The Effects of Income Transparency on Well-Being: Evidence from a Natural Experiment[†]

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In 2001, Norwegian tax records became easily accessible online, allowing everyone in the country to observe the incomes of everyone else. According to the income comparisons model, this change in transparency can widen the gap in well-being between richer and poorer individuals. Using survey data from 1985–2013 and multiple identification strategies, we show that the higher transparency increased the gap in happiness between richer and poorer individuals by 29 percent, and it increased the life satisfaction gap by 21 percent. We provide back-of-the-envelope estimates of the importance of income comparisons, and discuss implications for the ongoing debate on transparency policies. (*JEL* D31, H24, I31, K34)

The income comparisons model proposes that individual well-being largely depends on how that individual's income compares to the incomes of others (Luttmer 2005). This is a fundamental aspect of individual preferences, yet no consensus exists about the importance of income comparisons.[1] In this study, we offer novel evidence based on a unique natural experiment: in 2001, Norwegian tax records became easily accessible online, allowing everyone in the country to observe the incomes of everyone else quickly and easily. We test the hypothesis that, consistent with the model of income comparisons, increased income transparency widens the gap in well-being between richer and poorer individuals.

Tax records have been public in Norway since the nineteenth century, but they have not always been easily accessible. Before 2001, one had to make a formal request in person at the tax agency to see someone else's income. In the fall of 2001, the Norwegian media digitized tax records and created websites that allowed any

[1] This question is important to understand preferences more deeply, and also due to its implications for income taxation and other policies. For example, income comparisons can create positional externalities that reduce social welfare and could be corrected with taxes (e.g., Boskin and Sheshinski 1978, Frank 1985).

individual with internet access to search anyone's tax records. Every Norwegian was one click away from finding out the incomes of everyone else in the country.

We use various data sources to show the massive popularity of these online tax lists. During the busiest week of the year, these websites were more popular than YouTube. We also show that, rather than using the tax lists for legitimate goals (e.g., uncovering corruption or tax evasion), most used the websites to snoop on friends, relatives, and social contacts. For example, users could create leaderboards showing the highest and lowest earners among their Facebook friends or maps showing the incomes of everyone living around a specific location. This behavior became so pervasive that the Norwegian media dubbed it "tax porn."

Because income transparency facilitates income comparisons, it can widen the gap in well-being between richer and poorer individuals. Poorer individuals often lose this game of income comparisons. For example, if they learn that they are poorer than they thought (Cruces, Perez-Truglia, and Tetaz 2013), it can lower their self-esteem. If their social contacts learn how poor they are, it can reduce their social-esteem. In contrast, richer individuals often benefit from this game. Learning that they are richer than they thought can boost their self-esteem. And being looked up by their social contacts can boost their social-esteem.[2]

To test this hypothesis, we measure the effect of the increase in transparency on the gradient between subjective well-being and individual income rank (henceforth referred to as the *happiness-income gradient*). We use survey data from Norway from 1985–2013 that include the two most widely used measures of subjective well-being: happiness and life satisfaction. Despite some limitations of these subjective measures, evidence suggests that they contain useful information about well-being. For example, life satisfaction and happiness have been shown to be significantly correlated with objective measures of well-being and with decision utility (Di Tella, MacCulloch, and Oswald 2003; Benjamin et al. 2012).

Consistent with the hypothesis of income comparisons, we show that the 2001 income transparency change led to a 29 percent increase in the happiness-income gradient (p-value $= 0.005$) and a 21 percent increase in the life satisfaction-income gradient (p-value $= 0.026$).

We use multiple strategies to identify the causal effect of the transparency change of 2001. First, we conduct an event-study analysis and find that the happiness-income gradient stayed constant in the years before the change, increased in 2001, and persisted at the higher level during the subsequent 12 years of higher transparency.

Second, we identify individuals who were most likely to be exposed to the effects of online tax lists, based on observable characteristics that predict internet access. We show that, between 1985 and 2000, the happiness-income gradient remained stable for individuals with low and high internet access. After 2001, the happiness-income gradient remained at the pre-2001 level for individuals with lower internet access but increased substantially and persisted at the higher level for individuals with higher internet access.

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[2] Different individuals may react differently to the increased transparency. For example, while some rich individuals may feel happy that their neighbors caught a glimpse of their income, others may feel uneasy about the same situation (e.g., if they do not feel deserving of their high income). In this study, we can only measure which of these different mechanisms dominates on average.

Our third identification strategy reproduces the analysis using similar survey data from Germany, a country that was not affected by the Norwegian change in income transparency. Similar results for Germany would indicate that another factor, such as the dot-com bubble, caused the change in the happiness-income gradient in Norway. In sharp contrast to the Norwegian findings, however, the life satisfaction-income gradient did not change around 2001 in Germany. The event-study analysis shows that this gradient remained stable in Germany from 1985 to 2013, both in the population at large and in the subpopulations of individuals with higher and lower internet access.

Anecdotal evidence supports our finding that higher income transparency increased the well-being gap between richer and poorer households. For example, the media reported that the online tax lists led to bullying of kids from poorer households and that adults from poorer households felt that they disappointed themselves and others (Steinsland 2008, Associated Press 2009). Our findings also align with survey data indicating that, relative to richer households, poorer households were more likely to oppose the income transparency policy (Langset 2011).

The effects of income transparency may operate through multiple mechanisms. We provide suggestive evidence for one specific mechanism, self-perceptions. According to this channel, richer individuals may be happier because they learn that they are richer than they thought, and poorer individuals may be unhappier because they learn that they are poorer than they thought. We show that, indeed, transparency increased the gradient between perceived income rank and actual income rank by 8.5 percent (p-value < 0.001) and the gradient between the perceived adequacy of one's income and income rank by 4.7 percent (p-value $= 0.083$). This evidence cannot prove or rule out the self-perceptions channel, but it does serve as suggestive evidence. Moreover, the perceived rank and income adequacy gradients (8.5 percent and 4.7 percent) are smaller than the changes in the happiness and life satisfaction gradients (29 percent and 21 percent), which suggests the presence of other mediating factors besides self-perceptions.

We use the estimated effects of transparency to quantify the importance of income comparisons. Our back-of-the-envelope calculations suggest that, as a conservative lower bound, income comparisons accounted for 22 percent of the happiness that individuals in Norway derived from their incomes during the period of higher transparency. Moreover, we show that this lower bound is consistent with the effect of relative income on happiness, as reported in related studies.

Our evidence also relates to the ongoing debate on transparency. Technological advances have made it possible for everyone to know potentially everything about everyone else, sparking debates on whether the government should disclose its data, such as tax records. Some arguments that favor or oppose transparency are rooted in philosophical grounds.[3] However, most arguments seem to be based on the potential effects of transparency. In particular, detractors of income transparency argued that the tax lists were used in despicable ways to harm the well-being of poorer

[3]For example, some of the supporters of income transparency in Norway see it as a fundamental principle of democracy, while some opponents see it as a violation of privacy rights.

individuals.[4] This argument, however, was based on qualitative and anecdotal evidence. This study provides the first quantitative evidence on this matter.

Beyond the Norwegian experience, information disclosure may directly affect well-being in other contexts. In the 2000s, Sweden, Iceland, and Finland had to decide whether to make their tax records as easily accessible as in Norway. Outside of Scandinavia, governments disclose all sorts of sensitive information, such as the salaries of public employees (Card et al. 2012, Mas 2017), individual contributions to political campaigns (Perez-Truglia and Cruces 2017), and identities of criminals and tax delinquents (Linden and Rockoff 2008, Perez-Truglia and Troiano 2018). Our findings suggest that it is important to measure the well-being effects of disclosing sensitive data and to account for them in the cost-benefit analysis.

This paper relates to various strands of literature. Most important, it relates to a literature on the effect of relative income on well-being. In a seminal contribution, Easterlin (1974) showed evidence that happiness and income are positively correlated across individuals within a country but that average happiness in a country does not seem to rise over time as average income rises. One standard explanation for the paradox is that happiness depends on relative income. Within a given country, richer individuals have higher relative income, so they are happier. However, as every individual in the country becomes richer, the average relative income stays constant, and thus average happiness also remains constant. Consistent with this interpretation, several studies have shown that, holding own income constant, subjective well-being decreases with the mean income of neighbors (Luttmer 2005, Ferrer-i-Carbonell 2005).[5]

However, this evidence is subject to concerns about causal identification. For example, the Balassa–Samuelson model (Balassa 1964, Samuelson 1964) predicts that consumer prices should be higher in areas where nominal incomes are higher. Thus, even in the absence of income comparisons, happiness should be negatively correlated to the average income of neighbors, reflecting a higher cost of living. More generally, the average income in an area could be correlated with other unobservable attributes of the location that also affect well-being, thus generating omitted-variable biases. We contribute to this literature by presenting novel evidence on the effects of relative income on happiness that relies on a new identification approach, based on quasi-experimental variation in income transparency.

This study also relates to Bø, Slemrod, and Thoresen (2015), which measured the effect of the Norwegian disclosure of tax records on tax evasion. Disclosing tax records may deter tax evasion by encouraging others with relevant information about true tax liability to come forward and by threatening evaders with social sanctions (see also Perez-Truglia and Troiano 2018). Bø, Slemrod, and Thoresen (2015) found that the change in income disclosure increased reported income among business owners by 2.7 percent, resulting in a total gain of 0.2 percent in income tax

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[4] This was by no means the only negative consequence from income transparency that was debated. For example, some detractors of open disclosure argued that the tax records could be used by criminals to target rich individuals. However, in a letter to the Ministry of Justice, the Norwegian police noted that their investigations ruled this out as a significant source for concern (*Dagens Næringsliv* 2010).

[5] There are some conflicting accounts about the evidence. See, for instance, Hagerty and Veenhoven (2003), Stevenson and Wolfers (2008), Easterlin et al. (2010), and Easterlin (2017) on the effect of income growth on happiness, and Senik (2004), Clark, Westergård-Nielsen, and Kristensen (2009), and Deaton and Stone (2013) on the effect of relative income on happiness.

revenues. This evidence confirms a benefit of disclosure, as alleged by its supporters. We present evidence on an unintended effect, income comparisons, as alleged by detractors of income transparency.

This work also relates to the study by Card et al. (2012) of the effects of income transparency on job satisfaction. The researchers sent emails to a random sample of university employees with information on how to access a website that listed the wages of all employees working in the same university. In a follow-up survey, they found that for workers with below-median salaries within their unit and position, having access to the website decreased satisfaction with their wages and their jobs. Consistent with this finding, Rege and Solli (2015) shows evidence that the disclosure of tax records in Norway increased the probability of quitting among workers with lower salaries. Their findings suggest that some poor individuals may benefit from income transparency, because they can find out if they are underpaid and look for a better job. On the contrary, our evidence suggests that income transparency increased the well-being of richer individuals at the expense of the well-being of poorer individuals.

Last, this study relates to a literature documenting how individuals misperceive their positions in the income distribution and how providing objective information can correct these misperceptions (Cruces, Perez-Truglia, and Tetaz 2013; Karadja, Mollerstrom, and Seim 2017). These studies are based on artificial contexts in which researchers provide information through a survey. We contribute to this literature by exploiting the variation in information access in a natural, large-scale setting and by showing that correcting these misperceptions may affect well-being.

The rest of the paper proceeds as follows. Section I describes relevant details about the disclosure policy. Section II presents the econometric specification and the survey data. Section III presents the results. The last section concludes.

I. Relevant Institutional Details

A. *Origin of the Online Tax Lists*

Although tax records have been publicly available in Norway since the middle of the nineteenth century, they were not easily accessible before 2001. Individuals who wanted to learn about someone else's income had to visit the local tax office or city hall during a three-week period and search through a book with records for thousands of taxpayers from the same municipality.[6] In the fall of 2001, a Norwegian newspaper made these tax records searchable online for the first time so that any Norwegian with internet access could view them easily and at any time (see Figure 1 for a screenshot of this website). All major newspapers soon created their own websites, which remained popular in the country for the following decade.[7] These websites listed full names and net incomes (see Figure 2 for a sample search result), and

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[6] In selected municipalities and only shortly before 2001, some local organizations sold books with information from the local tax rolls. Bø, Slemrod, and Thoresen (2015) exploits variation across these municipalities to identify the effects of transparency. We cannot use this same identification strategy because we lack sufficient data (we would need a survey sample orders of magnitude higher and with a higher frequency).

