NBER WORKING PAPER SERIES

THE DETERMINANTS OF INCOME TAX COMPLIANCE: EVIDENCE FROM A CONTROLLED EXPERIMENT IN MINNESOTA

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Working Paper 6575
http://www.nber.org/papers/w6575

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
May 1998

We would like to thank the employees of the Minnesota Department of Revenue -- in particular Gerald Bauer, Bob Cline, Steve Coleman, Mary Kim, and Carole Wald -- who initiated and executed the experiment described in this paper. Expert research assistance was provided by Jon Bakija. Helpful comments on an earlier draft were received from Alan Macnaughton, Lillian Mills, Ann Dryden Witte, and attendees of workshops at the University of Michigan, the National Bureau of Economic Research, Northwestern University, Dartmouth College, University of Oklahoma, Texas Tech University, Arizona State University, and the University of Illinois. Any opinions expressed are those of the authors and not those of the National Bureau of Economic Research.

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NBER Working Paper No. 6575
May 1998
JEL No. H26

ABSTRACT

This paper reports on the results of a controlled experiment in Minnesota in which a random sample of taxpayers was informed that their income tax returns would certainly be closely examined. We analyze reported income of this sample of taxpayers, reported income on their previous year's returns, and reported income from the two corresponding years' returns of a control group of taxpayers that did not receive the letter.

We find that the treatment effect varies depending on the level of income. Low and middle income taxpayers increased reported income and tax liability relative to the control group, which we interpret as indicating the presence of noncompliance. The effect was much stronger for those with more "opportunity" to evade, as measured by their source of income. However, the reported income of the high-income treatment group *fell* sharply relative to the control group. We suggest a model based on tax audits as a negotiation that can explain this apparently perverse result.

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1. INTRODUCTION

Tax evasion is a quantitatively significant phenomenon that affects the equity, efficiency, and simplicity of a tax system. Because most taxpayers will not voluntarily pay taxes in the absence of an enforcement mechanism, the potential for evasion must be considered in the design of tax structure. The phenomenon of evasion also raises challenging questions about the appropriate design of the tax enforcement agency itself: how many resources should be devoted to auditing suspected evaders; how should these resources be allocated across classes of taxpayers; how many resources should be devoted to taxpayer assistance rather than monitoring; can evasion be reduced by appeals to taxpayers' conscience, or sense of duty?

The formulation of informed policy in these areas has been hampered by a paucity of reliable quantitative information on the likely effects of these alternative policies. The critical information is to what extent taxpayers would alter their compliance behavior in response to the set of possible policy alternatives. As we will argue below, existing empirical work--statistical, and experimental--is plagued by serious enough problems that the findings are subject to considerable doubt.

In this paper we discuss the results of a controlled experiment regarding income tax compliance that is not subject to the biases of other approaches to investigating the determinants of income tax compliance. In 1995 the Minnesota Department of Revenue conducted a series of income tax compliance experiments to test alternative strategies for improving tax administration and increasing voluntary compliance.[1] Approximately 47,000 Minnesota taxpayers who filed 1993 income tax returns were selected at random for one of five experimental 'treatments' that were administered at the beginning of the filing season for 1994 returns.

One treatment group was offered enhanced taxpayer assistance, including help with their *federal* return that was not previously offered by the State. Another received a redesigned Minnesota income tax return form with additional line items for reporting Minnesota adjustments to federal taxable income. Two additional large groups received 'educational' letters from the Commissioner of Revenue that appealed to their sense of equity or to their sense of social norms. Finally, a fifth group of 1,724 was informed by the Commissioner that the returns they were about to file, *both state and federal*, would be 'closely examined.' It is this last experimental treatment that is the focus of this paper. This is an especially interesting experiment because, under certain assumptions, the response of taxpayers provides an estimate of the extent of tax evasion.

The Department of Revenue sampled data from 1993 Minnesota income tax returns as they were filed during calendar year 1994. The 1993 data were matched to corresponding data from the 1994 returns of the same taxpayers after the experimental intervention. These two years of data from the same taxpayers enable comparisons of changes in reported

[1] The Department of Revenue contracted with an 'Advisory Board' of five academics in 1993 to assist in the experimental design and the subsequent data analysis. The Board was composed of the authors of this paper plus Kinley Larntz of the University of Minnesota and Daniel Nagin of Carnegie Mellon University. The Advisory Board met regularly in St. Paul with Department of Revenue executives and staff members and with representatives of the IRS St. Paul District during the design and administration of the experiment until the conclusion of the project in June, 1996. The Department of Revenue issued a report on the experiment in April, 1996. The Federation of Tax Administrators awarded the experiment its 1996 Award for Outstanding Research and Analysis in State Tax Administration. Coleman (1997) describes the experiment and the Minnesota

income, deductions, and tax liability between those taxpayers who received the treatments and similar groups of taxpayers who were not subject to any treatment (the control groups). Data from the 1993 and 1994 *federal* tax returns of the subjects of the audit experiment, both those who received the treatments and those who served as controls, were also made available. Data from the federal income tax returns include far more detail than the state tax returns because the Minnesota state income tax return is based on federal taxable income.

We find that the treatment effect varies depending on the level of income. Low and middle income taxpayers in the treatment group increased reported tax between 1993 and 1994 relative to the control group, which we interpret as indicating the presence of noncompliance. The effect was much stronger for those with more 'opportunity' to evade, as measured by their source of income. However, the reported tax liability of the high income treatment group *fell* sharply in 1994 relative to the control group; again, the effect was larger for those with 'opportunity.' We suggest a model that can explain this apparently perverse response.

2. THEORY

Suppose that the true tax base is not costlessly observable to the tax collection agency, although known to the taxpayer.[2] Then, under certain circumstances, the taxpayer may be tempted to report a taxable income below the true value. In the seminal formulation of Allingham and Sandmo (henceforth A-S) (1972), what might deter an individual from

Department of Revenue interpretation of the findings, which is similar but not identical to this paper's. Our views should not be taken to represent the views of the Minnesota Department of Revenue.

[2] More elaboration and qualification of this model can be found in Slemrod and Yitzhaki (1997).

income tax evasion is a fixed probability (p) that any taxable income understatement will be detected and subjected to a penalty (θ) over and above payment of the true tax liability itself.

In the A-S model, all real decisions, and therefore taxable income (y), are held fixed; only the taxpayer's report is chosen. The taxpayer chooses a report (x) and thus an amount of evasion $y - x$, in order to maximize

$$(1) \qquad EU = (1 - p)U(\nu + t(y - x)) + pU(\nu - \theta(y - x)),$$

where ν is true after-tax income, $y(1 - t)$, t being the rate of (proportional) income tax. Thus the choice of whether and how much to evade is akin to a choice of whether and how much to gamble. Each dollar of taxable income understatement offers a payoff of t with probability $(1 - p)$, but a penalty of θ with probability p. If and only if the expected value of this gamble, $(1 - p)t - p\theta$, is positive, a risk-averse taxpayer will chance some evasion, with the optimal amount depending on the expected payoff and the taxpayer's risk preferences.

