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The value of honesty: empirical estimates from the case of the missing children

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Abstract How much are people willing to forego to be honest, to follow the rules? When people do break the rules, what can standard data sources tell us about their behavior? Standard economic models of crime typically assume that individuals are indifferent to dishonesty, so that they will cheat or lie as long as the expected pecuniary benefits exceed the expected costs of being caught and punished. We investigate this presumption by studying the response to a change in tax reporting rules that made it much more difficult for taxpayers to evade taxes by inappropriately claiming additional dependents. The policy reform induced a substantial reduction in the number of dependents claimed, which indicates that many filers had been cheating before the reform. Yet, the number of filers who availed themselves of this evasion opportunity is dwarfed by the number of filers who passed up substantial tax savings by not claiming extra dependents. By declining the opportunity to cheat, these taxpayers reveal information about their willingness to pay to be honest. In our analysis, we develop a novel method for inferring the characteristics of taxpayers in the absence of audit data. Our findings indicate both that this willingness to pay to be honest is large on average and that it varies significantly across the population of taxpayers.

Keywords Tax evasion · Compliance · Honesty · Dependent exemption

JEL Classification H26 · H24

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1 Introduction

The rational actor model that forms the basis for microeconomics has been fruitfully extended to many realms of human behavior, including criminal activity. The benchmark model of crime goes back to Becker (1968), which posits that an individual will commit a crime when the expected benefits exceed the expected costs. This same framework has been used to study the decision to evade taxation, beginning with the seminal contribution of Allingham and Sandmo (1972). These models assume that individuals face no psychological cost of breaking the law—they bear no intrinsic cost for being dishonest, but instead make a purely pecuniary calculation.

In the area of tax compliance, existing research has demonstrated a strong negative correlation between evasion and the probability of being caught, which broadly supports a model of rational calculation without refuting the possibility of a preference for honesty. For example, audit data show that sources of income that are not subject to third-party reporting are far more likely to be underreported (Klepper and Nagin 1989), to the extent that less than half of all income from self employment is claimed (Slemrod 2007). Recent research has pushed this claim further. Based on a randomized audit experiment, Kleven et al. conclude that "overall tax evasion is low, not because taxpayers are *unwilling* to cheat, but because they are *unable* to cheat successfully due to the widespread use of third-party reporting" (p. 3). Phillips (2010) also finds additional support for the rational actor model by testing implications of a more realistic model of the relationship between evasion and audit probabilities.

Yet, other authors have concluded that the observed levels of tax compliance are too high to be explained by the standard Allingham and Sandmo (1972) framework, arguing that some desire to be honest or to comply with social norms must be important (Andreoni et al. 1998). Directly asking taxpayers does not clarify their motivations for compliance. In 2010, 87 % of taxpayers surveyed stated that it is not acceptable to cheat at all, and 97 % agreed with the statement that "It is every American's civic duty to pay their fair share of taxes." But 64 % said that fear of audit was important in inducing them to pay their taxes honestly (Internal Revenue Service Oversight Board 2011). This paper contributes to the literature on tax compliance by using a novel strategy for *detecting* tax evasion and *quantifying* the willingness to pay to be honest, based on an examination of taxpayer response to a change in enforcement.

In 1987, millions of children suddenly "went missing" from the rolls of federal income tax returns. The reason was a change in reporting requirements, which eliminated an important avenue for evasion. To claim a dependent prior to 1987, a filer needed only to list the dependent's first name on his tax return. Since the Internal Revenue Service had no easy way to verify that these dependents existed or to ensure that they were not listed on multiple returns, the system may have tempted filers to either invent dependents or to claim ineligible individuals as dependents. Below, we show that many filers availed themselves of this opportunity and claimed fictitious or otherwise ineligible dependents. On the other hand, we estimate that a majority of taxpayers were unwilling to cheat to gain around \$500 in 2010 dollars, equivalent to roughly 1 % of the mean after-tax income and 7 % of the mean taxes owed in 1986. We interpret this as evidence that many taxpayers have a substantial taste for honesty, and that an unwillingness to cheat is thus an important component of the economics of tax evasion, and perhaps crime more generally. We also demonstrate how the response to the enforcement change can be used to uncover differences in characteristics across cheaters and honest taxpayers without relying on audit data. Our findings suggest that cheaters and honest taxpayers are fairly similar in many observable characteristics, including the tax value of cheating.[1]

Our analysis is performed on a panel of tax return data that spans the Tax Reform Act of 1986 (TRA86), which included the reporting change. As of 1987, filers were newly required to report a Social Security Number (SSN) for all dependents over the age of 5. Given this information, it was relatively easy for the IRS to verify the existence of dependents and to check that they were not listed on multiple returns, and consequently the probability of cheating without detection fell precipitously. The response to this change in reporting rules was pronounced. Our data show that the number of dependents claimed in 1987 fell by 5.5 %, which is equivalent to 4.2 million "missing children."

We are not the first to document this decline in the number of dependents claimed. An IRS report detailed the motivation for the policy change and provided estimates of the change in the number of dependents claimed in response to the reform (Szilagyi 1990). Moreover, the episode is cited in two popular public finance texts (Slemrod and Bakija 2008; Gruber 2009) and a mainstream book on economics (Levitt and Dubner 2005). But, while the basic facts of this event are known, no prior work has studied the behavioral responses in detail, nor has any other research used this incident to quantify evasion, measure the willingness to pay to be honest, or estimate the differences in characteristics between honest taxpayers and cheaters, as we do here.

Early waves of empirical research on tax evasion were based on audit data, survey data, and laboratory experiments, each of which has strengths and weaknesses.[2] Our work relates more closely to a newer stream of research that uncovers indirect evidence of evasion, which Slemrod and Weber (2012) call "traces of evasion." An early example of this strategy is Pissarides and Weber (1989), which compares national income product account and reported taxable income to infer underreporting. Feldman and Slemrod (2007) infer evasion by comparing the marginal increase in charitable donations with respect to sources of income subject to different third-party reporting requirements. Our analysis differs from these by leveraging a natural experiment from a change in enforcement policy to uncover facts about evasion without the benefit of audit data.[3]

[1] A number of studies using audit data have tested for differences in evasion across income categories, gender, and tax rates (Clotfelter 1983; Feinstein 1991; Christian 1994). These articles do not, however, indicate whether these evasion differences are due to different opportunities to evade or different propensities conditional on opportunity. Our case study has the advantage of being an opportunity that is readily available to all taxpayers, which allows us to isolate evasion predilection.

[2] For a thorough discussion and critique of the literature, see Andreoni et al. (1998) and Slemrod (2007). Of particular interest to our work is the finding from laboratory studies that some people comply with the tax authority, even when the probability of audit is known to be zero, which implies a desire to be honest (Baldry 1987; Alm et al. 1992).

[3] Our approach also has an affinity with nonincome-tax studies that uncover cheating indirectly, such as Jacob and Levitt (2003), Fisman and Wei (2004), and Oliva (2010).

Researchers within and outside of economics have incorporated a variety of social factors into models of compliance. Tyler (1990) argues that citizens obey the law out of a sense of allegiance to a government they view as a legitimate authority. Smith (1992) applies this idea to the case of tax filing, documenting with survey evidence that self-reported voluntary compliance is positively correlated with viewing the tax authority as fair and responsive. Cowell (1992) considers a general model of evasion where the equity of the tax system influences preferences without specifying a functional form, and Bordignon (1993) models an environment in which social concerns create a constraint on the amount of evasion available. Related implications have been tested in laboratory experiments. There is laboratory evidence that evasion responds to cues about fairness (Spicer and Becker 1980) and to the uses of revenue (Becker et al. 1987). The models most closely related to our work incorporate honesty directly. Block and Heineke (1975) introduce a "preference for honesty" into a labor supply model, allowing the disutility of work to differ for activities that are legal versus illegal. Erard and Feinstein (1994) introduce compliance behavior by assuming that some fraction of consumers will never cheat. Gordon (1989), which is perhaps most similar to our model, adds a psychic cost to the Allingham and Sandmo (1972) framework where the consumer faces a continuous evasion choice. Individuals with the highest psychic costs rarely evade, but assuming decreasing absolute risk aversion, are predicted to evade more when the tax rate increases. Our model is distinct from existing work in analyzing a discrete choice to cheat (by claiming a dependent) and in providing a direct parameterization that we take to data in order to quantify the willingness to pay to be honest.

Our analysis proceeds as follows. In Sect. 2, we provide additional details about the change in reporting policy and the other tax law modifications in TRA86 relevant to our analysis. In Sect. 3, we describe our data, which is a panel of tax returns spanning the reform. In Sect. 4, we document the decline in the number of dependents claimed in 1987. We argue that the substantial decline cannot be accounted for by a delay in obtaining SSNs or by other changes in dependent rules that were part of TRA86.

In Sect. 5, we flip to the other side of the coin and document the tax savings foregone by the majority of filers who did not claim additional dependents and were therefore unaffected by the policy reform. We show that average tax savings given up by honest taxpayers from claiming one inappropriate dependent would have been roughly \$250 in 1986 dollars on average, which equates to \$500 in 2010 dollars, or 1 % of after-tax income and 7 % of the average total tax paid. We show that accounting for risk preferences has a limited impact on these magnitudes.

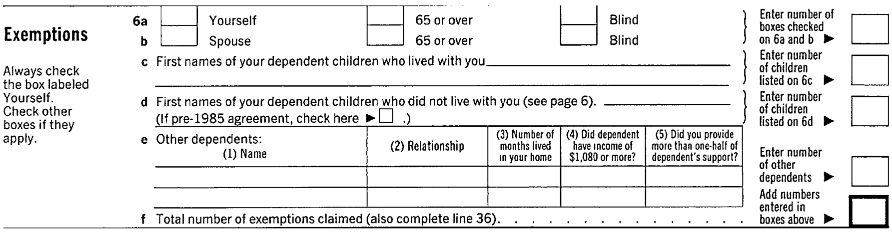
In Sect. 6, we impute the average characteristics of cheaters, as compared to groups of honest taxpayers, and conclude that they look different on several dimensions, including filer status and claiming of the child care credit. The average cheater does not, however, appear to have a higher monetary gain from cheating than the average honest taxpayer. This suggests that the variation in the decision to cheat is not driven primarily by the tax savings at stake, but instead by variation in the willingness to pay to be honest. We interpret this as a second piece of evidence in support of the notion that a taste for honesty is quantitatively important for the analysis of evasion. Section 7 concludes.