[7] The following are some sample websites: www.skattelister.no, www.nrk.no/skatt, www.tu.no/skattelister, and skatt.na24.no.

Figure 1. Screenshot of Search Tool for Tax Records

Note: Intro website from skattelister.no as of June 16, 2010.

Source: web.archive.org

could also list additional information such as taxes, net worth, birth years, cities, and postal codes. These websites allowed visitors to search by multiple fields. For example, visitors could search for their own last name to find relatives. Or they could search by postal code to find neighbors.

Although all other Scandinavian countries (except Denmark) make tax records publicly available, the Norwegian tax disclosure during 2001–2013 was exceptional because of its accessibility (Langset 2011).[8] In Finland, accessibility of tax records is similar to that in Norway prior to 2001, requiring individuals to visit the tax agency in person (Kostyukov 2018).[9] In Sweden, the requests for tax returns are not anonymous and must be done by phone, making this practice much less popular than it is in Norway.[10] In Iceland, tax records are also difficult to access and available during two weeks of the year only.

B. *Evolution of the Online Tax Lists*

Between 2001 and 2013 (the last year of our survey data), several factors may have contributed to increased or decreased use of online tax lists, though none of these changes in visibility was remotely comparable in size to the change of 2001.

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[8] In other countries, information about incomes can be easily accessed online for a subset of the population (e.g., public employees in some US states).

[9] In the day that the tax records are released, a few dozen Finnish journalists line up in the tax agency to look up the incomes of some newsworthy individuals. However, that number pales in comparison to the millions of searches conducted in Norway every year. One exception of the Finnish law is that the tax records are searchable online for the top 10,000 richest individuals. Also, there was a period in which requests for tax records could be done over the phone. However, to the best of our knowledge, this option was not nearly as widespread as the online searches were in Norway.

[10] In 2006, a credit reporting company called Ratsit published a website with a search tool for tax records similar to the ones offered in Norway (*The Local* 2015). However, it was taken down by the Sweden's Chancellor of Justice shortly thereafter. The website was later allowed, but the searches were non-anonymous and subject to a fee. In 2015, this same company began selling physical copies of the tax records at the municipality level, just like in some Norwegian municipalities prior to 2001.

FIGURE 2. SCREENSHOT OF A SAMPLE SEARCH RESULT FROM THE SEARCH TOOL

Note: Search result from skatt.na24.no as of August 1, 2015.

Source: web.archive.org

Some factors may have contributed to a gradual increase in income visibility. For example, the media added convenient and engaging ways to browse tax records. One newspaper released an app that connected to Facebook and automatically created leaderboards showing the highest and lowest earners among Facebook friends, as in Figure 3.[11] Another application allowed users to tap on a map to see the incomes of everyone living near that position.[12] Just like the websites, these smartphone apps were incredibly popular (Jørgenrud 2009, Teknologirådet 2010).[13] There was also a modest increase in internet access during the 2000s, which may have contributed to higher income visibility: according to Statistics Norway, the share of internet users increased from 72.8 percent in 2002 to 95.1 percent in 2013.

On the other hand, some government regulations may have decreased the degree of income transparency. From 2004 to 2006, regulators introduced restrictions to the use of the tax lists: visitors had to use an official search tool conduct searches, which was only available during three weeks of the year (Teknologirådet 2010). The official search tool was easy to use, and the newspapers seamlessly embedded it in their own websites. The three-week restriction may not have had significant effects either, as individuals could conduct the same number of searches, just in a concentrated period. Indeed, most searches occurred during that same three-week period even when the restriction was not in place, because the timing coincided with tax record updates. For example, about 60 percent of data searches published in October 2013 were conducted during the first three weeks after the tax lists were posted (*E24* 2014), even though individuals were allowed to search all year long.

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[11] Also, the top-right corner of Figure 1 shows an advertisement for one of these apps.

[12] Panel B of online Appendix Figure A.1 shows a screenshot of one of these apps.

[13] In addition to showing individual records, some of the websites and apps offered tools to navigate aggregate data. For example, panel C of online Appendix Figure A.1 shows the screenshot of one of the websites offering an interactive tool to figure out the user's position in the income distribution of the country or a city (*Dagens Næringsliv* 2014). Also, the data published on the tax lists were eventually indexed by all the popular search engines. As a consequence, searching for the name of a Norwegian citizen in Google would show the individual's tax record at the top of the search results (Teknologirådet 2010).
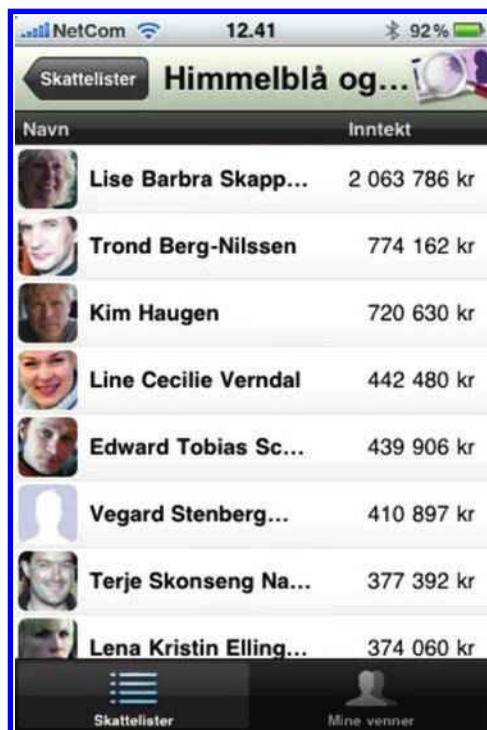
FIGURE 3. SCREENSHOT OF A SMARTPHONE APPLICATION SHOWING AN INCOME LEADERBOARD

Source: Jan Omdahl, "Nå får du skattesøk på iPhone," *Dagbladet*, October 15, 2009

The 2004 restrictions were removed in 2007, with no new restrictions added until 2011. From 2011 to 2013, the government required individuals who wanted to search tax records to log in to the official website of the tax agency using a PIN code and a password.[14] Most individuals presumably already had accounts for filing their taxes and other online services.[15] Search volume probably declined due to this added hassle, but it remained substantial (Thoresen 2013).

The final and most significant restriction was introduced in 2014, when the searches became non-anonymous. This change is not relevant for our empirical analysis because it occurred after the last year of the survey data. However, we discuss this regulatory change below, because it provides evidence about how individuals had been using the search lists.

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[14] The 2011 legislation also introduced a limit on the maximum number of searches per month (500), although it seems that such restriction would not be binding for the vast majority of individuals. Also, the government still allowed the media to disseminate some information from the tax lists, such as the lists of the top-100 richest individuals or break downs of average income by county; see, for example, the following website, which is still functional: www.vg.no/spesial/skattelister/. For reference, panel A of online Appendix Figure A.1 shows a screenshot of the search tool from the official website of the tax agency as of 2015.

[15] However, individuals who were not registered in the tax agency, such as minors or visitors from other countries, could not log into the website.

C. *Popularity of the Online Tax Lists*

We use three sources of data to assess the popularity of the tax lists. The most direct evidence comes from a 2007 survey conducted by Synovate, which was representative of the population of taxpayers. Around 40 percent of respondents reported to have used the online search tools (*Skattebetaleren* 2008). This behavior may be underreported in surveys because of social desirability bias. Thus, the true fraction of Norwegians using these websites may have been even larger than 40 percent.

Web traffic data confirm media claims about the massive popularity of the online tax lists, with one website reporting 29.4 million searches in the year after the publication of the tax records for 2007 (*VG* 2008). This figure implies 7.47 searches per capita among 3,935,000 internet users in Norway in 2007. Even if these statistics are inflated due to self-reporting by the website owners, this figure excludes traffic from other websites and smartphone apps offering access to the tax records, making the likely number of searches even higher.

There are also publicly available data from the period when tax records were accessible only from the tax agency's official website. According to the Norway Ministry of Finance (2014), 920,896 unique users conducted slightly more than 17 million searches in 2013.[16] In that year, only adults with a valid account could log in to the official website to conduct searches. Among the 3,797,822 adults in Norway, about 24.25 percent searched for at least one tax record in 2013, and the average user made 18.46 searches.

The statistics reported for 2007 and 2013 are not directly comparable to each other, because they come from different sources and are probably based on different definitions. With that caveat, the number of individuals conducting searches and the number of searches per capita decreased from 2007 to 2013. This difference is probably due to the 2011 requirement that users log in to the official tax agency website to search tax records.

We also assess the popularity of the income search tool using data from Google Trends, which include the number of times that a keyword is searched in the Google search engine.[17] For the main search category, skattelister, we include searches for the two words used most often to refer to the tax records, "skattelister" and "skattelistene," which both translate literally to "tax list." For instance, one popular website with access to the tax records was www.skattelister.no. As benchmarks, we use data on two keywords that are consistently among the most popular keywords around the world: "weather" and "YouTube." As a proxy for the general interest in information about taxes, we study the number of searches for "tax."

Figure 4 shows the popularity of selected keywords in 2010 (the last year when users could conduct searches outside of the official website of the tax agency). Panel A of Figure 4 shows the results for Norway. Google Trends does not provide information about the absolute number of searches, so the search totals are

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[16] The statistics that have been reported for 2011 and 2012 are similar in magnitude to those for 2013: *Bergens Tidende* (2014) reports over 700,000 unique visitors making over 13 million searches in 2011, and over 900,000 unique visitors making over 16.5 million searches in 2012.

[17] The data can be accessed at: trends.google.com. For a discussion of the advantages and limitations of this type of data, see Stephens-Davidowitz (2014).

Panel A. Annual search volumes, Norway versus Sweden
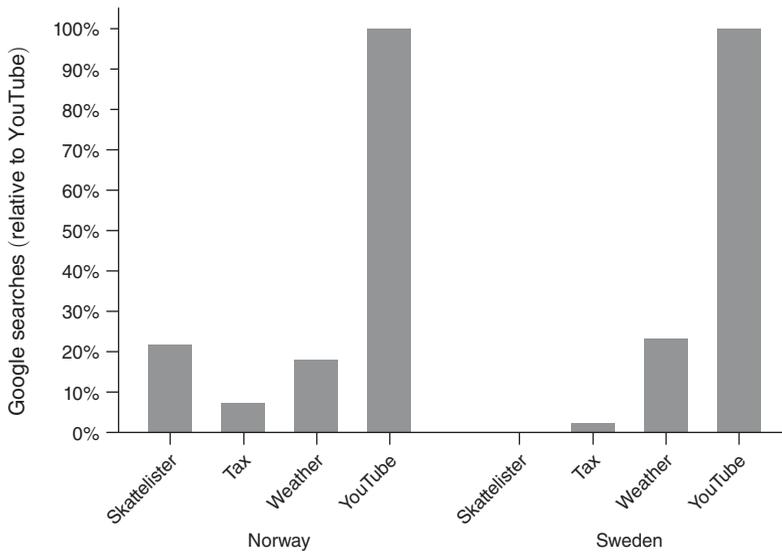
Panel B. Weekly search volumes, Norway
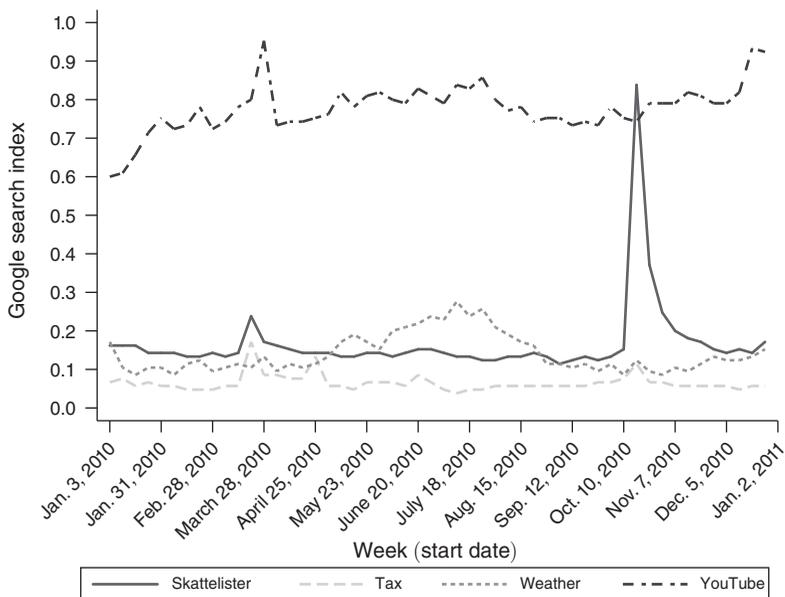
FIGURE 4. POPULARITY OF ONLINE TAX LISTS MEASURED BY GOOGLE SEARCH DATA