Yitzhaki (1974) amended the A-S formulation to allow the penalty for discovered evasion to depend on the *tax* (not, as in A-S, the income) understatement, as more accurately reflects practice in many countries. In this case, the maximand becomes $(1 - p)U(\nu + t(y - x)) + pU(\nu - \theta t(y - x))$. This is a critical change, because it means that the tax rate has no effect on the terms of the tax evasion gamble; as t rises, the payoff to a successful understatement of a dollar rises, but the cost of a detected understatement rises proportionately. This is seen by examining the first-order condition for optimal evasion, which becomes

$$(2) \quad \frac{U'(y_A)}{U'(y_U)} = \frac{(1-p)}{p\theta},$$

where y_U and y_A refer to net income in the unaudited and audited states of the world, respectively.

Regardless of whether the penalty depends on the tax understatement or income understatement, more risk-averse individuals will, *ceteris paribus*, evade less. Individuals with higher income will evade more as long as absolute risk aversion is decreasing; whether higher-income individuals will evade more, as a fraction of income, depends on relative risk aversion. Evasion relative to income will decrease, increase or stay unchanged as a fraction of income depending on whether relative risk aversion is an increasing, decreasing, or constant function of income. Regardless of preferences, increases in either p or θ will decrease evasion. *If p equals one, any rational taxpayer will report his or her true income.*

This model predicts that a risk-neutral individual would either, if the evasion has a positive expected payoff, remit no tax at all, or if evasion had a negative expected payoff, do no evasion. Either of two variations in the model can eliminate these 'either-or' predictions. In the first variation the probability of detection is an increasing function of the amount of evasion. The implications depend on the precise relationship between p and evasion. For example, consider the case (discussed in Yitzhaki (1987)) where a risk-neutral taxpayer confronts p being an increasing function of evaded income[3]. If $s = t(y - x)$, understated tax, then expected income is

$$\text{(3)} \qquad EY = (1-p[y - x])(\nu + s) + p[y - x](\nu - \theta s)$$

If $p' \equiv \partial p / \partial(y - x)$ is zero, then the risk-neutral taxpayer evades an unlimited amount as long as it has positive expected value, i.e., when $(1 - p) - p\theta > 0$. When p' is non-zero, the first-order condition becomes

$$\text{(4)} \qquad 1 - p - p\theta = p'(\theta + 1)(s / t).$$

In this case, evasion will be constrained by the fact that p increases to offset what would otherwise be an increase in expected income.

In the second variation there are distinct sources of income, each of which is subject to its own p. For example, employee labor income has a high p (due to information reporting by employers and computer matching), while 'moonlighting' income has a much lower p. Faced with this situation, a risk-neutral individual reports all or none of each of the several sources of income, but may certainly report a fraction of total income.

As has been pointed out by Skinner and Slemrod (1985) and others, these simple models are not adequate for explaining many aspects of noncompliance behavior. A host of extra-economic models have been offered, surveyed in Fischer, Wartick and Mark (1992) and

[3]The endogenous probability of detection can of course be applied to the case of a risk-averse taxpayer. In this case, at the margin the gain in expected value is offset by a combination of increased risk-bearing and an increased probability of detection. Cremer and Gahvari (1994) generalize this notion by introducing what they call a 'concealment technology,' which takes the form p(e, e/y, m), where e is the amount of income not reported (y - x in the notation used above), e/y is the ratio of unreported income to true income (which is endogenous in their model), and m is taxpayer expenditure on concealment.

Roth, Scholz, and Witte (1989). These perspectives add much insight into the problem of tax noncompliance, but are unlikely to significantly disturb the principal predictions that are relevant to this experiment: that, in the face of certain detection cum penalty, evasion is likely to be negligible, and that increases in the likelihood of detection with penalty will decrease evasion.

3. REVIEW OF EXISTING EMPIRICAL LITERATURE

3.1 Data problems

Ascertaining the extent, characteristics, and determinants of evasion immediately runs into two problems -- one conceptual and one empirical. The conceptual problem is that, although one can assert that legality is the dividing line between evasion and avoidance, in practice the line is often blurry; sometimes the law itself is unclear, sometimes it is clear but not known to the taxpayer, sometimes the law is clear but the administration effectively ignores a particular transaction or activity.

The other difficulty is that, by its nature, tax evasion is not easy to measure -- merely asking just won't do. Several different approaches have been attempted. One approach relies on inferring the level or trends in noncompliance from data on measurable quantities, such as components of the money supply or national income and product accounts. The monetary indirect estimates are based on the presumption that most unreported economic activity takes place in cash; discrepancies between actual currency in use and a forecast based on historical relationships is assumed to track the underground economy. This approach is not reliable, however, as its accuracy depends entirely on how well the demand for cash is estimated, and estimates of money demand are subject to substantial forecasting error. The indirect noncompliance estimates based on discrepancies between national income accounts, and income reported to the tax authority, are subject to similar, although less serious, problems.

The most reliable source of information about tax compliance concerns the U.S. federal income tax, and exists because of the IRS's Taxpayer Compliance Measurement Program (TCMP). Under this program, approximately every three years from 1965 until 1988 the IRS conducted a program of intensive audits on a large stratified random sample of tax returns, using the results to develop a formula used to inform the selection of returns for regular audits. The TCMP data consist of line-by-line information about what the taxpayer reported, and what the examiner concluded was correct. This data formed the basis for the IRS estimates of the aggregate 'tax gap' and provide much useful information about the patterns of noncompliance with respect to such variables as income, occupation, line item, region of the country, age, and marital status. While informative, it is widely recognized that even the intensive TCMP audits imperfectly reveal particular kinds of noncompliance, such as income from the underground economy.

The TCMP studies suggest the following set of stylized facts:

-- With audit coverage hovering around 1% and an extensive information reporting and matching program, the tax gap is estimated to be 17% of true tax liability. However, much of this tax gap refers to nonfilers and to estimates of noncompliance undetected by the TCMP. The TCMP-detected rate of noncompliance is 7.3%.

-- The extent of evasion varies widely across types of gross income and deductions; for example, the 1988 TCMP indicates that the voluntary reporting percentage was 99.5% for wages and salaries, but only 41.4% for self-employment income (Schedule C). These percentages clearly vary positively with the likelihood of income understatement being detected.

-- The fraction of income that is underreported declines with income. For example, Christian (1994) reports that, in 1988, taxpayers with (audit-adjusted) incomes over $100,000 on average reported 96.6 percent of their true incomes to the IRS, compared to 85.9 percent for those with incomes under $25,000.

-- Within any group defined by income, age, or other demographic category, there are some who evade, some who do not, and even some who (presumably inadvertently) overstate tax liability. For example, of middle-income (income between $50,000 and $100,000) taxpayers in 1988, 60% understated tax, 26% reported correctly, and 14% overstated tax (Christian, 1994, p. 39).