2 The introduction of Social Security information on tax returns

Prior to 1987, a tax filer needed to include only the first names of any dependent children he wished to claim on his return. In contrast, starting in 1987, the 1040 required the full name and SSN of all children over age 5. The relevant portions of the 1040 from 1986 and 1987 are shown in Fig. 1. The SSN requirement was extended to all dependents age 2 and older beginning in tax year 1989, to all dependents at least one year old in 1991, and finally to all dependents in 1995. The gradual reduction in ages affected by the SSN requirement was designed to give families time to obtain social security numbers for children.[4]

While the reporting requirement for claiming a dependent exemption changed in 1986, the underlying definition of a dependent did not. Five tests must be met for a taxpayer to claim another individual as a dependent. The dependent must be a US citizen or a resident of the US, Canada, or Mexico. The dependent cannot file a joint return. The dependent's income cannot exceed the personal exemption amount, with exceptions made for a taxpayer's own child under the age of 19 or under the age of 24 if a full-time student. The dependent must either live with or be related to the taxpayer

(a) 1986 Exemption Portion of the 1040



(b) 1987 Exemption Portion of the 1040

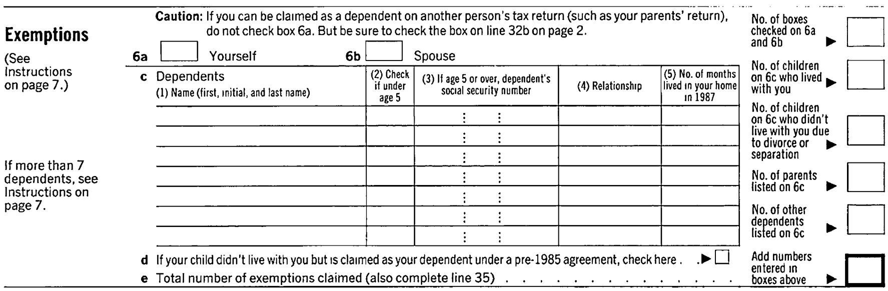


Fig. 1 The change in reporting required on the 1040. *Note*: The figure shows portions from the 1040 form in 1986 and 1987. The 1986 1040 required only the first names of dependents, whereas the 1987 required the full name, birth date and Social Security Number for all children over age 5

[4]Today more than 90 % of SSNs issued to children are issued at the time of birth, through the Enumeration at Birth program, which began in 1989 (GAO 2005).

claiming her. Finally, more than half of the dependent's support must be provided by the taxpayer claiming her. None of these tests changed between 1986 and 1987.[5]

The Tax Reform Act of 1986 did make changes that may have influenced the dependent status of some individuals. Prior to the reform, a child who was claimed as a dependent by his parents but who also filed his own return could claim a personal exemption for himself. This "double-dipping" was eliminated by TRA86. Beginning in 1987, dependents were not permitted to claim a personal exemption and in most cases could claim only a limited standard deduction. This may have created some negotiation between parents and children about how long to remain a dependent.[6] The value to parents of claiming an additional exemption also changed in 1986. The nominal value of a personal exemption rose from \$1080 to \$1900 while marginal tax rates fell for most taxpayers. We return to these changes below when interpreting our data.

3 Tax return panel data

We use data from the University of Michigan tax panel, compiled by the Office of Tax Policy Research (OTPR). The starting point for this dataset is the annual cross-sections of tax return data released by the Statistics of Income (SOI) division of the IRS. These cross-sections report information from most lines of the tax return and from many supporting schedules. There is information on the total number of exemptions (including extra exemptions for filers who are blind or over age 65), the number of exemptions for dependent children (reported separately for children living at home and children living away from home), and the number of exemptions for dependents other than children. To protect taxpayer confidentiality, income amounts are blurred and the number of exemptions is topcoded in some cases.[7]

Auten and Carroll (1999) describe the two methods used to select returns for inclusion in the annual SOI cross-sectional files. First, a nonstratified sample is drawn by choosing certain four-digit combinations and selecting all returns on which the primary filer's SSN ends with one of these combinations. This set of randomly chosen returns is known as the Continuous Work History Survey. Second, a stratified sample

[5]Recent research on dependent overclaiming focuses on the Earned Income Tax Credit (EITC). McCubbin (2000) describes data from a 1994 audit of randomly selected EITC claimants. Approximately 26 % of EITC dollars claimed were overturned upon audit, and about 70 % of these overclaims involved an error in claiming an EITC-qualifying child. McCubbin further finds that misclaiming a child is sensitive to the size of the associated benefit. She estimates that a \$100 increase in the tax savings from claiming a child increases the probability of erroneously claiming an EITC-qualifying child from a mean of 8 % to 8.4 %. Liebman (2000) estimates the extent of EITC misclaiming by matching March 1991 CPS respondents to 1990 tax return data. At the time, the EITC was exclusively available to filers with children. Liebman estimates that 11 to 13 % of 1990 EITC recipients did not have a child in their CPS household as of March 1991, and 10 % did not have a child in the household one year earlier.

[6]See Whittington and Peters (1996) for evidence that the tax effect on this decision is small.

[7]For returns with adjusted gross income (AGI) greater than \$200,000, the number of exemptions for children living at home is topcoded at 3. This topcoding is unlikely to affect our results, as only 7 filing units observed in both 1986 and 1987 are affected. The number of filing units affected in any other pair of years over which dependent loss is calculated is never greater than 22.

is drawn by sorting taxpayers on the basis of level and type of income and applying different sampling rates to filers in different strata. The OTPR panel uses only those individuals in the randomly selected CWHS portion of the SOI cross-sections (Slemrod 1992), and thus the panel does not oversample high-income returns. The number of four-digit combinations included in the CWHS sample changed over time, due to IRS budget constraints. This results in variation in the number of tax returns included in the panel in different years. The number of tax filing units present in all 12 years of the panel is 4,982, and the number present in both 1986 and 1987 is 9,099.

Christian and Frischmann (1989) analyze attrition from the OTPR panel. They find that younger, unmarried, and lower-income individuals are somewhat more likely to drop out of the panel. A change in marital status may cause an individual to be dropped from the panel, as only the SSN of the primary filer is used in linking returns across years. Because men are more likely to be listed as primary filers, men have lower rates of attrition from the tax panel.[8] The unbalanced nature of our panel leads us to use somewhat different samples for different parts of our analysis, which we detail in the relevant sections below.

4 Many people cheated on their tax returns

Our tax panel data allow us to look at the number of filers who lose net dependents each year by comparing the number of dependents in one year against the number claimed the following year. If many filers were cheating up until 1986, then we would expect to see a large number of filers losing dependents in 1987. Figure 2 plots the percentage of filers who lost at least one dependent in each year of our panel, conditional on claiming a dependent in the previous year. There is a dramatic spike in 1987, when 20.3 % of filers lost a dependent, compared to an average of 14.0 % in all other years.[9] If the 1987 surge in dependent losses is entirely due to cheating, then approximately 31 % $((20.3 - 14)/20.3)$ of the 1987 dependent losers are cheaters. The number of "excess" dependent-losing filers in 1987 is equal to 2.5 % of the filing population. Figure 2 also plots the fraction of filers who gain at least one dependent in each year, which shows no commensurate change between 1986 and 1987.[10] Some cheaters may have chosen to claim more than one fictitious dependent. The percentage of filers losing multiple dependents is 4.4 % in 1987, substantially greater than

[8] Because children are more likely to live with unmarried mothers than unmarried fathers, estimates of the total number of dependents from the tax panel are somewhat lower than the total number of dependents appearing in cross-sections of tax return data. This gap is visible in Fig. 3 below.

[9] There are other cases in which requiring a taxpayer to provide a straightforward piece of supporting evidence has generated a large change in reporting. Fack and Landais (2011) show that when France first required that receipts for charitable contributions be submitted with tax returns, reported charitable contributions fell by 75 %.

[10] Note that the shares losing and gaining dependents are calculated with different bases in Fig. 2. All filers observed in two consecutive years are included when computing the share of filers gaining a dependent. Only filers observed in two consecutive years and claiming a dependent in the first year are included when computing the share losing a dependent. If we compute the share losing a dependent without conditioning on previously claiming one, 1987 still stands out. About 8 % of filing units lose a dependent in 1987, compared to 5 to 6 % in all other years.

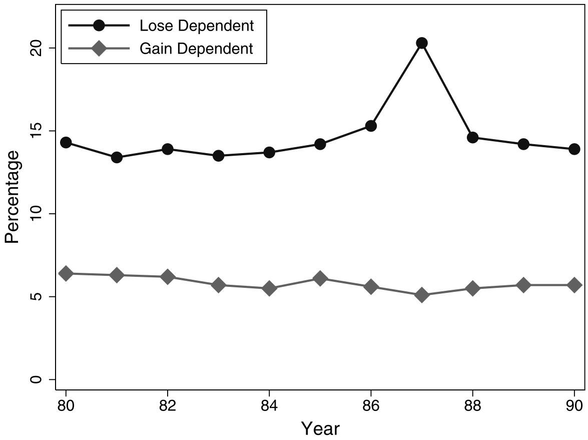


Fig. 2 Percentage of tax filers who lose and gain at least one dependent each year. *Note*: Estimates are the authors' calculations from OTPR data. Dependent exemptions claimed includ exemptions for children living at home, children living away from home, parents, and other dependents. In computing the share of returns losing dependents in year t, the denominator is the set of filing units observed in both t and $t-1$ claiming a positive number of dependent exemptions in $t-1$. In computing the share of returns gaining dependents in year t, the denominator is all filing units observed in both t and $t-1$

the 2.2 % to 2.9 % losing multiple dependents in any other year of the panel. We view this as further evidence of cheating, but throughout we focus on the decision to claim a marginal extra dependent.

The percentage of filers losing a dependent is already a bit higher than its long-run average in 1986. The Tax Reform Act was passed in October 1986, several months before tax returns were due, and the 1986 instruction booklet included information about the upcoming SSN requirements. If the slight uptick in losses in 1986 was due to *anticipation* of the reform by cheaters, then our calculation will underestimate the number of cheaters. It is clear from the figure, however, that any anticipatory effect is a small fraction of the total.

We can also compare the number of child dependents claimed on tax returns to the number of children in the population from census estimates, which we do in Fig. 3. Our tax data do not include the age of dependents, so we cannot perfectly match tax-based and census-based counts by age. Instead, we plot the ratio of all dependents to the total population under 19 and under 24. Dependents who meet the citizenship, joint return, residency, and support tests can be claimed as a dependent irrespective of their income if they are below age 19, or if they are full-time students under age 24. The majority of children under 19 will be claimed as dependents, and children of any age can be claimed if they have sufficiently low income and meet the other requirements. Children of nonfilers, however, will not appear on any return, so it is ambiguous whether we expect the total dependents claimed to be larger or smaller than these population bases. Our interest, however, is not in the ratio itself, but in whether or not the ratio changes suddenly in 1987.

