Once Bitten, Twice Shy?
The Lasting Impact of IRS Audits on Individual Tax Reporting*

Jason DeBacker†, Bradley T. Heim‡,
Anh Tran§, and Alexander Yuskavage**

January 8, 2016

Abstract

We study the impact of tax enforcement activity on subsequent individual taxpaying behavior. We exploit three waves of randomized Internal Revenue Service (IRS) audits of individual income tax filers during the 2006-2009 period to study both the short and long run effects of audits on taxpaying behavior. Rich and confidential IRS data allow us to discover several effects on post-audit taxpaying behavior. First, we find that taxpayers respond to audits by increasing reported income. This impact is stronger but more quickly diminishing for incomes not subject to third-party information. Second, individuals with higher income volatility are found to revert to their pre-audit behavior more quickly, conceivably because of a larger degree of asymmetric information between the filer and enforcement agency. Finally, we find larger and more persistent responses to audit among those who are less tax literate. Overall, the IRS's revenue gain from the long-term impact of audits is about twice as large as the revenue gained directly from the audit itself.

Keywords: tax audit, tax evasion, tax avoidance, individual income tax, IRS

JEL Classifications: H26, K42

---

* We thank Alan Plumley and John Guyton for helpful discussions, seminar participants at George Mason's SPGIA, Harvard Kennedy School, the Spring 2015 NBER Public Economics Program Meetings, and the 2014 National Tax Association Annual Meetings. The views expressed in this paper are those of the authors and do not necessarily reflect the views of the U.S. Department of the Treasury or the Office of Tax Analysis.
† Middle Tennessee State University, T: (615) 898-2528, E: jason.debacker@gmail.com; ‡ School of Public and Environmental Affairs, Indiana University, T: (812) 855-9783, E: heimb@indiana.edu; § School of Public and Environmental Affairs, Indiana University, T: (812) 855-0563, E: trananh@indiana.edu; ** Office of Tax Analysis, U.S. Department of the Treasury, T: (202) 622-0694, E: alexander.yuskavage@treasury.gov.

1. Introduction

According to the Internal Revenue Service, the tax gap - the difference between taxes paid and what taxpayers should be paying under law - was approximately $385 billion in 2006 (IRS 2012). The majority of the tax gap is associated with the individual income tax. To increase tax compliance, the IRS audits about 1% of individual filers each year (TRAC Reports, Inc. 2014). These audits have both immediate and long-term effects on tax filers. The immediate effect is that the taxpayer must make up tax discrepancies uncovered during the audits. The long-term effect is a deterrence of future behavior by the audited tax filers. This study focuses on these long-term effects of audits by examining changes in the taxpaying behavior of a large sample of tax filers who were randomly audited by the IRS.

In economics, the formal models of tax compliance date back at least to Allingham and Sandmo (1972) who build on the earlier work on the economics of crime by Becker (1968). While the theoretical literature on tax compliance has extended the seminal model of Allingham and Sandmo (1972) in a number of ways, empirical work on how audits shape the subsequent behavior of taxpayers is scant. More broadly, there have been very few rigorous empirical studies on how audits or other enforcement activities can affect long-run behaviors of different kinds.

Conceptually, there are two conflicting expectations about the effects of audits on taxpaying behavior. The first, and perhaps more intuitive, is that experiencing an audit leads taxpayers to revise their perceived audit probability up and therefore reduce their subsequent noncompliance. Afterwards, for each year they do not experience another audit, taxpayers may revise the perceived probability downward, encouraging noncompliance. Therefore the post-audit tax payment trend would consist of an immediate increase followed by a decrease. The second hypothesis predicts an opposite behavior. Taxpayers may, correctly or

incorrectly, believe that auditors rarely come back immediately after an audit, and therefore that it is safest to evade taxes immediately following an audit. As years pass, the risk that auditors will return increases and thus it is best to reduce noncompliance. In this case, the post-audit tax payment trend would be an immediate fall followed by an increase.

Empirically, two new studies using taxpayer data find support for both of these patterns of behavior. DeBacker et al. (2015) use U.S. tax data to show that corporations tend to pay *less* tax following an audit. In contrast, Advani et al. (2015) use U.K. data to show that self-employed individuals in the U.K. tend to pay *more* tax after an audit. Previous studies rely mostly on lab experiments and can show either pattern.[1] These conflicting expectations and patterns suggest that the important question of the long-term impact of audits is not yet resolved, either conceptually or empirically.

To tackle this question, we need an understanding of how taxpayers determine their level of compliance. For example, there is the observation of why, given the very low audit rates and penalties, people choose to evade so little tax. To explain this puzzle, some studies have shown that moral and culture factors can play an important role in mitigating people's willingness to evade tax.[2] Recently, Kleven et al. (2011) point out the importance of third-party information, which makes Danish taxpayers unable, instead of unwilling, to cheat. Kleven and his coauthors highlight the problem of information frictions in the area of tax compliance.

In this paper, we propose that information frictions may also help us to understand the long-term impact of audits. In fact, tax authority usually has imperfect information about the true income of a taxpayer. The tax authority makes attempts update this information from

[1] Notable lab studies are Alm and Mckee (2004), Fortin et al. (2007), Kirchler et al (2007) and Kastlunger et al (2009), and Choo et al. (2013).
[2] Notable studies in this area include Alm, McClelland, and Schulze (1992), Andreoni, Erard, and Feinstein (1998), and Feld and Frey (2002).

two main sources, namely, third-party reporting and tax audits. While the information from a third-party (e.g. an employer) tends to be updated annually, the audit information is updated only when an audit is conducted. When audits are not conducted, information from previous audits gradually becomes outdated. The speed of *updating* and *outdating* of income information allows those who wish to evade tax the opportunity to strategically manipulate their income reporting.

To test for the long-term effects of audits on tax reporting, we use data from the Internal Revenue Service's (IRS) National Research Program (NRP). The NRP began conducting random audits of individual tax filers starting in tax year 2001, and began conducting annual random audits starting in 2006. To these data, we merge returns from the universe of filers from 2000 to 2012, allowing us to examine the impact of audits on individual taxpaying behavior for a period of up to six years after an audit.[3] Importantly, we are able to determine changes in tax paying behavior for both the employed and self-employed and measure the responses to audit across a large set of income and deduction items. These data are instrumental in addressing the impact of legal enforcement on subsequent behavior for several reasons. First, the IRS conducts intermittent audits and keeps systematic records of them. Second, the IRS provides accurate data on subsequent tax payments in each year following the audit, even when there is no audit. Third, these data comprise a panel of the entire population of individual taxpayers over time, allowing for rigorous empirical analysis.

Since the treatment, an IRS audit, is randomized through the NRP, our empirical strategy is straightforward. Using the sampling weights that the IRS uses to select individuals

[3] The IRS defines an audit as "a review/examination of an organization's or individual's accounts and financial information to ensure information is being reported correctly, according to the tax laws, to verify the amount of tax reported is correct." (Internal Revenue Service (2014)).

for an NRP audit, we construct a nationally representative sample of audited individuals. We pair this with a random sample of individuals drawn from the same population of tax filers. Then, we compare the tax filings of these two groups before and after the audit year.

We begin by using this U.S. data to test for the main results that Kleven et al (2011) find among Danish taxpayers. We find that an audit increases subsequent reported wage income by 1.3% and sole proprietorship income, reported on Schedule C of Form 1040, by 14.2% in the US on average. Thus we find that the effect of audit on incomes subject to third-party information reporting is much smaller than on incomes that are not subject to third-party reporting. This result is consistent with Kleven et al (2011). Further, we find that audits have a long-term effect on tax reporting. An audit increases reported taxable income by an average of $1,185 per year, or equivalent to about 2.9% of the average taxable income. Thus the revenue gain from the audit adjustments understates the true revenue gain (which accounts for changes in subsequent tax reporting) by more than half when considering the five-year window after an audit.

Next, we study the persistence of the effect of the audit over time. The IRS data show that, after an audit, reported Schedule C income increases sharply by over 16% but then falls quickly in subsequent years. The effect of audit on reported Schedule C income in the fifth year after audit is only about one third of the effect in the first year after audit. In contrast, after an audit, reported wage income increase by only about 1% in the first year after audit, but continues to increase in the following two years to around 2% and only gradually declines.

We then consider the role of income volatility as another important factor in tax evasion. To illustrate, consider that when the tax authority finds it harder to predict the income of a plumber than the salary of a teacher, it is not only because the plumber may

have less income subject to third-party information reporting, but also because the plumber's income may be more volatile than that of a teacher. Such income volatility increases the rate at which the information gleaned from an audit becomes outdated. Thus, two factors, income volatility and third-party information, may correlate and confound previous studies that focus on the role of information reporting. We include measures of income volatility in our analysis, computed separately across demographic groups, in our analysis and that find volatility mitigates the persistence of the effects of audit on reported income. This empirical result holds even when we control for third-party information reporting. The implication is that the effects of audits on taxpaying behavior diminish more quickly for filer with higher income volatility, holding constant the level for information reporting across those filers.

Further, we find variation in the effect of audit across filers correlated with measures of tax literacy. While we cannot observe tax literacy directly, the results using several proxy variables related to the filer's understanding of the tax code are consistent with one another. These results show that those with a better understanding of the tax code respond more strategically to their audit experience.

The paper is organized into eight sections. Following this introduction, Section 2 describes the conceptual framework. Section 3 describes the data and Section 4 presents information on tax compliance in our data. Section 5 presents our main empirical results, and Section 6 reports results for particular income and deduction items and the relationship between tax compliance and income volatility. In Section 7, we offer evidence that the response to audit is affected to a great extent by the filer's tax literacy. Section 8 concludes.

2. Conceptual Framework

This paper addresses the question of how individuals respond to an audit. Specifically, we seek to understand how their taxpaying behavior changes in the years following an IRS audit. To understand the mechanisms which guide our empirical analysis, we provide an intuitive conceptual framework.

The workhorse model for tax compliance is that of Allingham and Sandmo (1972),. Several studies consider the impact of audit rates on individual taxpayer compliance. These include Tauchen, Witte, Beron (1989), Dubin, Graetz, and Wilde (1990), and Witte and Woodbury (1985), all of whom find that increases in audit rates increase compliance. Marginal tax rates also impact evasion since they affect the value of misreported income. Evidence of this has been provided by Clotfelter (1983) and Feinstein (1991). In addition, Feldman and Slemrod (2007) find that documented income is much less likely to be evaded, which has been corroborated in other studies such as Kleven et al (2011). Additional related studies are those that show evidence of the indirect effects of audits that we are interested in. These include Alm and Yunus (2010), who find a role for norms and learning in tax evasion in the U.S., and Dubin (2007), who calculates the deterrent effect of audits.