3.2 Determinants of Evasion

In sum, evasion is quantitatively significant and, furthermore, patterns in the compliance rate across items suggest that evasion can be quite responsive to the costs and benefits associated with it. However, empirical attempts to establish more systematically how compliance responds to aspects of the tax environment have met with limited success, primarily due to inherent data problems.[4] Cross-sectional and time-series data analysis, plus laboratory experiments, have been carried out.

3.2.1 Cross-sectional analysis

Clotfelter (1983) was the first attempt to make use of the TCMP data to investigate how noncompliance responded to changes in the environment. He estimated a tobit model, explaining, for each of ten audit classes, noncompliance as a function of the combined federal and state marginal tax rate, after-tax auditor-adjusted income, and a set of demographic variables available on tax returns. The most striking conclusion is that noncompliance is strongly positively related to the marginal tax rate, with the elasticity ranging from 0.5 to over 3.0. He did not, though, investigate the impact of the probability of detection on noncompliance, arguing that calculated arrest and conviction rates would probably not correspond closely to the perceptions of would-be evaders and would, in any event, not be exogenous; his regressions were carried out separately by audit class.[5]

Subsequent studies have produced mixed results. Of particular interest is work by Feinstein (1991), who performed a pooled cross-section analysis of 1982 and 1985 TCMP data, thus mitigating the problem that in a single cross-section (other than for cross-state differences) the marginal tax rate is a (complicated, non-linear) function of income, making it difficult to separately identify the tax and income effect. Feinstein's analysis suggests a negative impact of the marginal tax rate on evasion, contradicting Clotfelter's results but not inconsistent with the A-S model as adjusted by Yitzhaki. He does not, though, explicitly introduce the probability of audit.

Beron, Tauchen, and Witte (1992) investigate TCMP data aggregated by the IRS to the three-digit zip code level. In an attempt to deal with the problem of the endogeneity of a measure of p, they model the simultaneous determination of tax reporting and the log odds of

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[4] Harvey Galper once put the problem this way: "Regression analysis of tax evasion is straightforward, except for two problems: you can't measure the left-hand side variable, and you can't measure the right-hand side variables!"

[5] The IRS separates individual income tax returns into classes based on income and type of income, and occasionally releases the average audit rate by class.

an audit for each of several audit classes in each zip code area.[6] Their instrument for this is the level of IRS resources relative to the number of returns.[7] Of course, this may vary if the IRS targets its resources toward areas believed to be particularly noncompliant, thus invalidating its use as an instrument; Beron, Tauchen, and Witte argue that because the IRS has not been able to distribute its resources among districts so as to achieve its goals, it is a valid instrument. They find that increasing the odds of an audit significantly increases reported AGI and tax liability for some, but not all of the groups.

3.2.2 Time-series analysis

Dubin, Graetz, and Wilde (1990) make use of state-level time series cross-section data from 1977 through 1985 to investigate the impact of audit rates and tax rates on tax compliance. They do not, though, have a direct measure of noncompliance, but instead use tax collections per return filed and returns filed per capita as (inverse) measures of noncompliance. They conclude that the continual decline in the audit rate over this period caused a significant decline in IRS collections -- amounting to $41 billion by 1985. Note, though, that their measure of noncompliance will be affected by changes in the tax law as well as other changes in the economy, and their measure of the probability of detection is subject to the same endogeneity problems as the cross-sectional analyses.

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[6] For data availability reasons they do not consider the high-income groups (those with incomes over $50,000 in 1969 dollars).

[7] Dubin and Wilde (1988) perform a similar analysis on the zip-code-aggregated data, and use the same instrument. They defend this choice by claiming that, in an analysis of the time path of state-level IRS budgets, they were found to be independent of compliance levels, and predominantly determined by the share of total returns filed.

3.2.3 The Promise of a Controlled Experiment

A generic problem with both the time-series and cross-sectional studies is that the probability of detection (p) is difficult to measure and, furthermore, its variation may not be random but rather a response by the IRS to perceived variations in the extent of evasion or effectiveness of enforcement. The virtue of a controlled experiment is that the source of variation in p is unambiguous and is certainly not a response to the environment. Controlled experiments have been used fairly extensively since the late 1960's to investigate a wide range of economics issues; their strengths and weaknesses have been nicely surveyed by Burtless (1995) and Heckman and Smith (1995). Their applicability to tax compliance research was discussed in a paper by Boruch (1989) for the National Research Council's Panel on Taxpayer Compliance Research, which recommended that the IRS and external researchers collaborate to expand the use of field experiments to analyze the compliance effects of innovations in tax administration (Roth, Scholz, and Wilde, 1989, p.229).

One early example of this work is that of Schwartz and Orleans (1967), who, in cooperation with the IRS, contacted randomly selected groups of taxpayers with income generally above $10,000 during the month prior to filing their 1962 income tax returns. They asked 89 taxpayers questions that 'emphasized the severity of sanctions available to the government and the likelihood that tax violators would be apprehended' (the sanction group). Another group of 92 was asked questions focusing on moral reasons for compliance (the conscience group). A 'placebo' group of 92 was asked basic interview questions, and a fourth group of 111 served as an 'untreated' control. The empirical analysis was based on the change between 1961 and 1962 in reported AGI, total deductions, and income tax after credits. For AGI and tax after credits, only the 'conscience' group reported a larger increase in tax than the 'placebo' or 'untreated' control groups. Contrary to expectation, the 'sanction' group reported a larger increase in total deductions than the 'placebo' group. The authors speculate that subjects may have responded as if thinking, "You may beat me into admitting higher income, but I'll find a way of getting it back."

The present experiment differs fundamentally from Schwartz and Orleans because taxpayers were contacted by the taxing authority rather than the experimenters, and they were notified that their return would be 'closely examined.' Both make the present methods more appropriate for testing for the effects of enforcement actions on reporting behavior.

4. DESIGN OF THE EXPERIMENTS

4.1 Sample Design

The subjects for this experiment were selected randomly, subject to certain restrictions. Sampled were full-year 1993 Minnesota residents who filed Minnesota tax returns in 1994 for the 1993 tax year, and whose 1993 return was processed by the Minnesota Department of Revenue by the end of September, 1994. No amended returns were included; and matching federal income tax data had to be available for the taxpayers. About 1,853,000 Minnesota taxpayers met these conditions.

The portion of the sample used for the final analysis consisted of taxpayers whose 1994 Minnesota tax returns were filed and processed by the Department of Revenue by the end of December, 1995, or for whom federal tax returns were filed during 1995. Some loss of taxpayers in the sample was undoubtedly caused by taxpayers moving out of state or having too little income to file a 1994 return, among other possibilities. The December processing date, however, allowed us to include most of the taxpayers who might have filed late or requested an extension in 1995, perhaps as a result of the experimental treatment.

The sample was stratified by income and by a set of characteristics we refer to as opportunity. There were three stratifications by 1993 income: low-income, with AGI less than $10,000; middle-income, with AGI between $10,000 and $100,000; and high-income, with taxpayers with AGI over $100,000.