Panel (a) of Fig. 3 shows the ratio of the number of child dependent exemptions claimed (for children living at home and children living away from home) to the num-

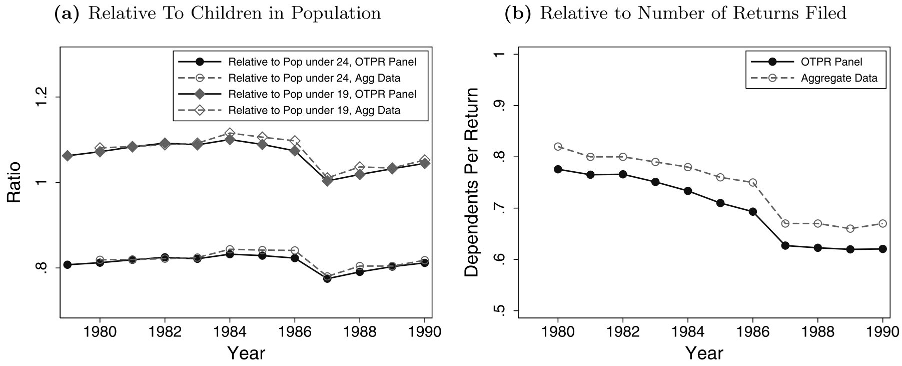


Fig. 3 Number of dependents claimed. In panel (**a**) estimates of the population under 19 and under 24 come from the United States Census Bureau. In both panels, estimates of the number of child dependents claimed on tax returns (including children living at home and children living away from home) come from the authors' calculations of OTPR data (labeled OTPR Panel) and IRS publication 1304 (labeled Aggregate Data)

ber of children under age 19 and to the number of children under age 24.[11] The figure shows both our calculations using the OTPR tax panel and aggregate totals reported by the IRS in publication 1304.[12] These data show that the relationship between the number of child dependents and the number of children in the population was quite steady in the early years of our analysis.[13] This is true whether the denominator is the population of children under 19 (the upper lines in the figure) or the population of children under 24 (lower lines), and whether the dependents are measured with the OTPR data (solid lines) or the IRS aggregates (dashed lines). In 1987, there was a significant fall in the number of dependents that does not correspond to any change in the underlying population. This pattern is striking: it is consistent across all four series, and it correlates exactly with the increase in dependent losses shown in Fig. 2.

The Tax Reform Act of 1986 increased the value of the standard deduction and the personal exemption, which increased the income at which a person is required to file a tax return. This could cause some low-income children to disappear from tax data. The coincident expansion of the EITC, however, will have worked in the opposite direction. Given these changes, it is useful to look also at the number of dependents claimed per return. In panel (b) of Fig. 3, we plot the ratio of the total number of dependents claimed to the total number of tax returns filed, using both OTPR panel

[11] The previous figure includes all dependents claimed, regardless of their relationship to the filer. This figure considers only dependents who are children of filers. Children account for 95 % of all dependents claimed. All returns observed in the OTPR panel in a given year are used to estimate the total number of child dependents, with different weights across years to account for the fluctuations in sample size.

[12] We thank Brian Erard for providing us with the publication 1304 data. Despite the small size of the OTPR panel, the values we impute from it are a close match to values computed from aggregate data.

[13] The slight increase over time in both series is consistent with rising college enrollment rates, which enables more children between 19 and 24 to be claimed. Data from the National Center for Education Statistics (1995) show that the college enrollment rate of 18- to 24-year olds increased from 25 % in 1979 to 28 % in 1986.

and SOI aggregate data. Both data sources indicate a sharp decline in dependents claimed in 1987, with no subsequent recovery.

A simple estimate of the number of inappropriate dependent claims can be derived under the assumption that filers lost more dependents in 1987 than in other years solely because many people were cheating prior to the reform, but stopped cheating in 1987. Under that assumption, the "extra" dependent losses observed in the panel between 1986 and 1987 correspond to 4.2 million improperly claimed dependents, or 5.5 % of all dependents claimed in 1986. The original IRS report suggested that the decline in dependents was equal to 7 million, based on the difference between a forecast of 76.7 million dependents and an actual number of 69.7 (Szilagyi 1990). The report does not, however, explain the nature of the forecast, and the citation for the actual number says simply "SOI data." SOI publication 1304 tables show the actual number of dependents claimed was 77.1 million in 1986 and 71.9 million in 1987, suggesting a decline of 5.2 million dependents. Our estimate of 4.2 million treats all excess dependent losses as cheating, but does not count any anticipatory changes that occurred in 1986, nor does it include anyone who continued to cheat in spite of the reform. Attributing the 1986 uptick in dependent losses wholly to the reform would raise this estimate to 5. Thus, estimates from the OTPR panel are broadly consistent with the decline documented elsewhere, though they are somewhat smaller than the original number reported in Szilagyi (1990), which we suspect was too high.

The policy reform in 1987 only required an SSN for children aged 5 and above. In 1989, the law required an SSN for children over the age of 2. Thus, in 1987, a devoted tax evader could have gained two more years of fake dependent claims by stating that their dependents were under 5. This would then lead to a drop in dependents claimed in 1989. In Fig. 2 we do not, however, see any evidence of extra "lost" dependents in 1989. (There are additional changes in 1991 and 1995, but these are outside the time span of our data.) Why not? Even though an SSN was not required for children under age 5, the 1987 form 1040 did require the dependent's full name and relationship to the filer for younger children, whereas it had previously required only the *first name* of dependent children (see Fig. 1). While the SSN is what enables the IRS to easily verify identity, the additional information required for all children made it more likely that the IRS could detect cheating, even for younger children. Moreover, taxpayers may have interpreted the change as indicating a broader effort to crack down on the inappropriate claiming of dependents.

It is also possible that the staunchest cheaters continued cheating through the 1989 change. If a cheater was willing to claim that a false dependent was under the age of 5 in 1987, he may have been bold enough to claim a new dependent under the age of 2 in 1989, or to write in a false SSN.[14] In sum, while it is plausible to have expected some decline in cheating in 1989, it is not necessarily surprising that any such change is too small to show up in the data.

[14]Using a fake SSN may have been a viable strategy because the IRS, lacking resources, only computer matched 3 % of SSNs in the years just after the change (General Accounting Office 1993).

4.1 Could the change in dependents be due to those lacking SSNs?

The 1987 reform required that filers record the social security number of dependents to be claimed. While it is now common for an SSN to be issued at the time of a child's birth, this was not standard practice in 1987. Instead, some children were first assigned an SSN when their parents established a savings account for them or when they first entered the labor market. Could the sharp decrease in dependents be due to many legitimate dependents who lacked an SSN being left off of returns?

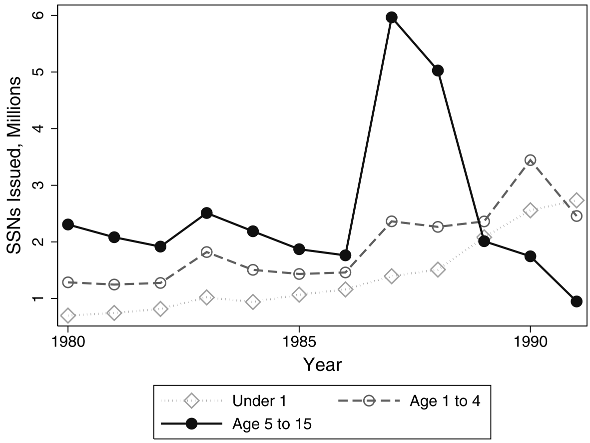
Several factors weigh against this explanation. First, the SSN application for a child was not particularly arduous. To apply, parents needed to submit a one-page form, the SS-5, along with proof of age, citizenship, and identity to the Social Security Administration. The form, which could be submitted by mail, was referenced in the 1987 1040 instruction booklet, and it was widely available in post offices, libraries, and other places where tax forms were distributed. A certified copy of a birth certificate was sufficient to establish both age and citizenship, and medical, school, or day care records could be used to establish identity. Applications were typically processed within 10 to 14 days (New York Amsterdam News 1988).

Second, the need for an SSN was well publicized before the requirement became effective. The law was passed in October of 1986, 18 months before the relevant filing deadline of April 15, 1988. In the fall of 1987, the IRS sent a mass mailing to 90 million taxpayers that highlighted the new SSN requirement and other major reforms (Los Angeles Times 1987). The upcoming SSN requirement was also described in the instruction booklet for 1986 returns, which would have given many filers advanced notice.

Even though the SSN requirement was announced long in advance of its implementation, was well publicized, and involved a straightforward and quick application process, some parents may still have been unaware of the requirement or may have procrastinated until it was time to file their taxes. Nevertheless, parents who had not obtained an SSN on time could still claim their children. The 1040 instruction booklet directed such parents to immediately apply for an SSN and to write "Applied For" on the corresponding line of the 1040. Thus, while recent research has demonstrated that procrastination and inertia can lead taxpayers to forego tax value by not optimally timing withholding (Jones 2012), we think this is of little concern here, both because the amount of money at stake is large and because parents had the option to apply for an SSN even after filing. Moreover, low-income families, whom we might suspect of facing the largest barriers to obtaining an SSN on time, likely already had SSNs for their children because all recipients of AFDC and Medicaid had been required to have SSNs since 1972 (Long 1993).

To further explore this issue, we analyzed data provided in Long (1993) on the number of new SSNs issued for three age categories: under 1, between 1 and 4, and between 5 and 16. If the decline in dependents claimed in 1987 was due to legitimate dependents who lacked SSNs, but later received them, then we would expect to see a rise in the number of SSNs issued to children above age 5 (the affected category in 1987) for several years after the reform. Alternatively, if nearly all legitimate dependents over age 5 obtained an SSN in order to be claimed on their 1987 tax returns, we would expect to see a surge in calendar year 1987 and 1988 issuances, followed by a return to prereform levels.

Fig. 4 Number of SSNs issued, by age of applicant. *Note*: data are from Long (1993)



The data are shown in Fig. 4. Prior to 1987, the number of SSNs issued is very stable in all three age categories shown.[15] There is then a tripling of claims for children over age 5 in the next 2 years, after which the series returns to the prereform level and begins to decline. The decline is consistent with the increase in issuances for younger age categories that began in 1987, which would leave fewer children in the older category in need of an SSN in the later years. These other age categories are similarly responsive to the tax law. The SSN requirement was extended to children ages 1 to 4 in tax year 1989, which led to a jump in issuances for that category in 1989 and 1990, despite downward pressure from the rise in newborn issuances. Overall, the data closely follow the pattern predicted by parents promptly obtaining an SSN as soon as it is required for tax purposes. The IRS itself was concerned with this issue and concluded that SSNs were not a significant barrier to legitimate claiming. They conducted surveys showing that, among taxpayers with children ages 5 and older, the share with SSNs for their children rose from 67 % in May 1987 to 89 % in January 1988—still several months before the 1987 filing deadline (Szilagyi 1990).

