Taxpayers' Response to Warnings of a Possible Tax Audit: Do They Change Their Compliance Behavior?

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Abstract

In 2008, the New York State Department of Taxation and Finance sent letters to clients of a fraudulent tax preparer, warning them of a possible audit and asking them to participate in the Department's Voluntary Disclosure and Compliance Program if they had filed inaccurate tax returns in the past. This study examines the impact of the letters on voluntary compliance in their future (2008 and 2009) returns. In this study, a simple method similar to "difference in differences", which we call "difference in positions", is applied. 10,000 samples are randomly drawn from the taxpayer population and the growth rates of Federal adjusted gross income (AGI) for these samples are put into relative frequency density graphs. We then examine the relative positions of the experiment group (the clients of the fraudulent tax preparer) within the normally distributed curves before and after the letters were sent. The change in the relative positions is regarded as the letter impact on voluntary compliance. It is found that the impact is significant in the first year (2008 tax returns) after the letters were sent. The impact is 17.49 percentage points on the AGI growth rate, which translates to $8.68 million of reported AGI for the 507 taxpayers in the experiment group. However, the impact is minimal in the second year (2009 tax returns), indicating that the long-run effect of the letter mailings may be weak.

1. Introduction

Since early 2007, the New York State Department of Taxation and Finance has been closely monitoring State tax returns prepared by paid tax preparers. Often in conjunction with other

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State and Federal law enforcement agencies, the Tax Department has successfully prosecuted and convicted some fraudulent tax preparers and fined some others. A number of fraudulent tax preparers chose to cooperate with the Tax Department and provided information about their clients. In other instances, taxpayers have provided information about their fraudulent tax preparers.

In 2008, one fraudulent tax preparer, whom we shall call Preparer X, made a plea bargain to cooperate with the Tax Department and agreed to provide a list of all his clients in order to avoid possible prosecution. In October 2008 the Tax Department mailed a letter to his clients to alert them that their tax preparer "has admitted to assisting taxpayers in the filing of fraudulent and inaccurate tax returns", and that their "tax return may be audited". A second letter was mailed a month later. Although the tones of the two letters were different, the first letter contained "harsher" language and the second letter contained "softer" language, both of them asked the taxpayers to come forward to correct the mistakes in their past tax returns, stating "If you believe that you may have filed inaccurate tax returns prepared by (the name of Preparer X), it is not too late for you to avoid penalties by participating in the Department's Voluntary Disclosure and Compliance Program."

Both letters were sent to the clients of Preparer X in the last quarter of 2008. In early 2009, when the 2008 tax returns were to be filed, those taxpayers who had received the letters had decisions to make. First, they must decide if they would prepare their tax returns themselves or hire a new tax preparer. Second, if they believed that they had filed inaccurate tax returns to reduce their tax liabilities in the past, they must decide if they would continue this practice in their new returns.

The purpose of this study is to examine if these taxpayers altered their behavior in preparing their tax returns after having received the targeted letters from the Tax Department. More specifically, the question we try to answer is, after the taxpayers had received the letters in October and November of 2008, did they, as a group, change their behavior and comply more strictly with tax laws on their 2008 and 2009 tax returns?

In the literature, there are two distinctive approaches to studying tax compliance. The first approach is based on theoretical grounds; and the second is applying laboratory experiments to analyze the behavior shifts in compliance after simulating tax policy and regulation changes.

In the first approach, the bases of theoretical grounds are not uniform among the researches in the literature. One group of studies is based on the classical microeconomic theories where taxpayers are assumed to be rational and follow the rule of utility maximization in making their tax compliance decisions. Their ultimate goal is to maximize their expected utility under uncertainty (For example, see Allingham and Sandmo 1972). Another group of the studies introduces sociological and psychological factors, such as moral, shame, trust, political power, and game theory, into their theoretical considerations. They hope these factors can explain some compliance phenomena which the simple utility models could not explain. (For example, see Bernasconi 1998, Kirchler, Hoelzl, and Wahl 2008, and Kirchler 2007). Under this approach, a wide range of interesting factors influencing tax compliance is studied. For example, some researchers propose that taxpayer uncertainty has a positive effect on compliance, (Alm, Jackson, and McKee 1992(2), and Beck and Jung 1989). Another example is the work by Erard and Feinstein (1994), who built a game-theoretic model of tax compliance which challenges the notion that honest taxpayers do not significantly influence most aspects of tax compliance systems.