One way in which we extend the Allingham and Sandmo (1972) model is to explicitly incorporate changes in subjective expectations of audit. To study the tax compliance behavior of taxpayers following audit, one must incorporate changes in the expected probability of audit conditional on having been audited. Conceptually, it is uncertain how audits would affect subsequent illegal tax behavior. On the one hand, those evading tax may update the audit risk in a Bayesian manner after experiencing an audit, and curb their misbehavior. On the other hand, tax evaders may perceive that inspectors are unlikely to follow immediately and therefore increase their evasion. Given these two potentially

opposing effects of an audit on the filer's expectations of future audits, the total effect of an audit on taxpayer behavior is ambiguous and an open empirical question.

Some interesting laboratory experiments lend some support to the second possibility. Guala and Mittone (2005) and Mittone (2006) find that lab subjects increase tax evasion immediately following an audit. Maciejovsky et al. (2007) also find their lab subjects to increase tax evasion following an audit, but to decrease it gradually over time to the pre-audit level. Kastlunger et al. (2009) show that this behavior is caused mostly by misperception of audit probability. Further, DeBacker et al. (2015) find evidence of such a hump-shaped effect on tax aggressiveness in their examination of corporate audits.

A second important extension of the basic Allingham and Sandmo (1972) model that is relevant to our study is the inclusion of different degrees of information reporting to the tax authority. Kleven et al. (2011) conceptualize third-party reporting of income as affecting taxpayers' ability to be noncompliant, and provide theoretical and empirical evidence that the degree of third-party reporting is a crucial determinant of the amount of taxpayer compliance. Their results imply that responses to audit will tend to be larger for income sources with less third-party reporting, for which the ability to be noncompliant is greater.

In reality, the tax enforcement agency has two main sources of information from which it can try to determine the distribution of the filer's true income. One source is from third-party income reporting and the other from audits. The key difference between these two sources of information is that third-party information is updated with each filing, but audit information is only updated when an audit is conducted. This leads to a gradual outdating of audit information after an audit. The degree to which the amount the information from audit diminishes over time is a function of the variability in the filer's income. For example, if small business income has larger year-to-year fluctuations than wage income, the

information gleaned from audit about the taxpayer's business income becomes less informative after a given amount of time than does the information about wage income.

Sanchez and Sobel (1993) show that in a model in which a fraction *p* of the population are willing to underreport income if it maximizes their expected payoff, and where the auditor conducts costly audits to maximize revenue subject to a budget constraint, the audit function results in cutoff rules, where certain segments of the population are audited at a positive rate and others not at all. In such a model, a more widely dispersed income distribution implies a lower cutoff point, with more scope for taxpayers to underreport income than if the income distribution were more compact.

To illustrate this argument, consider Figure 1. Figure 1 shows the conditional income distributions, as perceived by the auditor, for an income source that has a high variance. The distribution is most highly concentrated in the year after audit as the information from the audit is still relatively informative about the distribution of true income. However, as time passes, the spread of the distribution increases as the information from the audit becomes more outdated. This increase in the spread of the distribution results in more underreported income as the reduction in information to the IRS results in more scope for taxpayer noncompliance. Thus we expect to see reported income (denoted by Yr in Figure 1) decline as the spread in the conditional income distribution increases in the number of years since audit. For income sources that have less variance or volatility (or that are subject to more third-party reporting) the degree to which audit information becomes outdated is much lower. Thus we should expect a strong effect of audit on income sources with high variance and little third-party reporting (where the information gleaned from audit is particularly important), but that this effect declines more quickly than for sources of income with more third-party reporting and less variance or volatility.

[Figure 1 about here]

To summarize, our conceptual framework implies three propositions we can examine in the data. First, the direction of the effect of an audit on subsequent income reporting is ambiguous. Second, to the extent that income reporting responds to audit, we will observe more of a response among income sources that have less third-party reporting. Third, the responses to audit will be stronger and more transitory for those sources of income with less third-party reporting and more variance or volatility.

3. Data

Our data come from three sources. We discuss each data source in turn and then the process by which we merge the data and create our final sample.

First, we use data on audits from the IRS's National Research Program (NRP). Specifically, we use the taxpayer information generated by the audits conducted as part of the NRP's 2006, 2008, and 2009 waves.[4] Taxpayer information includes taxpayer identifiers (the social security number (SSN) of the primary filer), year of the audited return, and the resulting adjustment to the tax return by line on the Form 1040. Each of the 2006-2009 waves has approximately 15,000 observations.

The NRP conducts audits on a stratified, random sample of the filing population and includes in their sampling weights to allow researchers to create population-representative

[4] Note we exclude the NRP waves from 2001 and 2007 and those conducted after 2009. The NRP was not conducted in the years 2002-2005. Documentation suggests that the sampling frame and intent of the 2001 wave was sufficiently different from later waves to treat them as comparable. Further, constructing the control group for the 2007 NRP wave was complicated by the fact that stimulus rebate checks were sent out in 2008, and that to be eligible for a stimulus check one must have filed a year 2007 tax return. The resulting population of filers for tax year 2007 (who filed taxes in early 2008) was much different than in other years and did not closely resemble the weighted NRP sample. In particular, there was an increase in the number of people who typically did not file a tax return. We attempted to address this by using the methodology of Ramnath and Tong (2014) to identify those who filed only to claim the stimulus check, but the random sample was still sufficiently different than the NRP sample. NRP waves later than 2009 have been excluded to ensure that open audits were able to be completed before we drew our data.

statistics. Not all NRP audits are exactly the same. Most audits are conducted in-person by an auditor, while some are performed via correspondence, and some returns are simply accepted as-is without further contact with the taxpayer. This last type of audit presumably has no effect on the taxpayer as they are never notified of their selection, and accounts for approximately 5% of the audited sample. This presumably attenuates our observed effects towards zero.

Second, we use data from the IRS's population of individual income tax returns. These data include many items from the filer's Form 1040 and the associated forms and schedules, including all items on the front page of Form 1040 and the main line items from most associated schedules. We use these data from the years 2000 to 2012.

Finally, we use data from the IRS's Audit Information Management System (AIMS). The AIMS data contain detailed information on all IRS audits (including NRP and non-NRP audits) from 1996 to present. We use these data to augment the audit data from the NRP. In particular, the AIMS data allow us to observe variables such as the date the audit began and ended, the hours of examiner time put towards the audit, and examiner characteristics.

We construct our sample in the following way. We create a control group by randomly selecting a 0.1% sample of filers by choosing a different set of 10 four-digit SSN endings for each year 2006, 2008, and 2009.[5] For each of these years, we then select all primary filers who had one of these 10 four-digit endings from the universe of returns filed that year and which met the general sampling criteria of the NRP. We create our treatment group by finding the SSN of all primary filers in NRP waves from 2006, 2008, and 2009. Finally, we pull all tax returns from the 2000-2012 period for filers in either our sample or

[5] The sample size is dictated by computational constraints.

control groups[6]. Our final panel is thus comprised of a control group of randomly selected filers from the years of the NRP waves (followed over time) and a treatment group of randomly audited filers from the NRP waves (who are also followed over time). Creating our control group in this way (by ensuring that that those in the control group filed a return in the year the treatment group was audited) allows us to match attrition rates across treatment and control groups.

Using the SSN of the primary filer, we are able to link returns across the three data sources, the population files, the NRP, and the AIMS. Thus in our final panel, we have detailed information on each tax return filed from 2000-2012. For the treatment group, we also have detailed information on the characteristics of the audit and the adjustments to tax returns following audit, though we lack information on audits that are not closed by the time we pull data from the AIMS database. As such, information from audits not closed by October 2014 is missing in our sample. However, given that our last NRP wave is from 2009 and that well over 95% of audits are closed within two years, almost all audits have been closed. Table 1 summarizes our sample, noting weighted observations in the base year (i.e., NRP wave year) and across all years 2000-2012.[7] No filers in the random sample were selected for operational or NRP audits at any time in our sample. Filers in the NRP sample were not also selected for operational audits and we only subject to a single NRP audit.

[Table 1 about here]

While we do observe the date an audit was opened and closed, we do not know when the filer was notified of the audit or the results of the audit. Thus we use as our timing

[6] We also restrict these pre- and post- audit observations to those which would be consistent with the NRP sampling criteria.

[7] We use weights for both our randomly sampled control group and the treatment group. We weight the control groups by giving each filer equal weight to sum to the total population of filers in the base year. We weight the treatment groups using the NRP sampling weights. This gives us a number of weighted observations approximately equal to the population of filer in the base year for the NRP sample. We then apply these weights to the filing units for each year they are in the panel.

convention the number of years since the audited return was filed. For example, for the 2006 NRP wave, their tax year 2006 return was audited. Thus we consider their tax year 2007 return as being one year since the audited return was filed. We use this convention throughout the paper. As a result, one would not expect a sharp increases in reported income for all filers in a given NRP wave in a specific year since audit, since the duration of audits and the time when filers were notified varies. However, since the vast majority of audits are closed within two years, we do expect the effects of audits to fully materialize two to three years after the audited tax year.

Throughout, all monetary variables are deflated to 2005$ and 99% Winsorized. Winsorization of the data is necessary for dealing with outliers. The IRS does not edit most of our data sources, and thus data entry and calculation errors by the filers or the IRS agent entering the data are not uncommon. Winsorizing data inevitably removes some genuine variation from the sample; however, we think that Winsorization is acceptable to ensure that extreme outliers from coding errors do not influence our results. Further, the results reported in this paper are robust to different levels of Winsorization.[8]

4. Tax Compliance in the U.S.

The IRS's tax gap measure summarizes aggregate compliance with the U.S. income tax.[9] In 2006, the last year for which the IRS reports the tax gap, the net gap was $385 billion (Internal Revenue Service (2012)). This represents a compliance rate of 85.5%. Non-compliance with the individual income tax code represents the largest source of noncompliance, accounting for $235 billion of the $385 billion gap.

[8] Results with different levels of Winsorization are available from the authors by request.
[9] Note that the NRP data we use plays a large role in the IRS's estimation of the individual income tax gap.

Within the individual income tax, Internal Revenue Service (2012) shows that the lowest compliance rates come from income with less documentation. For example, the underreporting of business income, and in particular income from sole proprietors (as reported on Schedule C of Form 1040), accounts for about half of the individual income tax gap ($122 of $235 billion). Looking across income and deduction items, one can see the pattern that compliance rates fall as withholding and third-party verification decline. Such a pattern is also documented in Danish data by Kleven et al. (2011).