Previous research on tax evasion points to several factors associated with evasion, including income not subject to withholding tax and income from a sole proprietorship. The 'high-opportunity' group was a random sample from taxpayers who filed a federal Schedule C or F (indicating business or farm income) in 1993 *and* who paid Minnesota estimated tax. A Minnesota taxpayer is required to file and pay estimated tax quarterly if expected tax will be $500 or more above withholding and expected tax credits. The $500 threshold effectively eliminated taxpayers from the high-opportunity group who may have filed a Schedule C or F but expected to have little reported income from their businesses.[8] Taxpayers not in the high-opportunity category are referred to as low-opportunity.

The population count, sampling rate, and the resulting sample frequency for each stratum are presented for the treatment group in Table 1 and for the control group in Table 2.[9] Table 3 documents the further reduction in the sample by the elimination of returns 1)

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[8] An advantage of a sample based on estimated-tax payers is the possibility of tailoring interventions for this group in the future if the experiment proved a success, because these taxpayers are involved with the department throughout the year. The low-opportunity group selected to represent the general population may provide valuable information about what approach to compliance works best with people who rarely would be the target of an audit.

[9] The control group from the 'audit' experiment was combined with the control group from the 'appeal to conscience' experiment to increase precision. Both were randomly selected, and neither was contacted by the Department of Revenue during the experiment.

changing to, or from, married filing jointly, 2) filing for a different tax year, 3) not filing a 1994 tax return, or 4) having no positive income.[10] This produced a working sample of 22,368 returns.

4.2 Experimental Treatment

The treatment group received a letter by first-class mail from the Commissioner of Revenue in January of 1995.[11] Note that this treatment was administered after the tax year, and at the beginning of the filing season.[12] Thus, with a few exceptions (such as contributions to IRA or Keoghs) it could not have affected non-reporting behavior with tax consequences. The taxpayers were told: 1) that they had been selected at random to be part of a study "that will increase the number of taxpayers whose 1994 individual income tax returns are closely examined;" 2) that both their state and federal tax returns for the 1994 tax year would be closely examined by the Minnesota Department of Revenue; 3) that they will be contacted about any discrepancies; and 4) that if any 'irregularities' were found, their returns filed in 1994 as well as prior years might be reviewed, as provided by law. The taxpayers were given department phone numbers to call for information and assistance with

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[10] We also excluded a number of returns for which there was a single 1993 return associated with two 1994 returns, presumably due to divorce.

[11] This aspect of the experiment is consistent with the Allingham-Sandmo assumption of a fixed "true" taxable income.

[12] The letter was sent separately from the tax form itself, thus minimizing the possibility that taxpayers who use professional preparers would discard the letter without reading it.

their taxes. (A copy of the letter sent to taxpayers in the experiment is included in the Appendix.)[13]

To what extent the receipt of this letter corresponds to a 'p equals one' experiment is an open question. Some taxpayers may believe that some aspects of noncompliance would not be detected by an 'examination.' Others might have believed that this was an idle, or incredible, threat. In the concluding section we return to these issues in light of the results, which we discuss next.

5. RESULTS

5.1 Measuring Compliance

Compliance has three parts: timely filing, accurate reporting, and timely paying; this paper focuses on the second.[14] To measure accurate reporting, we use federal tax after credits as reported on the federal income tax return. The 1993 dollar amounts were adjusted for inflation to 1994 levels.

Because of the cross-sectional and longitudinal nature of the data, several research strategies are possible. We focus on comparing *changes* in reported tax liability between 1993 and 1994 for the treatment group relative to the control group. That is, we subtracted the (inflation-adjusted) 1993 tax from the 1994 tax to calculate the change. We also examine the change in tax scaled by total 1993 positive income as a relative measure of

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[13] On February 10, 1995 the St. Paul *Pioneer Press* carried a report on the experiment headlined, "Many Upset Over Letter Hinting Audit." This news item was picked up by the Associated Press, and it appeared later in several other Minnesota newspapers. We are uncertain how this might have affected the results of the experiment. If there was any effect, it seems likely that the report would have enhanced the credibility of the audit notice.

[14] There was no statistically significant difference in the date filed (technically, the date received by the IRS) between the control and treatment groups. For evidence on the determinants of filing date, see Slemrod *et al.* (1997).

noncompliance. If the average change for the treatment group was different than the average change for the control group, we inferred that the treatment had an effect, provided that the difference between groups was large enough to be statistically significant. Although, we cannot verify that changes in reported tax liability were due to changed compliance behavior, subjects were randomly assigned to treatment and control groups, so this inference seems unassailable.

5.2 Difference in Mean Differences

Much of the interest, and puzzles, regarding the results are apparent in the tabulation of means presented in Tables 4 through 6. Because for the most part Tables 4 through 6 all tell a similar story, we focus here on Table 5, which shows the results for federal tax liability (after credits). Consider first the low and middle-income groups. Notice first that the 1993 means for the control and treatment groups are very close, attesting to the randomness of the sample selection procedures. Among both the low- and middle-income strata, the audit notice apparently had a very large impact on the high-opportunity taxpayers. The average difference-in difference federal tax liability was $676 for the middle income group, compared to an average $5606 of 1994 tax liability for the control group, amounting to a 12.1% increase in tax. For the lower-income, high-opportunity group the apparent treatment effect is even more striking; the difference-in-difference was $843, compared to 1994 tax liability for the control group of $580, amounting to a 145.3% increase. However, only the middle-income result was statistically significant at a 10% level. Qualitatively the same results apply if we compare the average change in behavior relative to 1993 total positive income, or if we look only at the fraction of taxpayers for whom real tax payments increased from 1993 to

1994 -- a larger treatment effect among the high-opportunity taxpayers compared to the low-opportunity taxpayers.

Although these are striking results, they apply to a small fraction of Minnesota taxpayers -- just 53,040 out of 1,852,839, or 2.9%. To get a feel for the potential impact of increased enforcement on aggregate tax liability, we must turn our attention to the "low-opportunity" taxpayers. For the low- and middle-income members of this category, the mean treatment effect is positive, but is of a much smaller magnitude than for the high-opportunity taxpayers. The difference-in-difference averages $92 for the middle-income taxpayers, or 2.3% of the average 1994 tax liability of the control group. For the low-income group the absolute difference-in-differences is only $13, 3.5% of the average 1994 tax liability of the control group. Neither of these differences is different from zero at a high confidence level.

If we aggregate the difference-in-difference estimates for the four groups studied so far, we obtain $161 million, or just under 2% of the total 1994 tax liability of $8.15 billion. This is obviously much lower than the 17% aggregate income tax gap estimated by the IRS, and significantly lower than the TCMP- detected noncompliance of 7.3%.