The second approach, the laboratory experiment approach, has been gaining popularity. In this approach, experiment participants, either students or real taxpayers, are provided specific information regarding audit, amnesty, or other tax policies. Then they are asked to file tax returns (fake or real tax returns.) The tax return data are analyzed to reveal their compliance behavior. For example, Alm and McKee (2006) apply experimental methods to examine the individual compliance responses to a "certain" probability of audit, and conclude that the compliance rate rises if an individual knows he will be audited and the rate falls if he knows he will not be audited. Slemrod, Blumenthal, and Christian (2001) randomly select taxpayers and inform them that their filling will be "closely examined' and found evidence of taxpayers' behavior changes in response to an increased probability of audit, although the responses are not uniform among different groups of taxpayers. Another experimental research by Alm, McKee, and Beck (1990) finds that the effectiveness of an amnesty program depends on the design of the program and the enforcement efforts post the amnesty. Alm, Jackson, and Mckee (1992(1)) use data from laboratory experiments to estimate the effects on compliance of the major fiscal instruments and conclude that, among others, there is a positive relationship between audit rate and compliance, but they caution about the generalization from the estimates based on the experiments. Another study by Mittone (2006) concludes that early experience of audits in taxpayers' "tax life" is a more effective way to increase compliance than later audits. Yet another experimental research by Kastlunger, Kirchler, Mittone, and Pitters (2009) also suggests that, although the effectiveness of audits and fines cannot be completely confirmed, early audits in taxpayers' "tax life" have a positive impact on compliance. For detailed discussions on the pros and cons of the laboratory experiment approach, see Angrist and Pischke (2010), Leamer (1983, 2010), Keane (2010), and Sims (2010).

The study presented here is different from those in the literature in several ways. First, we build a model exclusively for investigating the compliance behavior shifts for a targeted group of taxpayers after they received letters from the Tax Department warning them of a possible audit. Second, a simple research method similar to "difference in differences", which we call "difference in positions", is applied. It is a new approach to the studies of tax compliance.

Third, real tax return data from taxpayers are used in this study instead of experimental data often used in the literature.

The remainder of this paper is broken down as follows: Section 2 discusses data, Section 3 discusses the methodology used in this study, Section 4 presents the estimation procedure and results, Section 5 performs the statistical significance test on the estimate, and Section 6 concludes the paper.

2. Data

The study period for this research is from 2006 to 2009. The data are from three data sets maintained by the New York State Department of Taxation and Finance. The first data set is from the Department's legacy production system, which contains the State personal income tax data, including taxpayers' names, addresses, IDs, and other State tax return information, such as Federal adjusted gross income, deductions, credits, and tax liabilities. From this data set, we obtain 2006 and 2007 tax return information for State taxpayers.

The 2008 and 2009 personal income tax data are from a data set named "Complete" and populated from the Tax Department's new production system. This data set contains complete State personal income tax return information for 2008 and 2009 at the individual level.

From these two data sets, four variables, taxpayers' names, ID numbers, tax liability years, and Federal adjusted gross income (AGI), are extracted.

The third data set is the one containing the tax return information on the 1,036 clients of Preparer X. The list of taxpayers was provided by Preparer X as part of a plea bargain and the data set was created by the Office of Tax Enforcement within the Tax Department.

3. Methodology

In this study, we will use a technique similar to "difference in differences" method to investigate the letter impact on the voluntary compliance for the clients of Preparer X. For this purpose, the taxpayers' Federal AGI reported in their State tax returns is chosen as the object of this study, since it is one of the best indicators of the voluntary compliance.

The State personal income taxpayers are classified into two categories: One is the experiment group which contains the taxpayers who received the Department letters; the other is the control group which contains the taxpayers who did not receive the letters.