Our NRP data allow us to delve more deeply into the data than the tax gap statistics provided by IRS. Table 2 documents the measures of compliance found in our NRP data. Column 1 reports means by income and deduction sources and the fractions with those sources of income. Columns 2-4 report audit adjustments. Column 2 shows the average audit adjustment by income/deduction item and the fraction of those who report non-zero values of that item for which there is a non-zero adjustment. Column 3 and 4 decompose the adjustments in Column 2 into underreporting of income (which results in upward adjustments in income/downward adjustments in deductions) and over reporting of income (which results in downward adjustments of income/upward adjustment of deductions).[10]

[Table 2 about here]

Consistent with the compliance results seen in the IRS tax gap reports, the pattern that emerges is that those sources of income with the most documentation show the lowest rates of tax non-compliance. We find that non-compliance rates (measured by the fraction of filers with adjustments) are largest for Schedule C income, which has no withholding and little third-party verification. Schedule C income is adjusted for about 73% of those filers who are audited. The rate of non-compliance is lowest for wage and salary income, which is

[10] Note that Column 2 is the weighted sum Columns 3 and 4.

adjusted for about 6.5% of audited filers. Underreported income is more frequent than over reporting of income for all sources and is highest for Schedule C income. The average amount of underreported Schedule C income is $8,483. This compares to a mean of $8,401 for reported Schedule C income. Wage income is underreported by $1,233 compared to an average of $42,657. Overall, compliance rates are highest for wage income, which has a high withholding rate. Compliance rates are also high for capital income (both capital gains reported on Schedule D and capital income reported elsewhere), which has third-party reporting on most items, but not withholding.

5. Effects of audits on subsequent reported income

With an understanding of our data and tax compliance in the U.S., we now turn to our research question. The objective of the paper is to understand changes in individual taxpaying behavior in response to audit. To ensure the robustness of the results, we will use different methods, which include post-treatment difference, difference-in-difference and within-filer estimations.

5.1 Post-treatment difference and difference-in-difference estimates

The randomized controlled trial nature of the NRP allows us to consider the effects of audit on taxpaying behavior using either a simple post-audit difference or difference-in-differences estimator. For outcome variables, we will look at the reporting of taxable income, wage income, and Schedule C income. These three measures provide a nice contrast in terms of the amount of third-party information the IRS has regarding each income source. For example, most wages are subject to withholding, Schedule C income has very little documentation, and taxable income is a broad measure of overall income, composed of

income with different reporting requirements and determined after deductions are reported. Our difference-in-differences estimator of the effect of audit is thus:

$$Effect\ of\ audit = \left(\bar{Y}_{B,2} - \bar{Y}_{B,1}\right) - \left(\bar{Y}_{A,2} - \bar{Y}_{A,1}\right), \qquad (1)$$

where B denotes the treatment group (i.e., the NRP sample) and A the control group. The subscripts 1 and 2 denote the pre-audit and post-audit periods respectively. For each, we consider the mean over a span of 3 years. Thus the $\bar{Y}_{B,2}$ is calculated as the mean of the income source of interest for the NRP sample over the three years after audit and $\bar{Y}_{B,1}$ is calculated as the mean of the income source of interest for the NRP sample over the three years prior to audit. The means for the control group are constructed in an analogous way.

[Table 3 about here]

Table 3 reports the post-treatment and difference-in-differences results. We present the results in percentage terms because the income sources have very different mean amounts. The top panel of the table shows that (reported) taxable income of the audited group increases by 3.8% when comparing the post-audit period to the pre-audit period, while that of the control group increases only 0.9%. These figures imply that audits increase taxable income by 2.9% on average ($1,185 per year). In the second panel of Table 3, however, the effect of audits on reported wage income is only 1.3% on average ($590 per year). This effect is consistent with the idea that it is more difficult to misreport income that is also subject to withholding and third-party verification (Kleven et al. (2011)). The largest effect of audits, in percentage terms, is on Schedule C income. The third panel in Table 3 shows this effect to be 14.2% ($1,156 per year). This large effect supports the view that it's relatively easier to manipulate Schedule C income than wage income, and that such income is more responsive after an audit.

5.2 Persistence of audit effect

Given these results, a question of further interest is how individuals change tax reporting *over time* after audit: do they increase reported income permanently, or does the initial effect decline as time passes? Figure 2 plots the differences between the mean reported incomes of the audited and control group. Reported taxable income increases in the first and second years and after an audit and remains elevated even after six years. Adjusted gross income (AGI) and wage income follow a similar pattern as taxable income.

As Figure 2 shows, the effect of audits on Schedule C income are strong in the first couple of years after audit. Following the initial upswing in reported Schedule C income, it then turns downward toward the pre-audit level. This result stands in contrast to the trends in taxable income, AGI, and wage income, and suggests further that income with less-third-party reporting responds differently to audit. We thus delve more deeply into the varying responses by type of income in Section 6.

For each of the four income sources considers, it is apparent from the figure that the pre-audit trends are similar across the NRP and non-NRP samples. Thus the common trends assumption needed for identification is satisfied. We next consider models with individual fixed effects, which will allow for the identification of the effects of audit from within filer variation in reported income.

[Figure 2 about here]

5.3 Within-filer estimates

Because we have a panel of tax returns, we can examine changes in individuals' behavior after an audit while controlling for time-invariant unobserved individual characteristics. We first estimate an equation of the form

$$Income_{it} = \beta PostAudit_{it} + \gamma_i + \eta_t + \varepsilon_{it}, \qquad (2)$$

where $Income_{it}$ denotes a measure of income for individual (taxpayer) *i* in year *t*; and $PostAudit_{it}$ denotes that the individual was audited during our sample period prior to year *t*, γ_i denotes an individual (taxpayer) fixed effect, and η_t denotes a year fixed effect. In this specification, identification of the effects of audit come from within filer changes in reported income between the pre and post audit periods, net of trends in income common across the treatment and control groups, which are accounted for by the year fixed effects.

[Table 4 about here]

Table 4 reports the results from regressions that estimate the effect of audits on taxable income. Column 1 shows that audits increase reported taxable income by $1,109, which is statistically significant at the 1% level. This result implies that, consistent with the simple difference-in-differences tabulations above, individuals tend to report more income after audit.

Column 2 examines whether, in our estimation framework, the effects of audit differ with the number of years since the audited tax year by estimating an equation of the form:

$$Income_{it} = \sum_{k=1}^{K} \beta_k \, (PostAudit_{it}) * (k \; Years \; Since \; Audit) + \gamma_i + \eta_t + \varepsilon_{it} \quad (3)$$

In this specification, the key explanatory variables are a series of dummy variables that show the difference between the audited and control group from Year 1 through (at most) Year 6 after the audited tax year. This column shows that reported taxable income increases quickly during the first two years after the audit, with an increase of around $1,200, and stays at this level until at least Year 6, with all of the effects being statistically significant. Figure 3 summarizes a further specification where we include dummies for the two years prior to audit. Neither of these pre-audit dummies are significant.

[Figure 3 about here]

Because of our data, we are able to observe the adjustment to each filer's return. One might suppose that the response to audit would be stronger for filers with a positive adjustment to tax liability than those with no adjustment. That is indeed that case. We provide evidence for this in Figure 4, where we estimate Equation 3 on taxable income separately for each of three groups: those with a positive adjustment to tax liability following the audit, those with no adjustment, and those with a negative adjustment. Note that in this specification, we are comparing each outcome to the population as a whole. We see the strongest and most statistically significant response from the group with positive adjustments. Almost all the coefficients for the other two groups are statistically insignificant and have much lower point estimates than for the positive adjustment group. It should be noted that the actual audit outcome is not exogenous to taxpayer characteristics. However, we believe these results are very suggestive of the expected behavior. In other words, while we find that the non-compliant increase compliance after audit, we do not find that the compliant increase non-compliance. These results are consistent with what Gemmell and Ratto (2012) using UK data.

[Figure 4 about here]

6. Effect under third-party information and income volatility

We now examine whether the effects of an audit differ with the level of third-party reporting by estimating separate impacts by the source of income or the type of deduction that is being claimed. Then we examine whether the response differs for taxpayers with more or less volatile income.

6.1 By Income Source

IRS data allow us to distinguish incomes from wages, Schedule C, D and E. Among those, wage income information can be easily cross-checked with data reported by employers. Information on incomes from Schedule C, D and E are harder to verify as little third-party information is available. We will examine the audit effect on incomes from these sources by both intensive and extensive margins.

6.1.1 Intensive Margin

In Table 5 (and Figure 5), we present estimates versions of Equation 3, where the dependent variable is income of a particular type. Note that Figure 5 presents the coefficients for the post-audit year indicator variables in percentage terms to make the responses more comparable across income sources with very different mean values. We also restrict our sample to those who have non-zero amounts of income of that particular type in the year of the audit, so these can be considered intensive-margin results.

[Table 5 about here]

[Figure 5 about here]

In Column 1, we repeat the results from Column 2 in Table 4, in which the dependent variable is taxable income. Column 2 presents results for total income.[11] The results in this column are slightly smaller than, though similar to, those for taxable income. This is not surprising due to the fact that taxable income also incorporates the filer's choice of deductions and so provides for more opportunity to manipulate taxes through reporting.

In Column 3, the dependent variable is wages and salaries. Similar to the results found above in the simple tabulations, this specification finds a small positive effect of an audit on reported wages, with an increase of $330-530 in the first three years after audits. However, the impact on wages dies out thereafter.

[11] Total income is from Form 1040, Line 22. It is AGI with the above the line deductions added back in.

Column 4 presents results when the dependent variable is Schedule C (sole proprietorship) income. As noted above, this source of income is not generally subject to third-party reporting, and so may be easier for taxpayers to manipulate. Consistent with this, the estimation results suggest that Schedule C income increases substantially after audit, by over $1,000 in the first two years. Interestingly, that effect diminishes 3 years after the audit, and is insignificant after four years. Further, in years 5 and 6, the estimated impact is actually negative. These results suggest that taxpayers with sole proprietorship income may be more careful in reporting income right after an audit, but may become more aggressive than they were in the years prior to the audit over time.

In Column 5, which presents results for Schedule D income (capital gains and losses), no significant effects of audits are found in any year. However, the results in Column 6 for Schedule E income, which includes partnership, S corporation, and rental income, mirror that for Schedule C income. Schedule E income, like Schedule C income, is largely self-reported. A significant positive impact of audits is found in the first two years, with the effect diminishing in the third year, and no longer significant in the fourth year. In years 5 and 6, the estimates turn negative, but (unlike for Schedule C income) are not statistically significant in those years.

6.1.2 Extensive margin effects

To understand the extensive margin effects of audit, we estimate linear probability models of the form:

$$I(Y \neq 0)_{it} = \sum_{k=1}^{K} \beta_k \, (PostAudit_{it}) * (k \; Years \; Since \; Audit) + \gamma_i + \eta_t + \varepsilon_{it}, \quad (4)$$

where $I(Y \neq 0)_{it}$ is an indicator function equal to one if Y is not zero. The variable Y represents income from Schedule C, Schedule E, or wages and salaries. The estimates of Equation 4 are reported in Figure 6, Panels A-C. Each panel reports results for three

groups, those who filed the schedule or, those who did not, and the full sample. The graphs then plot the change in the likelihood of filing the given form by year since the audited return was filed.