Notes: Google Trends data for 2010. Panel A shows the annual number of Google searches for each category of keywords, relative to the searches for "youtube." The *Skattelister* category is comprised by "skattelister+skattelistene," Tax is comprised by "skatt+skatter," *Weather* is comprised by "yr+ver" in Norway and "väder" in Sweden. Panel B shows the weekly number of searches for each keyword category in Norway, normalized so that total searches sum up to 1 in the first week of 2010.

normalized as a fraction of YouTube searches.[18] The data suggest a remarkable interest in the tax lists: for every five searches for YouTube, there was about one for skattelister. Norwegians were more likely to search for the tax records than to search for the weather. Searches for the tax lists were roughly three times higher than those for taxes, suggesting that a general interest in taxes does not explain the popularity of the search tool. As a robustness check, panel B of Figure 4 provides comparable search data for Sweden, where there is no reason for individuals to search for skattelister. The volumes of searches for weather, taxes, and YouTube are roughly similar between Norway and Sweden, but searches for skattelister are virtually non-existent in Sweden.

Panel B of Figure 4 shows the distribution of Google searches over the course of each week of 2010. Search volumes are normalized so that searches in all categories sum up to 1 in the first week of 2010. During most of the year, searches for the tax lists remained stable at roughly twice the volume of searches for taxes and at about the same as weather-related searches. In the third week of October, when data from the previous tax calendar year were released, searches for the tax lists increased sharply.[19] During that week, the number of searches for the tax lists exceeded the number of searches for YouTube, suggesting that Norwegians were more interested in learning about others' incomes than in watching videos on YouTube.

D. *Uses of the Online Tax Lists*

This section presents some evidence that the online tax lists were being used primarily to snoop on social contacts.

Perhaps the best piece of evidence comes from the regulatory change that happened in 2014, when searches for tax records stopped being anonymous. Specifically, any individual could use the same website to identify who searched for their tax records. This non-anonymity should have discouraged individuals from unsavory uses of the tax records, such as snooping, due to the threat of social sanctions. Consistent with this hypothesis, the tax agency reported that the number of searches dropped by 88 percent after the removal of anonymity. Furthermore, the number of users logging in to the system did not decrease much; however, instead of searching for others' incomes, most users logged in to find out who searched for them.[20]

The aforementioned 2007 Synovate survey offers more direct evidence about the uses of the tax lists (*Skattebetaleren* 2008). The survey asked whether respondents searched for specific types of individuals: 61 percent reported searching for close relatives, 53 percent for themselves, 42 percent for friends, 26 percent for work colleagues, 25 percent for other relatives, 23 percent for neighbors, 18 percent for celebrities, and 6 percent for politicians. This pattern is more consistent with snooping on social contacts than investigating corruption. Indeed, around 77 percent of respondents who used the tax records reported using them for curiosity or fun and

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[18] As an additional benchmark, the number of searches for YouTube are slightly higher than the combined searches for "porn" and its Norwegian translation, "porno."

[19] We find a consistent peak in Google Trends data for other years. This peak is also consistent with the internet browsing data discussed below.

[20] Some individuals started selling a search service under their names to allow users make anonymous searches, although the service has not met popular demand (Kvile 2014).

Panel A. Traffic by profile popularity
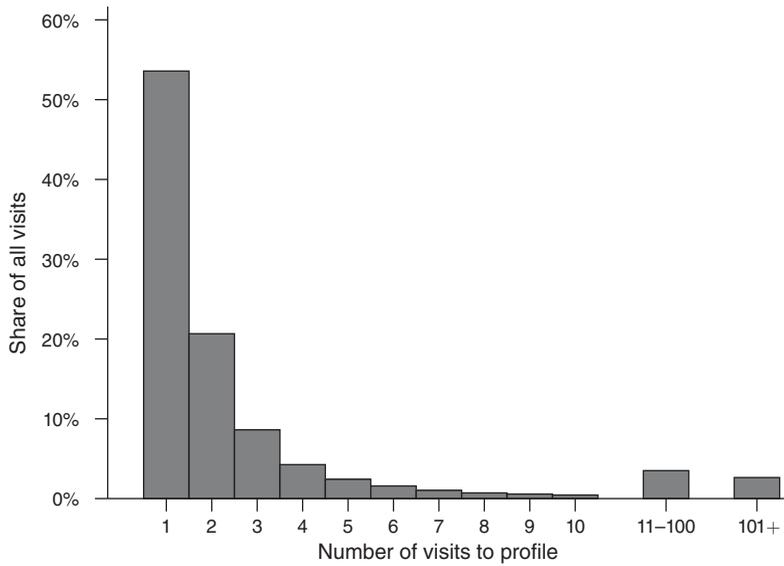
Panel B. Traffic by session length
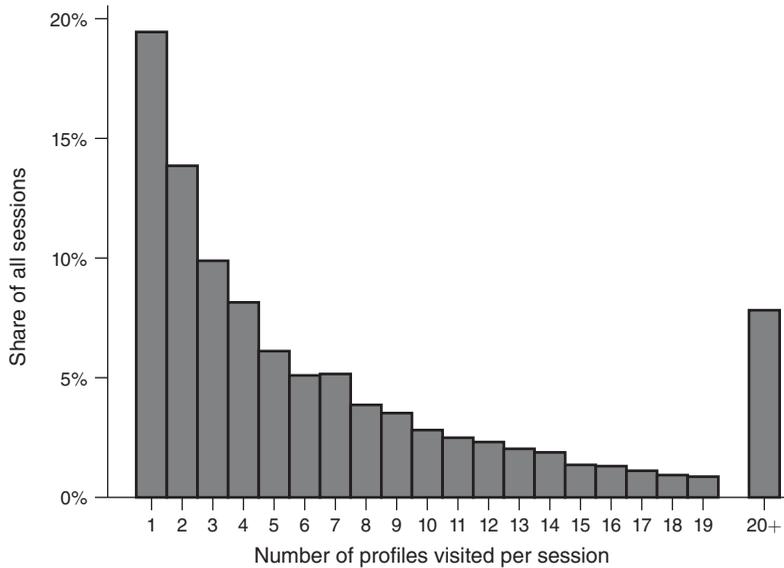
FIGURE 5. INTERNET BROWSING DATA ON THE USES OF THE ONLINE TAX LISTS

Notes: Data from a proprietary panel of internet users, on the visits to a popular website that offered a tool to search the Norwegian tax records during 2010. Panel A shows the number of repeated visits to profiles during 2010 (for profiles that have been visited at least once). Panel B shows the number of profiles visited per session, for sessions with at least one profile visited, corresponding to October 20, 2010 (the release date for the data on incomes corresponding to the 2009 tax calendar). A session begins when the user opens the internet browser and ends when the user closes the browser.

only 2 percent for monitoring, such as uncovering tax evasion (*Digi* 2008). And in another survey conducted by Synovate in 2011, only 15 percent of respondents believed that the tax lists provided useful information (*Sunnmørsposten* 2011).

We also present evidence based on internet browsing behavior from panel data covering a significant share of internet users in Norway in 2010. We focus on visitors to a popular website that provided access to the tax records. The data span 200,000 unique browser sessions with at least one visit to this website. Panel A of Figure 5 shows the distribution of total visits by the number of profile visits. The results suggest that most traffic is not directed to famous people, such as athletes and politicians, because visits to popular profiles (i.e., visited at least 100 times) account for less than 3 percent of total traffic. Panel B provides a histogram of the number of profiles visited per user session on the day of the release of the 2009 tax calendar data.[21] The data suggest that large-volume users, such as mass marketers, did not contribute heavy traffic to these websites. For example, users visiting more than 100 profiles per session account for only 0.27 percent of total visits. Panel B of Figure 5 also shows that individuals did not search only for their own incomes, as the typical session involved searching for several individuals. Even under the conservative assumption that all sessions with a single profile visit corresponded to individuals searching for their own incomes, this type of searches comprise just 2.62 percent of total traffic.

Last, we discuss the possibility that individuals used the online tax lists to learn information about salaries for salary negotiations and career choices (Cullen and Pakzad-Hurson 2018). In the previously mentioned survey data, only 26 percent of individuals searched for work colleagues in the tax lists, and they may have been snooping rather than researching. Moreover, due to the nature of the data, the Norwegian tax lists have been described as "completely useless" for salary comparisons (*NRK* 2008). As a benchmark, the website of state employees studied in Card et al. (2012) publishes information about salaries, with breakdowns by base salary and other forms of compensation. In contrast, the Norwegian tax records reveal the net income of the individual, which aggregates all salaried income, including bonuses and commissions, and non-salaried income, such as capital gains, self-employed income, and social benefits. Thus, if you found out that a coworker was listed in the tax records with a higher net income than yours, you would not be able to tell whether that coworker has a higher salary or whether he or she has additional sources of income.

II. Econometric Specification and Survey Data

A. *Econometric Specification*

The baseline specification is the following:

$$(1) \quad SWB_{i,t} = \alpha_1 \cdot IncomeRank_{i,t} + \alpha_2 \cdot IncomeRank_{i,t} \cdot I_t^{01-13} + X_{i,t}\beta + \delta_t + \epsilon_{i,t}.$$

Here, $SWB_{i,t}$ is a measure of subjective well-being of individual i in year t, with a higher value denoting higher well-being; $IncomeRank_{i,t}$ is the position of individual i in the national distribution of household income in year t, from 0 (the poorest household) to 1 (the richest); I_t^{01-13} is a dummy variable indicating the period

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[21] This is a lower bound on the number of profiles visited per user that day, for instance, because one may have visited the website from multiple devices.

of higher income transparency, equal to 1 if $t \in [2001, 2013]$ and 0 otherwise; δ_t denotes the year effects, $X_{i,t}$ is a vector with additional control variables, and $\epsilon_{i,t}$ denotes the error term.

The coefficient α_1 corresponds to the average gradient between $SWB_{i,t}$ and $IncomeRank_{i,t}$ in the period from 1985 to 2000. We expect this coefficient to be positive, meaning that being richer is associated to higher subjective well-being. This association may arise purely from intrinsic utility from consumption (e.g., richer individuals can afford nicer houses, food, and entertainment), or from a combination of intrinsic utility and income comparisons (e.g., richer individuals get higher self-esteem and social-esteem). The coefficient α_2 measures the change in the happiness-income gradient from 1985–2000 to 2001–2013. Our main hypothesis is that α_2 is positive: i.e., by facilitating income comparisons, the higher transparency increased the happiness-income gradient.

This regression has a difference-in-differences interpretation in which I_t^{01-13} corresponds to the indicator of post-treatment period and $IncomeRank_{i,t}$ corresponds to the intensity of treatment (from 0 to 1). An important concern with this specification, as in every other difference-in-differences design, is the possibility of differential pretrends. In other words, it is possible that the happiness-income gradient had been gradually increasing even before 2001, yielding $\alpha_2 > 0$, even if there was not a discontinuous change in this gradient around 2001. The following specification is a traditional way of addressing this concern, by allowing for differential trends:

$$(2) \qquad SWB_{i,t} = \alpha_1 \cdot IncomeRank_{i,t} + \alpha_2 \cdot IncomeRank_{i,t} \cdot I_t^{01-13}$$

$$+ \gamma \cdot IncomeRank_{i,t} \cdot (t - 1985) + X_{i,t}\beta + \delta_t + \epsilon_{i,t}.$$

In this specification, the coefficient α_1 corresponds to the happiness-income gradient in 1985. The coefficient γ corresponds to the linear trend for this gradient from 1985 to 2013. And the coefficient α_2 corresponds to the change in the happiness-income gradient around 2001, above and beyond the linear trend.

