Once Bitten, Twice Shy?
The Lasting Impact of Enforcement on Tax Compliance*

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Abstract

We examine the impact of enforcement on subsequent compliance behavior by taxpayers. Exploiting waves of randomized audits by Internal Revenue Service from 2006 to 2009, we find three long-run responses by taxpayers. First, audits increase tax payments substantially in following years, but this effect is short-lived when third-party reporting is not available. Second, taxpayers with high income volatility revert to their pre-audit behavior quickly. Third, sophisticated taxpayers are less affected by enforcement. These responses reveal how taxpayers perceive the enforcement risk and change their noncompliance according to the dynamics of the information barrier between them and the enforcement agency.

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

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I. Introduction

Tax noncompliance represents a significant legal issue. In the United States (U.S.), taxpayers 'voluntarily' pay only about 81.7% of their tax liability each year according the most recent estimate by the Internal Revenue Service. Tax enforcement, mostly via audits, helps to recover an additional 2.0 percentage points of tax liability. The remaining 16.3%, or roughly $406 billion each year, never reaches the federal budget due to tax noncompliance (IRS 2016). While we know enforcement directly improves tax collection by 2.0 percentage points, we have very little idea about the indirect deterrence effects that contribute to the 81.7% that is paid voluntarily.

Understanding deterrence effects has been a central question in the field of law and economics, particularly after the seminal article by Becker (1968). It has been a consensus that punishment risk provides the foundation for deterrent effects (Pogarsky & Piquero, 2003). However, there is no consensus on how enforcement deters subsequent crimes. For example, Gibbs (1975) and Tittle (1980) and ensuing studies argue that punishment discourages future illegal activities. Yet, Sherman (1993), Paternoster & Piquero, (1995), Piquero & Pogarsky, (2002) suggest that criminals are more likely to commit crimes again immediately following punishment.

Our understanding of why people pay or do not pay tax has grown over time. Starting from early conceptual frameworks (e.g. Allingham and Sandmo 1972), we have gained valuable insights from lab experiments (e.g. Alm and Mckee 2004, Fortin et al. 2007, and Choo et al. 2014). Most recently, a new strand of research using field data and experiments highlights the importance of third-party

information in ensuring that individuals pay tax (Kleven et al. 2011, Carrillo et al. 2014, Pomeranz 2015, and Slemrod et al. 2015).[1]

Tax compliance provides a useful setting for us to examine the deterrence effects of enforcement. Alm, Jackson and McKee (2009) note that audits can impact taxpayers in three ways: a direct effect (which consists of adjustments on the audited return), a direct (specific) deterrence effect (in which taxpayers who are audited change their subsequent behavior), and an indirect (general) deterrence effect (consisting of effects on those who are not audited).

Our focus is on the direct deterrent effects of audits; that is, the impact of audits on subsequent tax payments by the audited individuals. Three features of IRS policy and our data make this possible. First, the IRS conducts intermittent random 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.

Conceptually, the long-term response of taxpayers to an audit has not been settled. In theory, experiencing an audit would lead taxpayers to update up their perceived audit probability in a Bayesian manner and thus pay more tax (e.g. Dubin et al. 1990). However, Maciejovsky et al. (2007) and Kastlunger et al. (2009) argue for the opposite: a taxpayer would pay less tax right after an audit because she may believe that the auditor is unlikely to come back soon.

Empirically, three new studies on long-term tax responses using tax data also show very different results. DeBacker et al. (2015) find that U.S. firms reduce tax payment immediately after an audit, and then increase it gradually to pre-audit level. In contrast, a recent working paper by Advani et al. (2015) finds that self-assessed taxpayers in the United Kingdom increase their tax payments after an

[1] For recent surveys of this literature, see Alm (2012) and Slemrod (2016).

audit, while a report by the Internal Revenue Service (2015) that uses one wave of randomized audits finds increased reported taxable income among the self-employed who had positive audit adjustments, but found negative impacts among those who had no additional tax assessment from the audit.

We use randomized, comprehensive, multi-wave administrative audit and tax return data to improve on these studies in several important ways. In DeBacker, et al. (2015), a series of control variables had to be used to account for the fact that operational audit likelihoods are endogenous to the outcomes expected by the tax authority. By relying on randomized audits, the selection into the treatment and control groups is made exogenous and audited taxpayers can be compared directly to those not selected for audit. While Advani et al. (2015) does have randomly selected audits, they also only have data on those taxpayers who had to submit a self-assessment of their tax. The reliance on self-assessment taxpayers means that the results are not generalizable to the entire taxpaying population, only a self-selected group that accounts for about a quarter of the taxpaying population and mainly consist of the self-employed, company directors, pensioners, and very high-income taxpayers. They also construct their control group out of individuals who are audited in the future rather than those who were never selected for audit. This requires them to make assumptions about the intertemporal stability of the audit process and raises significant concerns about survivorship bias. In contrast, our use of the universe of all tax returns allows us to construct a control group that covers almost every taxpayer, and to follow them into the future for an arbitrary number of years. Finally, IRS (2015) focuses on Schedule C filers who were audited in 2007. In addition to expanding the scope of the type of returns audited, we also expend the scope over the number of waves of audits. Because of major macroeconomic events in the late 2000s, it is difficult to say to what extent any economic behaviors are typical. The more waves of audits we are able to incorporate, the more confident we can be that we are observing typical responses.

In this paper, we provide a theoretical framework and empirical results that extend the literature in several ways and that helps provide further evidence to answer some of these unresolved questions. To start, we consider the link between third-party information reporting and long-term responses to audit. This link has not been studied but we believe is central to understanding the deterrent effects of enforcement against tax evasion, and similar illegal behaviors.

Specifically, we propose that the difference in information held by the tax authority and taxpayers determines the long-term impact of audits. In practice, the tax authority usually has imperfect information about the true income of a taxpayer. The tax authority makes attempts to update this information from two main sources, namely third-party reporting and tax audits. Although 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 form a perceived audit risk and manipulate their income reporting strategically. Figure 1 describes the dynamics of this process.

We identify three drivers of the audit probability perceived by taxpayers. First, when *third-party reporting* is not available, audit information would be most useful for the enforcement authority but it can become outdated quickly. Second, higher *income volatility* would make this outdating process faster and therefore reduce the deterrence effect of tax audits. Third, more *sophisticated* taxpayers would be able to exploit the difference in information more effectively. Therefore, our predictions are that the effect of audits on tax compliance will be more short-lived and ineffective when there is less third-party reporting, when taxpayers have higher income volatility, and for taxpayers with higher levels of tax sophistication.

To test for these predictions, 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 implemented 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.[2] For the first time, data allow us to study the long-term responses to audit among both wage earners and filers who are self-employed, as well as measure the responses to audit across a large set of income and deduction items. This is key to understanding the interaction between third-party information and audits in the long run. The treatment, an IRS audit, is randomized through the NRP, thus our empirical strategy is straightforward. Using the sampling weights that the IRS uses to select individuals for an NRP audit, we construct a nationally representative sample of audited individuals. We pair this representative sample with a random sample of individuals drawn from the same population of tax filers. We then compare the tax filings of these two groups before and after the audit year. This exercise allows us to study the impact of three drivers of long-term tax behavior: third-party information, income volatility and taxpayer sophistication. We summarize the key results on these drivers below.

Third-party information. 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, the effect of audit on reported is much larger when there is less third-party reporting, which is consistent with Kleven et al. (2011). Importantly, we show that audits have a long-term effect on tax reporting. An audit increases reported taxable income by an average of $1,185 per year, equivalent to about 2.9% of the average taxable income. Thus, the direct revenue gain from the audit adjustments understates the true revenue gain.

[2] 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." See https://www.irs.gov/businesses/small-businesses-self-employed/irs-audits.

Accounting for changes in tax reporting in the five years following an audit increases the revenue gain by more than 100%.

An interesting and novel finding is that there are differential effects on the persistence of audits over time. The IRS data show that, after an audit, reported Schedule C income increases sharply by more than 16%, but then falls quickly in subsequent years. The effect of audit on reported Schedule C income in the fifth year after an audit is only about one third of the effect in the first year after an audit. In contrast, reported wage income increases by only about 1% in the first year following audit, but continues to increase in the following two years to approximately 2%, after which it remains relatively constant.

Income volatility. Beyond third-party information, we propose income volatility as important factor enabling 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 year-over-year than that of a teacher. If the plumber reports a large change in income, this volatility can reduce the auditor's certainty as to whether or not the change represents tax avoidance or real economic activity. A higher degree of 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 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 filers with higher income volatility, holding constant the level of third-party information reporting across those filers.

Taxpayer sophistication. Further, we find that variation in the effect of audit across filers is correlated with measures of tax sophistication. Tax experience, familiarity with audits and the use of tax preparers reduce the effectiveness of tax audits. These results show that those taxpayers with a better understanding of the tax code respond more strategically to their audit experience.

The results above contribute to an important literature on tax enforcement started by Allingham and Sandmo (1972). Several early studies consider the impact of audit rates on individual taxpayer compliance. These include Tauchen, Witte, and 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 may also impact evasion since they affect the value of misreported income. Evidence of this effect has been provided by Clotfelter (1983) and Feinstein (1991).

Our analysis is built on several important works on the role of third-party information. Feldman and Slemrod (2007) find that documented income is much less likely to be evaded. Kleven et al. (2011) point out the importance of third-party information, which makes Danish taxpayers unable, rather than unwilling, to cheat. Pomeranz (2015) and Slemrod et al. (2015) show that third-party information reduces tax evasion among Chilean and U.S. businesses. Our results support these previous findings, but we also show that the large audit impact on reported income for people without third-party reporting, found in previous studies, diminishes very quickly over time.

Our results also contribute to work that examines the indirect deterrence effect of enforcement against tax evasion and other crimes. These studies include Alm and Yunus (2009), which finds a role for norms and learning in tax evasion in the U.S., and Dubin (2007), which calculates the deterrent effect of audits. We show that this deterrent effect is mitigated by taxpayers' income volatility and sophistication.

The paper is organized into eight sections. Following this introduction, Section II describes the conceptual framework. Section III describes the National Research Program, and Section IV describes the data. Section V presents information on tax compliance in our data. Section VI presents our main empirical results, and Section VII reports results for particular income and deduction items and the relationship between tax compliance and income volatility. In Section VIII, we offer evidence that the response to audit is affected to a great extent by the filer's tax sophistication. Section IX concludes.

II. Conceptual Framework

To guide our empirical analysis of the tax compliance behavior of taxpayers following audit, consider the model of tax evasion introduced by Allingham and Sandmo (1972). The appendix presents an extension to this model where beliefs about audit probabilities evolve depending upon one's interaction with the tax system, including the amount of interaction with the tax authority and audit experiences.[3] Such a model yields a number of interesting comparative statics. As in Allingham and Sandmo (1972), reported income is always less than or equal to true income, reported income is increasing in the penalty rate, and the effect of the marginal tax rate on reported income is indeterminate unless more assumptions are made on the utility function of the taxpayer.[4]

In addition, and of more direct interest to our study, are the following propositions:

Proposition 1: Reported income is increasing in the perceived audit probability.

Corollary 1: The effect of an audit on reported income is ambiguous.

Corollary 2: Reported income is higher for 3rd party reported income.