So far we have not discussed the results relating to the high-income taxpayers, who comprise only slightly more than 3% of taxpayers, but who have $2.5 billion, or 30.1% of the federal tax liability. They deserve separate treatment because the results are so strikingly different. First of all, note that the 1993 means for the treatment and control groups are not very close. At a minimum, this testifies to the high variance in reporting behavior among this group: it suggests, furthermore, that the attrition in this sample might not have been random. Of most interest is the fact that, compared to the control group, on average the high-income treatment groups exhibit a lower change in reported tax liability from 1993 to 1994. The magnitude of the difference-in-differences is large, amounting to 34.8% of the 1994 control group average tax liability for the high-opportunity group, and 16.8% for the low-opportunity group: for both groups, the difference in means is significant. The unexpected behavior of the high-income groups is also evident in our simple non-parametric analysis -- the fraction of taxpayers for whom there was a real increase in tax paid was *lower* among the treatment groups.

5.3 Regression Analysis

We also estimated a regression model of the scaled response (1993-94 change per hundred dollars of 1993 positive income). This model controls for return characteristics that may better explain the response and improve our ability to test for experimental treatment effects. The regression model also allows for tests of interactions between the treatment effect and return characteristics other than income and opportunity.

The explanatory variables include dummy variables for ranges of total positive income, marital status, age, the presence of a paid preparer, marginal tax rate, and the presence of various supplemental schedules (Schedules A through F and Schedule ES) in addition to the treatment dummy. Interactions between the treatment dummy variable and each regressor are also included. Table 7 provides more detailed definitions of the regressors. Parameters of the models are estimated using unweighted OLS. The results are reported in Tables 8 through 10, for each of the three measures of tax reporting behavior. In each table, the left-hand panel reports on the results of regressions with the absolute (real) change as the dependent variable, while for the right-hand panel the dependent variable is the percent change relative to 1993 total positive income.

The regression results corroborate many of the patterns evident in the tabulations. In particular, there is a positive treatment effect associated with certain indicators of opportunities to evade, especially the presence of a Schedule A (itemized deductions), and Schedule ES (payroll tax on self-employment income). This treatment effect is, however, overwhelmed by a strong negative treatment effect for high-income taxpayers; in the level equations, the marginal tax rate has an incremental negative treatment effect. One other finding is worthy of note: in the absolute change, but not the percentage change, specification, being elderly is associated negatively with a treatment effect, consistent with earlier findings that elderly people are less likely to be noncompliant.

6. AN EXPLANATION FOR THE PERVERSE HIGH-INCOME RESPONSE

The surprising result, which runs counter to the basic theory of tax evasion, that very high income taxpayers would report less tax after receiving an audit notice deserves further attention. Note, first of all, that the TCMP results discussed above suggest that the extent of evasion relative to true income is in fact *lower* for high-income taxpayers. They do not, though, suggest that it is non-existent.

We have come up with two possible explanations for a perverse response of reported income to a notice of examination.[15] The first is that the audit notice letter induced taxpayers to seek out professional tax advisors who, among other things, uncovered legitimate ways to reduce taxable income that the taxpayer had previously been unaware of. A simple version

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[15] These explanations presume, of course, that the empirical finding is not spurious. One source of a spurious finding is differential attrition from the sample of the treatment and control groups: upon receiving the treatment letter, many high-income evaders simply did not file a 1994 return. There is some evidence that the attrition rate is higher for the treatment subset of high-income families, but it is impossible to assess the quantitative impact of this, and we are inclined to believe that this is not an important explanation for our findings.

of that hypothesis can be investigated by looking at the change in preparer use. Table 11 documents that the examination notice did increase the percentage of taxpayers (relative to the control group) that made use of professional tax assistance, for high-income taxpayers as well as the other income groups. Overall, the difference-in-differences amounts to 2.8 percentage points. However, the data on the high-income groups suggests that a shift toward preparer use is unlikely to be a significant part of the story of why the reported income of the treatment group declines because most of this group was already using a preparer for tax year 1993. This finding does not preclude that taxpayers who used professional tax preparers for tax year 1993 used better or more aggressive tax preparers in 1994, or received different advice in 1994 compared to 1993 from the same preparer.

Our preferred explanation relies on two extensions of the basic Allingham-Sandmo framework. The first, which was discussed in Section 2, is that taxpayers believe that the probability of audit depends on their report; this becomes even more salient if taxpayers believe that, upon audit, previous years' noncompliance may be detected and penalized. Other things equal, a higher value of p' reduces the amount of noncompliance.

The second extension we suggest relies on the idea that, even upon audit, 'true' tax liability is not ascertainable. The tax liability ultimately paid depends on, among other things, a process of negotiation between the taxpayer (and, potentially, his agent) and the IRS. In a costly negotiation under imperfect and asymmetric information, it may make sense to begin with a 'low' bid. This is true, for example, in Farber's (1980) model of final-offer arbitration, where each side's optimal offer trades off the expected benefits of a successful low bid and the fact that the lower the bid the less likely it is to be accepted. (Anecdotal evidence also suggests that IRS auditors are induced to find 'something' in an audit, so that leaving a few nuggets of noncompliance may facilitate a mutually advantageous resolution of the audit process.)[16]

In terms of the model presented in Section 2, we are arguing that the penalty upon audit is not θs, but rather $\theta F(s)$, where F is the penalty, relative to true after-tax income, expected from the negotiation process. Our assumption is that, for small values of s, $\partial F/\partial s < 0$, so that a lower bid (i.e., more apparent noncompliance) increases expected income.

Furthermore, we expect that past noncompliance, which would be uncovered upon audit, increases the expected penalty. Thus, we have extended the problem facing a risk-neutral taxpayer posed in equation (3) to be the following:

$$\text{(5)} \qquad \underset{s}{\text{Max}} \quad EY = (1-p[y-x])(v+s) + p[y-x](v - \theta F(s, s^p))$$

The first-order condition for s now becomes

$$\text{(6)} \qquad 1 - p - pF'\theta = p'\left(\frac{s+\theta F}{t}\right)$$

In the framework of this model, compare two environments. In the first, meant to approximate the 1994 situation for sophisticated taxpayers in the treatment group, $p = 1$ and $p' = 0$. In the second environment, the 1993 situation, $p << 1$ but $p' > 0$. It is possible that a rational taxpayer, faced with the maximization problem of expression (5), will initially report less income in the former case.

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[16] Many popular guides to taxpayer tactics during an audit suggest this kind of behavior. For example, consider the following passage in Strassels and Wool (1981), after the stereotypical IRS agent (Harry) has uncovered two items which lead to higher tax:.."Marvin [the taxpayer's advisor] is not feeling so badly at all. True, he has given Harry your undeclared interest income and energy credit, but he expected to give them both anyway

7. CAVEATS AND CONCLUSIONS

In retrospect, a larger sample size of high-income taxpayers would have increased the certainty with which inferences can be drawn.[17] This is less of a problem with the other income groups, for whom the variability of income reports is much lower. Also, if feasible, a follow-up experiment should begin at the start of the tax year, to allow the probability of audit to influence not only reporting decisions, but also real substitution and avoidance behavior. In the experiment discussed here the treatment was applied only after the tax year was completed, when most behavior with tax consequences (other than IRA or Keogh contributions) had already been carried out.

