

Do Normative Appeals Affect Tax Compliance? Evidence from a Controlled Experiment in Minnesota

Abstract - This paper explores one part of a 1994 Minnesota Department of Revenue field experiment designed to study the effectiveness of alternative enforcement strategies. Two letters containing different normative appeals were sent to two large groups of taxpayers; a control group received no letter. The impact of the letters on voluntary compliance is measured by comparing the change (for tax years 1994–93) in reported income and in taxes paid for treated versus control taxpayers (a difference-in-difference approach). We find little evidence of an overall treatment effect. However, the letters do appear to impact the compliance behavior of some groups of taxpayers.

Marsha Blumenthal

Department of
Economics,
University of St.
Thomas, St. Paul, MN
55105

Charles Christian

School of Accountancy
& Information
Management,
Arizona State
University, Tempe, AZ
85287-3606

Joel Slemrod

Office of Tax Policy
Research,
School of Business
Administration,
University of Michigan,
Ann Arbor, MI 48109-
1234

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INTRODUCTION

Recent public interest in the U.S. individual income tax has centered on the enforcement strategies employed by the Internal Revenue Service (IRS), widely viewed as overly aggressive, with Congressional hearings highlighting behavior bordering on the brutal. Members of Congress and the media argue for (and have effected!) substantial reform of the agency's structure and operations. In contrast, only a few years ago the IRS was criticized for its laxity, as estimates of the "tax gap" (the difference between taxes paid and the sums actually owed) topped \$100 billion. At that time, the agency was admonished to find new ways of identifying errant taxpayers and collecting the taxes (and penalties) they owed.

While tax administrators might well wonder just exactly what the American people do want from them, these political considerations tend to mask a fundamental problem: how best to enforce a tax system in which citizens play an important role in determining their own liabilities. With limited resources, state as well as federal tax agencies must decide whether the next budgeted dollar should be spent on aggressive enforcement (for example, auditing) or on more gently persuasive activities, such as friendlier interactions with taxpayers.

In a unique set of experiments during the 1994 and 1995 tax year filing seasons, the Minnesota Department of Revenue attempted to measure the effect on compliance of alternative enforcement strategies, including normative appeals to conscience, advanced notice of an increased audit rate, and enhanced taxpayer services. In an earlier paper (Slemrod, Blumenthal, and Christian, 2000), we reported that the experiment's higher audit rate increased the reported income and tax liability of low- and middle-income taxpayers, especially among those with greater opportunities to evade taxes. In contrast, high-income taxpayers responded by reducing the amount of reported income.

This paper presents and analyzes results from the normative appeal experiment. We conclude that neither of these normative appeals had a significant overall impact on reported income or tax liability. Although it is impossible to generalize from these findings to make conclusions about any and all normative appeals, no matter how constituted, we believe that there are no "dollars left on the table:" we find no evidence that inexpensive, mail-based appeals will significantly increase tax compliance.

RELATED LITERATURE

In a compliance system where taxpayers report their income and liabilities, a tax agency has at its disposal four strategies of enforcement (Roth, Scholz, and Witte, 1989). First, the agency can deter evasion by detecting and punishing non-compliance. Second, it can streamline its procedures to make compliance simpler for taxpayers.¹ Third, it can encourage normative support for compliance by re-

mindings taxpayers of their social commitments or of the services supported by tax payments. And fourth, the agency can increase compliance indirectly by working with tax practitioners. This paper attempts to assess the effectiveness of the third strategy.

One obvious route worthy of exploration is the set of attitudes, beliefs, and social norms about compliance held by taxpayers (see Roth, Scholz, and Witte, 1989, for a review of this literature). As Sheffrin and Triest (1992) point out, there are at least two reasons why attitudes could be important: first, because failing to consider the effect of an attitude might bias estimates of the impact of a correlated variable,² and second, because the impact of an attitude might go beyond the individualistic approach of most deterrence theory (as presented in Allingham and Sandmo, 1972), as attitudes are conditioned by membership in social networks and institutions or by perceptions of the extent to which the rest of the society complies. In related work, Kaplan and Reckers (1985) suggest that knowledge of widespread tax evasion could, depending upon the situation, move attitudes either way. Their laboratory experiments indicate that, when informed of widespread societal noncompliance, taxpayers are less inclined to evade when they believe it creates serious national problems, but are more inclined to do so when they experience unusual personal financial stress or when they perceive that evaders are highly moral persons.