Finally, if failure to obtain an SSN prevented parents of legitimate dependents from claiming them in 1987, we would expect to see a disproportionate number of those filers *gaining* a dependent in subsequent periods, once they had obtained a number. To examine this possibility, we use our OTPR panel data to plot the fraction of filers who gain a dependent, conditional on having lost a dependent in a particular year, in Fig. 5. Each line in the figure represents those who lost a dependent in a different year, with 1987 in bold, and each data point shows the fraction who gain a dependent t years before or after a loss. Because our panel is unbalanced, the sample used in each data point differs slightly. To be included, a taxpayer had to file in both the base year and the year before that (in order to determine if they lost a dependent in the base year), as well as the relative year shown and the year before that (in order to

[15] There is a noticeable uptick in SSN applications in 1983. This is the first year in which recipients of interest and dividend payments needed to provide an SSN to the financial institution issuing these payments (Long 1993). Parents who had established savings accounts in the names of their children would have needed SSNs for their children at this point.

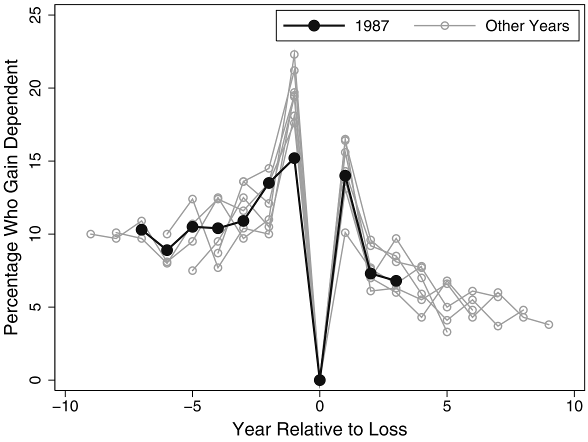


Fig. 5 Share gaining a dependent, among those losing dependent in year 0. *Note*: *Each line* in the figure corresponds to a different subsample from the OTPR panel: the set of filing units losing at least one dependent in a particular year. The *bold line* corresponds to the 721 filing units observed losing at least one dependent in 1987. We follow each sample over time, computing the share of these filing units gaining a dependent in each earlier and later year

determine if they gained a dependent). For example, the year 1 value for the sample shown in bold is calculated over filers observed in 1988 as well as 1986 and 1987. The year 3 value for the sample shown in bold is calculated over filers observed in 1986 and 1987 (allowing identification of a dependent loss in 1987) as well as in 1989 and 1990 (allowing identification of a dependent gain in 1990).[16]

The data show considerable churning. Relative to year zero (when a filer lost a dependent), filers are relatively likely to have gained a dependent immediately before or after that loss. This is true in every year, not just 1987.[17] If many filers lost a dependent in 1987 because of missing SSNs, we would have expected a large *increase* in dependent gains for those filers in subsequent years, relative to the gain experienced by filers who lost dependents in non-reform years. This is not the case; 1987 appears typical. If anything, gains are low immediately after 1987 losses, which is consistent with cheaters being less likely than legitimate losers of dependents to regain the dependent in the subsequent year.[18]

We cannot definitively eliminate the possibility that missing SSNs account for some of the decline in dependents in 1987. If there were some such cases, our estimates may overstate cheating. But, the administrative details, the pattern of SSN issuances, and the lack of a dramatic deviation in regaining suggest that this plays at most a minor role.

[16]Because underlying fluctuations in the sample may be affecting the patterns we observe, we have constructed a balanced-panel version of Fig. 5 that includes only those filing units observed in all years of the panel. This alternative figure is quite similar; it too shows that the group of filers who lose dependents in 1987 is not particularly likely to gain dependents in subsequent years.

[17]This may result in part from changes in who is claiming dependents following marital changes. In cases where a filing unit loses a dependent and gains a dependent in the following year, 25.3 % transitioned out of married filing jointly status at some earlier point in the panel. Among filing units losing a dependent in one year and not subsequently regaining, only 14.7 % had previously transitioned out of married filing jointly status.

[18]In the extreme case where cheaters have a *zero* chance of regaining a dependent, however, we would expect to see an even lower rate of regaining for 1987 dependent losers, given that nearly one-third of this group are estimated to be cheaters. This suggests that there could be modestly higher than average regaining rates among honest filers who lost a dependent in 1987.

4.2 Could the change in dependents be due to other changes in TRA86?

The Tax Reform Act of 1986 involved many other tax changes. Could any of these be the source of a large reduction in dependents? The most obvious feature of TRA86 relevant here is a change in the tax treatment of dependent filers. Prior to 1986, a child who filed his own return could claim a personal exemption on his return and still be claimed as a dependent by his parents. After 1987, this was not allowed, so children and parents had to choose whether the child would file with zero exemptions, and allow the parents to claim the exemption, or vice versa. If many families chose to allow income-earning children, who qualified as dependents, to claim themselves instead of claiming them as dependents on the parents' returns, then this could generate a one-time drop in dependents claimed in 1987.

In a tax-minimizing household, a child will claim an exemption for herself only when her tax savings from the exemption exceed the parents' tax savings from claiming an additional dependent.[19] The value of the exemption depends entirely on the marginal tax rate. For it to be tax-minimizing for a child to claim herself, that child would need to earn sufficiently more money than her parents so as to be in a higher tax bracket, and yet still live in her parents' home and obtain more than 50 % of her financial support from her parents, so as to qualify as a dependent.

Such arrangements are rare, as is shown in Table 1, which uses March Current Population Survey (CPS) data from 1987. We constructed a sample of children, be-

Table 1 Children's income relative to parents' income

	Full sample	Ages 15–18	Ages 19–23
A. Wage income			
% with any	54.7	47.6	77.8
Mean amount	1,317	836	2,883
% with wages > 0.75· parents' wages	6.2	5.5	8.5
% with wages > parents' wages	5.7	5.1	7.3
Median of (own wages/parents' wages)	0.007	0	0.049
B. Total income			
% with any	66.2	59.8	86.9
Mean amount	1,874	1,229	3,977
% with inc. > 0.75· parents' inc.	2.8	2.5	3.6
% with inc. > parents' inc.	2.0	1.8	2.6
Median of (own inc./parents' inc.)	0.015	0.005	0.068
N	11,369	8,853	2,516

Data are from the March 1987 CPS. Sample is restricted to children of household heads and to those either between the ages of 15 and 18 or between the ages of 19 and 23 and enrolled in school full-time

[19] A caveat here is that households may fail to minimize taxes in this manner under models of intrahousehold bargaining that lead to inefficiencies. A unitary household model, a model that results in efficient bargaining, or one that includes transferable utility will result in tax minimization because these models predict that families will maximize welfare and then bargain over the surplus.

tween the ages of 15 and 23, living with one or both parents. The lower age cutoff reflects the fact that only individuals aged 15 or older report income information in the CPS. All children between ages 15 and 18 are included, while children between ages 19 and 23 are included only if they are full-time students. Table 1 shows that 66.2 % of these potential dependents had some form of income, and 54.7 % had wage income. Not surprisingly, the median ratio of a child's income to his parents' income is 0.015. Only 2.8 % of children had income equal to 75 % of their parents' income, and just 2 % had income equal to or greater than their parents'. In sum, given the low probability that it would have been tax-minimizing for children to claim themselves, we expect that this aspect of the 1986 reform is unlikely to explain an important fraction of the drop in dependents.

5 Many other people paid to be honest

Our data suggest that around 2.5 % of taxpayers were cheating by improperly claiming dependents in 1986. Conversely, this means that 97.5 % were *not* cheating. This is perhaps the more surprising statistic, given the substantial amount of income that could have been taken at relatively low risk by claiming fraudulent dependents. Importantly, the 97.5 % of taxpayers who did not avail themselves of this opportunity to cheat implicitly demonstrated that they would rather give up several hundred dollars in income than cheat the government. Overall, we think this is striking evidence of a broad willingness to pay to be honest across the taxpayer base.

How much did honest taxpayers forego? To answer this question, we would like to eliminate the tax cheaters from the sample and tabulate the tax savings from claiming an extra dependent that would have been enjoyed by the honest taxpayers had they cheated. Unfortunately, there is no way to directly identify the cheaters in the data. We can, however, identify groups of filers who were almost certainly honest. One such group is the set of filers who did not lose a dependent between 1986 and 1987, who make up 92 % of filers, whom we analyze in Table 2.

Table 2 shows the average 1986 tax savings associated with the marginal dependent for filers who appear in the panel in both 1986 and 1987. These statistics are obtained by comparing the after-tax income of filers given the actual number of dependents reported in the data against their hypothetical after-tax income when they have one additional (or one fewer) dependent, as calculated by TAXSIM.[20] The after-tax income gain that would have accrued to families from claiming one additional child represents the gain to be had from cheating. On average, those who did not lose a dependent between 1986 and 1987 would have saved $275 on their 1986 taxes by claiming an additional dependent. The mean savings is even higher, $291, for those who claimed zero dependents. We view both of these groups as consisting of honest taxpayers. These savings are in 1986 dollars; inflating by the CPI to 2010 dollars doubles the estimates to $547 and $579.[21]

[20] The TAXSIM program is described by Feenberg and Coutts (1993).

[21] We use the CPI-U all items, which was 109.6 in 1986 and almost exactly double, 218.1, in 2010.

Table 2 The value of claiming a dependent

	N	Gain from additional dep., 1986	Loss from one fewer dep., 1986	After-tax income	Tax liability
Observed in 86 and 87	9,099	271		22,006	3,898
		(160)		(28,108)	(16,573)
No deps. in 1986	5,548	291		17,878	3,509
		(176)		(27,030)	(18,081)
Deps. in 1986	3,551	239	−271	28,457	4,505
		(123)	(126)	(28,546)	(13,875)
Lost dep. in 1987	721	224	−254	25,868	3,943
		(121)	(127)	(25,888)	(11,082)
Did not lose in 1987	2,830	243	−276	29,117	4,648
		(123)	(126)	(29,152)	(14,499)
Did not lose dep. in 1987	8,378	275		21,674	3,894
		(162)		(28,268)	(16,964)

Calculations of the gain (loss) associated with claiming one additional (fewer) dependent rely on TAXSIM estimates of tax liability, using income elements and household composition reported on 1986 tax returns. Standard deviations are in parentheses. After-tax income is defined as AGI minus total income tax liability, as reported on 1986 tax returns

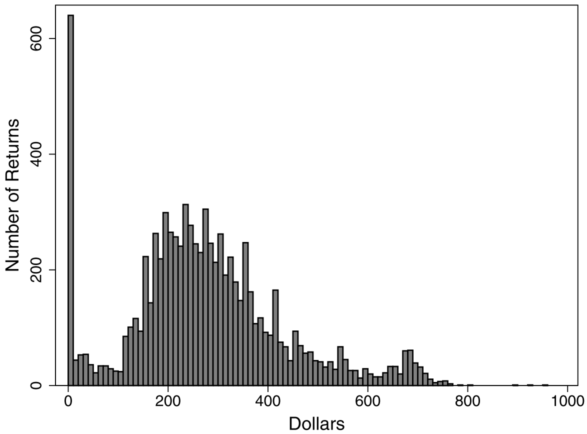


Fig. 6 Value of claiming one additional dependent, nonlosers, 1986. *Note*: Figure plots the difference between two TAXSIM computed tax liabilities for each filing unit, for units that appear in both 1986 and 1987 and do not lose a dependent. The first value is the tax liability that results from applying 1986 tax law to actual reported 1986 income and dependent exemptions. The second value adds one to the number of dependents claimed

Figure 6 shows the distribution of tax savings that would have accrued to filers if they had claimed an additional dependent in 1986, for the sample of returns that did not lose dependents between 1986 and 1987 (and are therefore assumed to be honest payers). Dependent exemptions are nonrefundable, so claiming an additional dependent has zero impact for low-liability filers. Among those not losing a dependent between 1986 and 1987, 7.3 % would have gained nothing. The average tax savings from an additional dependent among the remaining filers was larger. In this sample of presumably honest taxpayers, 69.4 % stood to gain at least $200, and 18.0 % stood to gain at least $400 by claiming an extra dependent.