[3] A formal treatment of this framework is provided in Appendix A1.

[4] One can see this from the first order condition. A higher marginal tax rate increases the benefits to evasions. However, since the penalty is proportional to the tax liability avoided, the expected costs to evading another dollar are also increasing in the marginal tax rate.

Corollary 3: Reported income decreases in the variance of the taxpayer's income.

Corollary 4: The change in the taxpayer's perceived probability is decreasing in the number of tax filings she has experienced.

(The proof of Proposition 1 and these corollaries are in Appendix A1)

Our empirical approach is centered around testing these relationships, in particular how the audit experience affects reported income and how this varies across income sources and filer types. Given the contrasting evidence from the field and laboratory, Corollary 1 leads to an ambiguous result. Our study will provide evidence on this empirical question. Corollary 2 suggests that filers with more 3rd party reported income are more compliant. We look at evidence of this, but also consider the differential impacts of audits across different sources of income. If audits induce compliance, we would expect the responses to be larger from those taxpayers or income sources that were less complaint prior to the audit. That is, we would expect larger changes in reported income after audit from those income sources with less 3rd party reporting.

In the context of the lasting effects of audits, we consider what Corollary 3 implies about the responses to audit from those with more highly variable income sources vis-à-vis those with less variable income sources. Finally, Corollary 4 suggests that we should find a smaller effect of audit amongst groups of filers who are more familiar with the tax system. We call this familiarity "tax sophistication". Our test of Corollary 4 is thus to test whether groups with more experience filing, i.e., more tax literate groups, respond less to the audit event than do other groups.

To summarize, our theoretical model implies four results that 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. Fourth, the effect of audit will be smaller among groups with more tax sophistication.

III. The National Research Program

The NRP conducts audits on a stratified, random sample of the filing population in order to inform IRS enforcement policy. In this section, we discuss the similarities and differences between NRP and operational audits so that the reader can judge the external validity of our research using NRP audits.

The NRP Examination Guidelines (IRS, 2012) state that NRP audits should be conducted in a manner similar to that of operational audits. The Guidelines specify this similarity in many aspects, including audit depth, required filing checks, amount of data collected, judgement standards, referrals to other IRS units, the examiner's responsiveness and availability to meet with the taxpayer.

NRP audits are also similar to operational audits in terms of how the examination location is determined. 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. Note that in the third style of audit, the taxpayer is generally unaware of the audit and it presumably has no behavioral effect.[5] All three methods are used in both operational and NRP audits.

NRP audits are, however, different from operational audits in several ways. Unlike the operational audits, NRP audits are conducted to collect detailed compliance data on all the potential issues on the return. As such, they may cover

[5] This third type of audit accounts for approximately 5% of the audited sample. We note that the NRP randomly chooses tax returns to audit but non-randomly determines the examination method. Therefore, we include this last type in the sample to ensure the randomness of the audit treatment. This means our estimates are the effect of the intent to treat with an audit, rather than being the effect of the treatment on the treated.

parts of the tax return for which there is no suspected noncompliance. Operational audits are focused more narrowly on areas of suspected noncompliance (Black et al, 2012). Interestingly, taxpayers report higher satisfaction from NRP audits than from operational audits (Brown and Johns, 2007).

The most important distinction between NRP audits and operational audits, which are relevant for the external validity of our study, is that the taxpayer is informed that an NRP audit is random, and therefore not related to any action taken by the taxpayer. Further, the infrastructure of the NRP system is set up for measurement and statistical analysis, not for building profiles of individual taxpayers. NRP audits are designed to identify problematic "issues and taxpayer segments" rather than "identify, locate, and monitor… the actual taxpayers whose returns are part of the NRP system" (IRS, 2017). While the understanding of what this means (and thus the behavioral response) varies between taxpayers, we would expect those taxpayers with greater tax sophistication to be better able to take advantage of these features.

However, the audited taxpayer may learn about the stringency of an audit or the ability of the auditor to identify problems in the tax return. Further, the taxpayer may learn about the tax code and how to file tax properly after an audit. We also refer to work such as Manoli and Turner (2014) which finds that contact from the IRS is perhaps more important in changing behavior than the content of that contact. While it is likely that an NRP audit will educate the taxpayer about a wider range of such topics, it is difficult to say whether NRP or operational audits educate the taxpayer on the most salient tax topics (specifically, those for which the taxpayer is currently noncompliant or is considering future noncompliance).

In sum, we speculate that after an NRP audit, taxpayers will update their perceptions about both audit probabilities and audit stringency. Taxpayer seem likely to gain information on audit stringency from both NRP and operational audits, though the taxpayer may learn less information about a broader swath of

topics under an NRP audit. For matters of audit probability, taxpayers seem likely to learn less about their audit risk from an NRP audit than an operational audit, primarily because undergoing an operational audit is *prima facie* evidence to the taxpayer that the IRS perceives them to be noncompliant in some way. However, those with lower tax sophistication seem more likely to conflate the two forms of audits and respond more strongly to the NRP audits than more sophisticated taxpayers who fully comprehend that NRP audits are random.

IV. Data

Our data come from three sources. We discuss each data source in turn and then describe 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.[6] Taxpayer information includes taxpayer identifiers (the social security number [SSN] of the primary filer), year of the audited return, and the resulting adjustment to selected lines on the Form 1040.[7] Each of the 2006-2009 waves has approximately 15,000 observations.

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.

[6] Note that 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 too 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 anomaly 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.

[7] Unfortunately, information on penalties and interest owed (if applicable) is not contained in these data.

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 with a control group and a treatment group. We create the control group by randomly selecting a 0.1% sample of filers, choosing a different set of 10 four-digit SSN endings for each year (2006, 2008, and2009).[8] 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. 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 control groups.[9] Our final panel thus comprises 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.

By combining the random sampling of the NRP with our method of constructing the control group, we are able to avoid many of the pitfalls that would typically affect a panel study of tax compliance. By ensuring that our control group was selected in waves in the same manner as the treatment group, differential attrition cannot be attributed to the fact that we are combining multiple waves of data.

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

[9] While we use pre-audit values in all years for consistency, we also estimated equations (11) and (12) using the after-audit values for the year they are available. The results are similar, except that the estimated coefficients are all approximately $2500 lower. This is consistent with Table 2, where the unconditional audit adjustment is $2419. In other words, taxpayers report larger tax payments after audit, but not as large as the discrepancy found by the auditor.

Furthermore, because selection into the NRP is random, any behavioral differences between the control and treatment group cannot be explained by selection into treatment based on observables. We believe that these two factors rule out the vast majority of purely mechanical explanations for observed outcomes.

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.[10] Operational audits are rare in both our treatment and control groups. The likelihood of receiving an operational audit in either group is under 1%, in line with overall IRS audit rates.

[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 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

[10] 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 method gives us a number of weighted observations approximately equal to the population of filers 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.

expect sharp increases in reported income for all filers in a given NRP wave in a specific year since audit, because 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 are 99% Winsorized. Winsorization of the data is necessary for addressing outliers.[11] 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. The effect of noisy data is compounded by the fact that the distribution of income in our population is quite skewed, making our estimates susceptible to quirks of sampling among the upper end of the income distribution. While our analysis relies on 99% winsorization, we are able to obtain very similar results at both 95% and 99.9% winsorization, and qualitatively similar results when income is winsorized at $10 million. These results are reported in Table A.1. & A.2. in the Appendix.

V. Tax Compliance in the U.S.

The IRS's tax gap measure summarizes aggregate compliance with the U.S. income tax.[12] During 2008-2010, the average tax gap was $458 billion (IRS 2016). This gap represents a compliance rate of 81.7%. Noncompliance with the individual income tax code is the largest source of noncompliance, accounting for $319 billion of the total gap.

With regard to the individual income tax noncompliance, IRS (2016) 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

[11] With 99% Winsorization, we are truncating the values at their 99th percentile. That is, all values at or above the 99th percentile are now assigned the 99th percentiles values.
[12] Note that the NRP data we use plays a large role in the IRS's estimation of the individual income tax gap.

proprietors (as reported on Schedule C of Form 1040), accounts for about half of the individual income tax gap. Looking across income and deduction items, it is evident 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 the IRS. Table 2 documents the measures of compliance found in our NRP data.[13] 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. Columns 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).[14]

[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 noncompliance. We find that noncompliance rates (measured by the fraction of filers with adjustments) are largest for Schedule C (sole proprietorship) 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 noncompliance is lowest for wage and salary income, which is 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 figure

[13] This table only includes adjustments found upon audit. It does not multiply these amounts to account for noncompliance that was not found in an audit.
[14] Note that Column 2 is the weighted sum of Columns 3 and 4.

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.

VI. 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, namely post-treatment difference, difference-in-differences, and within-filer estimations.

A. Post-treatment Difference and Difference-in-differences Estimates

The randomized controlled 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 useful 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:

$$Effect\ of\ audit = \left(\bar{Y}_{T,2} - \bar{Y}_{T,1}\right) - \left(\bar{Y}_{C,2} - \bar{Y}_{C,1}\right), \qquad (10)$$

where T denotes the treatment group (i.e., the NRP sample) and C 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 $\bar{Y}_{T,2}$ is calculated as the mean of the income source of interest for the NRP sample over the three years after audit and $\bar{Y}_{T,1}$ 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, the effect of audits on reported wage income is 1.3% on average ($590 per year). This effect is consistent with the understanding that it is more difficult to misreport income that is also subject to withholding and third-party verification (Kleven et al.2011), either before or after audit, and so the effect is small in percentage terms. Note, however, that this response is large relative to the IRS estimated tax gap for wage income.[15]

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).[16] This large

[15] The IRS estimates an evasion rate of 1% for wages and salaries. See https://www.irs.gov/pub/newsroom/overview_tax_gap_2006.pdf. Taking both estimates together could imply that experiencing an audit eliminates any remaining evasion. Alternatively, they could imply that individuals might respond to an audit by over-reporting income to reduce the risk of future audits.

[16] This effect is predominantly driven by Schedule C income declining substantially among the control group. This is likely due to the fact that the post-audit period spans 2007-2012, which includes the depths of the Great Recession. This recession had a particularly strong negative impact on the earnings of the self-employed, with business or professional net income reported on individual returns declining by almost 10% from 2008-09. See https://www.irs.gov/pub/irs-soi/15intaba.xls.

effect supports the perception that it is easier to manipulate Schedule C income than wage income, and that such income reporting is more responsive after an audit.

B. 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 after an audit and remains elevated even after six years. Adjusted gross income (AGI) and wage income follow a similar pattern to that of taxable income.

As Figure 2 shows, the effect of audits on Schedule C income is strong in the first few 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.[17] We thus delve more deeply into the varying responses by type of income in Section 6.

For each of the four income sources, it is apparent from the figure that the pre-audit trends are similar across the NRP (treatment) and non-NRP (control) samples. Thus the common trends assumption needed for identification cannot be rejected. We next consider models with individual fixed effects, which will allow for identification of the effects of audit from within-filer variation in reported income.

[Figure 2 about here]

[17] Note that these post-audit trends are not the result of compositional differences. To show this, we redo this analysis separately for the treatment and control groups from each NRP wave. The figures showing taxable income and Schedule C income are available in the appendix and document similar trends across each wave.