A person's moral obligation to pay taxes flows from feelings about right and wrong, ultimately from attitudes about the appropriateness of social norms and laws. Some writers question whether people commonly view tax evasion as an

¹ Of course, simplification of the tax code should also be a priority, but it is not in the power of the tax authority.

² For example, using a latent variable technique and Louis Harris survey data, Sheffrin and Triest find that those with negative attitudes toward government also perceive a lower probability of tax cheating detection. Omitting attitude, the predicted effect of a policy to increase the perceived detection probability would be biased upward.

immoral act (Klepper and Nagin, 1989). Survey work by Song and Yarbrough (1978) suggests that tax fraud is seen as less serious than most violent crimes and also less serious than many property crimes. Referring to the literature of moral development and mass communication research, Hasseldine (1998) points out that since the media generally portray the income tax system in negative terms (embodying, for example, loopholes for the rich and an intimidating bureaucracy), a moral appeal could be credible only if it successfully reframed tax compliance as a positive end.

Other authors marshal evidence that tax compliance behavior is responsive to moral reasoning. Reckers, Sanders, and Roark (1994) use a mailed experimental task instrument to study how the intention to underreport income would be affected by individual tax ethics, withholding position (refund or additional tax due), and the marginal tax rate. They found that the underlying propensity to underreport when owing additional tax or facing a higher marginal tax rate was reduced by a strong belief that tax evasion is morally wrong. Laboratory experiments by Kaplan, Newberry, and Reckers (1997) suggest that subjects using "high" moral reasoning are less likely to express evasion intentions but are also less responsive to communications seeking to induce greater compliance. Scholz and Pinney (1995) posit that compliance decisions are guided by a duty heuristic,³ embodying a subjective valuation of the net benefits of citizenship. From interviews with a sample of Long Island taxpayers, Scholz and Pinney argue that this duty heuristic affects tax compliance indirectly, through

the deterrent effect of an increased perceived threat of detection. Song and Yarbrough (1978) report that ethical considerations loom larger in the thinking of certain kinds of taxpayers. Their results suggest that those with high levels of income and education, who are between the ages of 40 and 65, or who are white, married or own homes have a high level of tax ethics.⁴

The closest antecedent to our work is the field experiment reported by Schwartz and Orleans (1967), who exposed three groups of randomly-selected middle-income taxpayers, numbering about 90 each, to different persuasive messages a month prior to filing their 1962 federal returns. One message emphasized social commitment, while the second emphasized deterrence, and a third "placebo" served as a control. Treatment effects were assessed by comparing the differences, between 1962 and 1961, in income reported, deductions taken, and taxes due for the two experimental groups, relative to the control group.⁵ While both experimental groups reported more income and more taxes due in 1962, the difference was larger (and statistically significant) for the social commitment group. Methodological criticisms of this study (Roth, Scholz, and Witte, 1989) have cited the low standards of statistical significance and raised questions concerning non-response bias.

McGraw and Scholz (1991) performed a conceptual replication of the work, studying the behavior of 154 taxpayers.⁶ One treatment group (normative) watched a videotape describing the rationale for the 1986 Tax Reform Act and emphasizing the importance of social responsibility to Americans. A second treatment

³ A heuristic is a cognitive short-cut which minimizes the effort necessary for making routine decisions.

⁴ A somewhat inconsistent finding, from analyses of the IRS' Taxpayer Compliance Measurement Program (TCMP) data, is that compliance is greater among households where the head or the spouse is older than 65 while noncompliance is more prevalent among households with a married head (Andreoni, Erard, and Feinstein, 1998).