The extensive margin results for Schedule C and E filers are reported in Figure 5, Panels A and B. These two graphs show sharp declines in the likelihood that a filer continues to file the relevant schedule after audit. Thus, the increases in taxable income following audit is only one effect of the audits on those with business income. The other effect is to make them less likely to claim business income. There are at least three possible reasons for this decline, though our data do not allow us definitively say how many filers are affected by each of the three possibilities. First, audits may result in increases in reported income and thus taxes. This pushes down the after-tax return on the business endeavors and may cause the filers to forgo them. Second, the Schedule C or E may have been filed for an activity that generated losses, which were used offset ordinary income elsewhere on the return. An audit may have found such a losses to be illegitimate and thus the filer discontinued their use of those losses and the filing of the associated schedule. Finally, and this is particularly relevant for Schedule C income, an audit may have found that the filer should not have been filing as an independent contractor, but instead should have been classified as an employee. The increase in compliance after an audit in this case would result in fewer Schedule Cs being filed. Some evidence of this effect can be seen in Panel C. This is the extensive margin effect of audits on filers' wage income. For those who do not report wage income in the year of audit, there is a strong increase in the probability they report wage income in subsequent years, which mirrors the results for the reporting of Schedule C or Schedule E income.

[Figure 6 about here]

Taken together, the intensive and extensive margin results are consistent with those of Kleven et al. (2011) who point out that taxpayers' compliance is strongly related to the ability to be noncompliant, as this ability is greatest with self-reported income such as that reported on Schedules C and E.

6.2 By Deduction Item

We next estimate variants of Equation 3 in which the dependent variables denote the amounts of particular types of deductions that filers claim. Column 1 of Table 6 presents the results for above the line deductions (i.e., deductions that figure into AGI) and Column 2 the results for total itemized deductions. Columns 3-5 present results for individual types of itemized deductions (charitable contributions, state and local income taxes, and mortgage interest). We also summarize the results in Figure 7, which plots the percentage changes in the deduction items following audit. Columns 1 and 2 show that both types of deductions decrease after an audit (which implies higher taxable income), with a larger decline for itemized deductions. For both types of deductions, the effects continue at a high level for up to six years after the audited return was filed. As with the income source results, the results for deductions are consistent with the idea that taxpayers manipulate their tax reporting where they are able to do so. Thus we see larger effects of audit on the more malleable, and less documented, itemized deductions category. Charitable contributions are estimated to fall by around $270-460 after an audit, with the size of the impact being consistent across years. Interestingly, although they are both subject to third-party reporting, state and local taxes and mortgage interest are also estimated to fall after an audit, with the effect increasing over time.

[Table 6 about here]

[Figure 7 about here]

6.3 By degree of volatility

The result that income sources such as Schedule C and Schedule E income have the largest responses to audit is not surprising given the limited amount of third-party reporting on these income sources. However, the transitory nature of the impact of audit on these income sources and on the taxable income of those deriving income from these sources is not as intuitive.

In addition to being subject to less third-party reporting, business income reported on Schedules C and E differs from labor income (which makes up the majority of total income) by its volatility. DeBacker, Panousi, and Ramnath (2014) document that business income exhibits both a higher variance and more volatility than labor income. We believe that the difference in income processes interacts with audits in an interesting way. In particular, we posit that higher income volatility allows taxpayers to change reported income from year to year more easily. The result is that the effects of audit are not as persistent for business income as for labor income. For example, consider an audit of a filer with only wage income. Since wage income is less volatile, that filer may believe that the IRS can infer that their wage income a given number of years in the future is within some range with a fair degree of accuracy. Thus the effect of audit on wage income will be smaller, and potentially more persistent. However, for the more volatile Schedule C income, the filer may believe that the same number of years from the audit, the IRS has very little ability to predict what the range of true Schedule C income is. Therefore the effect of audit on Schedule C income is more transitory.

To test this hypothesis we include a measure of volatility into our models and interact volatility with the audit indicator variable. We measure volatility by considering the variance in each individual filer's taxable income over time. We then group filers by filing

status, number of dependents, and schedules filed. Finally, we take averages over these groups and use that as the measure of volatility for each individual in that group.

Table 7 presents the results of this model, separately for all filers and for those with Schedule C income. In all cases, the interaction terms are negative, indicating that volatility reduces the effect of audits on subsequent taxpaying behavior. This is consistent with our hypothesis. Furthermore, the interaction terms show that the effect of volatility becomes more negative over time, which means the effect of an audit is more short-lived for filers with more volatile income.

[Table 7 about here]

7. The implications of tax literacy

As noted above, an NRP audit is random, and the letters NRP subjects receive explicitly mention that they were selected for a random audit used for IRS research purposes. Despite this, we find significant impacts of audit on filers. One reason for this may be that filers misunderstand the nature of the NRP audit. In particular, the result of an operation audit affects one's future probability of being audited, but one's experience in an NRP audit has no bearing on their future audit probability.[12] To test this, we look at the responses to audit across several subsamples of the tax filing population. Each of these subsamples is created based on characteristics that may be correlated with tax literacy or understanding of tax enforcement. These include income, age, the use of a paid tax preparer, and the use of e-filing of the tax return. Although none of these characteristics is a perfect proxy for tax literacy, consistent responses to audits across all these subsamples would suggest that tax literacy may play a role in how filers respond to audits.

7.1 Responses by Filer Age

Figure 8 splits the sample according to the age of the primary filer in the year of the audit.[13] Here, the three oldest age groups (35-44, 45-54, and 55-64) appear to be the most responsive after an audit, with estimated effects two years after audit in excess of $1,700, while the impact for the youngest group two to four years after audit is around $750. These results are in contrast to Kleven, et al. (2011) who find that the propensity to underreport income falls with age.

[12] This conditional probability is something we've confirmed in our panel. The unconditional probability of being selected for an operational audit is statistically indistinguishable from the probability of being selected for an operational audit conditional on having been selected for an NRP audit. This is true even if the NRP audit uncovered a positive adjustment to the filer's tax liability.

[13] For observations in the control group, we split based on age in the tax year for which they were drawn as a control observation.

[Figure 8 about here]

Considering the long-term patterns, we see that the youngest age group (25-34) responds significantly differently than the older age groups to audit. In particular, this age group does not show a return to the pre-audit trend in income reporting. Following an audit, those aged 25-34 report higher incomes and this effect persists over the next six years with increases over the entire window. In contrast, the older age groups show an increase in reported taxable income in years two and three after the audit, but a decline after that. Thus we find evidence that audits have more persistent deterrent effects on those who are relatively new tax filers. This may reflect differences in how those with a short filing history update there prior beliefs about IRS enforcement following an NRP audit as compared to those with longer filing histories and more experience with IRS enforcement.

7.2 Responses by Filer Income

Next, we show how the responses to audit differ across income groups. These results are shown in Figure 9. Income quintiles 1-4 show similar responses to audit, with persistent increases in reported taxable income following audit. Amongst these, the lowest income group has the strongest response to audit measured by the change in taxable income reported after audit. Note that this is true despite this group having the lowest taxable income at the time of audit. The top income quintile shows the opposite trend, with a persistent decline in reported income after the initial increase in income in the year after the audited return was filed. That those in the highest income quintile have such different responses from those in the lower income quintiles is consistent with these filers being more sophisticated and having a better understanding of the NRP audit process.

[Figure 9 about here]

7.3 Responses by Filing Method

We now condition samples on filing method and compare the responses to audit. By filing method, we mean the use of a paid preparer to file the tax return and the use of e-filing of a tax return. E-filing is highly correlated with the use of tax filing software. Our assumption is that those using a paid preparer or e-filing tend to be more sophisticated in their understanding of the tax code than those without, at least conditional on the personal or software assistance they receive in filing.

Figure 10, Panel A displays the results comparing those who used paid-preparers in the year the audited return was filed to those who did not. In the first three years after audit, these two groups have very similar responses, increasing reported taxable income by over $1,000 more than the comparable group who was not audited. However, after year three the post-audit trends diverge. In these years, the trend for those using a paid prepare moves taxable income towards its pre-audit level. While for those who did not use a paid preparer, the increase in reported taxable income is persistent.

[Figure 10 about here]

In Figure 10, Panel B, we see that a similar comparison can be made between those who used e-filing in the year the audited return was filed and those who did not. In particular, the changes in taxable income following audit are greater and more persistent for those who did not e-file.

A word of caution about these results is in order. Over our sample period, there was a sharp increase in the number of e-filed returns. Thus, those who e-file in 2006 (from whom the effect 6 years from audit is identified) may be a very different group of filers than those who e-file later in the sample period. Fixed effects do control for level differences in income between these groups, but the composition of the groups may also differ on

unobservables. Differences in unobservables that lead to heterogeneous audit treatment effects may skew the estimated effects of audit over time.

7.4 Summary of findings related to tax literacy

Although there exists no direct measure of tax literacy for the filers in our sample, in each case above, the results are consistent with smaller, and more transitory, responses from those with a better understanding of the audit process they are involved in: the young and inexperience respond more strongly and persistently than the old, those with low income respond to a greater extent than those with high income, those using paid-preparers have more fleeting responses than those who don't, and those who e-file have less of the long-run response than those who do not. While the proxies for tax literacy are imperfect, the robustness of these results should suggest that there is a relationship between tax literacy and the responses to audits.

8. Conclusion

Tax evasion is estimated to lower global tax collections by about 18% and result in about three trillions dollars in lost public funding worldwide (Murphy 2011). Among all countries, the U.S. suffers the largest loss in absolute terms because of the sheer size of the economy. One way to deal with this problem is through audits of tax returns. Auditing affects tax compliance through deterrence effects, which impact all filers. Auditing also has more direct effects on the audited. First, there is a static revenue gain when the auditors discover noncompliance. Second, those audited tend to report higher taxable income in subsequent years, resulting in further revenue gains. This study provides a rigorous

evaluation of the effectiveness of tax audits in reducing tax evasion, in the short and long term.

We examine the impact of audits on subsequent individual taxpaying behavior using data from the Internal Revenue Service (IRS) National Research Program on random audits on tax returns from 2006-2009 matched to returns from the universe of filers from 2000 to 2012. It is worth noting that the effects we measure are the effects of a random audit. Filers may respond differently to non-random audit; for instance, they can make some inferences about the auditing process from the fact that they were selected. However, because of this selection, the effects of non-random audits are more difficult to measure. One argument that suggests the effects of non-random and random audits may not be far apart is made by Manoli and Turner (2014). They provided evidence from a randomized field experiment showing that the content of contact between the IRS and tax filers is much less important than the existence of this contact.