C. 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

$$Income_{it} = \beta PostAudit_{it} + \gamma_i + \eta_t + \varepsilon_{it}, \qquad (11)$$

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 comes 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.[18] 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:

$$Income_{it} = \sum_{k=1}^{K} \beta_k \, (PostAudit_{it}) * (k \; Years \; Since \; Audit) + \gamma_i + \eta_t + \varepsilon_{it} \qquad (12)$$

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

[18] While we use pre-audit values in all years for consistency, we also estimated equation (11) using the after-audit values for the year they are available. The results were extremely close to those reported here and do not affect our findings.

reported taxable income increases quickly during the first two years after the audit, with an increase of approximately $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 in which we include dummies for the two years prior to audit. Neither of these pre-audit dummies is significant.

[Figure 3 about here]

Although this tells us about the average response, it does not tell us about how specific taxpayers decide what their response will be. If all taxpayers show the same response, this suggests that there is something in common to all audits which is driving their behavior. More likely, taxpayer response will be affected by which adjustments the auditor makes and how large those adjustments are. While it can be difficult to characterize such a multi-dimensional interaction as an audit, one obvious comparison we can make is whether the total adjustment to income is upwards or downwards, and how large that adjustment is.

First, we use the information from the audits in a simple way, distinguishing among those who have positive, negative, or no adjustment. We expect that the response to audit would be stronger for filers with a positive adjustment to tax liability than for those with no adjustment. That is indeed that case. We provide evidence for this effect in Figure 4, where we estimate Equation 12 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.[19] 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

[19] Regression coefficients are presented in Appendix Table A.3.

audit outcome is not exogenous to taxpayer characteristics. However, we believe these results are strongly suggestive of the expected behavior. In other words, while we find that noncompliant filers increase compliance after audit, we do not find that compliant filers increase noncompliance. These results are consistent with what Gemmell and Ratto (2012) observe using UK data.

[Figure 4 about here]

Second, we interact the amount of the audit adjustment with PostAudit dummy variable in Equation (12) to see if the adjustment amount magnifies the effect of PostAudit. We report the result of this interaction analysis in Appendix 2. In Table A.4, we can see that the interaction effects are not significant when all post-audit years are grouped together. Table A.5 examines this interaction effect over time and shows it is positive and significant in years 2 and 3 after the audit, though the magnitude of these interaction effects are also small compared to the main effect. In other words, if taxpayers receive a positive adjustment, they will increase their reported income significantly, as showed in Figure 4, but the size of the adjustment increases this effect but only marginally. This should not be surprising, as the average positive adjustment in our data is $5,167 while the largest effects audits have is an average increase in income after audit of around $2,850. What this means is that, on average, individuals report more income following audit, but they do not increase reported income by the same magnitude as the amount of underreported income found upon audit.

VII. 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. We then examine whether the response differs for taxpayers with more or less volatile income.

A. By Income Source

IRS data allow us to distinguish incomes from wages and Schedules C, D, and E. Among these sources, wage income information can be easily cross-checked with data reported by employers. Information on incomes from Schedules 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.

Intensive Margin Effect

In Table 5 (and Figure 5), we present estimates of versions of Equation 12, 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 filers who have non-zero amounts of income of that particular type in the year of the audit, and therefore these findings 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.[20] The results in this column are slightly smaller than, though similar to, those for taxable income. This outcome is not surprising due to the fact that taxable income also incorporates the filer's choice of deductions and so provides more opportunity to manipulate taxes through reporting.

[20] Total income is from Form 1040, Line 22, i.e. AGI with the above-the-line deductions added back in.

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.

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 expectation, the estimation results suggest that Schedule C income increases substantially after audit, by more than$1,000 in the first two years. Interestingly, that effect diminishes three 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 immediately after an audit, but over time may become more aggressively noncompliant than they were in the years prior to the audit.

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 those 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.[21]

[21] Appendix Tables A.6 and A.7 interact the PostAudit dummy variable with the amount of the audit adjustment to examine if the adjustment amount magnifies the effect of PostAudit differentially for different income sources. In Table A.6, the interaction the interaction effects are not significant except for Schedule C income, which is the income source with the highest rates of noncompliance (see Table 2). The estimate here indicates that when the audit adjustment increases by $100, taxpayers increase their reported incomes by $1.80, which is economically small. However, two caveats apply to this estimate. First, the total estimated effect of the audit would also include the fixed amount of $609. Second, this effect is estimated on a select sample of people found to have been noncompliant upon audit, and so the small effect may be reflecting that taxpayers who are more noncompliant have a greater propensity to continue to be noncompliant after audit, leading to the small change in Schedule C income. Table A.7 examines this interaction effect over time and shows it is in some years for most income sources, though the magnitude of these interaction effects are also small compared to the main effect.

Extensive Margin Effect

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

$$I(Y \neq 0)_{it} = \sum_{k=1}^{K} \beta_k \, (PostAudit_{it}) * (k \, Years \, Since \, Audit) + \gamma_i + \eta_t + \varepsilon_{it}, \quad (13)$$

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 13 are reported in Figure 6, Panels A-C. Each panel reports results for three groups: those who filed the schedule, 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.[22] 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 four possible reasons for this decline, though our data do not allow us to definitively say how many filers are affected by each of the four possibilities. First, audits may result in increases in reported income and thus taxes. This effect reduces 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 to offset ordinary income elsewhere on the return. An audit may have found such losses to be illegitimate, and thus the filer discontinued their use of those losses and the filing of the associated schedule.

[22] Coefficient estimates are presented in Appendix Table A.8.

Third, particularly for Schedule C filers, 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, which shows 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 that 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]

Fourth, for Schedule C filers, an audit might induce a taxpayer to change the form of their pass-through business from a sole proprietorship to a type for which income would be reported on Schedule E (such as an S corporation). Popular press reports note that Schedule C income is audited at a much greater rate than Schedule E income,[23] and so the audited taxpayer may aim to avoid future audits by making such a change. Evidence of such an effect can be seen in Figure 5, Panel D, in which Schedule C filers tend to be more likely to report Schedule E income after audit while non-Schedule C filers do not.

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.

B. By Deduction Item

We next estimate variants of Equation 12 in which the dependent variables denote the amounts of particular types of deductions that filers claim. Column 1 of

[23] See, for example, https://www.marketwatch.com/story/what-are-your-chances-of-getting-audited-2015-12-15?mg=prod/accounts-mw.

Table 6 presents the results for above-the-line deductions[24] and Column 2 presents 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 theory that taxpayers manipulate their tax reporting where they are more 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 up to $460 after an audit, with no decline in later 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]

C. 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

[24] These are deductions that are made from total income when calculating adjusted gross income (AGI), and include deductions for moving expenses, health savings accounts, self-employed contributions to retirement plans and health insurance premiums, alimony paid, amounts contributed to IRAs, student loan interest, and tuition and fees paid for secondary education.

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 will be able to estimate their wage income for a given number of years in the future 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 during the same post-audit period, the IRS has very little ability to predict the true range of Schedule C income. 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 calculation 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

finding 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]

VIII. The Implications of Tax Sophistication

As noted above, an NRP audit is random, and the NRP subjects receive explicitly state that they were selected for a random audit used for IRS research purposes. Despite this disclosure, we find significant impacts of audit on filers. One reason for this effect may be that filers misunderstand the nature of the NRP audit. In particular, filers may believe their returns are selected for operational audits because the IRS suspects some section of the return is incorrect, even though the letter makes clear that the IRS does not necessarily believe anyone selected for NRP audits is delinquent in any way.[25] Alternatively, though the IRS letter is silent about whether they will use noncompliance found in an NRP audit to improve the coverage of subsequent audits, some individuals may assume that the IRS will take such findings into account, and adjust their behavior accordingly. To test this possibility, 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 sophistication or understanding of tax enforcement. In particular, we use proxy for tax sophistication with measures related to the number of tax returns filed. These characteristics include age and the use of a paid tax preparer. Although none of these characteristics is a perfect proxy for tax

[25] While the contact letter does not make any claims about how the NRP audit will affect future audit rates, we find that the likelihood of future audit is very similar for those in either the control or treatment groups. We also see little difference in future audit rates whether the NRP found positive or negative adjustments.

sophistication, consistent responses across these groups would suggest that tax sophistication may play a role in how filers respond to audits.

A. Responses by Filer Age

Figure 8 splits the sample according to the age of the primary filer in the year of the audit.[26] 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 approximately $750. These results contrast with those of Kleven et al. (2011), who find that the propensity to underreport income falls with age.

[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 2 and 3 after the audit, but a decline afterwards. Thus we find evidence that audits have more persistent deterrent effects on those who are relatively new tax filers. This finding may reflect differences in how those with a short filing history update their prior beliefs about IRS enforcement following an NRP audit as compared to those with longer filing histories and more experience with IRS enforcement.

[26] For observations in the control group, we split based on age in the tax year for which they were drawn as a control observation. The Appendix Table A.9 shows the regression results from which Figure 8 is created. The table shows the significance of these post-audit effects.

B. Responses by Use of Paid Preparer

We now condition samples on whether the taxpayer used a paid preparer to file the tax. Our assumption is that those using a paid preparer results in more information about the tax system. The paid preparer is an expert and someone who likely has participated in the filing of a large number of tax returns. Thus a taxpayer employing the help of a paid preparer will be more a more tax literate filer since she has the assistance of the preparer.

Figure 9 displays the results comparing those who used paid preparers in the year the audited return was filed to those who did not.[27] In the first three years after audit, these two groups have very similar responses, increasing reported taxable income by more than $1,000 above that of the comparable group that was not audited. However, after year 3, the post-audit trends diverge. In these later years, the trend for those using a paid preparer 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. That is, the increase in reported income is much more transitory for taxpayers using a paid preparer. This is consistent with the theoretical prediction of a larger response from those with less experience with the tax system. In particular, it is likely that paid prepares better understand the random nature of the NRP audits and thus initially have their clients increase reported income (e.g., during the time the audits might still be underway), but then return to pre-audit reporting behavior knowing that the NRP audit signals nothing about future audit probabilities.

[Figure 9 about here]

[27] The Appendix Table A.10 shows the regression results from which Figure 9 is created. The table shows the significance of these post-audit effects.

C. Responses by Income

Finally, we might expect that taxpayers' sophistication may vary with their income. Taxpayers with higher incomes may rationally invest in a deeper understanding of the tax code and may therefore have a differential response to an audit. To understand these differences, we create treatment and control groups based on the quintile of their income immediately prior to audit.[28]

Figure 10 shows how the responses to audit differ across income groups. 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 in the difference in dollars of taxable income reported after audit. Note that this is true despite this group having the lowest taxable income and marginal tax rates at the time of audit. Most fascinatingly, 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. This response of the highest income group is similar to results in Slemrod, Blumenthal, and Christian (2001) who found that lower income taxpayers increased their tax payments when informed by the tax authority that their returns would be "closely examined," while high income taxpayers decreased their tax payments. The high income response is also similar to the response by corporations found in DeBacker et al (2015). High income individuals appear to be more likely to know, then, that these are random audits, whose results are not used to determine whether they are audited in the future, and so they do not increase their reported income in response. [29]

[28] A related topic of interest would be how audit response depends on marginal tax rate, since the predictions of the Allingham-Sandmo model are sensitive to details of the tax code. However, this introduces endogeneity concerns which would potentially swamp the usefulness of our randomized treatment. To that end, we point towards the positive relationship between income tax rates and income, and claim that results based on income quantiles are probably quite similar to results based on tax rates.