Are these tax savings, foregone in the interest of civic piety, large? To put the magnitudes in context, Table 2 also shows the average after-tax income and average income tax paid for filers across these categories. The foregone gains from cheating, among those who did not lose a dependent in response to the reform, amount to 1.3 % of after-tax income, and 7.1 % of taxes paid. These are substantial amounts. In real dollars, the average foregone tax savings are as large as the child tax credit when it was introduced in 1998, and they are 80 % of the size of the maximum tax rebate for single filers in the 2008 stimulus plan. Past research has found evidence of fertility responses to benefits of this size. Whittington et al. (1990) estimate that TRA86's increase in the personal exemption, from $1,080 in 1986 to the fully phased-in level of $2,000 in 1989, raised the number of births by 7.53 per thousand women at risk, though these estimates have been called into question (Crump et al. 2011). Dickert-Conlin and Chandra (1999) estimate that a $500 benefit would increase the fraction of mothers who give birth in the last week of December rather than the first week of January, so as to accelerate tax savings, by 26.9 %. The fact that existing research estimates that real behavioral responses are induced by tax savings of this magnitude suggests that these savings are "large" in some behavioral sense. This affirms our claim that foregoing these benefits due to a taste for honesty is a phenomenon of important magnitude.

We interpret the evidence in this section as showing that a large share of taxpayers did not cheat, even when there was substantial tax savings to be had from an evasion opportunity available to anyone. Our view is that it was possible for *any* taxpayer to invent a name and use that name to claim a dependent exemption prior to 1987, with a low probability of detection. An alternative view is that while the opportunity to evade in this manner existed for everyone, it was not an equal opportunity, but rather was easier for certain types of people. In particular, cheating in this way may have had lower costs for those who provided some level of care or support for a real child. Even so, much of the literature has focused on evasion opportunities that are strictly limited to itemizers or those with self-employment income, while our analysis applies to a broader slice of the taxpayer distribution.

5.1 Might audit risk explain a low prevalence of cheating?

Above, we calculate the tax value from cheating without accounting for the possibility of being caught and fined. To adjust for risk, one must use a parameterized model of utility. To that end, we adapt the Allingham and Sandmo (1972) model of a risk averse taxpayer by supposing that the taxpayer faces a discrete choice about whether or not to evade and introducing a nonpecuniary cost of cheating.

We model the filer's utility function using the constant relative risk aversion formula for utility U from net income z with coefficient of relative risk aversion γ: $U(z) = \frac{z^{1-\gamma}}{1-\gamma}$. Filers make a single discrete choice. They can claim the appropriate number of dependents, in which case they receive their true after-tax income Y with certainty. Alternatively, filers may choose to claim an additional, inappropriate dependent, which may reduce their taxes paid by amount ΔT but also triggers a psychological cost of cheating θ, measured in dollars. If a cheating filer is caught, which happens with probability p, he incurs an additional monetary penalty $\phi \Delta T$, which is a proportion of the taxes evaded.

In this model, an individual i will cheat if and only if:

$$(1-p)\frac{(Y_i+\Delta T_i-\theta_i)^{1-\gamma}}{1-\gamma}+p\frac{(Y_i-\phi\Delta T_i-\theta_i)^{1-\gamma}}{1-\gamma}>\frac{Y_i^{1-\gamma}}{1-\gamma}, \tag{1}$$

which says simply that the individual will evade when the expected payoff to cheating is greater than the expected payoff to being honest. For any given set of parameter values, there will be some value of θ, call it θ^*, that would make the taxpayer indifferent. If θ_i is greater than θ^*, then person i will not evade because the psychological cost makes honesty preferable.

This parameterization is useful in allowing us to estimate the quantitative importance of risk and in providing a precise definition of the honesty premium we describe above. Our calculations above, which show the amount of money foregone by an honest taxpayer, can be interpreted as saying that for any individual i who chooses to be honest, their θ_i is greater than the foregone tax savings we calculate. When evasion is a discrete choice, what we estimate from observing an honest taxpayer is a lower bound on his willingness to pay to be honest.

To parameterize our model, we substitute in values for income, tax savings, probability of audit, evasion penalties, and risk aversion. Then we calculate the value of θ^*, which tells us the psychological aversion to cheating that would make an individual indifferent to evasion given the other values. If this number is close to the nominal tax savings, then it implies that risk has little impact on the calculation, and that the nominal tax savings we presented above are good estimates of a lower bound on the psychological aversion to cheating. For heuristic value, we also assume that $\theta=0$, that there is no willingness to pay to be honest, and calculate the probability of audit required to make an individual indifferent to cheating, labeled p^*. With no honesty premium, an individual will cheat if they believe p to be below p^*.

Table 3 presents results for four scenarios. For Y, we use \$21,670, the mean after-tax income in our 1986 sample of filers who did not lose a dependent in 1987. Tax savings ΔT is set equal to the mean in our sample for these filers, \$275. The fraction of tax returns audited in 1986 was around 2 %, so we set $p=0.02$ as our probability of detection.[22] We set $\gamma=2$, which is close to estimates in Chetty (2006) and Attanasio and Paiella (2011). There are two classes of penalties for underpayment of taxes. Any part of underpayment due to "negligence or disregard of rules or regulations" is subject to a 20 % penalty, while any part of underpayment attributable to fraud is subject to a 75 % penalty. If taxpayers can claim plausible confusion about their mispayment, they will pay the 20 % rate, which applies to the vast majority of cases. We use this as our baseline, so $\phi=0.2$. Throughout this exercise, we assume filers are not cheating in any way other than misclaiming a dependent.[23]

[22] Between 1977 and 1986, the audit rate gradually fell from 1.88 % to 1 % (Dubin et al. 1990). Between 1988 and 1995, the audit rate ranged between values of 0.92 % and 1.67 % (General Accounting Office 1996).

[23] If filers are cheating on other margins, this creates background risk, which will have a small effect on our calculations. The effect is small because this other risk is born whether the individual decides to claim a false dependent or not. So long as the probability of audit is not a function of claiming a false dependent

Table 3 Sample values of the role of risk

Y	ΔT	γ	p	ϕ	θ^*	p^*
21,670	275	2	0.02	0.2	268	0.83
21,670	275	2	0.02	0.75	265	0.57
21,670	275	10	0.02	0.75	264	0.54
21,670	275	10	0.20	0.75	170	0.54

Table shows pretax income Y, the change in taxes paid from evasion ΔT, the coefficient of absolute risk aversion γ, the probability of audit p, the IRS penalty as a fraction of taxes evaded ϕ, the break-even willingness to pay to be honest, which makes a taxpayer indifferent between cheating and not θ^*, and the probability of audit that would make the taxpayer indifferent between cheating and not if $\theta = 0$, p^*

The first row of Table 3 shows our baseline estimates. An individual who could save \$275 in taxes by improperly claiming a dependent would be willing to do so as long as his psychological cost of cheating is below \$268. This suggests that, for our baseline parameters, risk is not an important explanation for why so many taxpayers failed to cheat. Another way to see this is to calculate the probability of audit that would make a filer indifferent between cheating and not cheating (p^*), assuming $\theta = 0$. For our baseline case, that break-even probability is an extremely high 83 %: If the taxpayer has no aversion to cheating ($\theta = 0$), then he has to believe that the probability of audit is at least 83 % to not cheat.

For robustness, we vary the risk coefficient γ, the probability of audit p, and the penalty fraction ϕ. We show a subset of representative results in Table 3. The only parameter that substantially changes the value of cheating is the probability of audit.[24] Combined with high risk aversion and criminal penalties, if filers believe the probability of being caught is 20 %, instead of 2 %, then the payoff to a nominal \$275 of savings falls to \$170. Even higher audit probabilities further erode the value of cheating, but they must be above 50 % for cheating to become a net negative proposition when $\theta = 0$, even with high penalties and risk aversion.

Might taxpayers believe the probability of audit is extremely high? Coarse information about perceived audit probabilities is available from the 1987 Taxpayer Opinion Survey. When asked about the likelihood of their 1986 returns being audited, 35 % of respondents said it was not very likely, 42 % said it was highly unlikely, and another 15 % were not sure.

This framework formalizes the economic interpretation of our data from the 1986 reform. Those people who did not cheat must have a $\theta_i \geq \theta^*$, where θ^* will be roughly 97.5 % of the estimated tax savings in Table 2 for our base case, and well over 90 % for even the most extreme scenarios, unless taxfilers believe their proba-

(which is likely because so many people claim dependents that this cannot be a useful audit criterion), our simplification is an important restriction only if falsely claiming a dependent makes other cheating easier to detect, conditional on audit.

[24] Estimates of the coefficient of relative risk aversion based on observed equity return premiums are in the neighborhood of 50 (DeLong and Magin 2009). Even numbers in this range have almost no impact on our estimates.

bility of being caught is much higher than the actual audit rates. Based on this, we conclude that the taste for honesty is substantial, even accounting for risk.[25]

5.2 Might ignorance explain a low prevalence of cheating?

An alternative interpretation is that filers who did not claim fictitious dependents prior to the reform were not doing so out of love of country, but instead because the opportunity to cheat had simply never occurred to them. Unfortunately, there is no way to directly measure the number of people who lacked the cleverness to cheat. That said, this evasion opportunity is very simple and easy. The box for dependents claimed is right at the top of the 1040, as shown in Fig. 1. Moreover, if filers do not have a distaste for cheating, then we would expect them to invest effort in searching for low-hanging evasion fruit, like the dependent exemption. If filers were ignorant because they had no intention of cheating and, therefore, did not look for easy ways to cheat, then this is a round about way of affirming our hypothesis that they are willing to pay to be honest.