⁵ The IRS provided the relevant aggregate data for 1961 and 1962.

⁶ The sampling method appears to match that of Scholz and Pinney (1995).

group (personal consequences) saw a videotape providing many examples of how taxpayers could use aggressive strategies to legally minimize their tax liabilities, while a third group, a control, saw no videotape. Federal tax return data (total reported income, adjusted gross income and income tax), aggregated separately for the three treatment groups, were made available for both the prior 1985 tax year and the subsequent 1987 tax year. While, as expected, those in the personal consequences group reported a significantly smaller increase in income than the controls, those in the normative group did not report a larger increase in income than the controls. Interviews with the participants suggested that the experimental manipulation did affect their knowledge and attitudes; the tax return data, however, imply that these effects did not translate into behavior. One explanation for the lack of response to the normative communication is the longer time between experimental manipulation and tax filing deadline (over three months for McGraw and Scholz, but one month for Schwartz and Orleans). Another possible explanation, that a normative appeal made by a tax authority might backfire, is suggested by Bardach (1989, p. 62): "... *any* moral appeal can be read as a sign that the enforcement system cannot cope and must resort to rhetoric instead."

In sum, the existing literature is rather cautious in its assessment of the role normative appeals can play in improving voluntary tax compliance. There is evidence that normative communication can affect attitudes, and that attitudes matter. The link between the two, however, seems tenuous. Support does exist for the propo-

sitions that moral persuasion will be more effective for certain groups of people, that an appeal will have more impact the closer it is to the time of the desired behavior, and that the half-lives of normative communications are likely to be short. Moreover, where field experimentation has been used with tax return data, as in Schwartz and Orleans (1967) and McGraw and Scholz (1991), the results have been conflicting, and the samples have been very small, too small to convincingly link behavior to individual taxpayer characteristics.

THE DESIGN OF THE FIELD EXPERIMENT

As one element of a larger field experiment, the Minnesota Department of Revenue explored the impact of moral persuasion communications on voluntary compliance with the income tax by sending alternative letters to two groups of randomly selected taxpayers.⁷ In this experiment, early in the 1994 tax year filing season (January, 1995), two groups of 20,000 taxpayers each received one of the treatment letters. Data was also collected from a third group of 20,000 people who received no letter, serving as controls for both letters.⁸ Both of the treatment letters began and ended with the same text: a reminder that the filing season had started, an admonition to carefully report all income and to take only the appropriate deductions, and some information regarding how to obtain assistance and additional forms and schedules. The two treatment letters each had a different middle paragraph. One (Letter1: Support Valuable Services), sent on January 27,

⁷ Unlike the sample selection in the Department of Revenue's audit experiment (see Slemrod, Blumenthal, and Christian, 2000), cases for this sermon experiment are a simple random sample, drawn from the population of Minnesota taxpayers who filed 1993 income tax returns.

⁸ Separate experiments were simultaneously conducted to explore whether an audit threat, an enhanced level of taxpayer service or a revised state tax form would impact compliance. The audit and enhanced service experiments shared one control group while the normative and form revision experiments shared another, separate control group. Control taxpayers received no treatment.

1995, described how income tax dollars are allocated amongst state services in Minnesota.⁹ The paragraph concluded, “So when taxpayers do not pay what they owe, the entire community suffers.” The intended message was that taxpayers should comply voluntarily in order to support the provision of socially valuable activities. The other letter (Letter2: Join the Compliant Majority), mailed on February 6, 1995, debunked the perception that there is widespread cheating on tax returns. The middle paragraph stated that IRS audits show that “people who file tax returns report correctly and pay voluntarily 93 percent of the income taxes they owe.” It concluded, “Although some taxpayers owe money because of minor errors, a small number of taxpayers who deliberately cheat owe the bulk of unpaid taxes.” Here the message was that if one wished to belong to the majority community of citizens one should comply with the tax laws. Copies of the letters may be found in the appendix.¹⁰