The “difference-in-differences” method has been applied broadly in economic impact analyses since the early nineties. Influential applications include, among others, Card and Krueger (1993, 1994), Eissa and Liebman (1996), and Blundell, Duncan , and Meghir (1998). This method is usually used to examine the impact of treatment by comparing the experiment group after treatment both to the experiment group before treatment and to some other control group. This method uses a control group to subtract out changes caused by factors other than the treatment, assuming that the changes caused by factors other than the treatment are identical among the experiment and control groups. For a detailed discussion of the “difference-in-differences” method, see Imbens and Wooldridge (2009).

In this study, we have similar assumptions as those underlying the “difference-in-differences” method and the main principles underlying this study are the same as those underlying the “difference-in-differences” method. In the “difference-in-differences” method, people examine changes “in differences”, while in this study, we examine changes “in relative positions” in normally-distributed observations. Therefore, we call our method “difference in positions”.

Specifically, we made three assumptions. First, macroeconomic indicators, like the national and state economic situations, have the same or similar impact on Federal AGI for both the experiment group and the control group. Second, the changes in Federal and State laws, policies, and regulations, such as changes in tax rates and deductions, have the same or similar impact on AGI for both the experiment group and the control group. And third, the ranking of AGI growth rate for a randomly-drawn sample of taxpayers among all samples, as long as the samples are large enough, is consistent over time, except in cases where there are factors which affect only the taxpayers in that sample. The third assumption is reasonable because 1) samples are randomly drawn; 2) the components of samples remain the same over time; and 3) all externalities with the exception of the letters have the same impact on the growth of AGI for all samples.

Based on these assumptions, it is clear that, if there is a significant change in the ranking of the AGI growth rate for the experiment group among all samples, the change must arise from factors which are only applicable to the experiment group. What is the difference between the experiment group and samples of the control group? The only difference is that the individuals in the experiment group are the taxpayers who received the Tax Department letters, while the individuals in other samples did not. Therefore, we attribute the ranking change, if any, to the letter impact.

4. Estimation Procedure

There are six steps used to estimate the letter impact. The first step is the classification of taxpayers. Initially, a group of more than 4.6 million New York State personal income taxpayers are selected from the whole population. These taxpayers are chosen because they meet the following two conditions: 1) they filed State tax returns for each of the four years in our study period (form 2006 to 2009); and 2) their identities can be explicitly verified. Cases in which two or more taxpayers share the same identification number or in which a taxpayer doesn't have a valid identification number are dropped. The total taxpayers are classified into two groups, the experiment group and the control group, as specified in Eq. 1.

$$p = c + e \qquad \text{(Eq.1)}$$

where p is the total population of the taxpayers, c is the number of taxpayers in the control group, and e is the number of taxpayers in the experiment group.

The experiment group, e, consists of 507 taxpayers, which we shall call Sample 1 (s_1), as described in Eq. 2. All of these taxpayers are on the list of clients provided by Preparer X and received the warning letters from the New York State Department of Taxation and Finance. Originally, there are 1,036 taxpayers on the list, but about half of these 1,036 taxpayers are dropped because either they don't have complete data for the whole study period from 2006 to 2009, or their identities could not be explicitly verified.

$$e = \text{Sample 1 } (s_1\text{, 507 taxpayers}) \qquad \text{(Eq. 2)}$$

The control group consists of taxpayers who have never been clients of Preparer X and did not receive the letters. In the second step, we draw 10,000 samples from the control group, as described in Eq. 3. Each sample contains 500 taxpayers who are randomly drawn from the population of 4,555,397 taxpayers (with replacement) in the control group.

$$c = \begin{cases} \text{Sample 2 } (s_2\text{, 500 taxpayers}) \\ \text{Sample 3 } (s_3\text{, 500 taxpayers}) \\ \text{Sample 4 } (s_4\text{, 500 taxpayers}) \\ \ldots\ldots \\ \text{Sample 10,001 } (s_{10001}\text{, 500 taxpayers}) \end{cases} \qquad \text{(Eq.3)}$$

In the third step, the individual AGI of the 500 taxpayers in each sample obtained from Step Two is summed up to aggregate AGI for each year from 2006 to 2009, as described in Eq. 4.

$$A_{st} = \qquad {}_{sit} \qquad \text{(Eq.4)}$$

where A_{st} is the total AGI for the taxpayers in sample s at time t, (s = 2,3, …, 10001, and t = 2006, 2007, …, 2009) and AGI_{sit} is the AGI for taxpayer i in sample s at time t, (i = 1, 2, …, 500).