[29] Slemrod et al. (2001) speculate that their perverse findings for high income taxpayers may be due to high income taxpayers believing that a higher report will lead to a lower probability of audit, and so if they know that they are going to be audited, it no longer makes sense to increase their report. Alternatively, they note that it may be that the taxpayer views the audit as a negotiation, and so it makes sense to start with a low bid. Although the first mechanism is unlikely to explain our results,

[Figure 10 about here]

IX. Conclusion

Tax evasion is estimated to lower global tax collections by about 18% and result in approximately $3 trillion 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 its economy. A key measure to confront this problem is through audits, which have two main effects on the audited. First, there is an immediate revenue gain when the auditors discover noncompliance. Second, those audited tend to report higher taxable income in subsequent years, resulting in further revenue gains. Our study rigorously evaluates these effects and identifies several factors determining the effectiveness of tax audits in the short and long term.

To do so, we examine the randomized audits by the Internal Revenue Service (IRS) National Research Program (NRP) during the 2006-2009 period. Our simple difference-in-differences specification indicates that an audit increases reported taxable income by more than $1,100 per year, 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 but 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 six years after an audit. Contributing to this increase, Schedule C and Schedule E income (which are not subject to third-party reporting or withholding) tend to sharply increase after an audit, but this increase diminishes (and turns negative) five or more years after audit, while the increase in wage and salary (which is subject to third-party

the second mechanism might, in that after going through an NRP audit, high income taxpayers might believe that they would pay less tax after audit if they reported less income on their return.

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 (e.g., state and local taxes and mortgage interest) that are subject to third-party reporting.

It is worth noting that the effects we measure are the effects of a random audit. In theory, 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, randomizing audits provides us a much more reliable setting to estimate the impact. 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 provide 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.

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 sophistication of the filer. These results help to better explain the information problem in law enforcement and identify what types of intervention may be most effective.

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Appendix

A1. A theory of responses to audit

In this appendix, we formalize the conceptual framework of Section II. We start with the seminal model of evasion by Allingham and Sandmo (1972) and modify it to incorporate changes in the expected probability of audit conditional on having been audited.

Consider a taxpayer who values after-tax income. Let the expected utility of the taxpayer be given as:

$$Eu(y,h,n,X) = \phi_{h,X}u((1-\tau)y - p\tau(y-R)) + \left(1-\phi_{h,X}\right)u(y-\tau R), \qquad (1)$$

where y is the taxpayer's true income, h is the taxpayer's audit history, n is a measure of taxpayer sophistication (which may be related to experience with the tax authority or reliance on tax professionals), X are observable characteristics of the taxpayer other than reported income, ϕ_h is the taxpayer's perceived probability of audit given her audit history h, p is the penalty rate (which is proportional to the amount of taxes evaded), and R is the taxpayer's reported income (and her only choice variable). Note that we have assumed a constant marginal tax rate and perfect enforcement upon audit (i.e., true income tax liability is determined) to simplify the analysis.

Let the taxpayer's perceived audit probability take the following form:

$$\phi_{h,X} = \phi_{0,X} + f(h,n) + g(E(y|X) - R, \sigma_X^2), \qquad (2)$$

where $\phi_{0,X}$is the filer's initial beliefs of the audit rate for taxpayers with characteristics X, $f(h,n)$ reflects the updating of these initial beliefs given audit history h and tax filing experience n, and $g(\cdot,\cdot)$ measures the perceived changes in audit rates as the taxpayer's reported income deviates from mean income for filer's with her characteristics and as the uncertainty of these expectations increases. We measure this uncertainty as the variance in true income, $\sigma_X^2 = var(y)$, which can be decomposed into two components that are conditional on X by the law of total variance:

$$var(y) = E\big(var(y|X)\big) + var\big(E(y|X)\big) \qquad (3)$$

We make the following assumptions about $f(\cdot,\cdot)$ and $g(\cdot,\cdot)$:

- A1: The perceived probability of audit increases as reported income falls relative to its expected value, $g_1 = \frac{\partial g(E(y|X)-R,\sigma_X^2)}{\partial(E(y|X)-R)} > 0$. This broadly describes a taxing authority with the goal of recovering tax revenue from under-reporters. Allingham and Sandmo (1972) adopt this assumption when considering endogenous audit rates.
- A2: The rate at which the perceived audit probability changes is increasing as reported income falls further below the expected value of income, $g_{1,1} = \frac{\partial^2 g(E(y|X)-R,\sigma_X^2)}{\partial(E(y|X)-R)^2} > 0$. Together, these first two assumptions describe a situation where taxpayers expect the taxing authority to disproportionately focus on the cases with the largest likely tax avoidance.

- A3: The change in the perceived audit probability from an additional audit is ambiguous: $\frac{\partial f(h,n)}{\partial h} \lessgtr 0$
    - If the filer bases her perceptions on the frequency of audits she has experienced, or is otherwise "scared straight" from the audit, it would be the case that the perceived audit probability is increasing in the number of audit experiences, $\frac{\partial f(h,n)}{\partial h} > 0$.
    - On the other hand, filers may react with a 'bomb-crater' effect, perceiving their likelihood of audit to decline if they have a very recent audit in their history. Thus, it may be that $\frac{\partial f(h,n)}{\partial h} < 0$.
- A4: The perceived audit probability declines as the variance in income increases, $g_2 = \frac{\partial g(E(y|X)-R,\sigma_X^2)}{\partial \sigma_X^2} < 0$. This will be true if the taxpayer believes that the enforcement agency is more likely to pursue cases where it is more certain that it will discover tax avoidance. For instance, it is sufficient for the taxpayer to believe that the enforcement agency faces some real cost of performing audits and that it is risk-averse in some institutional sense.
- A5: Absent collusion between the taxpayer and the third party, increases in third party reported income lower the variance of the conditional expectation of y.

That is, $\frac{\partial var(E(y|X))}{\partial 3rd\ Party} < 0$. This is because the tax enforcement agency has more information for conditioning their estimate of true income.

- A6: The more sophisticated the tax payer, the smaller her response to a change in her audit history, $\frac{\partial^2 f(h,n)}{\partial h \partial n} < 0$. We believe this assumption is reasonable since the more experienced taxpayer should generally learn less about tax enforcement than someone undergoing their very first audit.

The necessary condition for the taxpayer's optimization problem is:

$$\frac{\partial Eu(y,h,n,X)}{\partial R} = \phi_{h,X}\,\tau p u'\big((1-\tau)y - p\tau(y-R)\big) - \tau(1-\phi_h)u'(y - \tau R) - u\big((1-\tau)y - p\tau(y-R)\big)g_1 + u(y-\tau R)g_1 = 0 \quad (4)$$

Below, we will simplify notation by using the following shorthand: $\big((1-\tau)y - p\tau(y-R)\big) \equiv (Caught)$ and $(y-\tau R) \equiv (Avoided)$.

There are a number of results that fall out of this model. Since we follow Allingham and Sandmo (1972) closely, we get many of their same comparative statics. In particular, in this model, reported income is always less than or equal to true income, reported income is increasing in the penalty rate, and the effect of the marginal tax rate on reported income is indeterminate unless more assumptions are made on the utility function of the taxpayer.[30]

We now outline in more detail the implications of this model that are relevant for our study.

Proposition 1: Reported income is increasing in the perceived audit probability.

Proof: Totally differentiating the necessary condition from the taxpayer's problem we find:

$$\frac{\partial R}{\partial \phi_{h,X}} = -\frac{\overbrace{p\tau u'(Caught)}^{>0} + \overbrace{\tau u'(Avoided)}^{>0}}{\underbrace{\phi_{h,X}^2 p^2\tau^2 u''(Caught)}_{<0} + \underbrace{g_{1,1}[u(Caught)-u(Avoided)]}_{\leq 0} - \underbrace{g_1[\tau u'(Avoided)+p\tau u'(Caught)]}_{>0} + \underbrace{\tau^2(1-\phi_{h,X})u''(Avoided)}_{<0}} = -\frac{>0}{<0} > 0 \quad (5)$$

[30] One can see this from the first order condition. A higher marginal tax rate increases the benefits to evasions. However, since the penalty is proportional to the tax liability avoided, the expected costs to evading another dollar are also increasing in the marginal tax rate.

The result follows from assumptions A1 and A2. This proposition shows that taxpayers who perceive a higher audit probability will be more compliant. Four corollaries extend this result by considering how the perceived audit probability is affected by audit experiences and filer characteristics.

Corollary 1: The effect of an audit on reported income is ambiguous.

Proof: Totally differentiating the necessary condition, we find:

$$\frac{\partial R}{\partial h} = \underbrace{\frac{\partial R}{\partial \phi_{h,X}}}_{>0} \underbrace{\frac{\partial f(h,n)}{\partial h}}_{\lessgtr 0} \lessgtr 0 \qquad (6)$$

The result follows from assumption A3.

Corollary 2: Reported income is higher for 3rd party reported income.

Proof: Recall that the variance of y can be written as: $var(y) = E(var(y|X)) + var(E(y|X))$. Given this, we find that reported income is increasing in the amount of third party reporting:[31]

$$\frac{\partial R}{\partial\ 3rd\ Party} = \underbrace{\frac{\partial R}{\partial \phi_{h,X}}}_{>0} \underbrace{\frac{\partial g(E(y|X) - R.\sigma_X^2)}{\partial \sigma_X^2}}_{<0} \underbrace{\frac{\partial \sigma_X^2}{\partial\ 3rd\ Party}}_{<0\ by\ assumption} > 0 \quad (7)$$

The result follows from assumptions A4 and A5.

Corollary 3: Reported income decreases in the variance of the taxpayer's income.

Proof: Recall that the variance of y can be written as: $var(y) = E(var(y|X)) + var(E(y|X))$. An increase in the variance of the taxpayer's income source can be represented as an increase in$E(var(y|X))$. Given this, we find that reported income is decreasing in the variance of true income:

$$\frac{\partial R}{\partial \sigma_X^2} = \underbrace{\frac{\partial R}{\partial \phi_{h,X}}}_{>0} \underbrace{\frac{\partial g(E(y|X) - R.\sigma_X^2)}{\partial \sigma_X^2}}_{<0} < 0 \quad (8)$$

The result follows from assumption A4.

[31] Kleven et al. (2011) posit a similar result, although our derivation of why the audit probability is decreasing in 3rd party reporting is more explicit. Our model suggests that there may be an entire class of enforcement methods which operate by improving the tax authority's ability to forecast income, or which 3rd party reporting is just one.

Corollary 4: The change in the taxpayer's perceived probability is decreasing in the number of tax filings she has experienced.

Proof: Totally differentiating the taxpayer's first order condition, we find:

$$\frac{\partial^2 R}{\partial h \partial n} = \frac{\partial R}{\partial \phi_{h,X}} \frac{\partial}{\partial n}\left(\frac{\partial f(h,n)}{\partial h}\right) = \underbrace{\frac{\partial R}{\partial \phi_{h,X}}}_{>0} \underbrace{\frac{\partial^2 f(h,n)}{\partial h \partial n}}_{<0} < 0 \quad (9)$$

The result is driven by assumption A6.