Another standard method to assess differential pre-trends is based on the following specification:

$$(3) \qquad SWB_{i,t} = \alpha_1 \cdot IncomeRank_{i,t} + \alpha_2 \cdot IncomeRank_{i,t} \cdot I_t^{01-13}$$

$$+ \alpha_3 \cdot IncomeRank_{i,t} \cdot I_t^{97-00} + X_{i,t}\beta + \delta_t + \epsilon_{i,t}.$$

Where I_t^{97-00} is a "fake" treatment indicator that occurs just before the actual change in disclosure: i.e., a dummy variable that equals 1 if $t \in [1997, 2000]$ and 0 otherwise. In this specification, α_1 corresponds to the happiness-income gradient from 1985–1996, whereas α_2 measures the change in that gradient from 1985–1996 to 2001–2013, and α_3 measures the change in the happiness-income gradient from 1985–1996 to 1997–2000. If the happiness-income gradient changed sharply around 2001, we would expect $\alpha_2 > 0$ and $\alpha_3 = 0$. We also present event-study graphs, which extend this specification by including further interactions with I_t^{89-92}, I_t^{93-96}, and so on.

If the happiness-income gradient increased in 2001, it is possible that this increase was caused by another significant change besides transparency that occurred in 2001 and persisted over the following 12 years. To the best of our knowledge, there was no major event around that time that could have had such a large effect on the happiness-income gradient. We use two additional identification strategies to address this concern.

The first strategy consists of a "placebo" analysis that reproduces the regressions for another country (i.e., Germany), for which there are similar survey data but no change of disclosure around 2001. If the effects in Norway are due to an event that also happened in Germany, such as the growth of information technology or the dot-com burst of 2001, then the results in Germany should be similar to the results in Norway.

The second strategy consists of a triple-differences specification. Ideally, we would construct a variable indicating the type of individuals who would be most exposed to the effects of online tax lists. This exposure variable would identify individuals who are likely to search for themselves, to be searched for by their social contacts, to be aware that their social contacts are searching for them, and so on. Then, we could test if the well-being effects are stronger for these individuals. Unfortunately, we cannot construct this ideal exposure variable, because our survey data do not contain information such as whether a respondent visited the tax list websites. Instead, we construct our exposure variable based on internet access data.

Let the dummy variable $HigherInternet_{i,t}$ take the value 1 if individual i's observable characteristics in year t, such as the age and education, predict above-median internet access at home.[22] Consider the following triple-differences specification:

$$(4) \qquad SWB_{i,t} = \alpha_1 \cdot IncomeRank_{i,t} + \alpha_2 \cdot IncomeRank_{i,t} \cdot I_t^{01-13}$$

$$+ \alpha_3 \cdot HigherInternet_{i,t} + \alpha_4 \cdot HigherInternet_{i,t} \cdot I_t^{01-13}$$

$$+ \alpha_5 \cdot IncomeRank_{i,t} \cdot HigherInternet_{i,t}$$

$$+ \alpha_6 \cdot IncomeRank_{i,t} \cdot HigherInternet_{i,t} \cdot I_t^{01-13}$$

$$+ X_{i,t}\beta + \delta_t + \epsilon_{i,t}.$$

The coefficient α_2 is interpreted as the effect of the policy on individuals with lower internet access, which we expect to be small or even zero. On the other hand, the parameter α_6 measures the differential effect of transparency for individuals with higher internet access, relative to individuals with lower internet access. Our main hypothesis is that $\alpha_6 > 0$: i.e., the change in disclosure had a greater effect on individuals with higher internet access.

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[22] We cannot base the triple-differences strategy on a dummy variable for whether the respondent has internet access. First of all, the question about internet access was not added until 1999. Most important, the share of individuals with internet access has increased dramatically in the sample period. For example, a small share of the population had internet access before 1996: according to the World Telecommunication Development Report, only 6.4 percent of Norwegians had internet access in 1995. As a result, even if we had data on internet access for that period, it would make little sense to estimate the happiness-income gradient for individuals with internet access.

B. *Survey Data*

We use data from the Norwegian Monitor Survey, which was a repeated cross-sectional survey conducted by the market research institute Ipsos MMI. The data were collected every other year in 1985–2013 through a self-completion questionnaire sent by mail to a representative sample of Norwegians. This dataset has been used to explore the relationship between well-being and age (Hellevik 2002), between well-being and values (Hellevik 2003), and between well-being and sustainability (Hellevik 2015).

The final sample used in our regression analysis comprises 48,570 observations collected in 15 different years, implying an average of 3,238 observations per survey year. This sample seems to be representative of the general population in some observable characteristics. For example, in the year 2011, 53.0 percent of respondents were women, the median age was 37, and the mean gross household income was $129,684 (in 2011 US dollars). In comparison, administrative data from Norway for that same year suggest a share of women of 50.5 percent, a median age of 39.1, and a mean gross household income of $152,890.[23]

The survey team did not collect information about the date when each survey was completed or mailed back, but they believe that the questionnaires were completed between late September and early December.[24] Recall that the tax agency releases the income data for the previous fiscal year in mid-October. In the weeks following the data release, traffic to the online tax lists is highest. Thus, a substantial share of the respondents may have completed the survey during a time when income transparency was most salient. Our estimated effects of income transparency thus may overestimate the effects of income transparency on an average day of the year. On the other hand, a significant share of survey responses for 2001 may have been collected before the change in disclosure took place, thus leading to an underestimation of the effects of disclosure during the year 2001.

We discuss below the definitions of the main variables.

Subjective Well-Being.—The main outcome of interest is subjective well-being. The Norwegian Monitor Survey includes questions about happiness and life satisfaction, which are the two most widely used measures of subjective well-being (Easterlin 2004, Kahneman and Deaton 2010). The happiness question is: "Will you mostly describe yourself as: Very happy; Quite happy; Not particularly happy; Not at all happy." The life satisfaction question is, "How satisfied are you with your life? Very satisfied; Somewhat Satisfied; Neither satisfied nor dissatisfied; Slightly dissatisfied; Very dissatisfied." Happiness and life satisfaction are known as evaluative measures of well-being, because answering them requires respondents to think about their lives in general.[25] It is well established that evaluative measures do not

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[23] The data sources are: Central Intelligence Agency's World Factbook for the female share and median age, and the Euromonitor's World Consumer Income and Expenditure Patterns for the mean household income.

[24] According to private communications with the administrators of the survey, the questionnaires were typically sent to the respondents in the third week of September (following a national or local election), and the vast majority of surveys are sent back before the second week of December.

[25] The alternative to evaluative measures are hedonic measures, which are assessed by asking about the presence of various emotions in the experience of yesterday (e.g., happiness, sadness, worry), and they often have different

vary over the days of the week, are significantly correlated with income, and remain correlated with income even at high levels of income (Kahneman and Deaton 2010). We use the happiness question in our baseline regressions because it was asked in all survey waves from 1985 to 2013, whereas life satisfaction was asked starting in 1999.[26]

In the baseline specification, instead of arbitrarily assigning values 1, 2, 3, or 4 to the four possible answers to the happiness question, we employ the Probit-OLS method to assign these values (van Praag and Ferrer-i-Carbonell 2008). By construction, a higher value denotes higher happiness. We use this method with all subjective questions, including life satisfaction. Moreover, to facilitate the interpretation of the regression coefficients, we standardize all subjective outcomes to a mean of 0 and standard deviation of 1. Table 1 summarizes the definitions for happiness, life satisfaction, and all other main variables in the analysis. Table 2 provides the corresponding descriptive statistics.

Although subjective well-being measures have some well-documented limitations, a growing body of evidence indicates that they contain significant information about the individual's true well-being. Subjective well-being is positively correlated to objective measures of well-being, such as emotional expressions (Sandvik, Diener, and Seidlitz 1993), aggregate suicide rates (Di Tella, MacCulloch, and Oswald 2003), and activity in the pleasure centers of the brain (Urry et al. 2004). Subjective well-being also positively correlates with decision utility. For instance, Benjamin et al. (2012) conducted a survey in which subjects were shown pairs of hypothetical scenarios with trade-offs between two aspects (e.g., higher income versus longer workdays). They showed that, despite some deviations, most respondents choose a scenario that maximizes life satisfaction (see also Benjamin et al. 2014). Similarly, Perez-Truglia (2015) showed that the expenditure choices predicted by life satisfaction data are largely consistent with the actual expenditure behavior of the same individuals.

Income Rank.—The variable *Income Rank* is the position of the respondent in the distribution of household income for the current year.[27] As is typical in household surveys, respondents were asked about their annual gross household income using bins.[28] This question provides no information to rank households within a particular income bin and year. To ameliorate this measurement error, we follow the standard imputation method from the literature (e.g., Stevenson and Wolfers 2008, Kahneman and Deaton 2010), using information on other household characteristics

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correlates than evaluative measures (Deaton and Stone 2013). Unfortunately, our survey data do not include hedonic measures.

[26] For histograms of the responses to the *Happiness* and *Life Satisfaction* questions, see online Appendix Figure A.2.

[27] While this measure is based on the rank in the national income distribution, individuals probably care the most about the comparison to narrower reference groups such as their relatives, friends, neighbors, and coworkers (Clark and Senik 2010). We cannot construct measures of *Income Rank* based on these more specific reference groups because we do not know who the respondent's relatives, friends, or other social contacts are. Due to this source of measurement error, our results may underestimate the importance of income comparisons. Similarly, since the tax records disclosed the wealth of individuals, it is likely that individuals also engaged in wealth comparisons. This is another source of measurement error that may lead us to underestimate the overall importance of (income and wealth) comparisons.

[28] We drop 6 percent of the sample corresponding to individuals who did not respond the income question.

TABLE 1—DATA DEFINITIONS FOR THE MAIN VARIABLES FROM THE NORWEGIAN MONITOR SURVEY

Variable name	Definition
Happiness	Based on the following question: "Will you mostly describe yourself as: Very happy; Quite happy; Not particularly happy; Not at all happy." These four categories were assigned values using the Probit-OLS method, and then the variable was standardized to have mean 0 and standard deviation 1. Higher values denote higher happiness.
Income Rank	Estimated position in the national distribution of household gross income in a given year, from 0 (lowest income in the country) to 1 (highest). This rank is based on the following question: "What would you estimate the household's total gross income? That is, all total income before taxes and deductions: Less than NKR100,000; NKR100,000–199,000; NKR200,000–299,000; NKR300,000–399,000; NKR400,000–499,000; NKR500,000–599,000; NKR600,000–799,000; NKR800,000–999,000; Above NKR1,000,000." Before 1995, the bottom bin was split in two bins (0K–59K and 60K–100K). In 1995, they merged those two bins and added the seventh and eighth bins. They added the ninth bin in 1999. The within-bin ranks are imputed using interval regressions as in Stevenson and Wolfers (2008) and Kahneman and Deaton (2010).
Life Satisfaction	Based on the following question: "How satisfied are you with your life? Very satisfied; Somewhat satisfied; Neither satisfied nor dissatisfied; Slightly dissatisfied; Very dissatisfied." These five categories were assigned values using the Probit-OLS method, and then the variable was standardized to have mean 0 and standard deviation 1. Higher values denote higher satisfaction.
Perceived Rank	Based on question: "In comparison to other Norwegians, would you say that your economic situation is…? Much worse than average; Slightly worse than average; Average; Slightly better than average; Much better than average." These five categories were assigned values using the Probit-OLS method, and then the variable was standardized to have mean 0 and standard deviation 1. Higher values denote higher rank.
Income Adequacy	Based on the following question: "How do you feel about your economic situation? Do you really need more money than you have to be able to live a satisfying life, do you manage with your current income, or would you be able to cope with less if you had to?" The possible answers were "I need more money," "I manage with what I have," and "I could cope with less," which were assigned values using the Probit-OLS method, and then the variable was standardized to have mean 0 and standard deviation 1. Higher values denote higher income adequacy.
Internet Access	Dummy variable that takes the value 1 if the individual responds affirmatively to the following question: "Do you have Internet access at home?"
I{Higher Internet}	Dummy variable that takes the value 1 if the respondent has above-median predicted internet access in the current year. We estimated an OLS regression of internet access on age, age squared, and dummy variables for gender, marital status, education, household size, and number of workers in the household using the responses from 2001 (the results from this regression are presented in online Appendix Table A.8). Then, for each sample year, we used the estimated coefficients to generate predicted internet access and then split the observations for that year by the corresponding median value.

that correlate with income (e.g., education, age, county) to break ties within income bins.[29]

Higher Internet.—The goal of this variable is to split individuals by whether their observable characteristics are associated with higher internet access (once the internet becomes available). We base this exercise on the dummy variable *Internet Access*,

[29] The first step of this procedure consists in estimating, for each year, an interval regression of the logarithm of income on dummies for gender, education, marital status, age, number of household members, and county. The second step consists of using the estimated parameters to predict the logarithm of income for each individual, conditional on belonging to the reported income bracket. We can then construct *Income Rank* by ranking individuals based on their predicted household income.