Then the growth rate of the total AGI of each sample is calculated in Eq.5.

$$GA_{st} = A_{st} / A_{st-1} - 1 \qquad \text{(Eq. 5)}$$

where GA_{st} is the AGI growth rate for sample s at time t. For each sample, we have three years of AGI growth rates, from 2007 to 2009. In a similar manner, we calculate the growth rates of total AGI for the 507 taxpayers in the experiment group, s_1. The result is that for each year from 2007 to 2009, we have 10,001 AGI growth rates, 10,000 for the control group and 1 for the experiment group.

In the fourth step, we consider the distribution of the AGI growth rates of the 10,001 samples. Because all samples are randomly drawn except s_1, it is expected that the distribution is normal or nearly normal, excluding a few outliers. Figure 1 to figure 3 are the histograms of the AGI growth rate of the samples for each of the three years from 2007 to 2009, respectively. In these histograms, 9,859 samples (including s_1) are included while the 142 extreme outliers (less than 1.5 percent of the total) are dropped.

Figure 1.

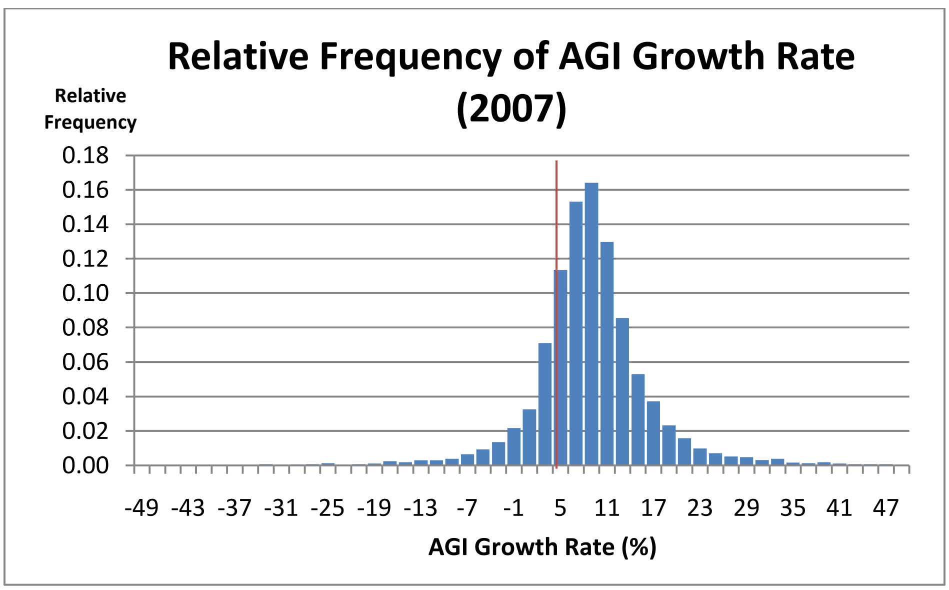


Figure 2.