The conclusions to be drawn from the results of this experiment depend critically on how taxpayers interpreted the treatment letters. While the treatments were, of course, designed with the purpose of signaling a certain, thorough audit, in actuality we may have had only very limited success in capturing the attention of taxpayers. The phrase "we will examine your 1994 tax return very closely" may have been less threatening than at first blush one might expect. Some people may already believe that their return is being examined "very closely." For such individuals the treatment would have no effect either because they are already deterred or because they have concluded, perhaps incorrectly, that such close inspection is not capable of detecting the type of noncompliance in which they engage. Others may simply not have believed the resource-constrained Minnesota tax enforcement agency could carry out such a large-scale audit program, or that even if they did believe that,

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[emphasis added]. And, since they both came at the very beginning of the audit, perhaps Harry will already feel less pressure to slice up all the other soft items that lie ahead." (p. 242) may have believed that even such an audit would not uncover their own evasion. Finally, the audit notice indicated that prior returns might also be examined. This element of the treatment may have backfired. Some individuals may have been fearful that if they changed their reporting patterns in 1994 by, for example, reporting income that they had previously not reported, the 1994 report would have given away their history of noncompliance.

These considerations argue against interpreting the difference-in-difference results as a measure of existing noncompliance. However, the results remain relevant as an indicator of the response of taxpayers to an increased enforcement probability, which in practice would be taken more seriously by some taxpayers than others, and whose impact would be conditioned on the taxpayers' history of noncompliance. This is, after all, the kind of information the Minnesota Department of Revenue was hoping to glean from this experiment.

What then, have we learned from this randomized, controlled experiment? A heightened threat of examination increases the reported income and tax liability of low- and middle-income taxpayers, especially those that have greater opportunities to evade taxes. The increased tax collections from this group are, though, likely to be fairly small, in this experiment amounting to less than 2% of total tax liability. Moreover, there is reason to suspect that high-income taxpayers may react by reporting even less income than before, based on a perception that an audit is in reality a negotiation process for which an initial 'low bid' may be optimal. If this last finding, which cries out for independent corroboration, is

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[17] One constraint on the size of the treatment group was the Advisory Board's insistence that the examination notice actually be carried out; limited Department of Revenue resources severely restricted the size of the treatment group.

correct, it suggests that a heightened audit threat be carried out simultaneously with a rethinking of how the audits themselves are carried out.

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Table 1
Treatment Group Sample Selection

Stratum	Population	Sampling Rate	n	Weight
Low Income / Low Opportunity	449,017	0.10%	460	976.12
Low Income / High Opportunity	2,120	2.74%	58	36.55
Medium Income / Low Opportunity	1,290,233	0.04%	575	2,243.88
Medium Income / High Opportunity	50,920	0.84%	430	118.42
High Income / Low Opportunity	52,093	0.22%	114	456.96
High Income / High Opportunity	8,456	1.03%	87	97.20
TOTAL	1,852,839		1,724	

Table 2
Control Group Sample Selection

Stratum	Population	Sampling Rate	n	Weight
Low Income / Low Opportunity	449,017	1.30%	5,823	77.11
Low Income / High Opportunity	2,120	6.56%	139	15.25
Medium Income / Low Opportunity	1,290,233	1.16%	14,955	86.27
Medium Income / High Opportunity	50,920	2.78%	1,414	36.01
High Income / Low Opportunity	52,093	1.44%	750	69.46
High Income / High Opportunity	8,456	3.18%	269	31.43
TOTAL	1,852,839		23,350	

Table 3
Sample Reconciliation

SAMPLE SELECTION	Treatment		Control		Total	
1993 Filers	1,724		23,350		25,074	
Filed Multiple TY94 Returns	-10	-0.6%	-165	-0.7%	-175	-0.7%
Changed 'To' or 'From' MFJ	-54	-3.1%	-973	-4.2%	-1,027	-4.1%
Return Received in '95 Was not for TY94	-1	-0.1%	-7	0.0%	-8	0.0%
Did Not File 94 Fedl Rtn	-122	-7.1%	-1,370	-5.9%	-1,492	-6.0%
TY93 TPI = 0	0	0.0%	-4	0.0%	-4	0.0%
Total	1,537		20,831		22,368	

Table 4
Average Reported Federal Taxable Income

WHOLE SAMPLE (Weighted)

	Treatment	Control	Tr-Ctl
1994	23,781	23,202	579
1993	23,342	22,484	858
94-93	439	717	-278
94-93/TPI	10.24	9.83	0.41
%w/Increase	54.4%	51.9%	2.5% ***
n	1,537	20,831	

LOW INCOME

	High Opportunity			Low Opportunity		
	Treatment	Control	Tr-Ctl	Treatment	Control	Tr-Ctl
1994	7,473	3,992	3,481	2,397	2,432	-35
1993	971	787	183	788	942	-154 **
94-93	6,502	3,204	3,298	1,609	1,490	119
94-93/TPI	54.38	44.14	10.23	32.86	34.68	-1.82
%w/Increase	65.4%	51.2%	14.2% *	52.2%	50.2%	2.0%
n	52	123		381	4,829	

MIDDLE INCOME

	High Opportunity			Low Opportunity		
	Treatment	Control	Tr-Ctl	Treatment	Control	Tr-Ctl
1994	33,280	31,191	2,089	24,316	23,669	646
1993	29,735	29,652	83	23,355	23,172	183
94-93	3,546	1,539	2,007	960	497	463
94-93/TPI	11.47	5.94	5.52	3.77	2.63	1.14
%w/Increase	57.2%	53.1%	4.1%	56.0%	52.8%	3.2%
n	397	1,318		520	13,636	

HIGH INCOME

	High Opportunity			Low Opportunity		
	Treatment	Control	Tr-Ctl	Treatment	Control	Tr-Ctl
1994	143,170	163,015	-19,845	146,198	145,161	1,037
1993	176,683	150,865	25,818	164,919	147,819	17,099
94-93	-33,513	12,150	-45,663 ***	-18,721	-2,659	-16,063
94-93/TPI	-11.15	0.28	-11.43 ***	-6.86	-2.52	-4.34
%w/Increase	37.5%	42.2%	-4.7%	32.7%	43.6%	-10.9% **
n	80	244		107	681	

* $p < .10$
** $p < .05$
*** $p < .01$

Table 5
Average Reported Total Federal Tax After Credits

WHOLE SAMPLE (Weighted)

	Treatment	Control	Tr-Ctl
1994	4,534	4,452	83
1993	4,520	4,250	270
94-93	15	202	-187
94-93/TPI	1.73	1.64	0.10
%w/Increase	53.2%	51.7%	1.6% **
n	1,537	20,831	