The results from a simple difference-in-differences specification indicate that an audit increases reported taxable income by over $1,100 per year, or equivalent to 2.9% of the average income. This effect is only 1.3% for wage income but is 14.2% for Schedule C income. Further, we find that the impact of auditing on reported wage lasts over time while it is fleeting for Schedule C income.

Similar results are found when controlling for individual fixed effects. These results suggest that Adjusted Gross Income increases for at least 6 years after an audit. Contributing to this increase, Schedule C and Schedule E income (which are not subject to third-party reporting or withholding) tends to sharply increase after an audit, but this increase diminishes (and turns negative) 5 or more years after audit, while the increase in wage and salary (which is subject to third-party reporting and withholding) is considerably

smaller. In addition, above the line and itemized deductions both decrease significantly after audit, and the decrease in deductions is apparent even among deductions (like state and local taxes and mortgage interest) that are subject to third-party reporting.

There are several clear implications one can draw from the results of this study. First, an audit of a randomly-selected individual tax filer increases reported taxable income by roughly 2.9%, and this effect appears to persist for at least six years. Second, audits produce more transitory effects on income sources that are subject to less third-party reporting. Third, income volatility provides an important shield for those willing to evade taxes. Fourth, responses to audits are affected by the tax literacy of the filer. These results help us to understand better the information problem in law enforcement and where intervention may be effective.

References:

Advani, Arun, Jonathan Shaw, and William Elming. 2015. "How long-lasting are the effects of audits?" *Working Paper*, Tax Administration Research Center, UK.

Allingham, Michael G and Agnar Sandmo. 1972. "Income Tax Evasion: A Theoretical Analysis." *Journal of Public Economics*, 1(3), 323-38.

Alm, James and Mohammad Yunus. 2009. "Spatiality and Persistence in Us Individual Income Tax Compliance." *National Tax Journal*, 101-24.

Alm, James, and Michael McKee. 2004. "Tax compliance as a coordination game." *Journal of Economic Behavior & Organization,* 54(3) 297-312.

Alm, James, Gary H. McClelland, and William D. Schulze. 1992. "Why do people pay taxes?." Journal of Public Economics 48(1): 21-38.

Alm, James. 2010. "Testing Behavioral Public Economics Theories in the Laboratory." *National Tax Journal*, 63(4), 635-58.

Andreoni, James, Brian Erard, and Jonathan Feinstein. "Tax compliance." *Journal of economic literature* (1998): 818-860.

Becker, Gary S. 1968. "Crime and Punishment: An Economic Approach," *Journal of Political Economy*, 76, 169-217.

Choo, Lawrence, Miguel A. Fonseca, and Gareth Myles. 2013. "Lab Experiment to Investigate Tax Compliance: Audit Strategies and Messaging." HM Revenue and Customs Research.

Clotfelter, Charles T. 1983. "Tax Evasion and Tax Rates: An Analysis of Individual Returns." *The Review of Economics and Statistics*, 363-73.

DeBacker, Jason Matthew; Vasia Panousi, and Shanthi Ramnath, 2014. "A Risky Venture: Income Dynamics within the Non-Corporate Private Business Sector", mimeo.

DeBacker, Jason, Bradley T. Heim, Anh Tran and Alexander Yuskavage. 2015. "The Impact of Legal Enforcement: An Analysis of Corporate Tax Aggressiveness after an Audit." *Journal of Law and Economics,* 58(2), 291-324.

Dubin, Jeffrey A. 2007. "Criminal Investigation Enforcement Activities and Taxpayer Noncompliance." *Public Finance Review*, 35(4), 500-29.

Dubin, Jeffrey A; Michael J Graetz and Louis L Wilde. 1990. "The Effect of Audit Rates on the Federal Individual Income Tax, 1977-1986." *National Tax Journal*, 395-409.

Feinstein, Jonathan S. 1991. "An Econometric Analysis of Income Tax Evasion and Its Detection." *The RAND Journal of Economics*, 14-35.

Feld, Lars P. and Bruno S. Frey. 2002. "Trust Breeds Trust: How Taxpayers Are Treated." *Economics of Governance* 3, 87-99.

Feldman, Naomi E and Joel Slemrod. 2007. "Estimating Tax Noncompliance with Evidence from Unaudited Tax Returns." *The Economic Journal*, 117(518), 327-52.

Fortin, Bernard, Guy Lacroix, and Marie-Claire Villeval. 2007. "Tax evasion and social interactions." *Journal of Public Economics,* 91(11): 2089-2112.

Gemmell, Norman and Marisa Ratto. 2012. "Behavioral Responses to Taxpayer Audits: Evidence from Random Taxpayer Inquiries." *National Tax Journal*, 65(1), 33-58.

Guala, Francesco and Luigi Mittone. 2005. "Experiments in Economics: External Validity and the Robustness of Phenomena." *Journal of Economic Methodology*, 12(4), 495-515.

Internal Revenue Service. 2012. "IRS Releases New Tax Gap Estimates."

Kastlunger, Barbara; Erich Kirchler; Luigi Mittone and Julia Pitters. 2009. "Sequences of Audits, Tax Compliance, and Taxpaying Strategies." *Journal of Economic Psychology*, 30(3), 405-18.

Kleven, Henrik Jacobsen; Martin B Knudsen; Claus Thustrup Kreiner; Søren Pedersen and Emmanuel Saez. 2011. "Unwilling or Unable to Cheat? Evidence from a Tax Audit Experiment in Denmark." *Econometrica*, 79(3), 651-92.

Maciejovsky, Boris; Erich Kirchler and Herbert Schwarzenberger. 2007. "Misperception of Chance and Loss Repair: On the Dynamics of Tax Compliance." *Journal of Economic Psychology*, 28(6), 678-91.

Manoli, Dayanand S. and Nicholas Turner, 2014. "Nudges and Learning: Evidence From Informational Interventions for Low-income Taxpayers." National Bureau of Economic Research, NBER Working Paper 20718.

Mittone, Luigi. 2006. "Dynamic Behaviour in Tax Evasion: An Experimental Approach." *The Journal of Socio-Economics*, 35(5), 813-35.

Sanchez, Isabel and Joel Sobel, "Hierarchical design and enforcement of income tax policies," *Journal of Public Economics*, March 1993, 50 (3), 345–369.

Tauchen, Helen V; Ann Dryden Witte and Kurt J Beron. 1993. "Tax Compliance: An Investigation Using Individual Taxpayer Compliance Measurement Program (TCMP) Data." *Journal of quantitative criminology*, 9(2), 177-202.

TRAC Reports, Inc. 2014. "Graphical Highlights of the Internal Revenue Service."

Witte, Ann D and Diane F Woodbury. 1985. "The Effect of Tax Laws and Tax Administration on Tax Compliance: The Case of the Us Individual Income Tax." *National Tax Journal*, 1-13.

Table 1. Number of observations

	Total observations
NRP Sample	
All Years	4,364,655,406
Base Year	404,252,738
Postive Adjustment to Tax Liability	164,338,287
Zero Adjustment to Tax Liability	210,135,968
Negative Adjustment to Tax Liability	29,778,483
Random Sample	
All Years	4,335,270,000
Base Year	405,187,000

Note: The National Research Program (NPR)'s sample is the treatment group, which has been audited. The random sample is randomly selected from the universe of tax files. Our data include 2006, 2008, 2009 NRP waves. The number of observations of the NRP sample is weighted according to the weights that were used by NPR.

Table 2. Summary of audit adjustments

	Pre-audit Income	Audit Adjustment	Underreported Income	Overreported Income
Taxable Income	$40,735	$4,031	$5,167	-$1,427
Non-zero fraction	76%	60%	83%	17%
Adjusted Gross Income	$48,417	$4,502	$5,893	-$1,512
Non-zero fraction	100%	43%	81%	19%
Deductions	$2,420	$58	$726	-$961
Non-zero fraction	25%	47%	60%	40%
Wages and Salaries	$42,657	$921	$1,233	-$148
Non-zero fraction	84%	7%	77%	23%
Sch C Income	$8,400	$7,116	$8,483	-$1,317
Non-zero fraction	16%	73%	86%	14%
Sch D Income	$3,066	$1,721	$3,133	-$886
Non-zero fraction	17%	16%	65%	35%
Sch E Income	$11,309	$4,512	$6,614	-$2,186
Non-zero fraction	12%	48%	76%	24%

Note: This table reports the measures of compliance found in our NRP data. The first column reports means by income and deduction sources and the fractions with those sources of income. The second column reports the average additional tax liability request during NRP Audits (often called audit adjustment), conditional on non-zero adjustment. The remaining collumns report the average underreported and overreported incomes, conditional on underreporting or overreporting.

Table 3. Single and double difference between the audited and non-audited

	NRP Sample	Non-NRP, Random Sample	% Difference
Taxable Income			
Pre-audit	41,144.14	40,879.17	0.6%
Post-audit	42,725.13	41,262.21	3.5%
% Diff	3.8%	0.9%	2.9%
Wage Income			
Pre-audit	43,951.25	43,787.59	0.4%
Post-audit	44,967.15	44,206.48	1.7%
% Diff	2.3%	1.0%	1.3%
Sch C Income			
Pre-audit	8,016.93	8,111.85	-1.2%
Post-audit	8,290.09	7,331.30	13.1%
% Diff	3.4%	-9.6%	14.2%

Note: This table reports reports the post-treatment and difference-in-differences between the audited and non-audited groups. The means are calculated over three years before and three years after audit (to define pre and post-audit period).