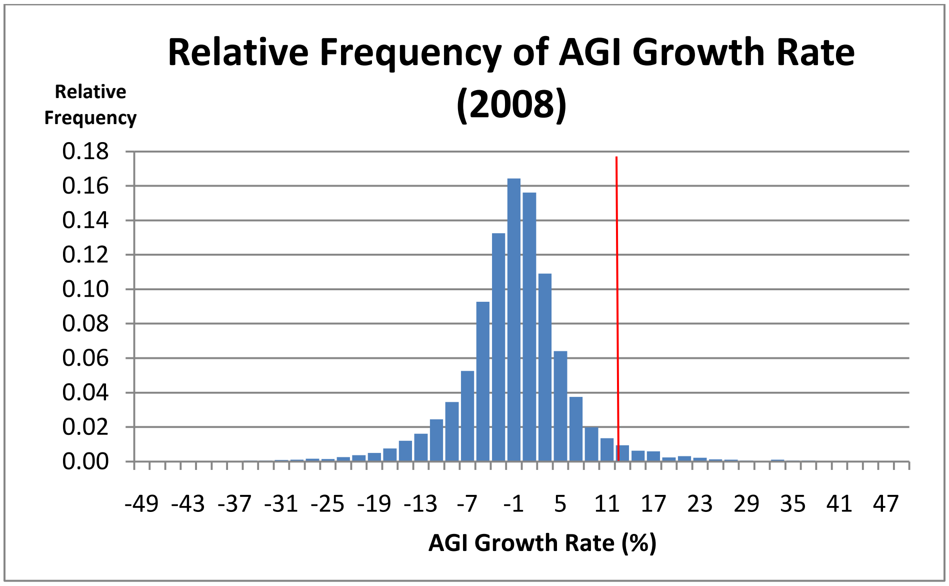
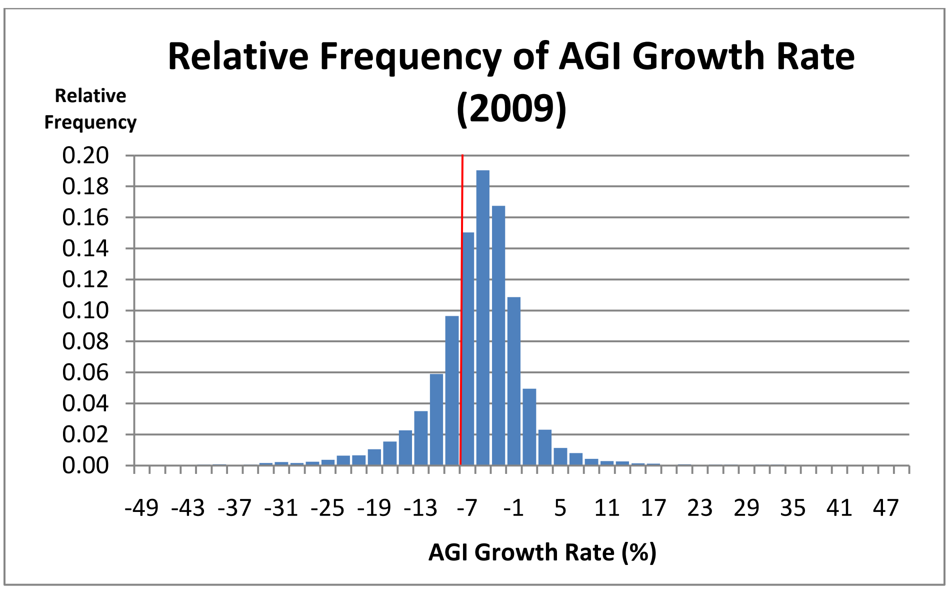


Figure 3.



In the fifth step, we draw a vertical reference line in each of these histograms to represent the growth rate of AGI of the experiment group (s_1) to see if there is a distinctive change in the relative positions for the experiment group over time. It is apparent that, from 2007 to 2008, there is a huge shift (relative to the normal curves) from left to right for the reference line, and, from 2008 to 2009, a huge shift from right to left. The graphs show that the position of the reference line in 2009 is very similar to that in 2007.

The letter impact on the reported AGI growth for the experiment group is calculated in step six. First, we calculate the percentile of the AGI growth of the experiment group among all samples in the base year (2007 tax year, before the letters were sent). Then we use the calculated percentile to find out what the growth rate would be in 2008 and 2009 (we call it assumed growth rate) if no letters had been sent, assuming there is a consistency in the relative position for the experiment group. The difference between the actual AGI growth rate of the experiment group and the assumed rate then is attributed to the letter impact on reported AGI. The estimation results are presented in Table 1 followed by a detailed explanation of the table.

Table 1.

