of an estimation that has 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 (Norwegian 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, Morse, and Tsoutsoura (2012). The latter study shows empirical evidence of more tax evasion among doctors, engineers, private tutors, financial services agents,

³⁴ See also Heckman, Ichimura, and Todd (1997) and Abadie (2005) for approaches to matching.

³⁵ This is estimated as just described under “Placebo tests.”

³⁶ We use a caliper (i.e., the largest allowed difference of propensity score between matches) of 0.025. Two treated municipalities do not have any matches within the 0.025 caliper, and are thus dropped.

³⁷ The propensity score is now respectively 0.66 and 0.65 for the control and treatment group.

³⁸ 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.

accountants, and lawyers.³⁹ Thus, we estimate our base specification on business owners only from these groups.⁴⁰

Table 5 shows a stronger effect than average for a group that includes doctors, lawyers, engineers, and other professions, though the effect is just outside of the range of significance at the 0.1 level. For taxi drivers and restaurant owners, however, the negative, nonsignificant 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 seems 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 also explore whether there are 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 dataset 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.

Tax Evasion or Signaling.—As noted in Section I, social bragging may be another explanation for the pattern we observe. It may be important for people to “keep up with the Joneses,” and if people use income as a signaling device,⁴¹ the change to Internet exposure may be used to demonstrate personal success. Note, though, that Hasegawa et al. (2013) find that both individuals and corporations underreport income to avoid disclosure, which is not consistent with the signaling theory.

It is hard to discriminate between the tax evasion explanation and the signaling effect for business owners. However, if bragging or signaling is the dominant explanation, we would also expect to see effects among wage earners, in the income definition including deductions.⁴² Even though most incomes and several deductions are third-party reported, wage earners may increase reported income by foregoing certain

³⁹ Artavanis, Morse, and Tsoutsoura (2012) use the larger loans given to the self-employed, compared to wage earners with similar incomes, as an indication of tax evasion. Their explanation for the difference in tax evasion between business categories is a difference in paper trail. Erard and Ho (2003) also find more tax evasion among lawyers and doctors.

⁴⁰ The categories included are: building of complete constructions or parts thereof; civil engineering; financial intermediation and activities auxiliary to financial intermediation; legal, accounting, bookkeeping, 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 organizations.

⁴¹ This is reminiscent of Glazer and Konrad (1996), who study charity contributions as status signaling. If charitable contributions are seen as an alternative form of signaling, one may expect that demonstrating prosperity through charitable donations is traded against public disclosure of income and wealth information. We do not have data on charitable giving and also do not believe that the effect on charity would be large; as there, in general, is little attention given to donors in Norway.

⁴² Note that our basic specification uses gross earned income (before deductions) as income definition, with no opportunity for overreporting for wage earners.

TABLE 6—EFFECT OF PUBLIC DISCLOSURE ON REPORTED ORDINARY INCOME
FOR BUSINESS OWNERS AND WAGE EARNERS

	Estimate	SE ^a
Ordinary income		
Business owners	2.93**	1.38
Wage earners	−0.23	0.32

Note: All specifications with individual fixed effects and year fixed effects.

^aStandard errors clustered at municipality level.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

deductions in income, such as the commuting deduction, deductions for charitable giving, and interest deductions for foreign or private debt. Under income reporting as signaling, wage earners and business owners are willing to pay for their increased status by reporting higher income after deductions (higher ordinary income). To further investigate this issue, we remove the occupational dimension from equation (1) and estimate it separately for business owners and wage earners, using ordinary income as the income concept. Even though business owners have more scope for adjusting reported income, if income signaling is important, we would expect to see a smaller, but positive effect for wage earners as well. Table 6 shows no signs of income as status demonstration, as the estimate for wage earners is not different from zero.

However, if such status signaling is more valuable for business owners than for wage earners, for example because high income may be interpreted as high quality of the services that are delivered, some part of the income responses reported in the present study could still be a result of signaling. In sum, although we cannot rule out entirely the signaling effect, we believe that tax evasion is the predominant explanation for our findings.

IV. Conclusion

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

APPENDIX A:
FIGURE APPENDIX

FIGURE A1. SPATIAL LOCATION OF CATALOG AND NONCATALOG MUNICIPALITIES

FIGURE A2. AVERAGE REPORTED INCOME 1993–2004,
WAGE EARNERS AND BUSINESS OWNERS

Note: Based on the 350,203 individuals present in all 12 years: 322,343 wage earners, 27,860 business owners.

FIGURE A3. DIFFERENCES IN LOG OF AVERAGE REPORTED INCOME BETWEEN NONCATALOG AND CATALOG MUNICIPALITIES, 1993–2004, WAGE EARNERS AND BUSINESS OWNERS (THOUSAND NORWEGIAN KRONER)

Note: Based on the 350,203 individuals present in all 12 years: 322,343 wage earners, 27,860 business owners.

APPENDIX B:
ESTIMATION RESULTS FOR OTHER SPECIFICATIONS

TABLE B1—EFFECT OF PUBLIC DISCLOSURE ON INCOME REPORTING.
ESTIMATION RESULTS FOR SPECIFICATION WITH NONCATALOG DIMENSION

Explanatory var.	(1)		(2)		(3)	
	Est.	SE	Est.	SE	Est.	SE
Business owner/ post-2001	−0.043***	(0.006)	−0.012**	(0.006)	−0.009	(0.006)
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 ²	0.048		0.080		0.080	

Notes: Standard errors clustered at municipality level. All specifications with individual fixed effects and year fixed effects.

^aAge, age squared, education at high school or university level, marital status, number of children.

^bFlexible trend in industry-specific growth; year interacted with municipality-industry classification.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

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 specifications that each remove one dimension from equation (1), the distinction between catalog and noncatalog areas and the occupational group dimension (restrict to business owners only), respectively. Thus, we present estimation results for $\log y_{it} = \alpha + \kappa(\text{bus}_i \times \tau_t) + X'_{it}\beta + \mu_i + \lambda_t + \varepsilon_{it}$ and for $\log y_{it} = \alpha + \eta(\text{nocat}_i \times \tau_t) + X'_{it}\beta + \mu_i + \lambda_t + \varepsilon_{it}$, respectively, where α , β , κ , and η are parameters. Variable explanations can be found in Section II.

The parameter estimate for $\hat{\kappa}$ in Table B1 shows that the identification of public disclosure in equation (1) rests on the information in catalog and noncatalog areas, and is not captured by a simple comparison of 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 many 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 A2, 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 noncatalog 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 for wage earners in

TABLE B2—EFFECT OF PUBLIC DISCLOSURE ON INCOME REPORTING.
ESTIMATION RESULTS FOR BUSINESS OWNERS ONLY

Explanatory var.	(1)		(2)		(3)	
	Est.	SE	Est.	SE	Est.	SE
Public disclosure ^a	0.032***	(0.011)	0.029**	(0.012)	0.023**	(0.009)
Indiv.-control var. ^b	No		Yes		Yes	
Industry-spec. gr. ^c	No		No		Yes	
Observations	236,672		236,672		236,672	
R ²	0.032		0.055		0.058	

Notes: Standard errors clustered at municipality level. All specifications with individual fixed effects and year fixed effects.

^aBusiness owners in the noncatalog area after Internet exposure.

^bAge, age squared, education at high school or university level, marital status, number of children.

^cFlexible trend in industry-specific growth; year interacted with municipality-industry classification.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

catalog and noncatalog areas (see Table 3), that the public disclosure effect in this more restricted sample is close to the estimates seen in Table 4. A somewhat smaller effect is obtained for the specification in column 3.

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