We suspect that people who claimed dependents at some point in time, and particularly those who had a change in the number of dependents, are aware of the tax value of dependents and the ease of claiming them. For filers who appear in all years of the tax panel, 66 % had a dependent at some point in time, and 59 % have a change in the number of dependents claimed during the sample period. Narrowing in on a group likely to be honest, filers who do not lose a dependent between 1986 and 1987, 34 % are claiming a dependent, and 32 % had changed the number of dependents before 1986. This suggests that there is a substantial opportunity to learn about the potential gains from claiming dependents and the relative ease of doing so.

In sum, there is no way to know whether any particular individual who failed to use the child loophole to lower his tax bill did so out of ignorance. Moreover, it is impossible to know what fraction of the honest taxpayers fall into that category. It seems likely, however, that filers with a thirst for cheating would have come across this possibility, particularly if they had experience with dependent exemptions, as most taxpayers do.

6 Who cheated and why?

6.1 Cheaters have differences in tax return characteristics

Who cheated? Are cheaters different from honest payers in observable characteristics? To answer such questions, we would ideally like to know who cheated in pre-reform years, and compare their characteristics to honest taxpayers. Our panel data allow us to identify those who lost a dependent in 1987, but there is no immediate

[25] In principle, estimates of the *distribution* of the willingness to pay to be honest could be estimated by comparing the amount of cheating at different tax values, given an additional assumption about the joint distribution of ΔT and θ. We pursued this empirical strategy but determined that the available tax panel data are of insufficient sample size given the observed variation in tax values to produce meaningful estimates.

way to distinguish those who lost a dependent for legitimate reasons from cheaters who reacted to the reform.

Fortunately, we can detect large differences in observable characteristics between cheaters and noncheaters by looking at changes in summary statistics across treatment years, where the number of cheaters present in particular subsamples varies widely and measurably. We pursue three different comparisons that use this basic approach. All three combine mild distributional assumptions with estimates of the number of cheaters in particular subsamples (taken from changes in observable statistics in response to the policy reform) to uncover facts about the characteristics of cheaters. These approaches may prove useful in other contexts. Here, they are especially compelling because all three approaches yield similar estimates, indicating that cheaters were more likely to be heads of household, less likely to be married filing jointly, and more likely to claim the child care credit. The main limitation to our exercise is that we are restricted to the variables that appear on tax returns, which excludes interesting demographics like age, education and occupation.

First, suppose that cheaters and non-cheaters have different distributions for some characteristic X. If the set of 1987 dependent losers includes a substantial number of cheaters, then the value of taxpayer characteristics among those who lost a dependent will be different in 1987 than in earlier years. This assumes that anyone losing a dependent in an earlier year must be honest.[26] Specifically, the mean value of characteristic X among all those who lost a dependent in 1987, denoted μ_x, can be written as a weighted sum of the mean of X for cheaters μ_x^c and noncheaters μ_x^h, where the weights are equal to the probability that someone in the sample is a cheater ρ:

$$\mu_x = \rho\mu_x^c + (1-\rho)\mu_x^h. \tag{2}$$

This equation can be rearranged to form an equation for the difference in means between cheaters and noncheaters:

$$\mu_x^c - \mu_x^h = \left(\mu_x - \mu_x^h\right)/\rho. \tag{3}$$

Thus, given an estimate of the pooled mean μ_x, the mean of honest taxpayers μ_x^h, and the proportion of the 1987 sample that were cheaters ρ, we can calculate an estimate of the difference in mean characteristics between cheaters and noncheaters. We have a ready estimate of μ_x from our sample of returns, since it is simply the mean of all 1987 dependent losers. In Sect. 4 we estimated that 31 % of those losing a dependent in 1987 were cheaters, which provides an estimate of ρ.

To obtain an estimate of μ_x^h we use the mean of characteristic X for filers reporting a dependent loss in an earlier year. TRA86 created many changes in the tax code that might cause differences in observable characteristics. To avoid conflating

[26] Assuming that all 1986 dependent losers are honest ignores any anticipatory changes in dependent claiming behavior. This will be problematic if some individuals gave up improper dependent claiming in the year the SSN requirement was announced rather than in the following year when it was implemented. We have addressed this concern by instead using the filing units losing dependents in 1985 to estimate means for honest dependent losers. The results of this analysis are quite similar to what we report in columns 1 and 3 of Table 4.

Table 4 Summary statistics for filers losing a dependent

	Lost dep. in 1986 $(\widehat{\mu_x^h})$ (1)	Lost dep. in 1987 $(\widehat{\mu_x})$ (2)	Mean diff.: cheaters − honest $(\widehat{\mu_x^c - \mu_x^h})$ (3)	Among 1987 losers: Lost previously? Yes (Honest) (4)	No (Cheaters) (5)
% Married, year $t-1$	69.9	62.3*	−24.5	71.1	54.5*
% Head of household, year $t-1$	23.3	31.1*	25.2	23.5	37.7*
% with change in filing status	21.1	24.7	11.6	19.0	29.6*
% with change in state	3.6	4.2	1.9	3.3	4.9
Mean dependents, year $t-1$	2.1	2.3	0.6	2.1	2.4*
Mean Δ in number of deps.	−1.2	−1.3*	−0.3	−1.2	−1.4*
Zero dependents after loss	47.9	44.1	−12.3	47.3	41.3
Head of HH to single after loss	10.4	13.2	9.0	10.4	15.6*
Last dependent had no tax value	7.3	5.4	−6.1	3.6	7.0*
% with child care credit, year $t-1$	7.8	12.8*	16.1	10.1	15.1*
% with EITC, year $t-1$	13.1	14.8	5.5	13.4	16.1
% age 65 or older as of 1987	6.0	4.4	−5.2	4.2	4.7
% age 65 or older as of 1990	9.5	7.5	−6.5	8.0	7.0
Mean AGI ($), year $t-1$	35,695	35,550	−468	40,250	31,448*
% itemizing, year $t-1$	53.6	49.0	−14.8	56.5	42.3*
% contrib. IRA, year $t-1$	21.7	16.5*	−16.8	20.2	13.2*
% contrib. election fund, year $t-1$	31.7	25.2*	−21.0	21.7	28.3*
N	549	721		336	385

Dollar amounts are real 1990 dollars. An * indicates that the difference between adjacent columns is statistically significant at the 10 % level. Column 3 is the estimated difference in means between cheaters and honest taxpayers, equal to (column 2–column 1)/0.31. Column 4 is hypothesized to have a substantially higher percentage of honest taxpayers than column 5, so that the sign of the difference is indicative of the difference in means between groups

these changes with the differences between cheaters and non-cheaters, we measure the characteristics of the 1987 dependent losers in 1986, the year prior. For consistency, we compute means in the year *before* dependent loss for the comparison group of honest taxpayers. We present unweighted means because the OTPR panel is constructed from a nonstratified sample.

Table 4 shows the results of this exercise. Column 1 shows the sample mean of characteristics measured in 1985 for taxpayers who lost a dependent in 1986. This is our estimate of the mean characteristic for honest taxpayers who lose a dependent. Column 2 shows the mean characteristic on 1986 tax returns (before the reform), for those taxpayers who lost a dependent in 1987 (when the reform took effect). Asterisks indicate that this value is statistically different from column 1 at the 10 % level based on a simple comparison of means. Column 3 shows our estimate of the difference in each characteristic between cheaters and honest losers of dependents, based on Eq. (3) and assuming $\rho = 0.31$.

Column 3 of Table 4 shows that there are significant differences between cheaters and those who lost a dependent for legitimate reasons. In particular, cheaters were much less likely to be married filing jointly, and much more likely to file as head of household. Moreover, more cheaters experienced a change in their filing status in 1987. This is driven in large part by 1986 head of household filers shifting to the less generous single filing status in 1987, though the difference is statistically imprecise. Cheaters were also much more likely to claim the child care credit, which is a tax credit for child care expenses incurred to enable parents or guardians to work. Taken together, the evidence shows that cheaters gained not only from claiming an additional dependent exemption, but also from moving to the head of household rate schedule and claiming (possibly fraudulent) credits for child care expenses.[27] Cheaters were also less likely to contribute to an IRA and to make a contribution to the presidential election campaign fund (which is plausibly correlated with a sense of civic engagement). Other characteristics fail to show a statistically significant difference.[28]

A second, related method of identifying differences in characteristics of cheaters and honest taxpayers comes from delineating two subgroups within the set of filers who lost a dependent in 1987. We hypothesize that having lost a dependent in years prior to the reform, when there was no change in the enforcement technology for detecting misclaimed dependents, is an indication of honesty. Thus, the group of individuals who lost dependents in 1987 *and* in earlier years likely contains a high proportion of honest taxpayers. The group of individuals who lost dependents in 1987 but never before likely contains a higher proportion of cheaters. Table 4 shows the means across these two subgroups, which are of roughly equal size, in columns 4 and 5. Asterisks in column 5 indicate that there is a statistically significant difference at the 10 % level using a simple comparison of means across the two columns.

This alternative procedure shows the same qualitative results about filing status as the analysis in columns 1–3. First-time dependent losers were less likely to be married filing jointly in 1986, and more likely to be filing as a head of household. Many more first-time losers experienced a change of filing status in 1987. Similarly, significantly more first-time losers claimed the child care credit in 1986. This alternative comparison also suggests that differences in AGI, the fraction itemizing, the fraction contributing to an IRA, and the fraction contributing to the election fund are statisti-

[27]The IRS began requiring a Social Security number or taxpayer identification number for the care provider for anyone claiming the child care credit in 1989. As discussed in O'Neil and Lanese (1993) and cited by Slemrod and Bakija (2008, pp. 238–239), this reform was followed by a substantial increase in the number of claims of self-employment income; a response similar in spirit to what we analyze here.

[28]We have also replicated the comparisons in columns 1 and 2 of Table 4 for every other year in our sample, as a placebo test. Overall, we find that there are 21 cases where the adjacent year means are statistically different at the 10 % level, out of 137 pairwise comparisons. In only one other year (1982) is there a statistical difference in the percentage of married filers, and in only two (1982 and 1989) is there a difference in share of head of household filers. Thus, we do reject equality more often than would be predicted by pure sampling variation, but the rejection is not concentrated in our variables of interest, and we believe that other tax reforms in other years will cause some statistics to differ. There are a number of significant differences between the groups of filers losing dependents in 1982 and in each of the adjacent years. We suspect this reflects the tax reform act that took effect in 1982. There are four significant differences in the IRA contribution variable, likely reflecting changes in IRA contribution rules.