Table 1. Number of observations

	Total observations
NRP Sample	
All Years	4,364,655,406
Base Years (2006, 2008, and 2009)	404,252,738
Positive 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 Years (2006, 2008, and 2009)	405,187,000

Note: The National Research Program (NRP)'s sample is the treatment group, which has been audited. The random sample is randomly selected from the universe of tax filers. 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 NRP.

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 columns 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 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.

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	1172.55	-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	1190.27	1155.673	3.137	-745.64	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	8201.87
	(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 variables are a series of dummy variables 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.

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	-2507.81	-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	-252.94	-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	1023.37	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 explanatory variable is a series of dummies indicating the number of years after the last audit. Standard errors clustered at the individual filer level are reported in parentheses below the point estimates.

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	3671.93	2614.918	8842.014
	(434.124)	(554.753)	(993.198)	(1374.814)
6 Year Post Audit	1190.27	2439.411	2120.874	6669.378
	(515.569)	(645.825)	(1052.582)	(1355.481)
1 Since AuditVolatility		-440.566		-523.944
		(94.452)		(163.790)
2 Since AuditVolatility		-580.613		-667.382
		(100.096)		(173.935)
3 Since AuditVolatility		-857.054		-1063.773
		(106.480)		(184.120)
4 Since AuditVolatility		-426.451		-764.784
		(136.857)		(230.788)
5 Since AuditVolatility		-866.402		-1395.788
		(179.025)		(323.981)
6 Since AuditVolatility		-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	34065.13	43563.92	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 clustered at the individual filer level are reported in parentheses below the point estimates.

Table A.1. Audit effect under different levels of Winsorization (average after audit)

Winsorization level	None	>10mil	0.1%	1%	5%
Post-Audit	-110.756	1180.830**	1100.357***	1109.329***	974.332***
	(1568.818)	(511.290)	(271.046)	(184.312)	(139.159)
Individual FE	yes	yes	yes	yes	yes
Year of Tax Return	yes	yes	yes	yes	yes
Constant	40274.859***	39326.291***	37760.768***	34098.286***	29573.839***
	(638.949)	(311.820)	(204.007)	(137.019)	(93.406)
R-squared	0.000	0.001	0.002	0.005	0.009
N	4771427	4771427	4771427	4771427	4771427

Notes: This table reports the regression of the duration since the last audit on repoted taxable income under different levels of winsorization. Standard errors clusted at the individual filer level are repoted in parentheses below the point estimates.

Table A.2. Audit effect under different levels of Winsorization (each year after audit)

Winsorization level	None	>10mil	0.1%	1%	5%
1 Year Post Audit	285.682	1667.715	856.733**	687.422***	656.934***
	(1992.150)	(1132.434)	(382.186)	(186.026)	(137.338)
2 Years Post Audit	2329.884	1492.201***	1285.728***	1203.155***	1110.014***
	(2867.748)	(526.734)	(310.808)	(201.095)	(155.487)
3 Years Post Audit	-2418.703*	765.208	1273.320***	1424.118***	1224.308***
	(1455.960)	(482.926)	(351.695)	(246.485)	(177.860)
4 Years Post Audit	-249.967	681.055	1235.173***	1480.547***	1069.996***
	(1850.060)	(657.672)	(467.398)	(328.235)	(232.083)
5 Years Post Audit	-4901.221	-25.066	713.511	1122.686***	1107.607***
	(3310.886)	(1029.872)	(603.287)	(434.124)	(327.745)
6 Years Post Audit	-2913.668	-565.026	777.436	1190.270**	846.231**
	(2795.738)	(1204.772)	(794.479)	(515.569)	(370.832)
Individual FE	yes	yes	yes	yes	yes
Year of Tax Return	yes	yes	yes	yes	yes
Constant	40268.995***	39323.949***	37760.422***	34098.639***	29574.002***
	(639.927)	(311.886)	(203.957)	(136.963)	(93.366)
R-squared	0.000	0.001	0.002	0.005	0.009
N	4771427	4771427	4771427	4771427	4771427

Notes: This table reports the regression of the duration since the last audit on repoted taxable income under different levels of winsorization, for each year after an audit. Standard errors clusted at the individual filer level are repoted in parentheses below the point estimates.

Table A.3. Effect of audit and reported taxable income by audit experience

	Type of Tax Adjustment		
	Positive	Zero	Negative
1 Year after	2065.827	-643.277	1755.373
	(296.999)	(239.672)	(1018.049)
2 Years after	2922.386	-32.348	-310.609
	(336.006)	(255.158)	(1000.503)
3 Years after	2649.289	450.225	793.552
	(390.910)	(316.991)	(1231.639)
4 Years after	2298.685	660.304	1908.434
	(534.140)	(368.955)	(1537.968)
5 Years after	1975.891	596.786	-575.319
	(687.271)	(484.729)	(2121.946)
6 Years after	2001.357	461.810	1108.057
	(800.131)	(578.894)	(2508.410)
Individual FE	yes	yes	yes
Year of Tax Return	yes	yes	yes
Constant	34117.05		
	(136.554)		
R-squared	0.005		
N	4,771,427		

Notes: This table reports the regression of the duration since the last audit on repoted taxable income by adjustment to tax liability determined in the NRP audit (positive, negative, or no adjustment). The coefficients were estimated in one regression interacting the audit adjustment with the post-audit dummy variables, though they are presented in three columns here for easier interpretation. Standard errors clusted at the individual filer level are repoted in parentheses below the point estimates.

Table A.4. Effect of auditing on taxable income, interacted with adjustment

	Taxable Income
Post-Audit	781.525***
	(221.116)
Interaction Effect	0.015
	(0.011)
Individual FE	yes
Year of Tax Return	yes
Constant	39222.706***
	(151.755)
R-squared	0.009
N	3840937

Notes: This table reports the effect of audit on repoted income of difference sources. Standard errors clusted at the individual filer level are repoted in parentheses below the point estimates.

Table A.5. Effect of auditing on taxable income, over time, interacted with adjustment

	Taxable Income
1 Year Post Audit	756.181***
	(224.417)
2 Years Post Audit	857.894***
	(243.089)
3 Years Post Audit	950.409***
	(296.544)
4 Years Post Audit	777.211**
	(392.339)
5 Years Post Audit	82.709
	(515.877)
6 Years Post Audit	-60.433
	(608.932)
1 Year Interaction Effect	0.015
	(0.012)
2 Year Interaction Effect	0.032*
	(0.019)
3 Year Interaction Effect	0.025*
	(0.014)
4 Year Interaction Effect	0.010
	(0.012)
5 Year Interaction Effect	-0.062***
	(0.023)
6 Year Interaction Effect	-0.032
	(0.027)
Individual FE	yes
Year of Tax Return	yes
Constant	39221.401***
	(151.701)
R-squared	0.009
N	3840937

Notes: This table reports the regression of the duration since the last audit on repoted income of difference sources. The key explainatory variables are a series of dummy variables 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.

Table A.6. Effect of auditing on different sources of income, interacted with adjustmen

	Total Income	Wage Income	Schedule C	Schedule D	Schedule E
Post-Audit	952.926***	350.290*	608.727***	-21.377	922.992***
	(211.371)	(210.487)	(125.176)	(95.496)	(258.054)
Interaction Effect	0.013	0.149	0.018***	0.000	0.007
	(0.011)	(0.101)	(0.006)	(0.003)	(0.006)
Individual FE	yes	yes	yes	yes	yes
Year of Tax Return	yes	yes	yes	yes	yes
Constant	50411.793***	41058.868***	5906.057***	5728.971***	8202.260***
	(152.363)	(129.027)	(72.701)	(94.034)	(129.266)
R-squared	0.008	0.010	0.008	0.040	0.006
N	4763014	3996502	849576	956067	695152

Notes: This table reports the effect of audit on repoted income of difference sources. Standard errors clusted at the individual filer level are repoted in parentheses below the point estimates.

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Table A.7. Effect of auditing on different sources of income, over time, interacted with adju

	Total Income	Wage Income	Schedule C	Schedule D	Schedule E
1 Year Post Audit	588.610***	333.150*	1027.837***	-231.415	1014.659***
	(203.503)	(182.106)	(129.855)	(146.564)	(275.661)
2 Years Post Audit	979.240***	446.469**	862.803***	-91.207	1466.875***
	(223.897)	(220.504)	(147.338)	(118.308)	(304.030)
3 Years Post Audit	1230.805***	505.236*	349.648**	136.194	817.225***
	(278.703)	(262.304)	(155.570)	(131.780)	(311.491)
4 Years Post Audit	1298.205***	76.882	17.570	283.897	309.248
	(368.899)	(356.078)	(197.873)	(178.518)	(416.615)
5 Years Post Audit	896.901*	-50.709	-713.581***	137.482	-373.028
	(502.891)	(505.334)	(263.002)	(213.930)	(518.446)
6 Years Post Audit	1272.894**	-11.256	-759.072***	138.237	-261.461
	(592.252)	(588.419)	(287.500)	(249.897)	(584.379)
1 Year Interaction Effect	0.008	-0.026	0.026***	-0.001	-0.002
	(0.011)	(0.105)	(0.006)	(0.002)	(0.007)
2 Year Interaction Effect	0.024*	0.075	0.030***	0.011	0.012**
	(0.014)	(0.121)	(0.007)	(0.010)	(0.006)
3 Year Interaction Effect	0.020*	0.258**	0.021***	-0.001	0.012*
	(0.012)	(0.110)	(0.006)	(0.002)	(0.007)
4 Year Interaction Effect	0.025*	0.413***	-0.002	0.001	0.021*
	(0.013)	(0.150)	(0.007)	(0.002)	(0.012)
5 Year Interaction Effect	0.016	0.163	-0.002	0.013	-0.014
	(0.021)	(0.149)	(0.008)	(0.017)	(0.009)
6 Year Interaction Effect	-0.040***	0.339**	0.002	-0.008	-0.011
	(0.010)	(0.157)	(0.008)	(0.021)	(0.011)
Individual FE	yes	yes	yes	yes	yes
Year of Tax Return	yes	yes	yes	yes	yes
Constant	50412.465***	41058.120***	5904.151***	5729.096***	8201.892***
	(152.291)	(128.913)	(72.677)	(94.025)	(129.273)
R-squared	0.009	0.010	0.009	0.041	0.006
N	4763014	3996502	849576	956067	695152

Notes: This table reports the regression of the duration since the last audit on repoted income of difference sources. The key explainatory variables are a series of dummy variables 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.