Tax return data for tax years 1993 and 1994 (from returns filed during 1994 and 1995, respectively) for the treatment and control groups were generously made available to the authors, who served as consultants during the design and execution of the experiment,¹¹ by the Minnesota Department of Revenue. Federal tax return data from the Internal Revenue Service were merged with the state data. In

this study, the impact of the experimental treatment on voluntary compliance is measured by comparing the change between 1993 and 1994 in income reported and taxes paid for treated versus control taxpayers.¹² We posit that a letter had a positive influence on compliance if those receiving it report a larger year-to-year increase (for example, in reported income) compared to those serving as controls. Using this “difference-in-difference” approach allows us to distinguish a true treatment effect from any other reason that reported income or tax liability might have changed between 1993 and 1994, such as a tax law change.

The income variable we study is federal taxable income, as reported on the Minnesota individual income tax form. The tax variable is Minnesota tax liability (also from the state form). In addition, we investigate the change in the dependent variables as a percentage of federal Total Positive Income (TPI)(provided by the IRS) and the proportion of participants reporting larger values of the dependent variables in 1994 compared to 1993.¹³ The data contain observations of 60,061 Minnesota taxpayers regarding over 300 variables. Of particular interest for the purposes of this research are: the date of filing, the taxpayer’s filing status, the presence and magnitude of particular sources of income (e.g., self-employment income or income not subject to information re-

⁹ 30 percent to education, 18 percent to health care and support for the elderly and needy, 12 percent to local governments for law enforcement, parks, libraries and snow removal, and some more to highways and the environment.

¹⁰ A second phase of the experiment was conducted during the 1995 filing season, early in 1996. In it, about half of the 1994 control taxpayers were randomly selected and sent a copy of Letter2. Additionally, about half of the 1994 Letter2 group were sent another copy of Letter2. This left a group of 8,275 who remained controls in the second phase. The purpose of the second phase was to investigate whether sustained moral persuasion has a differential effect, as well as to investigate the half-life of moral persuasion that is not sustained. However, despite assurances that taxpayers were randomly assigned to the three groups, there were statistically significant differences in their 1993 tax profiles, raising doubts about the validity of the second phase. For this reason, this paper addresses only the results of the first phase.

¹¹ Several others also contributed substantially to the work of the advisory board, including Daniel Nagin, Kinley Larntz, and Mike Gregory.

¹² All nominal amounts were expressed in 1994 dollars, in order to correct for inflation over the period.

¹³ Finding no statistically significant effects when the dependent variable is measured relative to TPI, those results are not presented here.

porting) and of itemized deductions (e.g., for medical expenditures or a home mortgage), the use of a practitioner, and whether the taxpayer was entitled to a refund or obliged to pay more tax at the time of filing.

In order to enhance the integrity of the analysis, certain criteria were imposed on the observations used for this study. First, a case was deleted if the taxpaying entity filed one return in 1993 but filed duplicate returns (for that Social Security number) in 1994.¹⁴ Second, only those with positive Minnesota tax liability in 1993 were included. Third, those cases with changes in filing status that involved either moving to or from "married, filing joint" status were deleted. Previous work on the data (Slemrod, Blumenthal, and Christian, 2000) suggested that such filing status changes were strongly associated with changes in reports of taxable income and tax liability, quite independent of the experiment. Including these cases would therefore increase the noise accompanying measurement of any treatment effects. Fourth, a case was deleted if the date on which the 1994 tax year return was filed occurred before the end of calendar year 1994; we did this because it seemed unlikely that the returns filed in these cases could fairly represent tax year

1994 behavior. And, finally, only those cases in which a 1994 Minnesota return was filed were included. Table 1 records this sample selection process for the experiment, enumerating the number of cases deleted at each step. To obtain attrition rates, we calculated the percent of unique 1993 filers who did not file in 1994.¹⁵ A chi-square test revealed no significant differences between the attrition rate in the control group and of those receiving either of the letters.