Table 4: Effect of audit on reported taxable income

	Average effect	Effect over time
Post-Audit	1109.329***	-
	(184.312)	-
1 Year Post Audit		687.422***
		(186.026)
2 Year Post Audit		1203.155***
		(201.095)
3 Year Post Audit		1424.118***
		(246.485)
4 Year Post Audit		1480.547***
		(328.235)
5 Year Post Audit		1122.686***
		(434.124)
6 Year Post Audit		1190.270**
		(515.569)
Individual FE	yes	yes
Year of Tax Return	yes	yes
Constant	34098.286***	34098.639***
	(137.019)	(136.963)
R-squared	0.005	0.005
N	4,771,427	4,771,427

Notes: This table reports the regression of the duration since the last audit on repoted taxable income. The first column uses a simple post-audit dummy variable. The second column uses a series of dummies indicating the number of years after the last audit. Standard errors clusted at the individual filer level are repoted in parentheses below the point estimates. The asterisks denote the level of statistical significance: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 5: Effect of auditing on different sources of income

	Taxable Incom	Total Income	Wage Income	Schedule C	Schedule D	Schedule E
1 Year Post Audit	687.422***	606.842***	331.796*	1172.550***	-231.878	1011.329***
	(186.026)	(202.347)	(181.705)	(129.573)	(146.579)	(277.308)
2 Year Post Audit	1203.155***	1036.045***	452.527**	1035.716***	-86.169	1498.063***
	(201.095)	(222.467)	(220.212)	(146.667)	(118.565)	(305.301)
3 Year Post Audit	1424.118***	1280.403***	522.173**	477.465***	135.764	845.633***
	(246.485)	(277.113)	(262.200)	(154.856)	(131.805)	(313.095)
4 Year Post Audit	1480.547***	1363.217***	103.304	4.185	284.549	356.594
	(328.235)	(367.693)	(355.860)	(196.367)	(178.504)	(417.286)
5 Year Post Audit	1122.686***	939.992*	-43.082	-723.781***	145.590	-405.062
	(434.124)	(500.293)	(505.031)	(261.934)	(212.183)	(518.825)
6 Year Post Audit	1190.270**	1155.673*	3.137	-745.640***	133.558	-288.744
	(515.569)	(591.324)	(588.270)	(284.947)	(249.393)	(584.497)
Individual FE	yes	yes	yes	yes	yes	yes
Year of Tax Return	yes	yes	yes	yes	yes	yes
Constant	34098.639***	50411.774***	41058.233***	5903.185***	5729.068***	8201.870***
	(136.963)	(152.286)	(128.911)	(72.712)	(94.025)	(129.265)
R-squared	0.005	0.008	0.009	0.008	0.040	0.006
N	4,771,427	4,763,014	3,996,502	849,576	956,067	695,152

Notes: This table reports the regression of the duration since the last audit on repoted income of difference sources. The key explainatory variable is a series of dummies indicating the number of years after the last audit. Standard errors clusted at the individual filer level are repoted in parentheses below the point estimates. The asterisks denote the level of statistical significance: * p<0.10, ** p<0.05, *** p<0.01.

Table 6: Effect of auditing on different deduction items

	Total Adjustments	Itemized Deductions	Charitable Contributions	State & Local tax	Mortgage interest
1 Year Post Audit	12.351	-562.917***	-267.326***	-39.360	82.500
	(26.780)	(119.826)	(26.915)	(28.098)	(70.714)
2 Year Post Audit	-75.077**	-1406.712***	-358.449***	-91.159***	-415.768***
	(30.054)	(128.412)	(29.488)	(32.039)	(76.328)
3 Year Post Audit	-132.297***	-1690.529***	-361.669***	-118.969***	-636.392***
	(31.941)	(139.131)	(31.393)	(37.640)	(78.949)
4 Year Post Audit	-169.009***	-1845.593***	-356.883***	-119.188**	-785.216***
	(42.228)	(172.254)	(40.350)	(52.516)	(95.295)
5 Year Post Audit	-261.008***	-2507.810***	-463.804***	-337.769***	-1069.046***
	(57.608)	(234.789)	(55.961)	(67.109)	(127.859)
6 Year Post Audit	-290.208***	-2012.379***	-436.051***	-252.940***	-920.873***
	(58.736)	(255.428)	(60.477)	(78.813)	(132.118)
Individual FE	yes	yes	yes	yes	yes
Year of Tax Return	yes	yes	yes	yes	yes
Constant	1023.370***	13934.544***	2130.039***	3026.309***	6506.456***
	(13.565)	(57.258)	(13.775)	(17.492)	(36.246)
R-squared	0.031	0.064	0.012	0.021	0.076
N	1,323,803	1,931,745	1,607,515	1,859,451	1,551,268

Notes: This table reports the regression of the duration since the last audit on different deductions claimed. The key explainatory variable is a series of dummies indicating the number of years after the last audit. Standard errors clusted at the individual filer level are repoted in parentheses below the point estimates. The asterisks denote the level of statistical significance: * p<0.10, ** p<0.05, *** p<0.01.

Table 7. Effect of audit and income volatility on subsequent taxable income

	All Filers	All Filers, Volatility Controls	Sch C Filers	Sch C Filers, Volatility Controls
1 Year Post Audit	687.422***	1938.009***	1221.978***	3492.192***
	(186.026)	(267.303)	(418.918)	(572.575)
2 Year Post Audit	1203.155***	2865.275***	2860.921***	5781.773***
	(201.095)	(289.397)	(468.607)	(630.918)
3 Year Post Audit	1424.118***	3875.297***	2639.729***	7303.227***
	(246.485)	(344.742)	(526.023)	(708.186)
4 Year Post Audit	1480.547***	2743.992***	3117.531***	6558.445***
	(328.235)	(426.973)	(715.627)	(894.466)
5 Year Post Audit	1122.686***	3671.930***	2614.918***	8842.014***
	(434.124)	(554.753)	(993.198)	(1374.814)
6 Year Post Audit	1190.270**	2439.411***	2120.874**	6669.378***
	(515.569)	(645.825)	(1052.582)	(1355.481)
1 Since Audit*Volatility		-440.566***		-523.944***
		(94.452)		(163.790)
2 Since Audit*Volatility		-580.613***		-667.382***
		(100.096)		(173.935)
3 Since Audit*Volatility		-857.054***		-1063.773***
		(106.480)		(184.120)
4 Since Audit*Volatility		-426.451***		-764.784***
		(136.857)		(230.788)
5 Since Audit*Volatility		-866.402***		-1395.788***
		(179.025)		(323.981)
6 Since Audit*Volatility		-415.373**		-1003.972***
		(197.789)		(321.598)
Individual FE	yes	yes	yes	yes
Year of Tax Return	yes	yes	yes	yes
Constant	34098.639***	34065.130***	43563.920***	43524.824***
	(136.963)	(135.957)	(281.317)	(279.661)
R-squared	0.005	0.006	0.003	0.004
N	4,771,427	4,771,427	849,576	849,576

Notes: This table reports effect of the interaction of the duration since the last audit and income volatility on reported income. The first two columns analyze the full sample. The last two columns restrict to only Schedule-C filers. Column 2 and 4 includes average volatility of the demographic group of the tax filers. Standard errors clusted at the individual filer level are repoted in parentheses below the point estimates. The asterisks denote the level of statistical significance: * p<0.10, ** p<0.05, *** p<0.01.