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Table A.8. Effect of audit on probability of claiming different income sources

	Schedule C			Schedule E			Wage Income		
	All Filers	Sch C Filers	Non-Sch C Filers	All Filers	Sch E Filers	Non-Sch E Filers	All Filers	Filers With Wages	Filers Without Wages
1 Year Post Audit	-0.003	-0.025	0.002	0.001	-0.001	0.002	0.001	-0.001	0.002
	(0.002)	(0.006)	(0.002)	(0.001)	(0.006)	(0.001)	(0.002)	(0.002)	(0.007)
2 Year Post Audit	-0.007	-0.109	0.013	0.003	-0.043	0.01	0.002	-0.007	0.046
	(0.002)	(0.007)	(0.002)	(0.002)	(0.006)	(0.002)	(0.002)	(0.002)	(0.007)
3 Year Post Audit	-0.006	-0.134	0.019	0.004	-0.056	0.014	0.003	-0.011	0.072
	(0.002)	(0.007)	(0.003)	(0.002)	(0.007)	(0.002)	(0.002)	(0.002)	(0.008)
4 Year Post Audit	-0.007	-0.153	0.022	0.002	-0.088	0.016	0.004	-0.01	0.081
	(0.003)	(0.009)	(0.003)	(0.002)	(0.009)	(0.002)	(0.003)	(0.003)	(0.010)
5 Year Post Audit	-0.01	-0.201	0.027	0.005	-0.139	0.027	0.007	-0.015	0.125
	(0.004)	A	(0.005)	(0.003)	(0.012)	(0.004)	(0.004)	(0.004)	(0.015)
6 Year Post Audit	-0.01	-0.182	0.025	0.001	-0.139	0.023	0.005	-0.015	0.12
	(0.005)	(0.013)	(0.005)	(0.004)	(0.013)	(0.004)	(0.004)	(0.004)	(0.016)
Individual FE	yes	yes	yes	yes	yes	yes	yes	yes	yes
Year of Tax Return	yes	yes	yes	yes	yes	yes	yes	yes	yes
Constant	0.133	0.445	0.077	0.104	0.549	0.044	0.897	0.969	0.487
	(0.001)	(0.003)	(0.001)	(0.001)	(0.003)	(0.001)	(0.001)	(0.001)	(0.004)
R-squared	0.002	0.115	0.007	0.005	0.120	0.007	0.021	0.014	0.171
N	4,771,427	849,576	3,921,851	4,771,427	695,152	4,076,275	4,771,427	3,996,502	774,925

Notes: This table reports effect of the duration since the last audit on the extensive margin, using fixed effects linear probability models. The first three columns consider the effect of audit on the reporting of any Schedule C income separtely for the full sample, those who claimed Schedule C income in the audit year, and those who did not claim Schedule C income in the audit year. The next three columns report the analogous results for Schedule E income and the final three columns report the same for wage income. Standard errors clustered at the individual filer level are reported in parentheses below the point estimates.

Table A.9. Effect of audit on reported taxable income by age

	25-34	35-44	45-54	55-64
1 Year Post Audit	660.877	826.844	1248.041	957.229
	(357.188)	(385.022)	(388.656)	(499.963)
2 Year Post Audit	1065.106	1806.904	1985.568	1522.915
	(433.036)	(452.523)	(416.199)	(622.674)
3 Year Post Audit	1044.372	1765.737	1694.176	2082.874
	(575.795)	(548.342)	(507.696)	(728.791)
4 Year Post Audit	791.317	1628.196	1445.681	678.218
	(838.000)	(713.112)	(662.705)	(833.229)
5 Year Post Audit	1049.906	-53.485	581.540	-716.594
	(1141.905)	(915.059)	(883.505)	(1135.599)
6 Year Post Audit	1578.192	-148.546	-349.598	520.382
	(1379.439)	(1111.340)	(931.124)	(1424.121)
Individual FE	yes	yes	yes	yes
Year of Tax Return	yes	yes	yes	yes
Constant	9103.537	33009.715	45421.386	55325.951
	(194.875)	(286.403)	(272.908)	(330.991)
R-squared	0.113	0.022	0.004	0.012
N	891,240	914,464	985,524	756,908

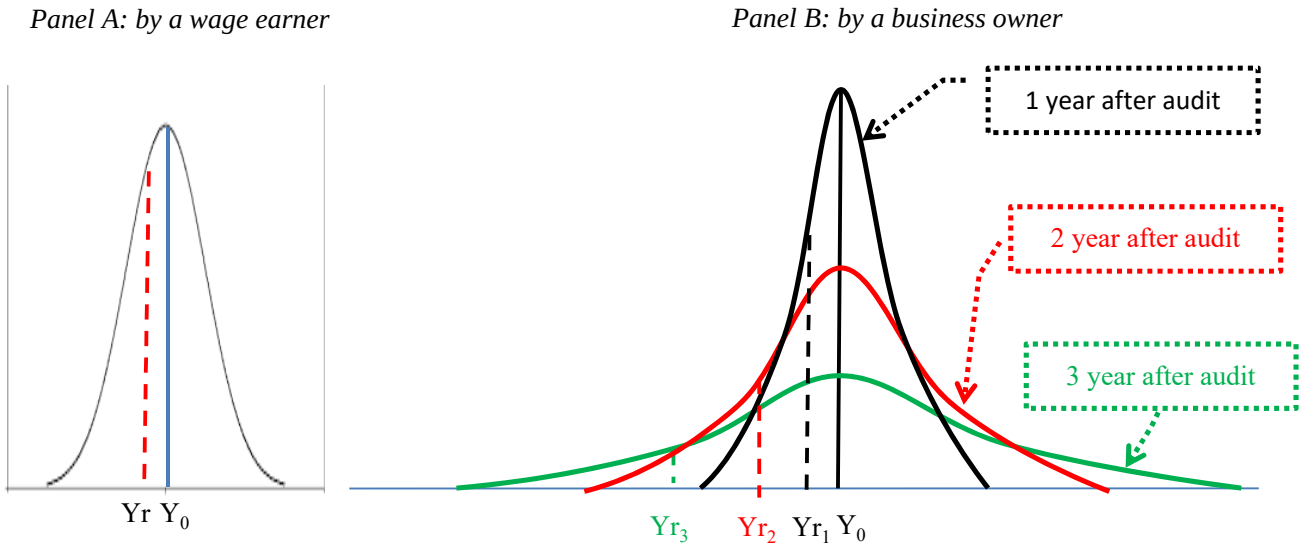
Notes: This table reports the regression of the duration since the last audit on repoted taxable income by the age of the primary filer in the year of the audit. Standard errors clusted at the individual filer level are repoted in parentheses below the point estimates.

Table A.10. Effect of audit on reported taxable by use of paid preparer

	Paid Preparer	
	Used	Did not Use
1 Year Post Audit	764.086	557.632
	(245.363)	(283.549)
2 Year Post Audit	1180.291	1224.826
	(260.859)	(313.797)
3 Year Post Audit	1613.843	1146.351
	(332.936)	(360.037)
4 Year Post Audit	1247.256	1859.363
	(400.332)	(557.415)
5 Year Post Audit	A	2044.845
	(539.482)	(716.934)
6 Year Post Audit	635.691	2028.902
	(628.964)	(873.370)
Individual FE	yes	yes
Year of Tax Return	yes	yes
Constant	37360.797	28978.283
	(182.733)	(202.110)
R-squared	0.003	0.016
N	2,949,779	1,821,648

Notes: This table reports the regression of the duration since the last audit on repoted taxable income by the use of a paid preparer in the year of the audit. Standard errors clusted at the individual filer level are repoted in parentheses below the point estimates.

Figure 1: Long-term effect of an audit on underreporting income



Note: This figure shows the conditional income distributions of a tax payer as perceived by the auditor. Panel A is for an income source that has a low variance; Panel B is with a high variance. In Panel B, 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 also expect that this effect declines more quickly than the effect on sources of income with more third-party reporting and less variance or volatility.