LOW INCOME

	High Opportunity			Low Opportunity		
	Treatment	Control	Tr-Ctl	Treatment	Control	Tr-Ctl
1994	1,451	580	871	360	369	-10
1993	146	118	27	119	142	-22 **
94-93	1,305	462	843	240	228	13
94-93/TPI	10.59	6.52	4.07	4.86	5.27	-0.41
%w/Increase	63.5%	51.2%	12.2%	50.1%	49.2%	0.9%
n	52	123		381	4,829	

MIDDLE INCOME

	High Opportunity			Low Opportunity		
	Treatment	Control	Tr-Ctl	Treatment	Control	Tr-Ctl
1994	6,201	5,606	595	4,065	3,992	73
1993	5,082	5,162	-80	3,818	3,837	-19
94-93	1,120	444	676 *	247	155	92
94-93/TPI	2.97	1.34	1.63	0.86	0.58	0.28
%w/Increase	56.7%	53.3%	3.4%	55.0%	52.8%	2.2%
n	397	1,318		520	13,636	

HIGH INCOME

	High Opportunity			Low Opportunity		
	Treatment	Control	Tr-Ctl	Treatment	Control	Tr-Ctl
1994	38,703	45,597	-6,894	40,577	40,339	239
1993	49,637	40,671	8,966	47,190	40,194	6,996
94-93	-10,934	4,925	-15,860 ***	-6,613	144	-6,757 *
94-93/TPI	-3.10	0.50	-3.60 ***	-2.08	-0.38	-1.70 *
%w/Increase	36.3%	42.2%	-6.0%	33.6%	43.6%	-10.0% *
n	80	244		107	681	

* p < .10
** p < .05

*** $p < .01$

Table 6
Average Reported Minnesota Tax Liability

WHOLE SAMPLE (Weighted)

	Treatment	Control	Tr-Ctl
1994	1,752	1,688	64
1993	1,732	1,639	93
94-93	20	49	-29
94-93/TPI	0.66	0.62	0.04
%w/Increase	52.7%	50.5%	2.1% ***
n	1,518	20,708	

LOW INCOME

	High Opportunity			Low Opportunity		
	Treatment	Control	Tr-Ctl	Treatment	Control	Tr-Ctl
1994	487	239	248	140	143	-3
1993	57	43	13	46	54	-9 *
94-93	430	195	234	94	89	5
94-93/TPI	3.55	2.78	0.77	1.98	2.13	-0.15
%w/Increase	59.6%	48.0%	11.6%	48.3%	47.3%	0.9%
n	52	123		373	4,767	

MIDDLE INCOME

	High Opportunity			Low Opportunity		
	Treatment	Control	Tr-Ctl	Treatment	Control	Tr-Ctl
1994	2,370	2,201	169	1,702	1,649	53
1993	2,093	2,093	0	1,627	1,609	18
94-93	277	108	169	75	41	35
94-93/TPI	0.85	0.41	0.44	0.30	0.19	0.11
%w/Increase	56.6%	51.7%	4.9% *	54.8%	51.9%	3.0%
n	394	1,313		516	13,582	

HIGH INCOME

	High Opportunity			Low Opportunity		
	Treatment	Control	Tr-Ctl	Treatment	Control	Tr-Ctl
1994	12,397	13,825	-1,428	12,870	12,274	596
1993	15,854	12,798	3,056	14,448	12,541	1,907
94-93	-3,457	1,027	-4,484 ***	-1,578	-267	-1,311
94-93/TPI	-1.00	0.02	-1.03 ***	-0.58	-0.19	-0.39
%w/Increase	36.4%	41.8%	-5.4%	33.0%	42.9%	-9.8% *
n	77	244		106	679	

* $p < .10$
** $p < .05$
*** $p < .01$

Table 7
Variable Definitions for Regression Models

(All Control Variables Are Based on 1993 Values)

INC1 - (1/0) TPI < \$20,000
INC2 - (1/0) \$20,000 ≤ TPI < \$50,000
INC4 - (1/0) \$100,000 ≤ TPI < \$200,000
INC5 - (1/0) TPI ≥ \$200,000

MFJ - (1/0) Married Filing Jointly / All Other
AGE - (1/0) Age 65 or Over / All Other
PREP - (1/0) Signed by a Paid Preparer / All Other
BALDUE - (1/0) A Balance Due Was Reported Upon Filing

SCHA - (1/0) Presence of a Schedule A (Itemized Deductions)
SCHB - (1/0) Presence of a Schedule B (Interest & Dividends \$400 or More)
SCHC - (1/0) Presence of a Schedule C (Self-Employment Income & Deductions)
SCHD - (1/0) Presence of a Schedule D (Capital Gains & Losses)
SCHE - (1/0) Presence of a Schedule E (Rents, Royalties, Trusts & Estates, and Partnerships)
SCHF - (1/0) Presence of a Schedule F (Farm Income or Loss)
SCHES - (1/0) Presence of a Schedule ES (Estimated Payments)

MTR - Combined Statutory Minnesota and Federal Marginal Tax Rate (including the Self-Employment Tax) as a Percentage

RX - (1/0) Treatment / Control Group

RX*• - Multiplicative interaction of the RX treatment dummy and the independent variable •

Table 8
Regression Coefficient Estimates for Change in Federal Taxable Income

	(94-93)		(94-93)/TPI/100	
	β	t	β	t
INTERCEPT	-561	-0.41	37.18	11.94
INC1	3,148	2.98	6.53	2.70
INC2	1,729	2.32	-5.73	-3.35
INC4	-2,891	-2.45	4.24	1.57
INC5	15,049	7.44	14.27	3.08
MFJ	1,576	2.77	-2.87	-2.20
AGE	-2,996	-4.02	-14.58	-8.55
PREP	-249	-0.54	-2.55	-2.42
BALDUE	-3,028	-6.26	-5.61	-5.07
SCHA	1,589	2.70	3.42	2.54
SCHB	2,113	3.04	1.66	1.04
SCHC	2,306	3.05	13.94	8.07
SCHD	-4,095	-5.85	-5.11	-3.19
SCHE	4,021	5.16	3.03	1.70
SCHF	-1,908	-1.35	-6.00	-1.85
SCHES	-389	-0.53	4.06	2.41
MTR	-34	-1.15	-1.02	-15.24
RX	7,380	1.40	-14.47	-1.20
RX*INC1	-2,836	-0.68	15.09	1.59
RX*INC2	-2,425	-0.85	4.37	0.67
RX*INC4	-9,628	-2.82	-15.88	-2.03
RX*INC5	-66,232	-13.33	-20.07	-1.77
RX*MFJ	-517	-0.22	-2.19	-0.42
RX*AGE	-4,668	-1.83	0.59	0.10
RX*PREP	-1,484	-0.82	-1.82	-0.44
RX*BALDUE	2,416	1.31	-0.48	-0.11
RX*SCHA	5,636	2.47	15.20	2.91
RX*SCHB	-5,679	-2.38	0.80	0.15
RX*SCHC	35	0.01	2.03	0.33
RX*SCHD	3,257	1.35	0.52	0.09
RX*SCHE	-1,578	-0.63	-0.91	-0.16
RX*SCHF	128	0.03	9.46	0.82
RX*SCHES	4,147	1.67	6.91	1.21
RX*MTR	-229	-2.12	0.17	0.68