Impact of Department Letters on Reported AGI
(For the 507 Taxpayers in the Experiment Group)

Year	AGI ($millions)	Actual Growth Rate (%)	Rank in Growth*	Growth Percentile *	Assumed Percentile (2007)*	Assumed Growth Rate (%)	Impact on Growth (percentage Points)	Impact on AGI ($millions)
(a)	(b)	(c)	(d)	(e)	(f)	(g)	(h)	(i)
2006	47.50							
2007	49.60	4.43	1,935	19.6	19.6	4.43		
2008	55.67	12.23	9,522	96.6	19.6	-5.26	17.49	8.68
2009	51.28	-7.89	2,723	27.6	19.6	-9.37	1.48	0.82

* Rank and percentile refer to those of the experiment group among all samples.

In Table 1, Column (b) is the reported AGI for the 507 taxpayers in the experiment group (s_1) and Column (c) is the AGI growth rate. The rank of the growth rate of the experiment group among the 9,859 samples (from lowest to highest) is presented in Column (d). For example, in 2007, the growth rate of 4.43 percent for the experiment group ranks 1,935th among the 9,859 samples. Then we calculate the percentile of the growth by dividing the rank by 9,859, which is presented in Column (e). For 2007, the growth rate for the experiment group is 19.6th percentile (1,935/9,859).

Based on our assumptions presented in Section 3, we have already reasoned that, if there is no letter impact, there would be no significant changes, compared with 2007, in the ranking of the AGI growth rate in 2008 and 2009 for the experiment group. That just means the percentile in the growth rate of the experiment group for 2008 and 2009 would remain the same as or similar to that for 2007, 19.6th percentile. Therefore, we apply the 19.6th percentile to both 2008 and 2009 as the assumed percentile, as presented in column (f). Then we use the growth rate of the sample with 19.6th percentile in 2008 as the assumed growth rate of the experiment group for

2008. Also, we apply the same method to obtain the assumed growth rate of the experiment group for 2009. In 2008, the growth rate of the sample with 19.6th percentile is -5.26 percent; while in 2009, the growth rate of the sample with 19.6th percentile is -9.37 percent. The assumed growth rate is presented in Column (g). We call the -5.26 percent for 2008 and -9.37 percent for 2009 "the assumed growth rate" because these would be assumed as the real growth rates for the experiment group if the taxpayers in this group had not received the Department letters.

The letter impact on AGI growth rate is presented on Column (h), which is the difference between the actual growth rate, Column (c), and the assumed growth rate, Column (g). For example, in 2008, the AGI for the experiment group would grow -5.26 percent if the taxpayers had not received the Department letters. Instead, the actual growth rate for this group is 12.23 percent. The difference, 12.23 - (-5.26) or 17.49 percentage points, should be regarded as the letter impact on the growth rate.

The dollar amount of the impact on AGI is presented in Column (i), which is obtained by multiplying the impact on growth rate, Column (h), by the AGI (Column (b)) in the previous year,.

It is clear in Table 1 that the letter impact is huge in 2008. Because of the letters, the AGI growth rate is 12.23 percent instead of -5.26 percent. The difference is 17.49 percentage points. Because of the letters, the 507 taxpayers in the experiment group reported $8.68 million more in AGI, or $17,210 per taxpayer. After applying the median State personal income tax rate, 5.25 percent, to the increased AGI, the State generated $0.46 million more in personal income tax because of the letters, averaging $899 per taxpayer.

Table 1 also shows that the impact diminishes in the following year. In 2009, the impact is only 1.48 percentage points on AGI growth rate and $0.82 million on reported AGI. After applying the median State personal income tax rate, 5.25 percent, the State generated only $0.043 million more in personal income tax. One explanation for this is that because most taxpayers in the experiment group did not see any action by the Tax Department in 2009, they would think the warning in the letters was just a way for the Department to generate revenue without doing any actual audit work. Therefore, the taxpayers resumed their prior behaviors in order to reduce their tax liabilities on their 2009 tax returns.

5. Statistical Significance of the Estimation

In this study, we attribute the relative position change in the AGI growth for the experiment group to the letter impact. In this section, we will use the results of the 2008 estimation to check if the change is statistically significant compared with the changes of the control group by applying a statistical tool called “p-value”.

A p-value is a measure of how much evidence we have against the null hypothesis. The null hypothesis, traditionally expressed by the symbol H0, represents the hypothesis of no change or no impact. The smaller the p-value, the more evidence we have against H0. It is also a measure of how likely we are to get a certain sample result or a result “more extreme,” assuming H0 is true.