Table 5 Probit regression predicting dependent loss

	Coefficient	Coefficient on 1987 interaction
Married, year $t-1$	-0.026	0.006
	(0.031)	(0.068)
Head of household, year $t-1$	0.070**	0.184**
	(0.034)	(0.081)
Change in filing status	1.049***	0.234***
	(0.025)	(0.084)
Number of dependents, year $t-1$	0.185***	0.111***
	(0.006)	(0.023)
Any child care credit, year $t-1$	-0.543***	0.059
	(0.028)	(0.075)
Any EITC, year $t-1$	-0.059***	0.048
	(0.022)	(0.086)
Age 65 or older as of 1987	0.624***	0.016
	(0.036)	(0.150)
Any IRA contribution, year $t-1$	0.183***	-0.103
	(0.026)	(0.075)
Real AGI, year $t-1$ (1000 s)	0.0003**	-0.0003
	(0.0001)	(0.0006)
N	55032	

The sample includes filers at risk of losing a dependent between 1980 and 1987, those who claimed at least one dependent in the previous tax year. The dependent variable is a dummy variable coded as 1 if the filer lost a dependent between years $t-1$ and t. The omitted other marital status category contains mainly single filers and a handful of married filing separate returns. Slightly different definitions are used to determine when a child can be claimed as a dependent and when that child allows an unmarried parent to use the head of household filing status (Holtzblatt and McCubbin 2003). This results in about 4 % of filers claiming dependents while using single filing status. The *, ** and *** indicate statistical significance at the 10 %, 5 %, and 1 % levels

cally different. With the exception of election fund contributions, the differences go in the same direction as in the previous comparison of cheaters and honest filers.

To address likely correlations across taxpayer characteristics, we next estimate a probit regression predicting dependent loss. We pool filers who are observed in two consecutive years up to and including 1987, and restrict the sample to those claiming at least one dependent in the earlier year of each pair. We control for a number of taxpayer characteristics, and interact each of these characteristics with a dummy for 1987 observations. This allows us to test whether a given characteristic affects the probability of dependent loss *differently* in the year when many losses represent cheating. The results are shown in Table 5. In this specification, only measures of household composition appear to be associated with cheating. While filing as head of household, relative to filing as a single taxpayer, is always associated with a significantly higher probability of dependent loss, this effect is larger in 1987. The same is true for having experienced a change in filing status between the two consecutive tax years. Other characteristics appear to affect dependent loss in the same way in 1987 as in earlier years. Broadly, the results corroborate our conclusions above.

Third, we use a different decomposition to infer the difference in characteristics across honest taxpayers and cheaters. The prior two methodologies were premised on a comparison of taxpayers who lost a dependent at different times, under different enforcement regimes. We can also compare filers who lost a dependent to filers who gained a dependent.[29] We start by noting that the mean of some characteristic X, among those who gain a dependent (henceforth "a gainer") in a given tax year t, can be written as the weighted sum of the mean of that characteristic for cheaters and honest taxpayers, where the weight is the probability that a gainer in year t is a cheater:

$$\mu_{x,t}^{g} = \rho_t^g \mu_{x,t}^{g,c} + \left(1 - \rho_t^g\right)\mu_{x,t}^{g,h}. \tag{4}$$

Here, $\mu_{x,t}^{g}$ is the average characteristic for gainers in year t (which is directly observable), $\mu_{x,t}^{g,c}$ is the average characteristic for gainers who are cheaters in year t, $\mu_{x,t}^{g,h}$ is the average characteristic for gainers who are honest in year t, and ρ_t^g is the probability that a gainer is a cheater in year t. The same decomposition can be written for those who lose a dependent ("losers") in a given year: $\mu_{x,t}^{l} = \rho_t^l \mu_{x,t}^{l,c} + (1 - \rho_t^l)\mu_{x,t}^{l,h}$.

If we make assumptions about the stability of mean characteristics for cheaters and honest taxpayers, then a comparison of the change in mean characteristics of gainers and losers spurred by a change in enforcement (which is observable) can be used to infer the difference in mean characteristics of cheaters and honest types (which is not directly observable). Specifically, we assume that cheaters have the same characteristic mean, in both 1986 and 1987, regardless of whether they are gainers or losers. That is, $\mu_{x,87}^{g,c} = \mu_{x,87}^{l,c} = \mu_{x,86}^{g,c} = \mu_{x,86}^{l,c} \equiv \mu_x^c$. We make a parallel assumption for honest taxpayers: $\mu_{x,87}^{g,h} = \mu_{x,87}^{l,h} = \mu_{x,86}^{g,h} = \mu_{x,86}^{l,h} \equiv \mu_x^h$. Under these assumptions, it is straightforward to show that the *difference* in the mean characteristics between gainers and losers can be written as

$$\mu_{x,t}^{g} - \mu_{x,t}^{l} = \left(\rho_t^g - \rho_t^l\right)\left(\mu_{x,t}^{c} - \mu_{x,t}^{h}\right). \tag{5}$$

This same expression can be calculated for years before and after a reform that changes the probability that gainers and losers are cheaters in estimable ways. Writing out the difference-in-difference between characteristics of gainers and losers across years $((\mu_{87,t}^{g} - \mu_{87,t}^{l}) - (\mu_{86,t}^{g} - \mu_{86,t}^{l}))$ using Eq. (5) and rearranging yields an expression for the difference in means across cheaters and honest taxpayers, our object of interest:

$$\mu_x^c - \mu_x^h = \frac{(\mu_{x,87}^{g} - \mu_{x,87}^{l}) - (\mu_{x,86}^{g} - \mu_{x,86}^{l})}{(\rho_{87}^g - \rho_{86}^g) - (\rho_{87}^l - \rho_{86}^l)}. \tag{6}$$

The left-hand side of Eq. (6) is exactly the same object as the one estimated by our first method and shown in column 3 of Table 4. The numerator on the right-hand

[29] We are especially grateful to Damon Jones for suggesting this approach.

side is the difference-in-difference across gainers and losers from 1986 to 1987. Each object in the numerator is directly observable. The denominator is the difference-in-difference in the probability that gainers and losers are cheaters between 1986 and 1987. Intuitively, this inference procedure examines the difference-in-difference in a characteristic across gainers and losers—groups whose fraction of cheaters should change sharply in 1987. The difference-in-difference inflated by the change in fraction of cheaters present in each pool yields an estimate of the difference between cheaters and honest taxpayers.

The probabilities in the denominator are not observable directly, but they can be inferred with relative precision under the assumption that the only reason that there were more losers and fewer gainers in 1987 was the reaction of cheaters to the introduction of the SSN requirement for dependents. Then the change in the number of taxpayers gaining or losing a dependent provides an estimate of the *change* in the probability that a gainer (loser) was a cheater.[30] Proceeding under that assumption, we can use Eq. (6) to calculate an estimate of the mean difference in characteristics across cheaters and honest taxpayers.

Table 6 shows the results of this exercise. Our previous estimates of $\mu_x^c - \mu_x^h$, from Table 4, are shown in column 1. Several characteristics from Table 4 are omit-

Table 6 Differences between cheaters and honest taxpayers

	Based on timing of dependent loss (1)	Based on comparison of gainers and losers (2)
% Married, year $t-1$	−24.5	−29.8
% Head of household, year $t-1$	25.2	34.6
% with change in filing status	11.6	14.6
% with change in state	1.9	−6.0
Mean dependents, year $t-1$	0.6	0.9
% with child care credit, year $t-1$	16.1	7.4
% with EITC, year $t-1$	5.5	13.7
% age 65 or older as of 1987	−5.2	−8.3
% age 65 or older as of 1990	−6.5	−9.8
Mean AGI ($), year $t-1$	−468	−4,723
% itemizing, year $t-1$	−14.8	−27.1
% contributing to IRA, year $t-1$	−16.8	−26.2
% contributing to election fund, year $t-1$	−21.0	−25.0

Column 1 repeats the estimate of the difference in the mean of the characteristic for cheaters and honest types from Table 4. The second column is an estimate of the same parameter (the difference in the mean of the characteristic for cheaters and honest types) based on the difference-in-difference statistic from Eq. (6)

[30] An assumption about exactly how many cheaters were in each group in 1986 is still required because this influences the denominator in the calculations. We assume that no one losing a dependent in 1986 is a cheater, and that no one gaining a dependent in 1987 is a cheater. Modifying these assumptions has a very modest impact on our estimates.

ted because they apply only to dependent losers, and the second methodology requires means for gainers. The second column shows estimates from the difference-in-difference decomposition for dependent gainers and losers. Overall, the results are strikingly similar, given that they come from the comparison of different subsamples and are motivated by different assumptions. The sign of the estimated difference is the same for all characteristics except our measure of geographic mobility (changing states), which was not statistically different in our first comparison. Moreover, the magnitude of the difference is qualitatively similar for most variables. This reaffirms our conclusion that cheaters were less likely to be married, more likely to be heads of household and more likely to claim the child care credit.

Our first decomposition relied on the assumption that the mean characteristic for taxpayers who lost a dependent in 1986 was an unbiased estimate of the mean characteristic of honest taxpayers who lost a dependent in 1987. Our second method assumed that 1987 dependent losers were less likely to be cheaters if they had lost a dependent in the past. In our third decomposition, we instead make the assumption that honest taxpayers have the same mean characteristic in 1986 and 1987, regardless of whether they are gainers or losers. (As a reminder, we are measuring characteristics in year $t-1$, so the mean characteristics are *not* influenced by the changes in the tax code included in TRA86 in either method.) None of these assumptions is free of potential criticism, but the fact that all three provide similar results is compelling.[31]

We have documented a sharp decline in the number of dependents claimed in 1987 and noted that the decline is sharper for head of household filers than for others, using several strategies. We interpret this as evidence of cheating. An alternative interpretation is that individuals impacted by the policy were *unintentionally* claiming dependents incorrectly prior to the policy change. Changes coincident with the introduction of the SSN requirement caused individuals to realize their mistake and change their claiming behavior. Under this interpretation, the policy did indeed reduce incorrect tax claims, but our empirical estimates are not so much about honesty as they are about mistakes. Is this alternative likely?

We think the role of confusion is minor for two reasons. First, as discussed in Sect. 4.1, the IRS ran an information campaign and changed the 1040 instructions ahead of the 1986 tax filing season. If information were the key driver, we would expect to see a major dip in dependents claimed in 1986. Second, confusion is most plausible for divorced (or never married) parents of a child, where both parents try to claim the same child. We cannot identify such people in the data, but we can identify the set of people who have no changes in marital status throughout the panel, prior to the tax reform, whom we believe will be less susceptible to confusion. If we replicate the analysis in Table 4 for this sample, our results change very little, which weighs against confusion.