Figure 2: Effects of Audit by Income Source

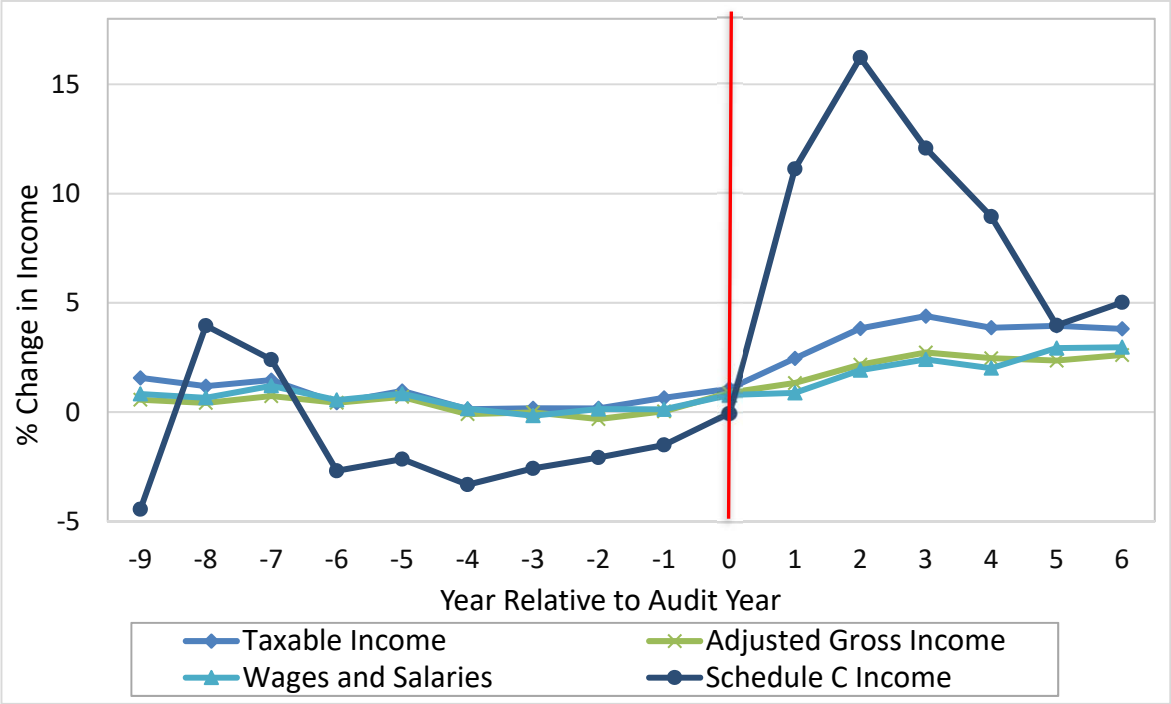


Figure 3: Effects of Audit on Taxable Income

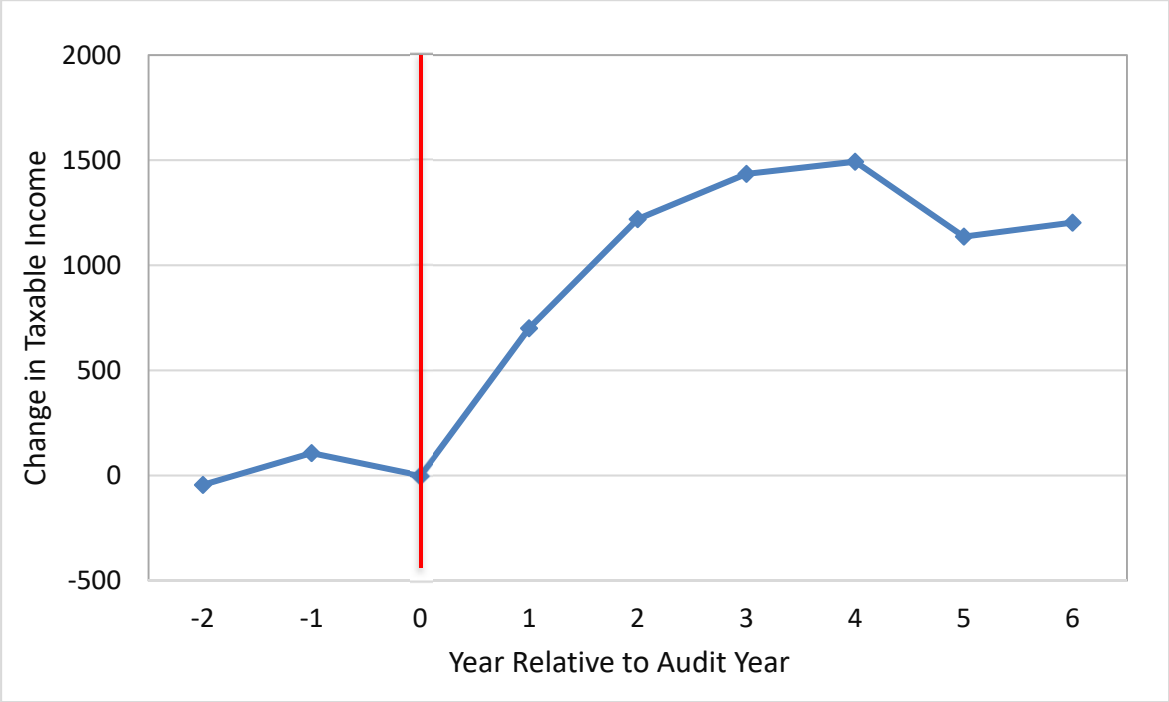


Figure 4: Effects of Audit on Taxable Income by Audit Experience

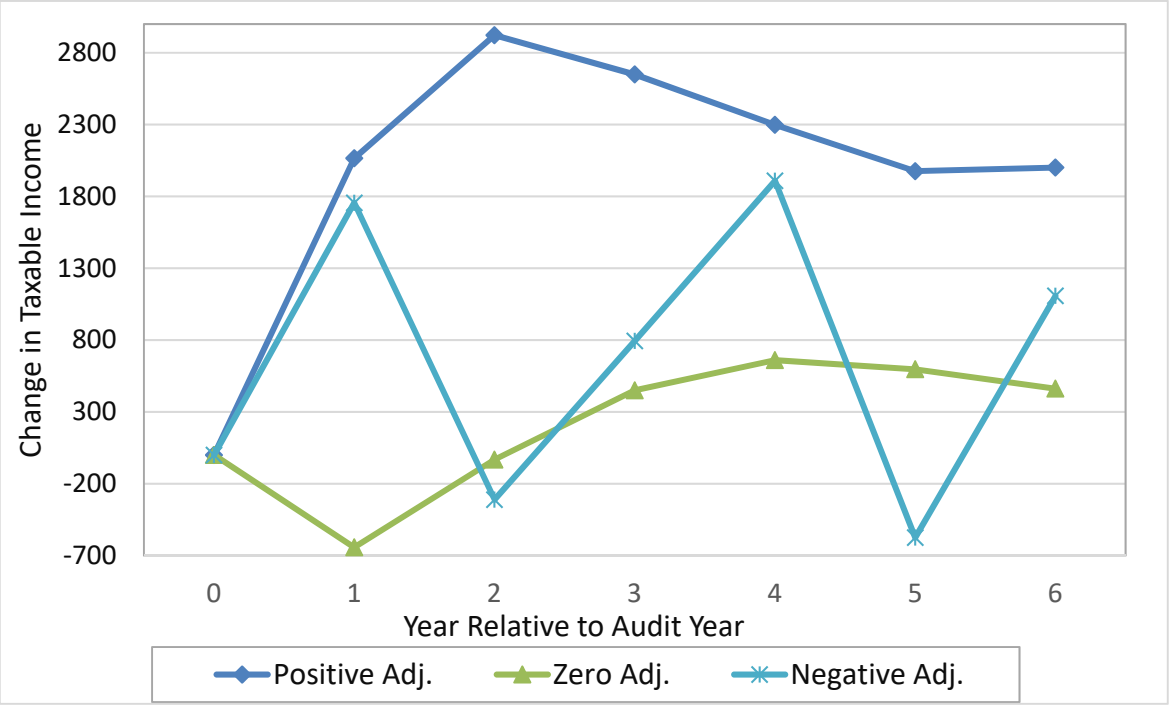


Figure 5: Effects of Audit by Income Source

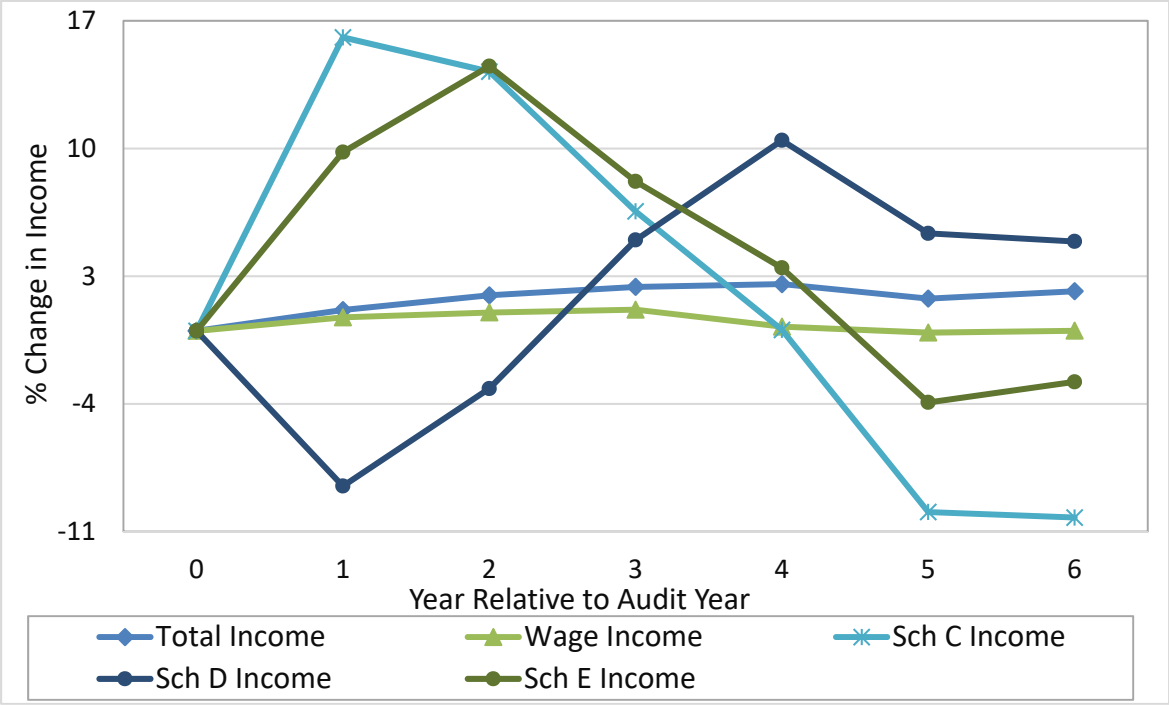
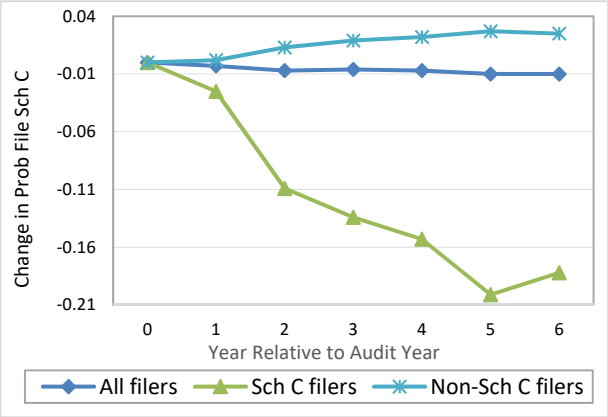
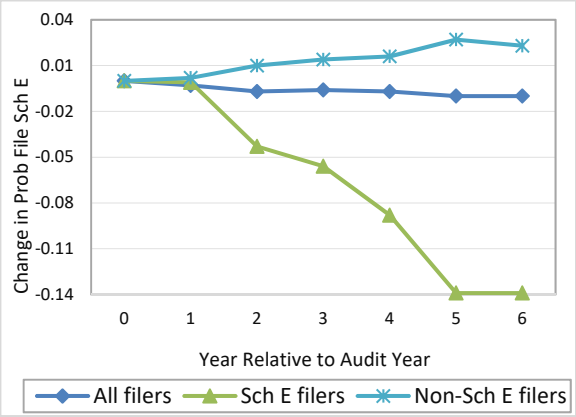