EXPERIMENTAL RESULTS

The Overall Treatment Effect

We first examine whether those taxpayers that received a normative communication changed their compliance behavior, as measured by income reported or tax paid, compared to taxpayers that received no such communication. We test it by looking at average differences-in-differences and by doing a multiple regression analysis with a treatment dummy, controlling for a set of taxpayer characteristics.

Table 2 presents mean federal taxable income (FTI) and Minnesota tax liability (MnTx) separately for those taxpayers who received each of the two treatment letters, in the aggregate for those who re-

TABLE 1
SAMPLE SELECTION STATISTICS

	Letter 1	Letter 2	Control
1993 filers	20,013	20,009	20,039
Less:			
Filed duplicate TY94 returns	256	234	276
Zero MN tax liability	3143	3197	3107
Changed "To" or "From" MFJ	653	699	664
94 return not for TY94	6	10	6
Did not file 94 return	340	333	362
Yields:			
Sample size	15,615	15,536	15,624
Attrition rate(%)	3.79	3.87	3.90

¹⁴ These cases were flagged for us by the Department of Revenue, to maintain anonymity.

¹⁵ Because taxpaying entities with duplicate 1994 returns appear twice in the data set, we subtracted half of them in order to arrive at the number of unique 1993 filers. The number of nonfilers was taken before applying any of the criteria listed in Table 1.

Do Normative Appeals Affect Tax Compliance?

TABLE 2
CHANGE IN REPORTED FEDERAL TAXABLE INCOME AND MINNESOTA TAX LIABILITY
IN TREATMENT AND CONTROL GROUPS

Letter 1						
	Federal Taxable Income			MN Tax Liability		
	Treated	Control	Treated–Control	Treated	Control	Treated–Control
1994	\$26,947	\$26,940	\$7	\$1,943	\$1,954	\$–11
1993	\$26,236	\$26,449	\$–213	\$1,907	\$1,934	\$–26
1994–1993	\$711	\$491	\$220(352)	\$35	\$20	\$15(29)
% with 94–93 increase	54.1	53.9	0.2	52.6	52.3	0.3
n	15,613	15,624		15,613	15,624	
Letter 2						
	Federal Taxable Income			MN Tax Liability		
	Treated	Control	Treated–Control	Treated	Control	Treated–Control
1994	\$26,906	\$26,940	\$–34	\$1,949	\$1,954	\$–4
1993	\$26,457	\$26,449	\$8	\$1,930	\$1,934	\$–3
1994–1993	\$449	\$491	\$–42(299)	\$19	\$20	\$–1(25)
% with 94–93 increase	54.6	53.9	0.7	53.1	52.3	0.8
n	15,536	15,624		15,536	15,624	
Either Letter						
	Federal Taxable Income			MN Tax Liability		
	Treated	Control	Treated–Control	Treated	Control	Treated–Control
1994	\$26,927	\$26,940	\$–14	\$1,946	\$1,954	\$–8
1993	\$26,346	\$26,449	\$–103	\$1,919	\$1,934	\$–15
1994–1993	\$580	\$491	\$89(270)	\$27	\$20	\$7(22)
% with 94–93 increase	54.3	53.9	0.4	52.8	52.3	0.5
n	31,149	15,624		31,149	15,624	

Notes:

Number in parentheses is the standard error.

The mean of “Treated–Control” may differ from the mean of “Treated” minus the mean of “Control” due to rounding error.

ceived either letter, and for those who served as controls.¹⁶ Consistent with the random assignment of cases to experimental groups and a lack of attrition bias, the 1993 treated and control means are not significantly different. For Letter1 (Support Valuable Services), the mean differ-

ence-in-difference for FTI¹⁷ was \$220, or those receiving the letter increased their report, on average, by \$220 more than did the controls. While the result suggests a successful moral persuasion, equal to about 0.8 percent of average income, it is not statistically significant. For Minnesota

¹⁶ We have excluded two Letter1 recipients whose reported income and taxes over the period were inconsistent: one reported 73 percent less FTI but only 35 percent less MnTx while the other reported 1.4 percent less FTI but 25 percent less MnTx. The preliminary analysis which included them yielded regression coefficients for the MnTx and FTI equations which were of widely varying proportions (i.e., the MnTx coefficients ranged from –10 to 134 percent of the FTI coefficients, while the state marginal tax rate varied only between 6 and 8.5 percent). Excluding these two treated recipients, the two sets of coefficients are more uniformly proportional.