TABLE 2—DESCRIPTIVE STATISTICS FOR THE MAIN VARIABLES
FROM THE NORWEGIAN MONITOR SURVEY

Variable name	Availability	Observations	Mean (standard deviation)
Happiness	1985–2013	48,570	0.00 (1.00)
Income Rank	1985–2013	48,570	0.50 (0.29)
Life Satisfaction	1999–2013	29,655	0.00 (1.00)
Perceived Rank	1993–2013	38,938	0.00 (1.00)
Income Adequacy	1993–2013	38,950	0.00 (1.00)
Internet Access	1999–2013	29,875	0.76 (0.43)
I{Higher Internet}	1985–2013	48,570	0.50 (0.50)

Notes: See Table 1 for a summary of data definitions for all of the variables listed above. Data from the Norwegian Monitor Survey.

which equals 1 if the individual has internet access at home and 0 otherwise. Using survey responses for 2001, we estimate an ordinary least squares (OLS) regression of *Internet Access* on a series of observable characteristics: age, age squared, and dummy variables for gender, education, marital status, household size, and number of working household members. Online Appendix Table A.8 reports the results from this auxiliary regression. The coefficients suggest that individuals with higher internet access are, on average, more likely to be male, educated, and young, and their households are likely to be larger with more working members. These correlations are largely consistent with the correlations reported in other studies of internet access and internet use in developed countries (File and Ryan 2014). We use the estimated coefficients to predict *Internet Access* for the entire survey sample. The dummy variable *I{Higher Internet}* equals 1 if the individual's own predicted internet access exceeds the median for the current year.[30]

Perceived Income Rank and Income Adequacy.—Starting in 1993, the survey included a subjective question about self-perceived income rank: "In comparison to other Norwegians, would you say that your economic situation is …? Much worse than average; Slightly worse than average; Average; Slightly better than average; Much better than average." We construct the variable *Perceived Rank* using responses to this question, which are coded with the Probit-OLS method and then standardized. By definition, higher values of this variable denote a higher perceived rank. For an additional test of the self-perceptions channel, we use data on another question that was added to the survey in 1993: "How do you feel about your economic situation? Do you really need more money than you have to be able to live a satisfying life, do you manage with your current income, or would you be able to cope with less if you had to?" The possible answers are "I need more money," "I manage with what I have," and "I could cope with less." We construct the variable *Income Adequacy* using the Probit-OLS method and then standardize it so that its

[30] This definition guarantees that the distribution of *I{Higher Internet}* will be stable over time: i.e., in any given year, one-half of the sample has *I{Higher Internet}* = 1 and the other half has *I{Higher Internet}* = 0.

mean is 0 and standard deviation is 1. Higher values of this variable indicate that one's income is more adequate.[31]

Control Variables.—We include a standard set of control variables used in studies of subjective well-being: age, age squared, and dummies for gender, education, marital status, total number of household members, and number of working household members.

German Data.—For the placebo test, we employ data from the German Socio-Economic Panel survey, collected every year from 1985 to 2013.[32] The data do not include a question on happiness but do include a question on life satisfaction: "How satisfied are you with your life, all things considered?" Responses are measured on a 11-point scale ranging from "Completely dissatisfied" (0) to "Completely satisfied" (10). We code and standardize this outcome using the same method as for the Norwegian data. Moreover, we reproduce the same regression specification for Germany, including all the same control variables and the same procedure to create $I\{Higher\ Internet\}$.[33] The final number of observations in Germany (108,209) is more than twice that of the Norwegian Monitor Survey (48,570).

III. Results

A. *Effects on the Happiness-Income Gradient*

Table 3 explores the effects of the change of disclosure on the happiness-income gradient. The dependent variable in column 1 is *Happiness*. This column uses the simplest specification from equation (1). The estimated coefficient on *Income Rank* (0.311) is positive, precisely estimated, and statistically significant (p-value < 0.001). This coefficient implies that during 1985–2000, going from the lowest to the highest income rank in Norway was associated with an increase in happiness of 0.311 standard deviations. This happiness-income gradient is in the same order of magnitude as the corresponding gradients reported in other studies.[34]

Column 1 of Table 3 also reports the coefficient on the interaction between *Income Rank* and $I\{2001–2013\}$. This estimated coefficient (0.090) is positive,

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[31] For histograms of the responses to the *Perceived Income Rank* and *Income Adequacy* questions, see online Appendix Figure A.2.

[32] To maximize power, we use all the years available in the German data: online Appendix Section A.1 shows that the results are robust if we focus on responses on odd-numbered years, like in the Norwegian survey.

[33] There are a couple of differences between the German and Norwegian surveys. In Germany, we have to include dummies for years of education instead of the dummies for levels of educational attainment used in Norway. And while in Norway we use *Internet Access* for the year 2001, that question was not included in Germany in 2001 so we have to use the responses for the year 2002 instead. Last, we restrict the German data to household heads in West Germany.

[34] For example, results reported in Table 2 from Stevenson and Wolfers (2008) suggest that, using data for a number of countries from the World Values Survey, the ordered probit regression of happiness on the logarithm of household income yields a coefficient of 0.244 (SE 0.008). We can provide a direct comparison by estimating the same regression with our Norwegian data, which yields a coefficient in the same order of magnitude (0.307; SE 0.008). This gradient for Norway is higher than, and statistically different from, the corresponding gradient from Stevenson and Wolfers (2008). However, we would not expect them to be exactly equal: there is no reason to believe that Norway should be representative of the world average; additionally, these differences may be due to differences in how income and subjective well-being are measured in the two datasets.

TABLE 3—EFFECTS ON THE GRADIENT BETWEEN SUBJECTIVE WELL-BEING AND INCOME RANK

	Happiness (1)	Happiness (2)	Happiness (3)	Happiness (4)	Life Satisfaction (5)	Life Satisfaction (6)	Life Satisfaction (7)	Life Satisfaction (8)
Income Rank	0.311 (0.028)	0.315 (0.040)	0.310 (0.032)	0.331 (0.040)	0.585 (0.056)	0.526 (0.085)	0.539 (0.018)	0.646 (0.025)
Income Rank $\times I\{2001\text{--}2013\}^{(i)}$	0.090 (0.032)	0.098 (0.059)	0.090 (0.037)	−0.004 (0.051)	0.122 (0.055)	0.050 (0.088)	0.018 (0.021)	−0.049 (0.035)
Income Rank $\times I\{2001\text{--}2013\}$ $\times I\{Higher\ Internet\}$				0.217 (0.073)		0.169 (0.131)		−0.011 (0.046)
Income Rank $\times (Year - 1985)$		−0.001 (0.004)						
Income Rank $\times I\{1997\text{--}2000\}^{(ii)}$			0.001 (0.048)					
p-value (i) = (ii)			0.043					
Country	Norway	Norway	Norway	Norway	Norway	Norway	Germany	Germany
Period	1985–2013	1985–2013	1985–2013	1985–2013	1999–2013	1999–2013	1985–2013	1985–2013
Observations	48,570	48,570	48,570	48,570	29,655	29,655	108,209	108,209

Notes: Heteroskedasticity-robust standard errors in parentheses. Each column corresponds to a separate OLS regression. *Happiness* and *Life Satisfaction* are subjective well-being measures normalized to have mean 0 and standard deviation of 1, with higher values denoting higher happiness/satisfaction. *Income Rank* denotes the position of the respondent's household relative to all the other respondents for that year, from 0 to 1. $I\{2001\text{--}2013\}$ takes the value 1 for 2001–2013. $I\{1999\text{--}2000\}$ takes the value 1 for 1997–2000. $I\{Higher\ Internet\}$ is a dummy variable that takes the value 1 if the respondent's predicted *Internet Access* is above the median value for a given year. All regressions control for year dummies, age, age squared, a gender dummy, three education dummies, four dummies for marital status, four dummies for household size, and three dummies for number of working household members. Column 4 also controls for all the additional interactive terms listed in equation (4). Columns 1 through 6 are based on data from the Norwegian Monitor Survey, collected every other year in 1985–2013. See Table 1 for a summary of data definitions and Table 2 for descriptive statistics. Columns 7 and 8 are based on the same specifications used in columns 1 and 3, only that using data from the German Socio-Economic Panel, collected every year in 1985–2013.

large, and statistically significant (p-value = 0.005). These findings suggest that the happiness-income gradient increased substantially (by 29 percent, from 0.311 to 0.401) from 1985–2000 to 2001–2013.

The first concern is that the coefficient on the interaction of *Income Rank* with $I\{2001\text{--}2013\}$ may not correspond to the 2001 change in disclosure, but instead results from a gradual change in this gradient that started years before 2001. To address this concern, column 2 of Table 3 shows results for the specification corresponding to equation (2), which includes the interaction between *Income Rank* and the time trend. The coefficient on the interaction between *Income Rank* and the time trend (−0.001) is close to zero and statistically insignificant (p-value = 0.877), whereas the coefficient on the interaction of *Income Rank* with $I\{2001\text{--}2013\}$ (0.098) remains positive and statistically significant (p-value = 0.099). Indeed, we cannot reject the null hypothesis that the latter coefficient (0.098) equals the corresponding coefficient of 0.090 from column 1 (p-value = 0.877).[35]

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[35] This is an equality test between two coefficients based on the same data but different regressions. To allow for a nonzero covariance between these two coefficients, we estimate a system of seemingly unrelated regressions. In the remainder of the paper, when comparing coefficients from the same data but different regressions, we always use this method.

In turn, column 3 of Table 3 presents results from the specification corresponding to equation (3), which introduces the interactions of *Income Rank* with $I\{2001\text{–}2013\}$ and $I\{1997\text{–}2000\}$ simultaneously. The coefficient on the interaction of *Income Rank* with $I\{2001\text{–}2013\}$ (0.090) reported in column 3 is statistically significant (p-value $= 0.015$) and identical in magnitude to the corresponding coefficient from column 1 (0.090). On the contrary, the coefficient on the interaction of *Income Rank* with $I\{1997\text{–}2000\}$ is close to zero (0.001) and statistically insignificant (p-value $= 0.975$). Furthermore, in column 3 the coefficients on the interactions with $I\{1997\text{–}2000\}$ (0.001) and $I\{2001\text{–}2013\}$ (0.090) are statistically different from each other (p-value $= 0.043$).

Panel A of Figure 6 takes the last specification a step further by means of an event-study analysis. This figure shows the evolution of the happiness-income gradient over the entire 1985–2013 period. Each coefficient denotes the change in the happiness-income gradient relative to 1997–2000. Thus, the coefficient on 1997–2000 is normalized to zero.