Figure 1: Income Variance and the Long Term Effects of Audit

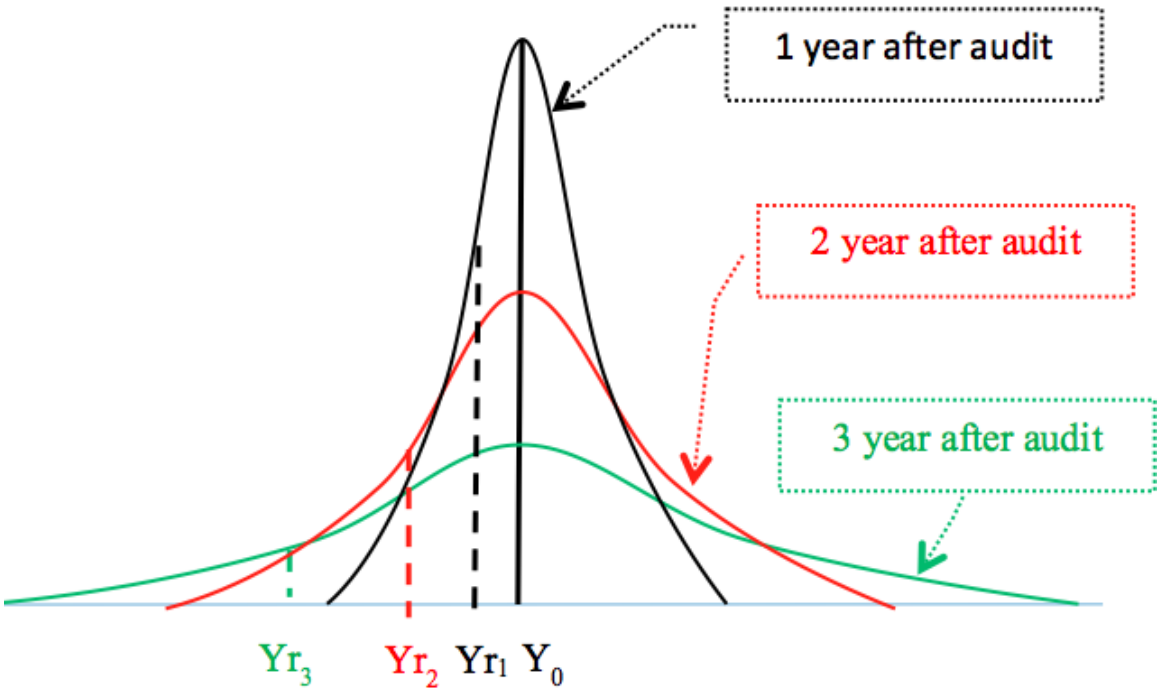


Figure 2: Effects of Audit by Income Source

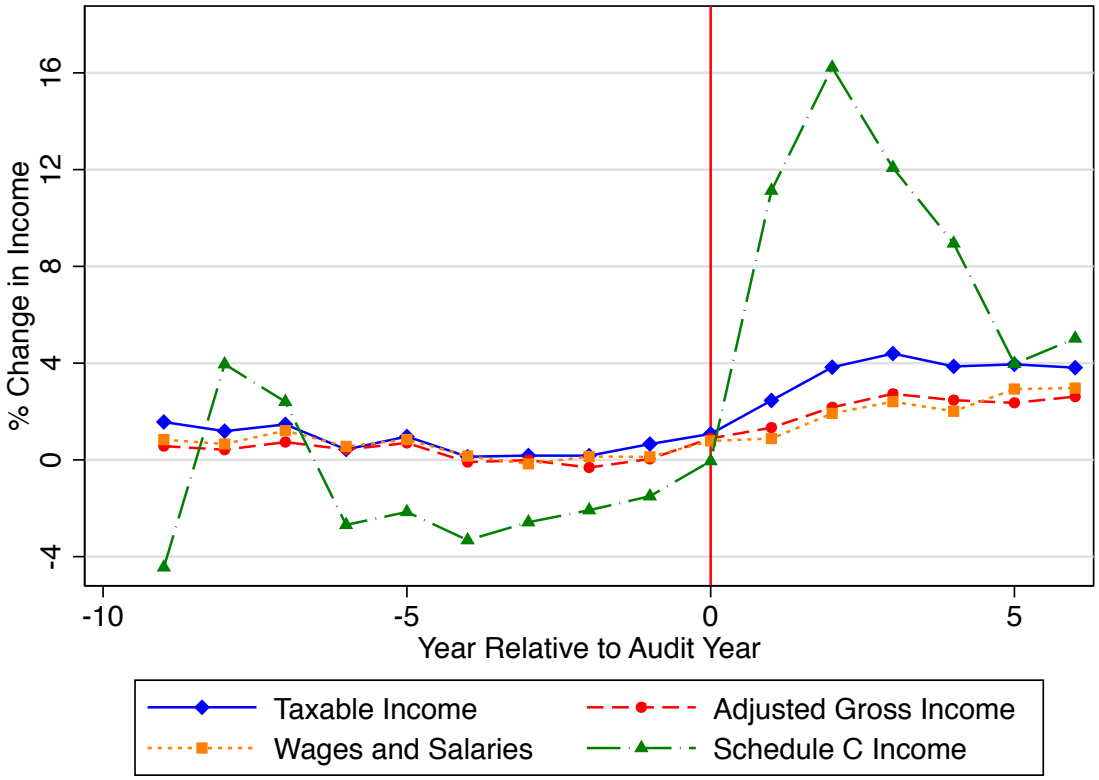


Figure 3: Effects of Audit on Taxable Income

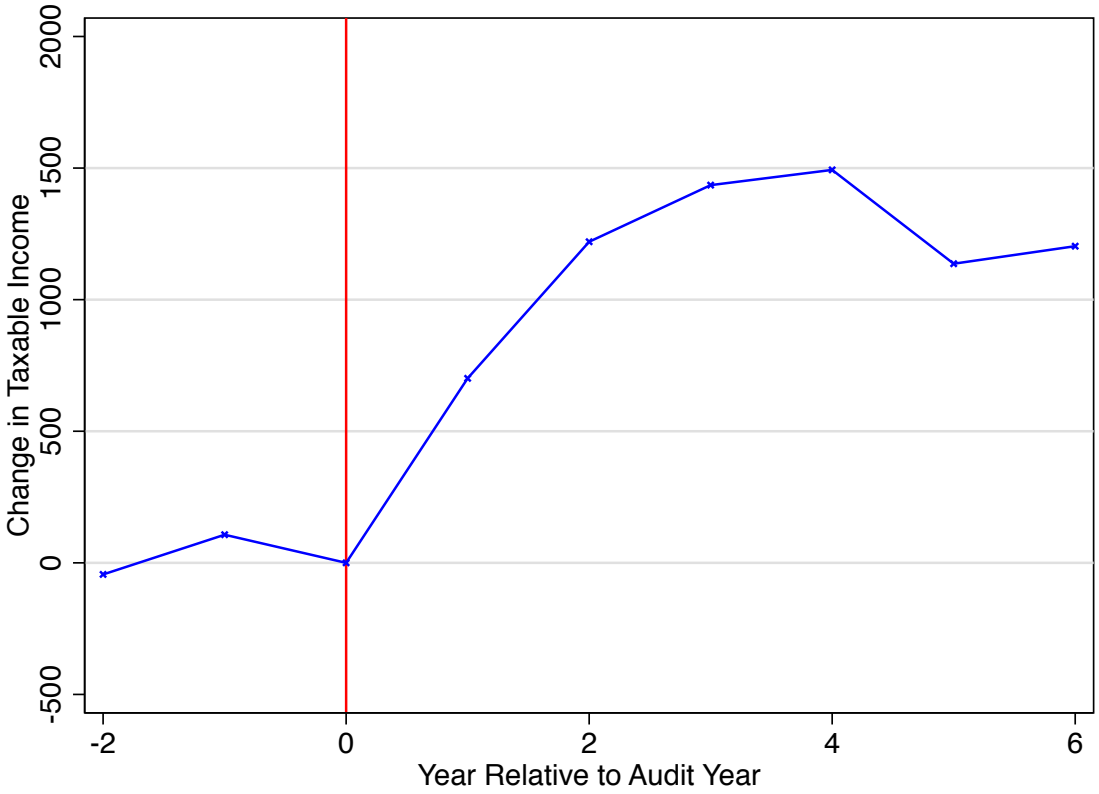


Figure 4: Effects of Audit on Taxable Income by Audit Experience

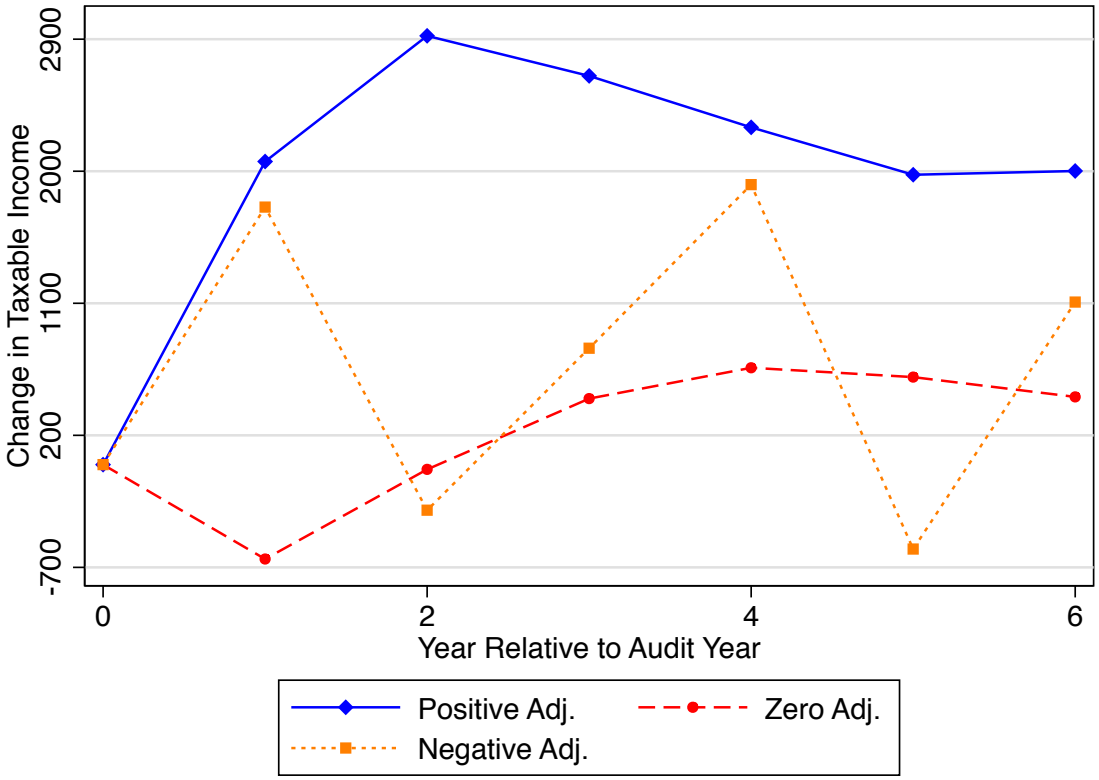


Figure 5: Effects of Audit by Income Source

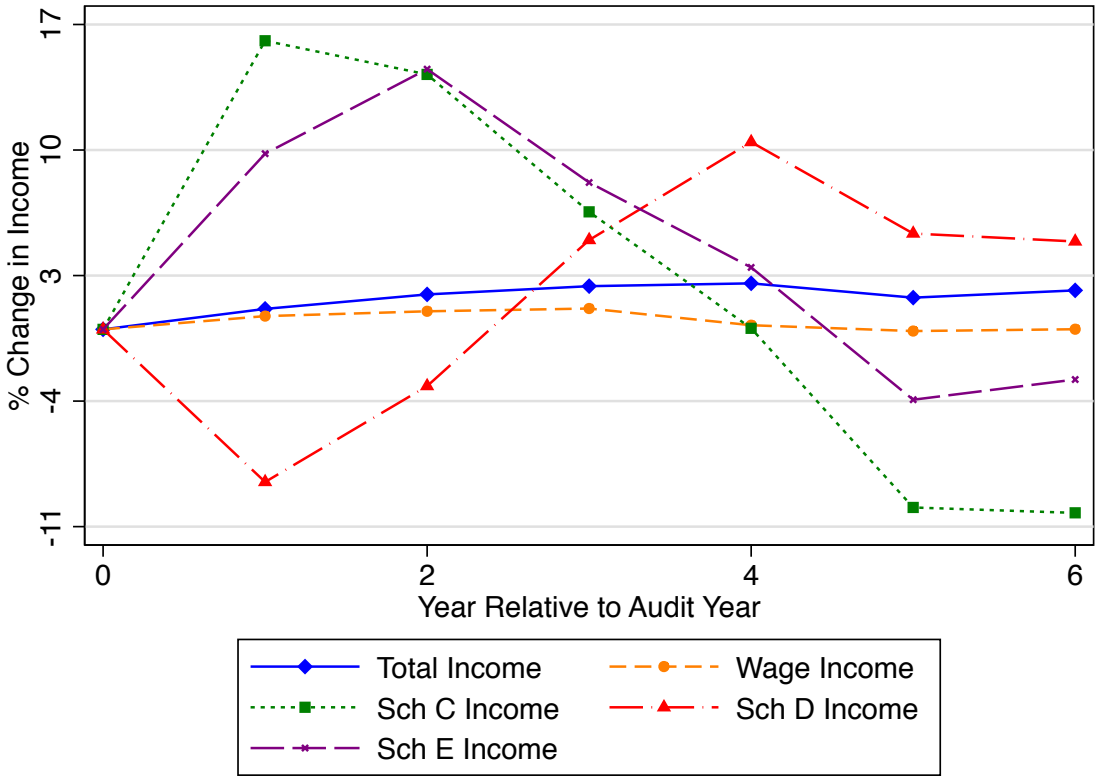
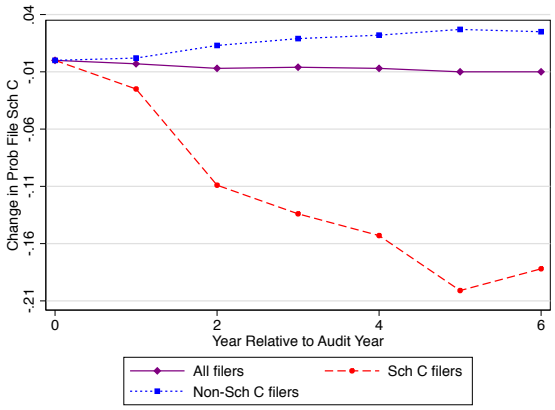
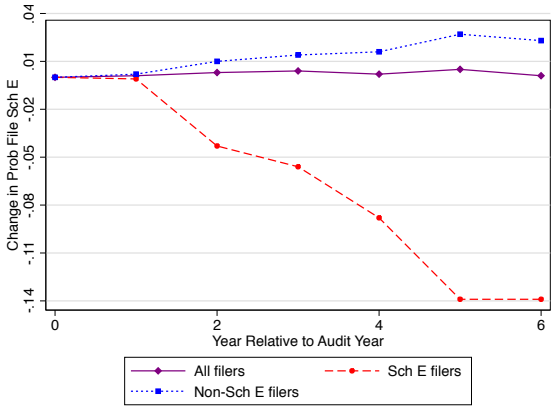