¹⁷ The data contain two sources of FTI observations, one from the Minnesota return and, in 1993 and 1994, one from the federal return. In the analyses which follow, we use the Minnesota FTI data, except for those cases in which it is missing on the state return but available from the federal return.

tax liability, the letter recipients raised their payments, on average, by \$15 more than did the controls, also a statistically insignificant difference. Similarly, the percentages of taxpayers in the treatment group reporting increases in FTI and tax liability are insignificantly larger than for the controls.

Recipients of Letter2 (Join the Compliant Majority) reported increases in FTI and MnTx which were \$42 and \$1 lower, respectively, than those reported by the controls. Neither result is statistically significant, however. The percent reporting increases in both FTI and MnTx were larger among the treated recipients, but not sufficiently larger to be statistically significant. We therefore cannot conclude that this letter had an impact on tax compliance.

The bottom panel combines the two treatment groups. Not surprisingly, there is no significant difference in the change in reported income or liability between those taxpayers who received either letter and the control group.

Sources of Income

We also performed the same type of analysis for particular sources of income. The results of these analyses are not reported here, however, because almost without exception there was no statistically significant increase for the treated group relative to the control group in the income reported for these sources.

Differential Impact

Although we found no overall treatment effect, it is possible that the normative appeal was successful for some subgroups of the population. Since the average measures aggregate the behavior of taxpayers with very different demographic and tax characteristics, the previous analysis would not reveal this. However, a multiple regression approach, in

which a treatment dummy variable is interacted with characteristics of taxpayers, can address that issue.

Table 3 contains the results of such an analysis for Minnesota tax liability; the results for federal taxable income are qualitatively the same. The dependent variable is the difference between 1994 and inflation-adjusted 1993 Minnesota tax liability. The independent variables include a constant plus dummy variables for income ranges, filing status, age, preparer use, presence of a tax balance due, dummy variables for the presence of each of eight tax schedules or types of income, medical expense and home mortgage deductions, filing date, and the marginal tax rate. In addition, each of these variables is interacted with a dummy variable, denoted RX, which takes the value of one for those taxpayers in either treatment group. Only the coefficients of the treatment term, by itself and interacted with the return characteristics, are reported in Table 3.

It is fair to say that most of the return characteristics we investigated do not seem to be associated with the magnitude of a treatment effect. There is a suggestion that upper-middle-income taxpayers (those in the INC4 class, with total positive income between \$100,000 and \$200,000) were influenced by each letter to report more income. The highest income group appears to have reacted perversely to Letter2 (Join the Compliant Majority), however. A similar perverse response of very high-income individuals to an audit threat letter was observed by Slemrod, Blumenthal, and Christian (2000). The response in the present situation may be due to the fact that recipients may believe that the sermon letters may augur an unannounced change in aspects of the enforcement policy.

Another finding that is consistent across the two letters, if not always significant, is that the receipt of Schedule C income (self-employment) and either of two components of income reported on Schedule