In Table 1, we have already calculated the relative change of the AGI growth rate due to the letter impact for the experiment group. For 2008, the change is 17.49 percentage points (Column (h) of Table 1). However, from 2007 to 2008 the relative position of each of the samples of the control group may have also changed. If we use the same method applied to the experiment group to calculate the “letter impact” for the control group samples, we may find that the “letter impact” may be even larger for some control group samples than that for the experiment group. Here, we use quotation marks for ‘letter impact” because they are actually not letter impact but impact caused by other factors (other than letters) pertaining only to the particular samples. For example, if a sample contains a taxpayer who was newly appointed in 2008 to a CEO position in a big financial firm, then it may make the AGI growth rate of the sample much higher, significantly changing the relative position of the sample. Under these circumstances, it is appropriate to use p-value to check the statistical significance of our estimate.

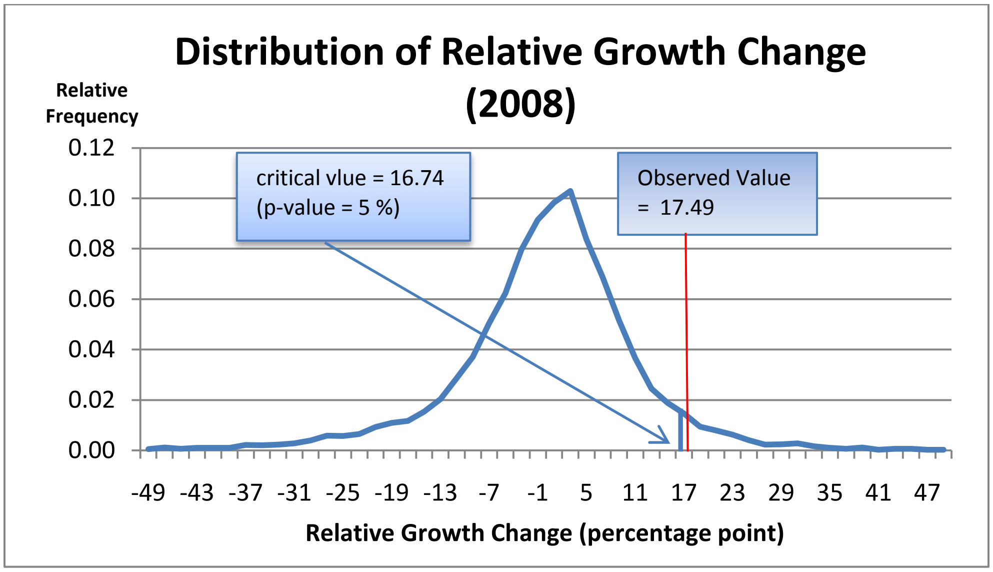
In the test, the samples, one for the experiment group and 9858 for the control group, are regarded as a population while the experiment group (s_1) is regarded as a random sample. Our aim is to detect if the sample is distinctly different from the population. We will do the test based on the following:

- Null hypothesis (H0): the change in AGI growth of the experimental group (s_1) is not distinctly different from that of the population;
- Observation O: the change in reported AGI of the experimental group (s_1) is 17.49 percentage points.

A p-value of 5 percent or less would reject the null hypothesis (H0) "at the 5 percent significance level’; otherwise, H0 would not be rejected.

First, the change of each of the control group samples is calculated in the same way as that of the experiment group presented in Table 1 and the probability density of these changes is presented in Figure 4. Next, we calculate the critical value of 5 percent for the relative growth change, which leaves 5 percent of total samples at the right tail of the curve in figure 4.

Figure 4.



The critical value is obtained by examining the ranking of the AGI growth changes presented in Figure 4. First we multiply the number of the samples by 95 percent to obtain the ranking of the sample with 95 percentile, which is 9,366 (9,859x0.95=9,366). Then we sort the AGI growth changes of the samples from low to high and obtain the AGI growth change of the sample which is ranked 9,366th. It is found that the AGI growth change for this sample is 16.74 percentage points, which is considered to be the critical value corresponding to the 5 percent p-value. In Figure 4, the area under the curve on the left-hand side of the critical value represents 95 percent of the samples, while the area on the right-hand side of the critical value represents the remaining 5 percent of the samples.