[31] We have calculated parallel comparisons that compare 1987 to pooled means across 1980–1986, which has the advantage of increasing the sample size. Most of our findings are robust to this change in comparison group, but the difference between the two methodologies is inflated for several characteristics, including claiming of the child care credit, itemizing, and IRA contributions. This may not be surprising, however, because there are other changes in the tax code in that time period which make assumptions about stability of means across different time periods much stronger than in the 2-year comparison.

6.2 Cheaters do not have particularly large tax benefits from dependents

What explains the fact that some people cheated on their taxes while others did not? Is the difference driven mostly by variation in the willingness to pay to be honest, or mostly by variation in the tax savings associated with claiming an additional dependent? One possibility is that there is a limited amount of variation in the willingness to pay to be honest. In the extreme, suppose that all people have the same willingness to pay to be honest. Then the only determinant of whether or not a person cheats is his tax gain from doing so. This implies that cheaters will have noticeably higher tax savings from the marginal dependent than will honest payers.

A second possibility is that there is considerable variation in the willingness to pay to be honest. In this scenario, the link between the monetary gain from cheating and the probability of cheating might be weak, depending on the correlation between tax savings (largely a function of income) and honesty. Assuming that tax savings and honesty are not too tightly correlated, as the variation in the willingness to pay to be honest rises, the relationship between cheating and tax savings from an additional dependent will dissipate. Thus, qualitatively, if there is a weak relationship between tax savings and cheating, we will conclude that heterogeneity in honesty is the primary determinant of cheating behavior, but we will conclude the opposite if the link is strong.[32]

To test whether or not tax savings are closely correlated with cheating, we first refer to Table 2, which shows the tax savings from changing the number of dependents for different subsamples. We compare the average tax savings of filers who lost a dependent in 1987, about one-third of whom are cheaters, to other groups of filers that contain few cheaters. Table 2 shows that those losing a dependent in 1987 had a mean tax savings for their last dependent claimed in 1986 of \$254 (the benefit from cheating for an observed cheater is the tax change associated with *losing* the marginal dependent claimed). Groups devoid of cheaters had similar, or even slightly larger, tax savings from cheating. Those who claimed no dependents in 1986 clearly were not cheating. The benefit from cheating for that group is equal to the value of *gaining* a first dependent, which is on average \$291 in 1986. Similarly, those who had a dependent in 1986 but did not lose a dependent in 1987 are unlikely to be cheaters. This group had a mean tax value of \$243. The last row in Table 2 shows these two groups combined. Together, they could have gained \$275 on average from claiming a false dependent. This indicates that cheaters and honest filers faced similar incentives.

An alternative approach involves comparison across years. The top panel of Table 7 presents the average tax savings associated with claiming a dependent in each year. The values in the $t-1=1986$ row of columns 2 and 4 contain a large number of cheaters. If cheaters have systematically higher tax savings from claiming a dependent, and if those who lost a dependent for legitimate reasons in 1987 had tax savings similar to those who lost a dependent in prior years, then we would expect

[32] Note that this setup assumes relatively little variation in risk preferences and the probability of audit. Because risk has little impact on the valuation of evasion, this portion of the assumption is innocuous, but if there is substantial heterogeneity in the subjective probability assigned to being audited, then this could also play a significant role in explaining the decision to evade.

Table 7 Tax savings from claiming a dependent, all years

Year $t-1$	Gain from one more dependent		Loss from one fewer dependent	
	No loss in t	Lost dep. in t	No loss in t	Lost dep. in t
	(1)	(2)	(3)	(4)
A. Mean dollar values				
1979	354	341	−414	−401
1980	324	319	−375	−366
1981	306	286	−348	−336
1982	272	261	−307	−287
1983	251	243	−285	−274
1984	235	231	−266	−256
1985	236	222	−271	−258
1986	243	**224**	−276	**−254**
1987	337	334	−413	−400
1988	315	290	−404	−361
1989	307	292	−404	−359
B. Median percent of AGI				
1979	1.1	1.1	−1.2	−1.2
1980	1.1	1.1	−1.2	−1.2
1981	1.1	1.0	−1.1	−1.1
1982	1.0	1.0	−1.0	−1.0
1983	0.9	0.9	−1.0	−1.0
1984	0.8	0.8	−0.9	−0.9
1985	0.8	0.8	−0.9	−0.9
1986	0.8	**0.9**	−0.9	**−0.9**
1987	1.0	1.1	−1.2	−1.2
1988	1.0	1.0	−1.1	−1.1
1989	1.0	1.0	−1.1	−1.1

Estimates are author's calculations using TAXSIM. Dollar amounts are in real 1986 dollars. Bold font values are samples with many cheaters

particularly high tax savings in the $t-1=1986$ sample. Such a pattern is not evident. This sample had an average tax value of $254 from losing their last dependent. This is nearly the same as the average value in the previous year, and is actually smaller than all of the values stretching back to the beginning of our sample in 1979. This is further evidence that cheaters and honest taxpayers had similarly-sized tax benefits from claiming a dependent.

The bottom panel of Table 7 repeats this exercise with savings as a percentage of AGI, to capture the possibility that lower income people are more responsive to a given level of tax savings. Because there are outliers in AGI, we report the median of the tax changes as a percentage of AGI. The patterns here are the same. If anything, 1986 appears to be a somewhat low value year, but the difference from prior years is not statistically significant.

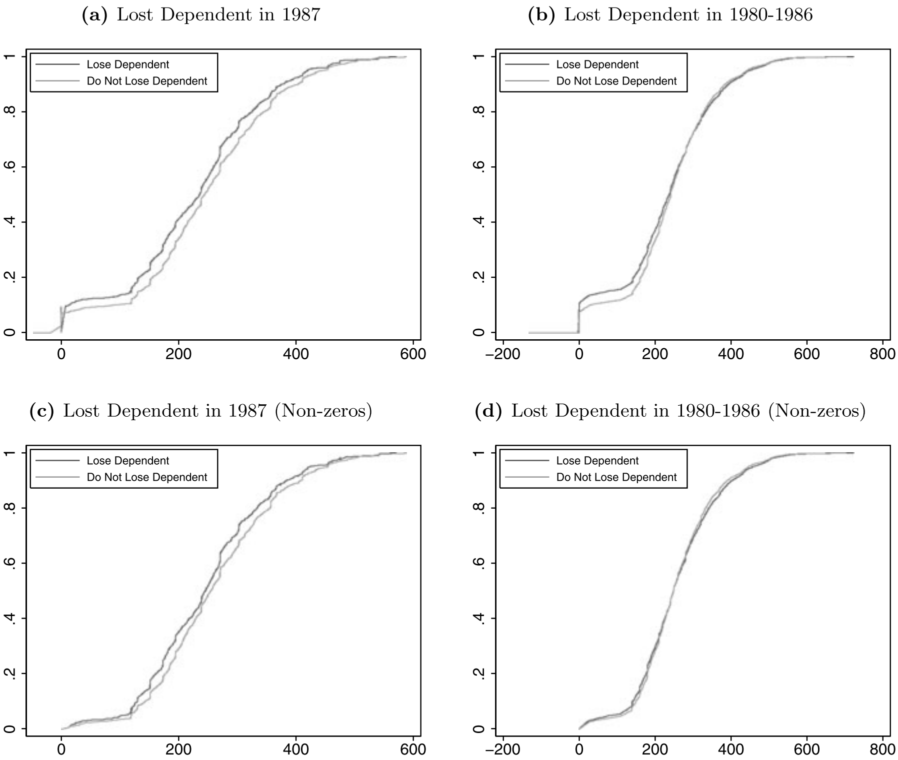


Fig. 7 Distribution of tax values. Figure shows empirical distributions of the tax savings associated with a marginal dependent for those losing a dependent and those not losing a dependent in the policy change year (**a**) and earlier years (**b**). Panels (**c**) and (**d**) show the same statistics for the subset of filers that have a nonzero tax value. All calculations are for tax values in year $t-1$, where t is the year when the dependent was lost

Figure 7 goes beyond comparisons of means by plotting the empirical cumulative distribution of tax values for relevant subsamples. Figure 7a shows the distribution for those who lost a dependent in 1987 and those who did not lose a dependent in 1987. If the driving force behind cheating is tax savings, we would expect that those who lost a dependent in 1987—many of whom were cheaters—would have higher tax values than those who did not cheat. This is not the case. The distribution for those losing a dependent lies above the distribution for those who did not lose a dependent, which implies that they had *lower* gains from claiming an additional dependent.

Figure 7b shows the same comparison for years before the policy reform. The distributions are fairly similar, though there is a difference in the number with a zero tax value—more of those who lost a dependent in pre-1987 years had a zero value from an additional dependent. To eliminate any differences in the distribution due strictly to the zero values, Figs. 7c and 7d show the same distributions, limited to those with positive estimated tax values. Now, the prereform years look very similar, which suggests that when there is no change in the incentive to cheat, the tax value distributions of those losing a dependent and those not losing are quite similar. Nevertheless, the policy reform year still shows a difference—those who lost a dependent had lower tax values.

Overall, this evidence suggests that cheaters did not have especially large gains from claiming additional children. This implies that the main driver of the decision to cheat was not the amount of money at stake. Since risk aversion plays a minor role in modifying valuations, it is implausible that risk aversion heterogeneity is a dominant driver, but we cannot rule out that heterogeneity in the perceived probability of audit and detection is key. It seems that either variation in the taste for honesty or variation in the perception of audit probabilities must be large.

7 Conclusion

The case of the missing children provides a unique opportunity for examining taxpayers' evasion behavior. Two main lessons emerge from our analysis. First, this episode demonstrates that while a substantial number of taxpayers lied to the IRS in order to save on their taxes, a much larger fraction of taxpayers passed up an easily accomplished evasion opportunity. These honest taxpayers gave up hundreds of dollars of tax savings, which amounted to an average of 7 % of their tax bill. This choice reveals a substantial and widespread willingness to pay to be honest. We think this is an important addendum to recent research that concludes that evasion is driven primarily by access to evasion opportunities and monitoring efficacy, as in Kleven et al. (2011). The evidence here suggests a greater role for a preference for honesty.

Second, the available evidence indicates that heterogeneity in willingness to cheat outweighed heterogeneity in the tax savings associated with children in determining who cheated and who did not. Cheaters do appear to be different from honest taxpayers in some key characteristics, like filing status, but these differences do not translate to substantial differences in the average tax savings from claiming children. This suggests that something other than variation in the tax benefit of a child plays an important role in determining who cheated and who did not. We interpret this as evidence that there is important variation in the willingness to pay to be honest.

Finally, we believe that our methodology for inferring the characteristics of cheaters, our use of a natural experiment, and our analysis of an evasion opportunity available to virtually all taxpayers constitute contributions to the larger literature on the economics of income tax evasion. In particular, our work contributes to the emerging stream of research that studies "traces of evasion" (Slemrod and Weber 2012) by showing an additional way of learning about evasion in the absence of audit data.

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