Table 9
Regression Coefficient Estimates for Change in Federal Tax Liability After Credits

	(94-93)		(94-93)/TPI/100	
	β	t	β	t
INTERCEPT	-54	-0.12	5.77	10.61
INC1	459	1.28	0.82	1.95
INC2	228	0.90	-0.92	-3.09
INC4	-712	-1.78	0.28	0.59
INC5	6,448	9.41	2.36	2.93
MFJ	308	1.59	-0.51	-2.24
AGE	-701	-2.78	-2.19	-7.35
PREP	-54	-0.35	-0.30	-1.62
BALDUE	-790	-4.83	-1.00	-5.20
SCHA	261	1.31	0.58	2.48
SCHB	631	2.68	0.42	1.52
SCHC	537	2.10	2.28	7.55
SCHD	-1,018	-4.30	-1.10	-3.94
SCHE	1,324	5.01	0.94	3.03
SCHF	-526	-1.10	-0.90	-1.59
SCHES	-117	-0.47	0.56	1.91
MTR	-1	-0.11	-0.15	-12.94
RX	3,049	1.70	-2.74	-1.30
RX*INC1	-1,532	-1.09	3.24	1.95
RX*INC2	-1,113	-1.16	0.69	0.61
RX*INC4	-2,624	-2.27	-4.03	-2.96
RX*INC5	-25,756	-15.31	-6.43	-3.24
RX*MFJ	-416	-0.53	-0.74	-0.81
RX*AGE	-1,937	-2.24	-0.37	-0.37
RX*PREP	-210	-0.34	-0.79	-1.09
RX*BALDUE	858	1.37	-0.24	-0.32
RX*SCHA	1,378	1.78	4.02	4.40
RX*SCHB	-2,143	-2.65	0.04	0.04
RX*SCHC	144	0.16	0.52	0.49
RX*SCHD	1,270	1.55	0.50	0.52
RX*SCHE	-170	-0.20	-0.58	-0.58
RX*SCHF	-391	-0.23	1.35	0.67
RX*SCHES	1,098	1.30	2.16	2.18
RX*MTR	-82	-2.24	0.03	0.60

Table 10
Regression Coefficient Estimates for Change in Minnesota Tax Liability After Credits

	(94-93)		(94-93)/TPI/100	
	β	*t*	β	*t*
INTERCEPT	-25	-0.23	2.38	10.92
INC1	192	2.27	0.37	2.19
INC2	115	1.93	-0.34	-2.85
INC4	-227	-2.40	0.24	1.27
INC5	1,021	6.32	0.90	2.81
MFJ	111	2.43	-0.12	-1.35
AGE	-237	-3.97	-1.03	-8.64
PREP	-24	-0.66	-0.17	-2.34
BALDUE	-197	-5.08	-0.36	-4.64
SCHA	87	1.84	0.17	1.79
SCHB	187	3.37	0.12	1.06
SCHC	194	3.20	0.90	7.46
SCHD	-333	-5.93	-0.37	-3.31
SCHE	336	5.37	0.27	2.19
SCHF	-156	-1.38	-0.41	-1.80
SCHES	-60	-1.02	0.25	2.17
MTR	-2	-0.88	-0.06	-13.86
RX	431	1.02	-1.33	-1.57
RX*INC1	-102	-0.31	1.34	2.02
RX*INC2	-138	-0.60	0.44	0.97
RX*INC4	-737	-2.68	-1.21	-2.21
RX*INC5	-6,240	-15.64	-1.82	-2.29
RX*MFJ	54	0.29	-0.16	-0.43
RX*AGE	-434	-2.12	-0.01	-0.02
RX*PREP	-94	-0.64	-0.23	-0.80
RX*BALDUE	300	2.02	0.09	0.31
RX*SCHA	446	2.44	1.26	3.44
RX*SCHB	-513	-2.68	0.11	0.29
RX*SCHC	-109	-0.52	0.03	0.07
RX*SCHD	343	1.77	0.16	0.41
RX*SCHE	-170	-0.84	-0.17	-0.42
RX*SCHF	-362	-0.90	0.42	0.52
RX*SCHES	309	1.55	0.60	1.50
RX*MTR	-16	-1.87	0.02	0.91

Table 11
Practitioner Use For Treatment and Control Groups
Tax Years 1993 and 1994

Whole Sample (Weighted)

	Treat	Control	Tr-Ctl
1994	53.8%	52.8%	1.0%
1993	50.4%	52.2%	-1.8%
94-93	3.4%	0.6%	2.8%

Low Income

	High Opportunity			Low Opportunity		
	Treat	Control	Tr-Ctl	Treat	Control	Tr-Ctl
1994	76.9%	81.3%	-4.4%	46.4%	42.0%	4.4%
1993	75.0%	82.1%	-7.1%	45.1%	40.8%	4.3%
94-93	1.9%	-0.8%	2.7%	1.3%	1.2%	0.1%

Middle Income

	High Opportunity			Low Opportunity		
	Treat	Control	Tr-Ctl	Treat	Control	Tr-Ctl
1994	82.2%	82.2%	0.0%	54.1%	53.8%	0.3%
1993	82.1%	84.1%	-2.0%	49.8%	53.3%	-3.5%
94-93	0.1%	-1.9%	2.0%	4.3%	0.5%	3.8%

High Income

	High Opportunity			Low Opportunity		
	Treat	Control	Tr-Ctl	Treat	Control	Tr-Ctl
1994	88.3%	89.3%	-1.0%	70.6%	73.2%	-2.6%
1993	88.8%	91.0%	-2.2%	68.2%	74.0%	-5.8%
94-93	-0.5%	-1.7%	1.2%	2.4%	-0.8%	3.2%

APPENDIX

MINNESOTA Department of Revenue

January, 1995

Dear Taxpayer:

This year we are doing a study that will increase the number of taxpayers whose 1994 individual income tax returns are closely examined by the Minnesota Department of Revenue. You have been selected at random from a list of all Minnesota taxpayers to be in this study.

The examination of your 1994 tax returns will include both your state and federal returns. After a close review of your returns, we may write you for additional information about them or arrange a face-to-face audit. If the examination of your 1994 returns finds any irregularities, we may also review tax returns you filed in prior years, as provided by law.

When you prepare your 1994 return, or give information to your tax preparer, pleas be very careful to report all your income and take only the deductions to which you are entitled. Remember to attach a copy of your federal return to your state return.

The Minnesota Department of Revenue tries to help taxpayers comply with the law. If you have questions about your Minnesota income tax return, please call us at these numbers:

Order Forms and Schedules	296-4444 from the Twin Cities metro area, or 1-800-657-FORM (toll-free) from elsewhere.
Information and Assistance	296-3781 from the Twin Cities metro area, or 1-800-652-9094 (toll-free) from elsewhere.

Sincerely,

Matthew G. Smith
Commissioner