TABLE 3
REGRESSION RESULTS: TREATMENT EFFECT, BY ITSELF AND INTERACTED
WITH RETURN CHARACTERISTICS

Independent Variable	Letter 1	Letter 2	Either Letter
RX	74.2 (0.32)	7.2 (0.035)	40.3 (0.20)
RXINC1	-80.1 (0.56)	-2.6 (0.02)	-43.8 (0.36)
RXINC2	-21.8 (0.21)	25.7 (0.29)	1.8 (0.02)
RXINC4	263.6 (1.56)	240.5 (1.63)	258.5 (1.78)
RXINC5	-206.6 (0.69)	-2,451.2 (9.48)	-1,343.8 (5.23)
RXMFJ	-110.9 (1.40)	74.8 (1.05)	-17.4 (0.26)
RXAGE	31.6 (0.28)	-23.2 (0.23)	0.48 (0.00)
RXEARLY	-3.2 (0.04)	-12.5 (0.20)	-8.4 (0.13)
RXPREP	82.5 (1.30)	-13.9 (0.25)	34.3 (0.63)
RXBALDUE	48.4 (0.07)	-2.9 (0.05)	21.0 (0.38)
RXSCHA	-335.6 (2.24)	267.4 (2.05)	-41.2 (0.32)
RXSCHB	98.6 (1.27)	-18.2 (0.27)	41.9 (0.63)
RXSCHC	-61.4 (0.60)	-171.2 (1.93)	-113.5 (1.30)
RXSCHD	6.3 (0.07)	2.9 (0.04)	-7.0 (0.09)
RXSCHRR	-124.7 (1.13)	-190.4 (2.00)	-158.8 (1.68)
RXSCHPS	-438.2 (3.38)	-81.8 (0.72)	-270.0 (2.42)
RXSCHF	290.8 (1.94)	-69.9 (0.53)	110.8 (0.82)
RXSCHES	4.5 (0.04)	100.7 (1.08)	53.9 (0.59)
RXMED	-0.03 (1.59)	0.01 (0.43)	-0.01 (0.68)
RXHOM	454.1 (3.12)	-334.3 (2.64)	63.1 (0.90)
RXMTR	-1.73 (0.30)	0.78 (0.16)	-0.33 (0.07)
n	31,221	31,148	46,752
R ²	0.0065	0.0181	0.0086

Note: The dependent variable is the change between 1993 and 1994 of reported Minnesota tax liability, in 1994 dollars. Also included in the regressions, but not reported here, are a constant and all of the return characteristics not interacted with the treatment dummy variables. The absolute value of t-statistics are in parentheses.

TABLE 3 (continued)

Variables are defined as follows:

RX	= 1 if taxpayer received treatment letter
INC1	= 1 if 1993 TPI \leq \$20,000
INC2	= 1 if \$20,000 < 1993 TPI \leq \$50,000
INC4	= 1 if \$100,000 < 1993 TPI \leq \$200,000
INC5	= 1 if 1993 TPI \geq \$200,000
MFJ	= 1 if 1993 MN filing status is married, filing jointly
AGE	= 1 if taxpayer or spouse is older than 65 in 1993
EARLY	= 1 if 1994 return was filed within one month of the date the treatment letter was mailed
PREP	= 1 if used practitioner to file 1993 MN return
BALDUE	= 1 if 1993 federal balance due > 0
SCHA	= 1 if 1993 Schedule A itemized deductions > 0
SCHB	= 1 if 1993 net taxable interest and dividends > \$400
SCHC	= 1 if 1993 Schedule C net profit/loss is non-zero
SCHD	= 1 if 1993 capital gains is non-zero
SCHRR	= 1 if 1993 rents and royalties is non-zero
SCHPS	= 1 if 1993 partnership and S corp. income is non-zero
SCHF	= 1 if 1993 Schedule F net profit/loss is non-zero
SCHES	= 1 if 1993 estimated tax is non-zero
MED	= 1993 Schedule A medical expenses
HOM	= 1 if 1993 Schedule A interest paid to banks or individuals > 0
MTR	= Federal + state marginal income tax rate

Notes: All dummy variables equal 0 if criterion is not met. TPI refers to total positive income, which is the sum of those income components that are positive. RX before a variable name refers to a multiplicative interaction of the treatment dummy and the independent variable.

E (rents and royalties as well as partnerships and S corporation income) are negatively associated with the magnitude of a treatment effect. This is consistent with the hypothesis that those with greater opportunity to evade will be less susceptible to a normative appeal.