To attribute the coefficient on the interaction between *Income Rank* and $I\{2001\text{–}2013\}$ to the effect of the disclosure policy, the happiness-income gradient should be stable during 1985–2000 and then increase after 2001. This is exactly the pattern shown in panel A of Figure 6. All pre-treatment coefficients are close to zero and statistically insignificant (p-values of 0.749, 0.737, and 0.612), suggesting that the happiness-income gradient remained constant during 1985–2000. And the three coefficients on the post-treatment period are positive and mostly statistically significant (p-values of 0.057, 0.024, and 0.311), suggesting that the happiness-income gradient increased after 2001 and remained high.

One challenge in interpreting these findings is that the effects may result from something other than the transparency change in 2001. To the best of our knowledge, there were no events that could explain these patterns, such as major changes to the income tax schedule or welfare benefits. To address this concern more directly, we use the triple-differences design corresponding to equation (4).

The dummy variable $I\{Higher\ Internet\}$ equals 1 if the individual's characteristics, such as being younger and more educated, predict higher internet access. For short, we refer to individuals with $I\{Higher\ Internet\} = 1$ as individuals with higher internet access. Our triple-difference strategy is then based on the assumption that the individuals with higher internet access, such as younger and more educated individuals, are the same types of individuals who are more exposed to the effects of the online tax lists.

Several arguments support this assumption. The websites require internet access, thus individuals with higher internet access and internet use are more likely to use the online tax lists, more aware that they exist, and more aware that their social contacts may be searching for them. Due to homophily, individuals with higher internet access have social contacts who also have higher internet access, and thus are more likely to be searched in the tax lists by their social contacts. Beyond internet access itself, individuals with higher internet access may be the type of individuals who care more about income comparisons. For example, Clark and Senik (2010) shows that individuals with higher internet access report that income comparisons are more important to them. As final evidence consistent with our assumption, the individual characteristics associated with higher internet access in our data (disproportionately
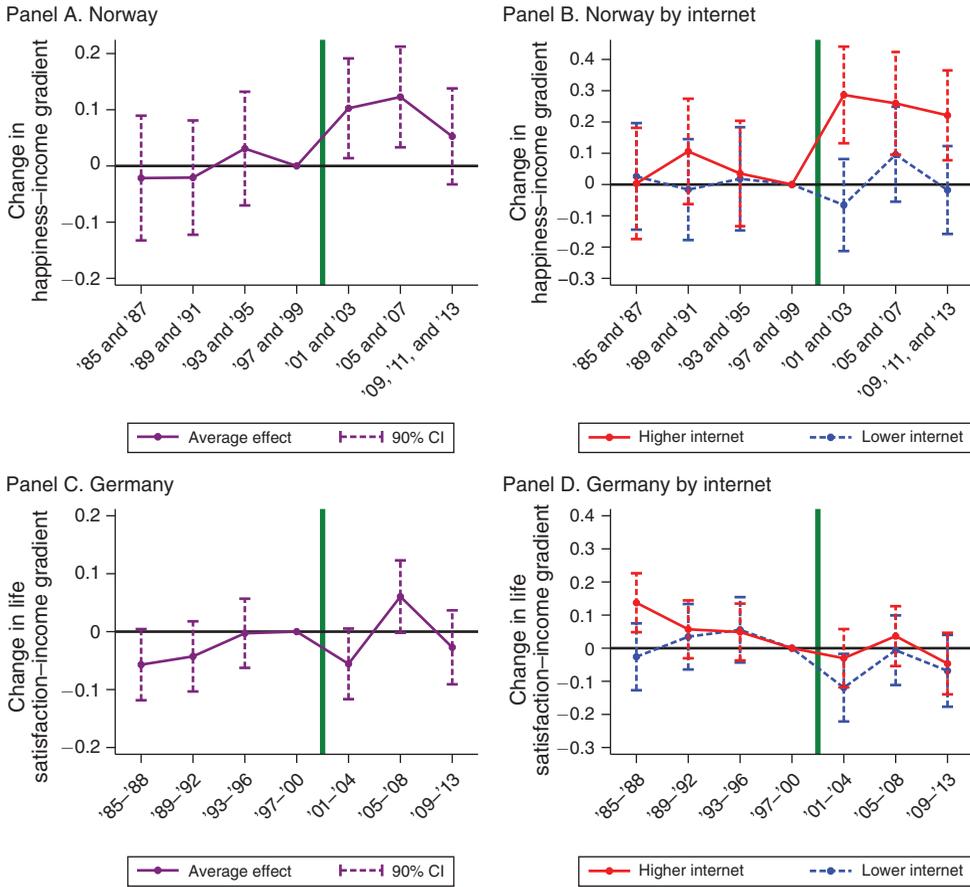
FIGURE 6. EVENT-STUDY ANALYSIS OF THE GRADIENT BETWEEN SUBJECTIVE WELL-BEING AND INCOME RANK

Notes: Each coefficient corresponds to the change in the gradient between subjective well-being and *Income Rank* relative to the period 1997–2000 (by construction, the coefficient for these years is normalized to 0). The green vertical line represents the change in disclosure that took place in Norway in 2001. The measure of subjective well-being in Norway is *Happiness* (panels A and B) and *Life Satisfaction* in Germany (panels C and D). Both of these outcomes are normalized to have mean 0 and standard deviation of 1, with higher values denoting higher happiness/satisfaction. *Income Rank* denotes the respondent's household rank for that year, from 0 to 1. All regressions control for year dummies, age, age squared, a gender dummy, three education dummies, four dummies for marital status, four dummies for household size, and three dummies for number of working household members. Panels A and B are based on 48,570 observations from the Norwegian Monitor Survey, collected every other year in 1985–2013: see Table 2 for more detailed data definitions and Table 3 for descriptive statistics. Panels C and D are based on 108,209 observations from the German Socio-Economic Panel Survey, collected every year in 1985–2013. Panel B is similar to panel A, only that two regressions are estimated separately for individuals with $I\{Higher\ Internet\} = 1$ and $I\{Higher\ Internet\} = 0$. Likewise, panel D is similar to panel C, only that based on two separate regressions by $I\{Higher\ Internet\}$.

younger, more educated, and male) also are associated with self-reported use of the online tax lists (Skattebetaleren 2008).

Column 4 of Table 3 reports the results from the triple-differences specification. The evidence indicates that, consistent with the hypothesis that the change in transparency caused the effects, changes in the happiness-income gradient were concentrated entirely among individuals with higher internet access. The coefficient on

the interaction between *Income Rank* and *I*{2001–2013} is close to zero (−0.004), statistically insignificant, and precisely estimated. This coefficient suggests that the happiness-income gradient did not change in 2001 for individuals with lower internet access. The coefficient on the triple interaction between *Income Rank*, *I*{2001–2013}, and *I*{*Higher Internet*} (0.217) is positive, large, and statistically significant (p-value = 0.003). This coefficient indicates that the happiness-income gradient increased substantially for individuals with higher internet access.[36]

Panel B of Figure 6 provides the event-study equivalent of the triple-differences identification strategy. The results show that, for individuals with lower internet access, the happiness-income gradient was stable prior to 2001 and remained the same after 2001. For individuals with higher internet access, the happiness-income gradient was stable prior to 2001, increased after 2001, and remained at a higher level for the subsequent 12 years.

As discussed in Section IB, during the 12 years following the change of disclosure, some factors may have increased or decreased the degree of income transparency. Due to the precision of the estimates, we cannot rule out ups and downs in the effects of the policy during the twelve years. However, based on panel B of Figure 6, the best guess is that the effects of transparency were stable: we cannot reject the null hypothesis that the three post-treatment coefficients (0.286, 0.260, and 0.221) are equal (p-value = 0.757).[37]

As an additional robustness check, we compare the effects on happiness with the effects on life satisfaction. Both outcomes are evaluative measures of well-being, and they are normally found to be significantly correlated to income. Furthermore, despite conceptual differences, happiness and life satisfaction often are treated as interchangeable in the literature on subjective well-being (Easterlin 2004). We thus expect similar effects of transparency across these two outcomes.

Columns 5 and 6 of Table 3 show results using *Life Satisfaction* as the dependent variable instead of *Happiness*. We are cautious when interpreting this evidence, however. Whereas *Happiness* has been available since 1983, the *Life Satisfaction* question was not added to the survey until 1999 and thus has only one year of pre-treatment data. Consequently, the standard errors for the *Life Satisfaction* regressions are almost twice as large as those for the *Happiness* regressions.

Column 5 of Table 3 presents the results for *Life Satisfaction* under the baseline specification from equation (1). The coefficient on *Income Rank* (0.585) is large and statistically significant (p-value < 0.001). Indeed, this gradient is larger than the corresponding gradient for *Happiness* (0.311, from column 1). This difference in gradients is not unreasonable, given that the two questions are supposed to measure somewhat different aspects of well-being and even use different scales. Most important, column 5 of Table 3 shows that the coefficient on the interaction between *Income Rank* and *I*{2001–2013} (0.122) is positive and statistically significant (p-value = 0.026). These estimates imply that higher income transparency increased the life satisfaction–income gradient by 21 percent, from 0.585 to 0.707.

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[36] According to column 4 of Table 3, the average effect of the income disclosure on the happiness-income gradient is 0.104 ($= -0.004 + 0.5 \cdot 0.217$) and statistically significant (p-value = 0.005). As expected, this average effect is close to the average effect reported in the baseline specification (0.090, from column 1 of Table 3).

[37] See online Appendix Section A.1 for a more detailed discussion, including a more disaggregated event-study graph.

Moreover, we cannot reject the null hypothesis that the 21 percent increase in the life satisfaction–income gradient equals the corresponding 29 percent increase in the happiness–income gradient (p-value $= 0.645$).

For the sake of completeness, column 6 reports the triple-differences specification for the *Life Satisfaction* outcome. The key coefficient on the triple interaction between *Income Rank*, *I*{2001–2013}, and *I*{*Higher Internet*} (0.168) is positive, large, and statistically indistinguishable from the corresponding *Happiness* coefficient (0.217, from column 4). However, due to the lower precision of the *Life Satisfaction* results, the coefficient is statistically insignificant (p-value $= 0.195$).

The next robustness check involves estimating the same regressions in a placebo country, Germany, which was not exposed to the Norwegian change in income transparency of 2001. A similar change in the happiness-income gradient in Germany around 2001 would imply that the effects for Norway must be explained by a factor different than the change in disclosure. In turn, finding no effects in Germany would rule out any shocks that began in 2001 and were common between Norway and Germany, such as the dot-com burst or the growth of information technology.

Columns 7 and 8 of Table 3 report the results for Germany, using *Life Satisfaction* as the dependent variable. Column 7 reports the results under the basic specification from equation (1). The results indicate that, prior to 2001, the gradient between income and life satisfaction was similar between Germany and Norway: the coefficient on *Income Rank* (0.539, from column 7) is statistically significant (p-value < 0.001) and in the same order of magnitude as the corresponding coefficient for Norway (0.585, from column 5).[38] Most important, column 7 indicates that, unlike in Norway, there was no significant change in the life satisfaction-income gradient in Germany around 2001. The coefficient on the interaction of *Income Rank* and *I*{2001–2013} (0.018) is close to zero, statistically insignificant, and precisely estimated. We can reject the null hypothesis that this coefficient for Germany (0.018, from column 7) equals the corresponding coefficient estimated in Norway (0.090, from column 1), with a p-value of 0.030.[39] These findings are consistent with panel C of Figure 6, which reproduces the event-study analysis for Germany (equivalent to panel A for Norway). In Norway, the happiness-income gradient was stable prior to 2001, then increased, and remained at the higher level. On the contrary, the life satisfaction-income gradient in Germany was stable prior to 2001 and remained at the same level after 2001.

Column 8 of Table 3 reports the results for Germany under the triple-differences specification. The coefficient on the triple interaction between *Income Rank*, *I*{2001–2013}, and *I*{*Higher Internet*} (-0.011) is close to zero, statistically insignificant, and precisely estimated. This coefficient indicates that in Germany, there was no differential change in the life satisfaction-income gradient between individuals with lower versus higher internet access. Indeed, we can confidently reject the null hypothesis that the coefficient for Germany (-0.011, from column 8) equals the corresponding coefficient for Norway (0.217, from column 4), with a p-value of

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[38] Modest differences should be expected because these are two different countries and also because the life satisfaction and income questions are elicited in different ways.