Figure 6: Effects of Audit by Filer Type, Extensive Margin

(a) Likelihood of Filing Schedule C



(b) Likelihood of Filing Schedule E



(c) Likelihood of Reporting Wage Income

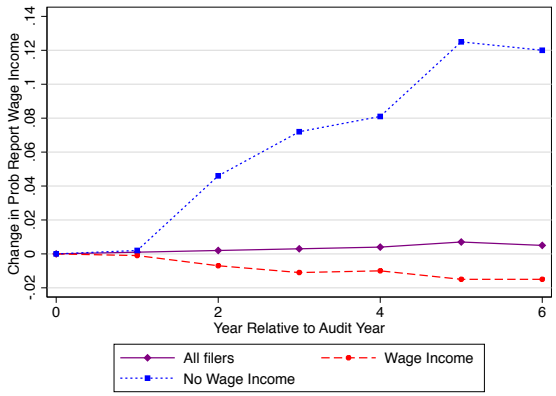


Figure 7: Effects of Audit on Deduction Amounts by Deduction Type

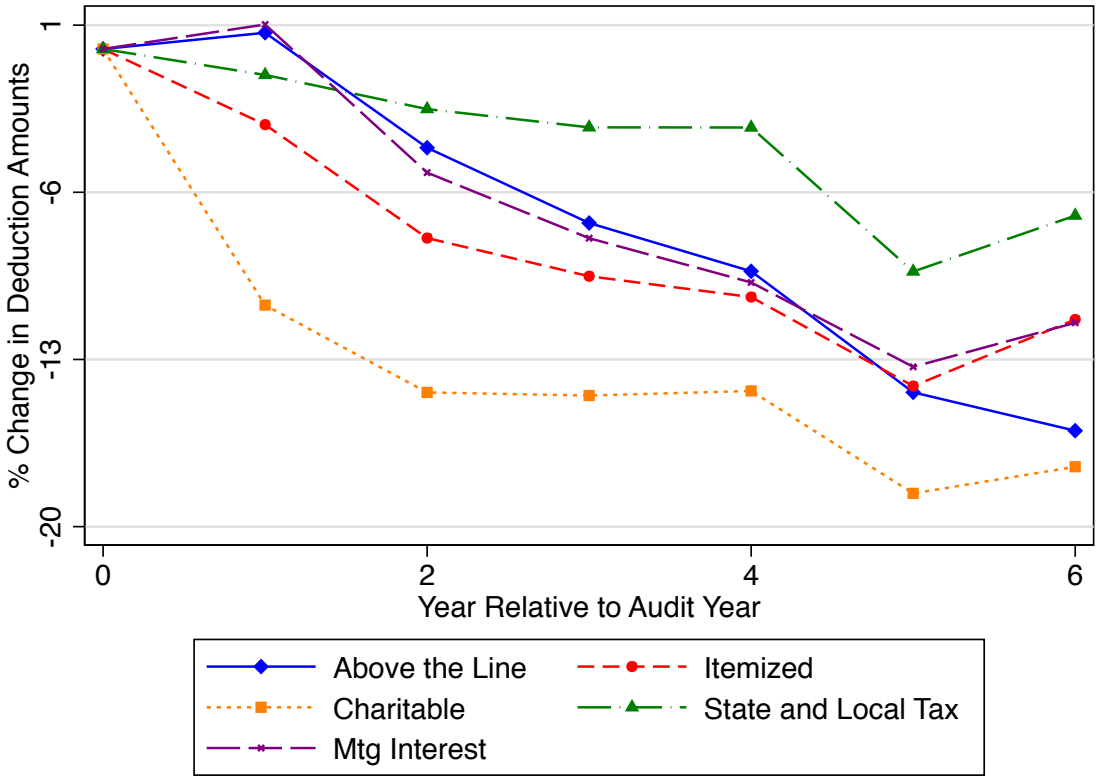


Figure 8: Effects of Audit onTaxable Income by Filer Age

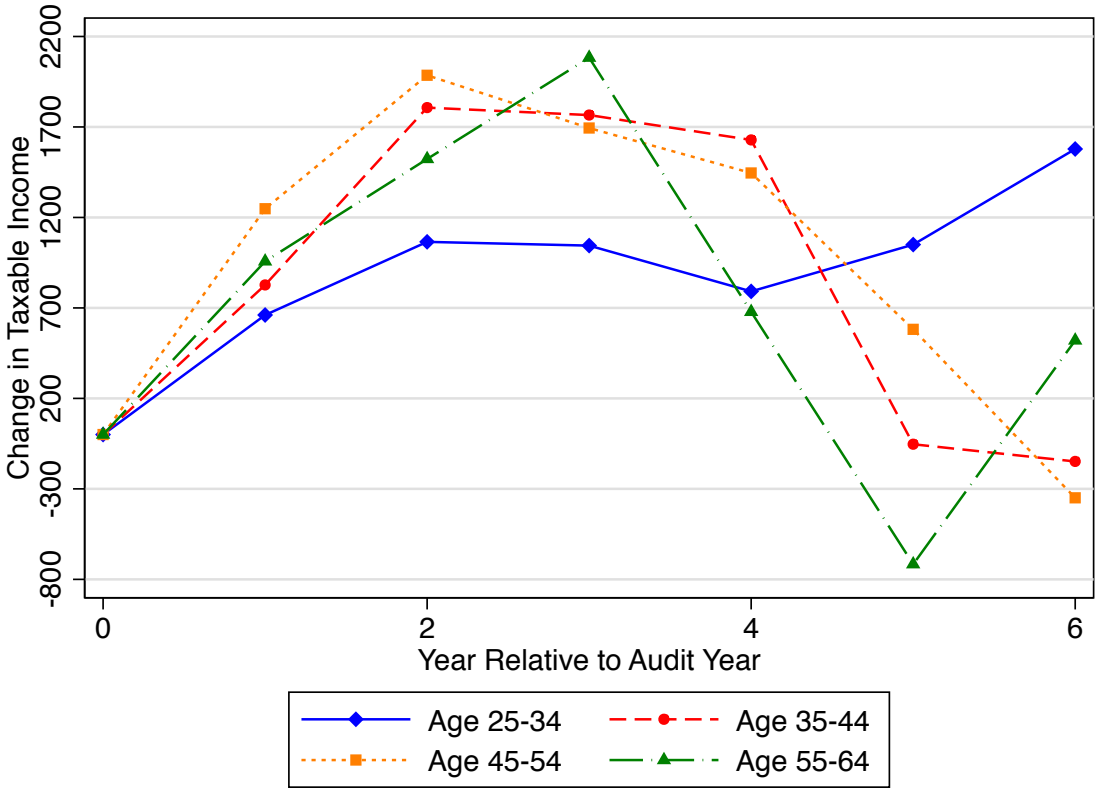


Figure 9: Effects of Audit on Taxable Income by Filer Income

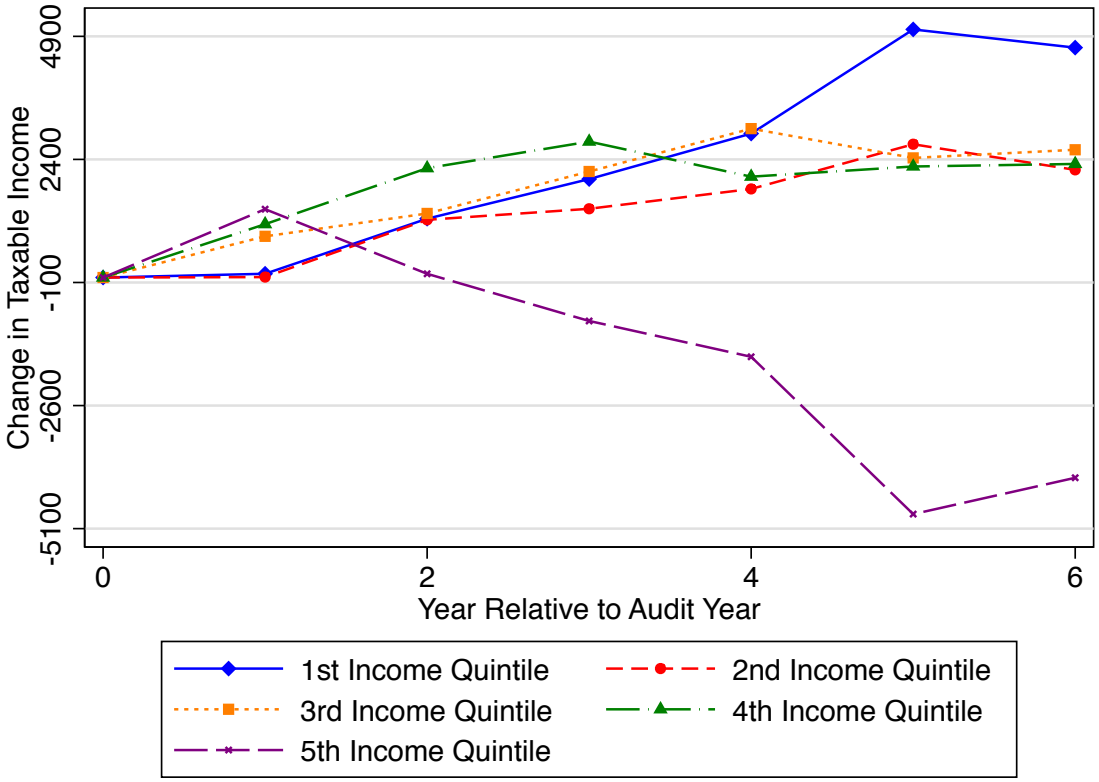
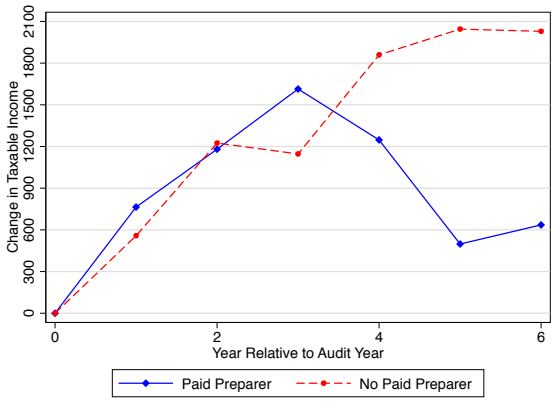


Figure 10: Effects of Audit on Taxable Income by Filing Method

(a) Filers Who Used Paid Preparers vs. Others



(b) E-filers vs. Non-E-filers