One striking and surprising result is that our indicator of paying mortgage interest is associated with the treatment effect, but positively for Letter1 (Support Valuable Services) and negatively for Letter2 (Join the Compliant Majority). Recall that Song and Yarbrough's argument that homeowners, feeling more connected to society, embrace a higher level of tax ethics. This evidence suggests that homeownership makes people susceptible to one type of normative appeal but makes them resent (and react negatively to) another, and is a fascinating subject for further research.

CONCLUSION

In the first large-scale controlled experiment of this kind, we find little or no

evidence that either of two normative appeals delivered by letter affects aggregate tax compliance behavior. One could interpret the dearth of evidence for an overall treatment effect in at least two ways. One interpretation is that the normative appeals used in this experiment did not have any effect on taxpayer attitudes, and therefore were not successful in changing compliance behavior. Another possibility is that the letters did have an impact on attitudes, but that those attitudinal adjustments did not translate into behavioral changes, either because the attitudinal shifts were too small or because the affected attitudes are not related to compliance behavior. Some interaction or communication with subjects during the experiment might have made it possible to distinguish between these interpretations.

We recognize that this conclusion may apply only to policies that are similar to the field experiments we carried out. In this exercise, taxpayers received a letter from the state revenue department once, after the tax year had

ended.¹⁸ Communications of a different sort, delivered in a different way, or with greater frequency might still produce a compliance effect.¹⁹ Second, as Bardach (1989) points out, normative communications may not always deliver the message intended. As an example, when a state revenue department tells a taxpayer (as in Letter 2) that “people who file tax returns report correctly and pay voluntarily 93 percent of the income taxes they owe,” the statement may be interpreted to mean that the revenue department is unable to detect cheating. This possibility makes the results of controlled experimentation with normative appeals difficult to interpret. Nevertheless, we conclude that these experimental results yield no evidence for policy makers that normative appeals will bring in additional tax revenues and, for researchers, no evidence that this kind of normative appeal affects tax compliance.

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¹⁸ While we attempted to repeat the treatment a year later, the research design failed to produce the randomization we had expected.

¹⁹ Whether one-shot communications can be effective is also controversial among advertising researchers. The minimalist school argues that one exposure may be enough to trigger maximum response to advertising. Repetitionists believe that advertising repetition is essential for optimal consumer response. Tellis (1997) reviews this controversy and concludes that single ad exposures are effective only in specific situations defined by relatively simple messages that are not particularly novel, and that reflect “reminder” ad campaigns for mature brands in mature categories. Our appeal to conscience treatment does not seem to fit this category, so our failure to find a significant response seems to be consistent with Tellis’ conclusion.

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MINNESOTA Department of Revenue

January, 1995

Dear Taxpayer:

The income tax filing season has started. We at the Minnesota Department of Revenue want to remind you that filing before April 17 will let us process your return faster.

Your income tax dollars are spent on services that we Minnesotans depend on. Over 30 percent of state taxes go to support education. Another 18 percent is spent on health care and support for the elderly and the needy. Local governments get about 12 percent of the state tax money, supporting services in your community such as law enforcement, parks, libraries and snow removal. Other tax dollars pay for highways and for cleaning up the environment. So when taxpayers do not pay what they owe, the entire community suffers.

As you prepare your return, or give instructions to your tax preparer, please be very careful to report all your income and take only the deductions to which you are entitled.

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:

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| 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

MINNESOTA Department of Revenue

January, 1995

Dear Taxpayer:

The income tax filing season has started. We at the Minnesota Department of Revenue want to remind you that filing before April 17 will let us process your return faster.

According to a recent public opinion survey, many Minnesotans believe other people routinely cheat on their taxes. This is not true, however. Audits by the Internal Revenue Service show that people who file tax returns report correctly and pay voluntarily 93 percent of the income taxes they owe. Most taxpayers file their returns accurately and on time. Although some taxpayers owe money because of minor errors, a small number of taxpayers who deliberately cheat owe the bulk of unpaid taxes.

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