[39] We can also reject the null hypothesis that the coefficient from Germany (0.018, from column 7) is equal to the corresponding coefficient reported in column 5 (0.122) for Norway (p-value $= 0.090$).

0.009. These results are consistent with those in panel D of Figure 6, which reproduces the event-study analysis for Germany by internet access (equivalent to panel B, for Norway). As in Norway, the gradient in Germany between well-being and *Income Rank* was stable prior to 2001 for individuals with higher and lower internet access. In Norway, these two gradients diverged after 2001, whereas in Germany they continued to be similar after 2001.

Last, these results do not address the effect of higher transparency on the average level of well-being. The happiness-income gradient may have increased because richer individuals became happier, because poorer individuals became unhappier, or a combination of the two. Online Appendix Section A.2 presents some estimates of the average effects on well-being with a difference-in-differences estimator using the exposure indicator based on internet access. The results suggest that the change in disclosure did not have a significant effect on average happiness and life satisfaction. These findings suggest that the disclosure policy resulted in a transference of well-being from poorer to richer individuals. Also, this view that the change of disclosure created roughly as many winners as losers is consistent with data from the 2007 Synovate survey indicating that about one-half of the Norwegian population (46 percent) opposed the income transparency policy (Langset 2011).[40]

B. *The Self-Perceptions Channel*

We cannot measure all possible channels that could explain the effects of transparency on well-being, but we provide some suggestive evidence of the role of self-perceptions.

There is abundant evidence that individuals perceive themselves to be closer than they are to the middle of income distribution (Cruces, Perez-Truglia, and Tetaz 2013).[41] This middle-class bias is believed to arise due to assortativity neglect: rich people look around and see other rich people, so they incorrectly conclude that they are middle class; likewise, poor individuals see other poor people around them and believe that they are middle class.[42] Unfortunately, the question on perceived income rank included in the Norwegian Monitor Survey uses a subjective scale, so we cannot measure the middle-class bias directly, as in Cruces, Perez-Truglia, and Tetaz (2013). However, we observe suggestive signs of this bias: most respondents (89.7 percent) believe that their incomes are slightly above, slightly below, or about average, and this tendency is true even for individuals in the tails of the income distribution.[43]

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[40] This share was still similar (49 percent) when measured again in 2011 (*Sunnmørsposten* 2011).

[41] Cruces, Perez-Truglia, and Tetaz (2013) first documented this bias using data from Argentina. Since then, other studies have documented this same middle-class bias in other countries: see, for example, Poppitz (2016) and Bublitz (2017).

[42] Indeed, Frick, Iijima, and Ishii (2018) shows that, under some assumptions about payoffs and the information structure, this type of "assortativity neglect" is not only one possible equilibrium, but the unique equilibrium.

[43] Only 16 percent of households from the top income decile report to be much better than average, and only 25 percent of households in the bottom income decile report to be much worse than average. However, there are several reasons why this misalignment may not be attributed to misperceptions. In the extreme case, if individuals interpret the range from "slightly below average" to "slightly above average" as between −90 percent and 1,000 percent of the average, then almost everyone would be right to pick those categories. Additionally, part of the misalignment may reflect measurement error in the actual income rank.

Under a middle-class bias, a higher income transparency would increase the gradient between perceived income rank and actual income rank. That is, poorer individuals would realize that they are poorer than they thought, and richer individuals would realize that they are richer than they thought. We test this hypothesis using the same regression specification from before but with *Perceived Rank* as the dependent variable instead of *Happiness*. Columns 1 through 4 of Table 4 present the regressions with this dependent variable. These results are based on smaller time frames and sample sizes, because *Happiness* has been collected since 1985, but *Perceived Rank* has been measured only since 1993.

Column 1 of Table 4 presents the results for the simplest specification from equation (1). The coefficient on *Income Rank* (2.172) is positive, large, and statistically significant (p-value < 0.001). This coefficient implies that, during 1993–2001, moving from the poorest to the richest household was associated with an increase of 2.172 standard deviations in perceived income rank. This strong gradient between perceived and actual income ranks suggests that self-perceptions of income rank were at least somewhat accurate. Most important, column 1 shows that the coefficient on the interaction between *Income Rank* and $I\{2001–2013\}$ (0.185) is positive, large, and statistically significant (p-value < 0.001). These results imply that higher income transparency increased the gradient between perceived income rank and actual income rank by 8.5 percent (from 2.172 to 2.357). The fact that perceptions became more correlated to reality suggests that perceptions became more accurate.

Column 2–4 of Table 4 assesses the robustness of the results with the other specifications. Column 2 adds the interaction between the time trend and *Income Rank*. In this alternative specification, the coefficient on the interaction between *Income Rank* and $I\{2001–2013\}$ (0.135) is still positive, large, and statistically significant (p-value $= 0.015$). Column 3, which adds the fake treatment interaction, also suggests that the results are robust: the coefficient on the interaction between *Income Rank* and $I\{1997–2000\}$ is statistically insignificant and statistically different from the coefficient on the interaction between *Income Rank* and $I\{2001–2013\}$ (p-value < 0.001). The results reported in column 4, corresponding to the triple-differences specification, are less robust. The triple interaction between *Income Rank*, $I\{2001–2013\}$, and $I\{Higher\ Internet\}$ (0.092) is statistically insignificant (p-value $= 0.228$). However, because the coefficient is not precisely estimated, we cannot rule out that this coefficient equals the 0.185 coefficient from the baseline specification reported in column 1 (p-value $= 0.267$).[44]

To assess the self-perceptions mechanism further, we measure the effects on *Income Adequacy*. Individuals may form their income aspirations by looking at the incomes of others. Richer individuals, who found out that they were richer than they thought, may have felt that their incomes were more adequate; poorer individuals, who learned that they were poorer than they thought, may have felt that their incomes were less adequate. To test this hypothesis, columns 5–8 present results with *Income Adequacy* as the dependent variable. Column 5 corresponds to the simplest regression specification. The coefficient on *Income Rank* (1.290) is positive, large, and statistically significant (p-value < 0.001). This coefficient

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[44] For the sake of completeness, online Appendix Section A.1 presents the event-study graphs for *Perceived Rank* and *Income Adequacy*.

Table 4—Effects on the Additional Outcomes: Perceived Income Rank and Adequacy of Own Income

	Perc Rank (1)	Perc Rank (2)	Perc Rank (3)	Perc Rank (4)	Income Adequacy (5)	Income Adequacy (6)	Income Adequacy (7)	Income Adequacy (8)
Income Rank	2.172	2.117	2.130	2.275	1.290	1.326	1.249	1.300
	(0.032)	(0.060)	(0.047)	(0.048)	(0.035)	(0.065)	(0.050)	(0.049)
Income Rank $\times$ I{2001–2013}[i]	0.185	0.135	0.228	0.138	0.061	0.094	0.101	−0.013
	(0.033)	(0.056)	(0.047)	(0.053)	(0.035)	(0.060)	(0.050)	(0.054)
Income Rank $\times$ I{2001–2013} $\times$ I{Higher Internet}				0.092				0.101
				(0.077)				(0.083)
Income Rank $\times$ (Year − 1985)		0.005				−0.003		
		(0.004)				(0.005)		
Income Rank $\times$ I{1997–2000}[ii]			0.069				0.066	
			(0.055)				(0.059)	
p-value (i) = (ii)			<0.001				0.396	
Country	Norway	Norway	Norway	Norway	Norway	Norway	Norway	Norway
Period	93–13	93–13	93–13	93–13	93–13	93–13	93–13	93–13
Observations	38,938	38,938	38,938	38,938	38,950	38,950	38,950	38,950

Notes: Heteroskedasticity-robust standard errors in parentheses. Each column corresponds to a separate OLS regression *Perceived Rank* and *Income Adequacy* are subjective measures normalized to have mean 0 and standard deviation of 1, with higher values denoting higher rank/adequacy. *Income Rank* denotes the rank of the household income for that year, from 0 to 1. I{2001–2013} takes the value 1 for 2001–2013. I{1999–2000} takes the value 1 for 1997–2000. I{Higher Internet} is a dummy variable that takes the value 1 if the respondent's predicted *Internet Access* is above the median value for a given year. All regressions control for year dummies, age, age squared, a gender dummy, three education dummies, four dummies for marital status, four dummies for household size, and three dummies for number of working household members. Columns 4 and 8 also control for all the additional interactive terms listed in equation (4). All regressions are based on data from the Norwegian Monitor Survey, collected every other year in 1985–2013. See Table 1 for a summary of data definitions and Table 2 for descriptive statistics.

suggests that moving from the poorest to the richest household is associated with an increase of 1.290 standard deviations in the adequacy of own income. Column 5 also suggests that the higher transparency increased the gradient between *Income Adequacy* and income rank: the coefficient on the interaction between *Income Rank* and I{2001–2013} (0.061) is positive and statistically significant (p-value = 0.083). These estimates suggest that the higher transparency increased the gradient between income adequacy and income rank by 4.7 percent (from 1.290 to 1.351). Indeed, this 4.7 percent effect is statistically indistinguishable from the 8.5 percent increase in the gradient between perceived and actual income ranks (p-value = 0.149).

Columns 6–8 of Table 4 assess the robustness of the results with the other specifications. In column 6, which introduces the interaction with the linear trend, the coefficient on the interaction between *Income Rank* and I{2001–2013} is even larger (0.094) than in the baseline specification of column 5. However, due to the loss in precision, this coefficient is statistically insignificant (p-value = 0.120). The results from column 7, which includes the fake interaction term, are mixed. On the one hand, the coefficient on the interaction between *Income Rank* and I{2001–2013} becomes larger (0.101) and more statistically significant (p-value = 0.044). On the other hand, we cannot reject the null hypothesis that this coefficient equals the coefficient on the fake interaction (p-value = 0.396). Last, the results from column 8 also are mixed: the coefficient on the interaction between *Income Rank*, I{2001–2013}, and I{Higher Internet} (0.101) suggests effects that

are larger than in the baseline specification (0.061, from column 5), but due to the lack of precision, this coefficient is statistically insignificant (p-value $= 0.225$).

The effects on well-being coincided with the effects on perceived relative income and adequacy of own income. However, this finding does not constitute definitive proof of the self-perceptions channel. Indeed, even if no effects on perceived relative income had been found, the self-perceptions channel would have not been ruled out. For example, transparency may affect well-being by making self-perceptions more salient, or it may operate through other self-perceptions such as whether individuals believe that others perceive them as rich. However, the evidence from this section suggests that some effects of transparency on well-being operate through changes in self-perceptions. The change in the perceived rank and income adequacy gradients (8.5 percent and 4.7 percent) are smaller in magnitude than the changes in the happiness and life satisfaction gradients (29 percent and 21 percent). This finding suggests that the self-perceptions channel cannot fully account for effects on well-being and thus additional mechanisms must be at play.

C. *Additional Robustness Checks*

In this section, we provide a brief discussion of the robustness checks that, due to space constraints, are reported in online Appendix A.

In the baseline specification, we code the happiness and life satisfaction questions using the Probit-OLS method. Online Appendix Section A.3 shows nearly identical results, both in terms of magnitude and statistical significance, under two alternative specifications: coding these variables with consecutive integers and using an ordered Probit model instead of OLS.

The baseline specification also assumes a linear relationship between subjective well-being and *Income Rank*. We use this linear specification because it fits the data well and, presumably for that reason, it is widely used in the literature (Ferrer-i-Carbonell 2005). In online Appendix Section A.4, we present results under a more flexible specification based on binned scatter plots. The findings confirm that the linear specification provides a fair approximation and that the results are not driven by outliers or nonlinearities. Relatedly, online Appendix Section A.5 compares the results to an alternative definition of *Income Rank* based on the rank within the county of residency instead of the national rank. The results are qualitatively and quantitatively consistent across the two definitions. If anything, the estimated effects of transparency are slightly larger under the local definition of *Income Rank*.