It is clear that our observation O, 17.49 percentage points (vertical line in Figure 4), is on the right-hand side of the critical value. Therefore, the null hypothesis that "the change in AGI growth of the experimental group is not distinctly different from that of the population" is rejected at the 5 percent significance level. What does this mean in plain English? If the letters have no impact (that is, if H0 were true), there would have been only less than 5 percent probability of observing the AGI growth change as large as 17.49 percentage points. Therefore, it can be concluded that our estimation of the AGI growth change for the experiment group is statistically significant.

6. Summary and Conclusions

In 2008, the New York State Department of Taxation and Finance sent two letters to clients of a fraudulent tax preparer warning them of a possible audit and asking them to come forward to participate in the Department's Voluntary Disclosure and Compliance Program if they had filed inaccurate tax returns in the past. This study examines the impact of the letters on voluntary compliance in their future (2008 and 2009) tax returns.

In this study, a research method similar to "difference in differences", which we call "difference in positions", is applied. 10,000 samples are randomly drawn from the taxpayer population and the AGI growth rates of these samples for each year are put into relative frequency density graphs. We then examine the relative positions of the experiment group (the clients of the fraudulent tax preparer) in the normally distributed curves before and after the letters were sent. The change in the relative positions is regarded as the letter impact on voluntary compliance.

It is found that the impact is significant in the first year (2008) after the letters were sent. Because of the letters, the clients of the fraudulent tax preparer reported a Federal AGI growth rate of 12.23 percent in 2008 tax returns, compared with the growth rate of -5.26 percent if there had been no letters sent to them. The impact therefore is 17.49 percentage points on the AGI growth rate, which translates to $8.68 million more reported AGI for the 507 clients. After applying the median State tax rate to the $8.68 million, the State collected $0.46 million more in personal income tax because of the letters.

It seems that the letters have no long-run impact on voluntary compliance. The impact diminishes quickly in the following year. For 2009 tax returns, the clients of the fraudulent tax preparer reported a Federal AGI growth rate of -7.89 percent, compared with the growth rate of -9.37 percent if there had been no letters sent to them. The impact, therefore, is only 1.48 percentage points on the AGI growth rate, which translates to $0.82 million more reported AGI for the 507 clients. The State collected only $0.043 million more in personal income tax in the second year after the letters were sent.

There is a tendency for this study to underestimate the letter impact because it uses changes in the reported Federal AGI to examine the impact. Components not included in the Federal AGI but included in the State tax returns, such as State deductions and credits, are not considered. In reality, we know that if a taxpayer makes fraudulent claims in the components of Federal AGI, he will have a tendency to make fraudulent claims in other components outside Federal AGI both in the Federal and State tax returns.

Furthermore, the intention of this study is to investigate the behavior changes of the clients of a fraudulent tax preparer. Though this tax preparer provided information on 1,036 clients, this study only includes 507 of them because of data limitations. If the letters had an impact on the 507 taxpayers, then they must also have had an impact on the other 529 taxpayers not included in this study. Therefore, the dollar amount of the impact in this study is most likely underestimated. For this reason, we think the impact on AGI growth rate may be a better measurement than that on the dollar amount of AGI.

It should also be noted that this study concerns the letter impact on voluntary compliance in future tax returns after the letters were sent, not the total letter impact. The total letter impact should be higher because it includes both the impact on the voluntary compliance in future tax returns and the impact on the pre-2008 returns for some taxpayers who decided to participate in the Tax Department's Voluntary Disclosure and Compliance Program and correct the inaccuracies in their pre-2008 returns.

The methodology used in this paper, "difference in positions", may be applied to further studies on the voluntary compliance. For example, we use Federal AGI as the study object in this paper. In the future, we may apply the methodology to total taxable income or total New York State taxes reported in the State income tax returns. Furthermore, the methodology may be easily applied to other impact studies, especially in cases where the regression analysis is not appropriate due to a short span of data.

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