A potential concern is that *Income Rank* is measured with a survey question, which may introduce measurement error.[45] This concern is probably minor, because survey measures of income correlate highly with their counterparts from the administrative records (Karadja, Mollerstrom, and Seim 2017). Moreover, measurement error in *Income Rank* does not necessarily challenge the validity of our findings. To explain our findings, the measurement error must have experienced a large, sudden, and permanent reduction in 2001. Moreover, this reduction must be present for individuals

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[45] Ideally, we would like to merge the survey responses to the income data from the administrative records. Unfortunately, this is not possible because the Norwegian Monitor Survey does not collect individual identifiers.

with higher internet access but not for individuals with lower internet access. Given that individuals with higher and lower internet access answered the same question about income, this confounding factor seems unlikely.

To address these concerns more directly, online Appendix Section A.6 presents results under alternative definitions of *Income Rank*. As the income question is elicited in bins, our baseline specification uses the standard method from the happiness literature to impute the values of *Income Rank* within each bin (Stevenson and Wolfers 2008, Kahneman and Deaton 2010). In online Appendix Section A.6, we show that the results are robust under the non-imputed version of *Income Rank*. This result should not be surprising, because the imputed and non-imputed versions correlate highly with each other (correlation coefficient of 0.984). A related potential concern is that the ninth bin in the income question was added in 1999, which could contaminate the comparison of the happiness-income gradient around 2001. Online Appendix Section A.6 mitigates this concern: the results are almost identical if we pool the ninth and eighth bins, as if the ninth bin was never introduced. This result should not be surprising either, because only 1.55 percent of individuals fell in the ninth bin in 1999.

Online Appendix Section A.7 assesses the robustness of the results under alternative definitions of *I{Higher Internet}*: using responses to *Internet Access* for 1999 or 1999–2013 (instead of 2001, as in the baseline specification), dividing the two groups by the median for the whole sample (instead of year-specific medians), and using a Probit model (instead of OLS). The results are quantitatively and qualitatively robust for all definitions. Moreover, we use the same method to construct *I{Higher Internet}* in Germany as in Norway. As a result, if the definition of *I{Higher Internet}* generated spurious effects in Norway, it should have introduced the same spurious effects in Germany.

Another potential concern is that the change in the happiness-income gradient is mechanically driven by an increase in income inequality. This possibility seems highly unlikely, given that it requires a large, sudden, and persistent increase in inequality that would be unprecedented in a developed country. Online Appendix Section A.8 addresses this concern more directly. Using survey data from the Norwegian Monitor Survey and administrative data from Statistics Norway, it shows that all measures of income inequality remain remarkably stable in Norway, not only around 2001, but during the entire 1985–2013 period. Relatedly, online Appendix Section A.8 shows that the composition of the sample of survey respondents did not change significantly around 2001 and, consistent with that fact, the regression results are not sensitive to the introduction of sampling weights.

D. *Interpretation of the Findings*

Our evidence suggests an increase in the gradient between subjective well-being and income in 2001 that is probably due to the increase in income transparency. Although we cannot cover every mechanism that may have mediated this effect, we can briefly discuss some plausible channels.

Individuals may be affected by the online tax lists because, as a result of them, they are treated better or worse by others. In other words, rich individuals may have benefited from having their incomes made public, because others recognize them as

rich and treat them better (e.g., maybe agreeing to favors or dating them). In turn, poorer individuals may have been treated worse by others. This interpretation aligns with evidence showing that individuals are treated better when they wear expensive clothing (Fennis 2008) and drive expensive cars (Doob and Gross 1968). It also aligns with evidence suggesting that individuals will pay more for highly visible goods, such as clothing and cars, to signal their income to others (Charles, Hurst, and Roussanov 2009).

Another possible mechanism states that individuals care directly about whether they are richer than others, because they get a psychological utility from holding that belief (Senik 2009). Richer individuals may be happier because they find out that they are richer than they thought: for example, they may form income aspirations by looking at the incomes of others. Alternatively, richer individuals may be happier by merely thinking that a social contact will find out how rich they are. Some evidence supports this interpretation: even in contexts of anonymity and privacy, individuals seem to care about how their own payoffs in the laboratory compare to the payoffs of other subjects. For example, individuals become less risk averse to avoid being ranked last (Kuziemko et al. 2014). According to brain imaging data, individuals seem to be displeased to learn that other subjects in the lab earned higher rewards (Fliessbach et al. 2007).

Another potential channel is that income transparency focused societal or individual attention onto income, thus increasing the marginal utility from income. Individuals use the search tool, read about it in the media and discuss it with others. Thus, the search tool may make it more likely for individuals to think about their own incomes, or about the incomes of others, at any given moment. If well-being depends on what comes to mind, this channel could explain the increased gradient between well-being and income.

There are other channels unrelated to income comparisons that may affect the happiness-income gradient but we do not believe to have played a substantial role in explaining our findings. For example, although the higher transparency reduced tax evasion (Bø, Slemrod, and Thoresen 2015), the magnitude of these effects (US$33 per household in 2001) are tiny relative to the magnitude of the change in the happiness-income gradient. Also, if individuals with lower pay used the tax lists to get a raise or a job with higher pay (Rege and Solli 2015), the resulting effects on the happiness-income gradient should be small and point in the opposite direction.[46]

Regarding the external validity of the findings, Norway is different from other countries in many dimensions, and thus income transparency effects may be more or less pronounced. To assess whether Norway is an exceptional context, we exploit data from the 2006–2007 wave of the European Social Survey. Following Clark and Senik (2010), we use the question, "How important is it for you to compare your income with other people's incomes?" The possible answers ranged from 0 (Not at all important) to 6 (Very important). This question was asked in Norway as well as

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[46] As discussed in Section ID, the tax lists are far from ideal for salary comparisons, and thus this change in behavior may only affect a small fraction of individuals. And, if anything, this channel would predict effects in the opposite direction: if lower-paid individuals are moving to higher-paying jobs, then that should reduce the gap in well-being between richer and poorer individuals.

in 21 other European countries.[47] The importance of income comparisons seems to be reasonably homogeneous across these countries, ranging from an average score of 1.95 in the Netherlands to an average score of 2.82 in Slovakia. Most important, the evidence indicates that Norway is not special in terms of income comparisons, as the average score in Norway (2.25) is close to the average score across the other 21 countries (2.30).

E. *Back-of-the-Envelope Calculations and Comparison to Related Studies*

We start from the framework that income can affect happiness through two channels, intrinsic utility and income comparisons. We use the previously discussed estimates to assess the relative contribution of income comparisons, ranging from 0 percent (only intrinsic utility matters) to 100 percent (only income comparisons matter). To estimate a lower bound, we start from the worst-case scenario that the happiness-income gradient was entirely due to the intrinsic utility channel prior to 2001. Additionally, we assume that the 29 percent increase in the happiness-income gradient (column 1 of Table 3) was due to the income comparisons channel. Under those two assumptions, it follows that 22 percent $(= 0.29/(1 + 0.29))$ of the happiness-income gradient after 2001 is due to income comparisons. As this is based on a worst-case scenario, the 22 percent provides a lower bound to the importance of income comparisons.[48]

We provide a less conservative lower bound by focusing on individuals with higher internet access. We assume that the pre-2001 happiness-income gradient was entirely due to the intrinsic utility channel and that the 56 percent increase in this gradient (column 4 of Table 3) was entirely due to income comparisons. Under these two assumptions, it follows that among individuals with higher internet access and after 2001, at least 36 percent $(= 0.56/(1 + 0.56))$ of the happiness-income gradient can be attributed to income comparisons.

We also benchmark our results with estimates obtained in other studies. Countless studies can be used for this comparison, but we focus on a set of five studies that are used as benchmarks in Bottan and Perez-Truglia (2017). Three are based on happiness regressions using data from the United States, Germany, and Japan, respectively: Luttmer (2005); Ferrer-i-Carbonell (2005); and Clark, Senik and Yamada (2017). The other two studies are based on hypothetical trade-offs between absolute and relative incomes: Johansson-Stenman, Carlsson, and Daruvala (2002) and Yamada and Sato (2013), based on data from Sweden and Japan, respectively. Based on the key estimates reported in these studies, the role of income comparisons is estimated at 82.0 percent in Luttmer (2005); 49.6 percent in Ferrer-i-Carbonell (2005); 52.8 percent in Clark, Senik, and Yamada (2017); 35.0 percent in Johansson-Stenman, Carlsson, and Daruvala (2002); and

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[47] In addition to Norway, the question was asked in the following countries: Austria, Belgium, Bulgaria, Denmark, Estonia, Finland, France, Germany, Hungary, Ireland, Netherlands, Poland, Portugal, Russian Federation, Slovakia, Slovenia, Spain, Sweden, Switzerland, Ukraine, and the United Kingdom.

[48] We are assuming the worst-case scenario that income comparisons did not matter at all prior to 2001. If, instead, we were to assume that income comparisons explained 50 percent of the happiness-income gradient prior to 2001, then we would have concluded that 61 percent $((0.5 + 0.29)/(1 + 0.29))$ of the post-2001 gradient was due to the income comparisons channel. Online Appendix B provides a simple model to formalize these back-of-the-envelope calculations.

45.8 percent in Yamada and Sato (2013).[49] These estimates are in the range of 35 to 82 percent and are then consistent with the lower bound of 22 percent reported in our study.

IV. Conclusions

In 2001, Norwegian tax records became easily accessible online, allowing everyone in the country to observe the incomes of everyone else. We propose that, because of income comparisons, higher income transparency can increase the differences in well-being between richer and poorer individuals. Using survey data and multiple identification strategies, we present evidence that higher income transparency caused an increase of 29 percent in the happiness-income gradient and an increase of 21 percent in the life satisfaction-income gradient. We provide evidence that some, although probably not all, of these effects operated through changes in self-perceived income rank. We also provide back-of-the-envelope calculations suggesting that income comparisons play a significant role in the relationship between well-being and income.

We conclude by discussing some implications for designing disclosure policies. We provide unique evidence that, as argued by those who opposed it, income transparency had a negative effect on the well-being of individuals with lower incomes. However, this result does not imply that transparency is bad. Alternative ways of publicizing and disseminating information can reduce these adverse consequences while preserving the desirable effects of transparency. For example, in 2014, Norway made searches of the tax records non-anonymous, which seems to have successfully leveraged social norms to discourage unintended uses of the data, such as snooping on friends.

However, in presence of strong privacy norms (Cullen and Perez-Truglia 2018), a policy of non-anonymous searches can discourage legitimate uses of the data such as for salary negotiations and career planning. Governments may want to complement the non-anonymous search tools by offering anonymous access to de-identified datasets. For example, some US states list the salaries of all public employees including identifiable information such as full names. Instead, they could offer aggregate data such as average salaries or salary ranges by organization, occupation, and unit. These aggregate data can provide most of the information that individuals need while avoiding harmful effects on the well-being of the lowest earners.[50]

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[49] Let y be an individual's own income and $\bar{y}$ the average income in the individual's reference group. These studies are based on utility functions of the following form: $U = a \cdot \log(y) - b \cdot \log(\bar{y})$, with $a > 0$ and $b \in [0, a]$. We can use the ratio between the two coefficients, b/a, to measure the fraction of the utility from income that is due to income comparisons. Luttmer (2005) reports $a = 0.361$ and $b = 0.296$ in column 3 of Table 1; Ferrer-i-Carbonell (2005) reports $a = 0.456$ and $b = 0.226$ in column 1 of Table 2; Clark, Senik, and Yamada (2017) reports $a = 0.290$ and $b = 0.153$ in column 1 of Table 3; Johansson-Stenman, Carlsson, and Daruvala (2002) reports $b/a = 0.35$ in page 373; and Yamada and Sato (2013) reports $a = 0.048$ and $b = 0.022$ in column 1 of Table 4.

[50] A similar recommendation is provided in Cullen and Perez-Truglia (2018). They conducted a survey of employees in a large firm with standard pay secrecy. They show that a vast majority of employees would prefer the firm to disclose anonymous salary information, such as average salaries per position. However, most employees would prefer the status quo of pay secrecy over the disclosure of salary records with personally identifiable information.

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