Figure 6: Effect of Audit by Filer Type, Extensive Margin

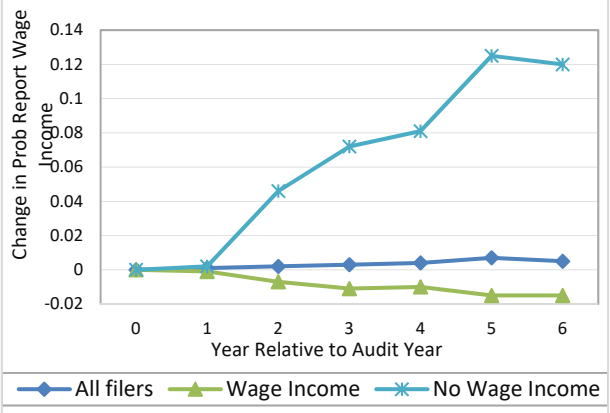
(a) Likelihood of Filing Schedule C



(b) Likelihood of Filing Schedule E



(c) Likelihood of Reporting Wage Income



(d) Likelihood of Filing Schedule E

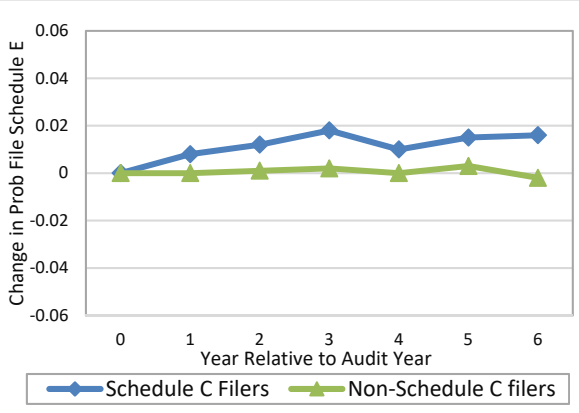


Figure 7: Effects of Audit on Deduction Amounts by Deduction Type

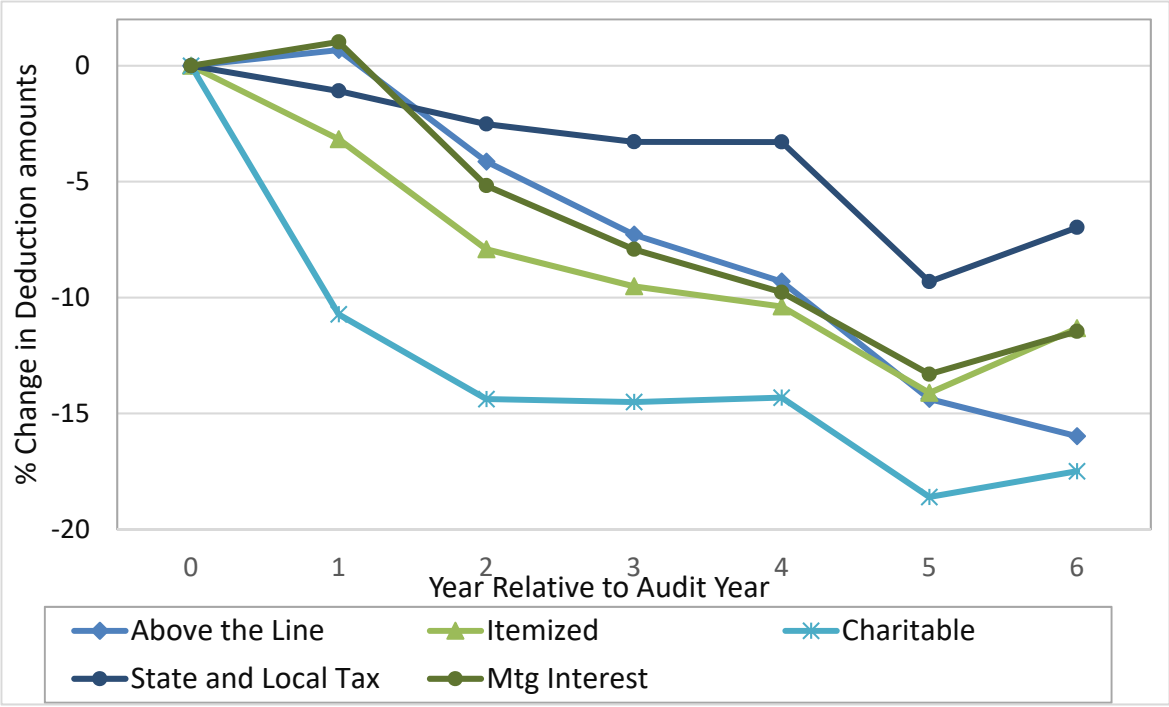


Figure 8: Effects of Audit on Taxable Income by Filer Age

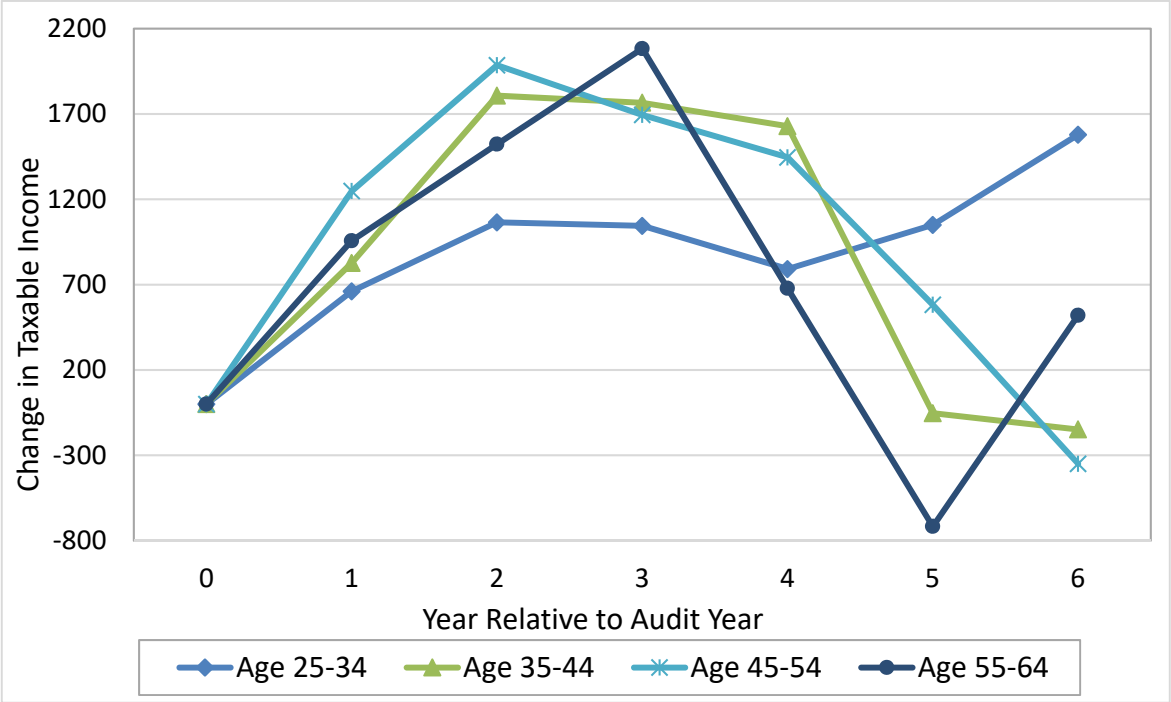


Figure 9: Fillers Who Used Paid Preparers vs. Others

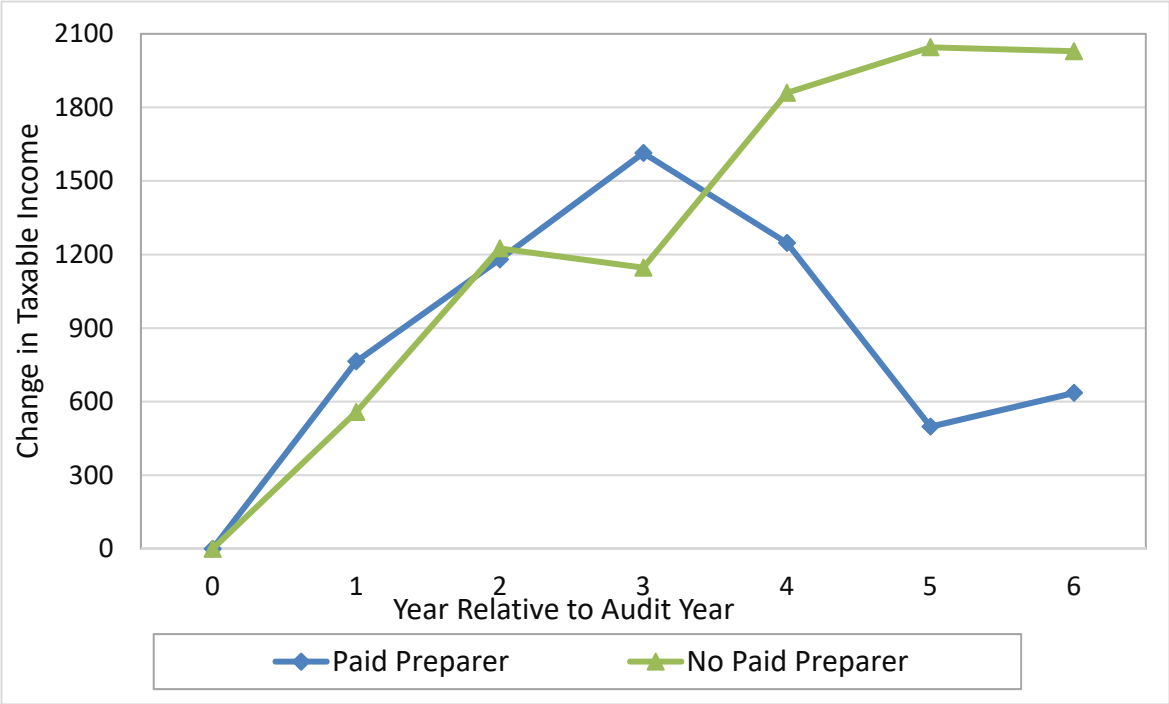
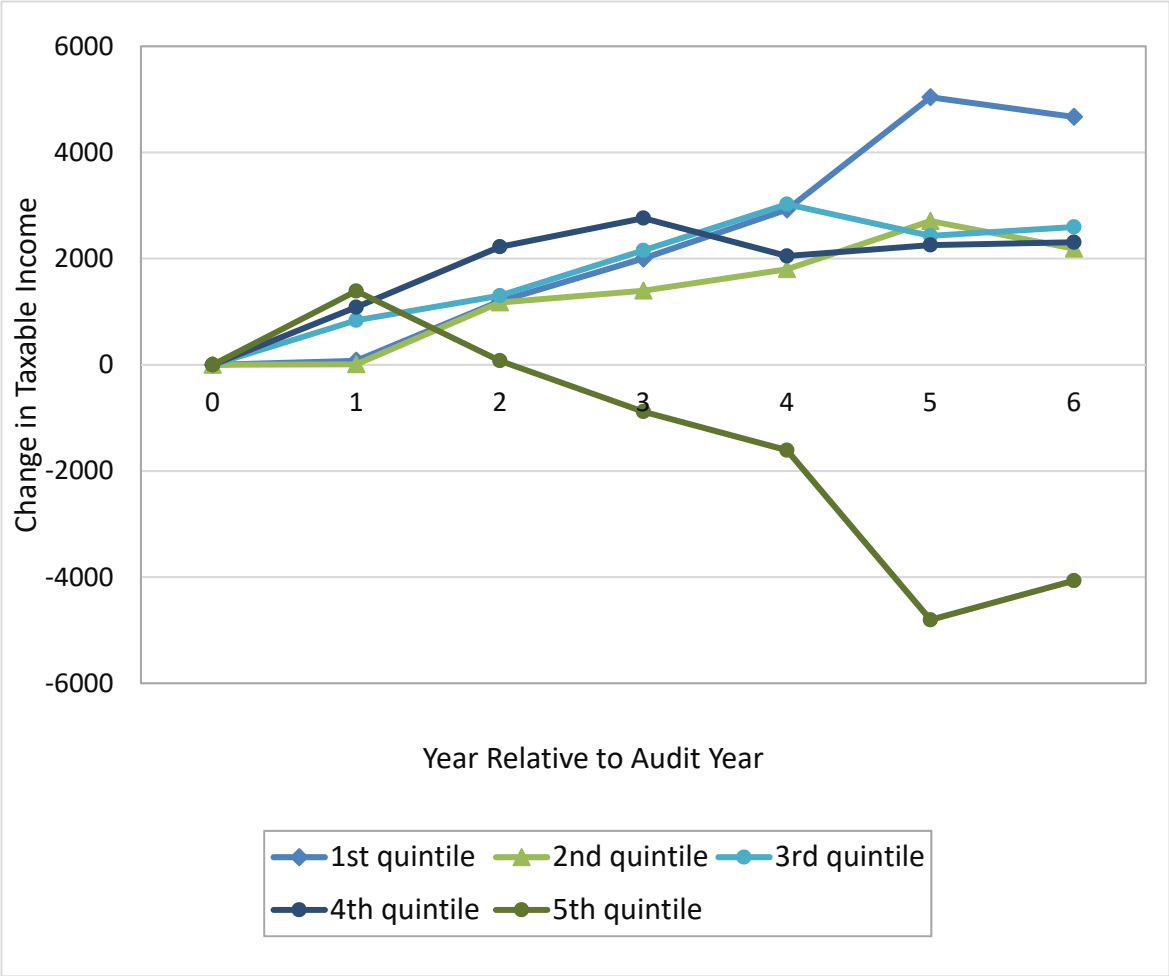


Figure 10: Income quintiles



APPENDIX

Figure A.1: Filers Who Used Paid Preparers vs. Others

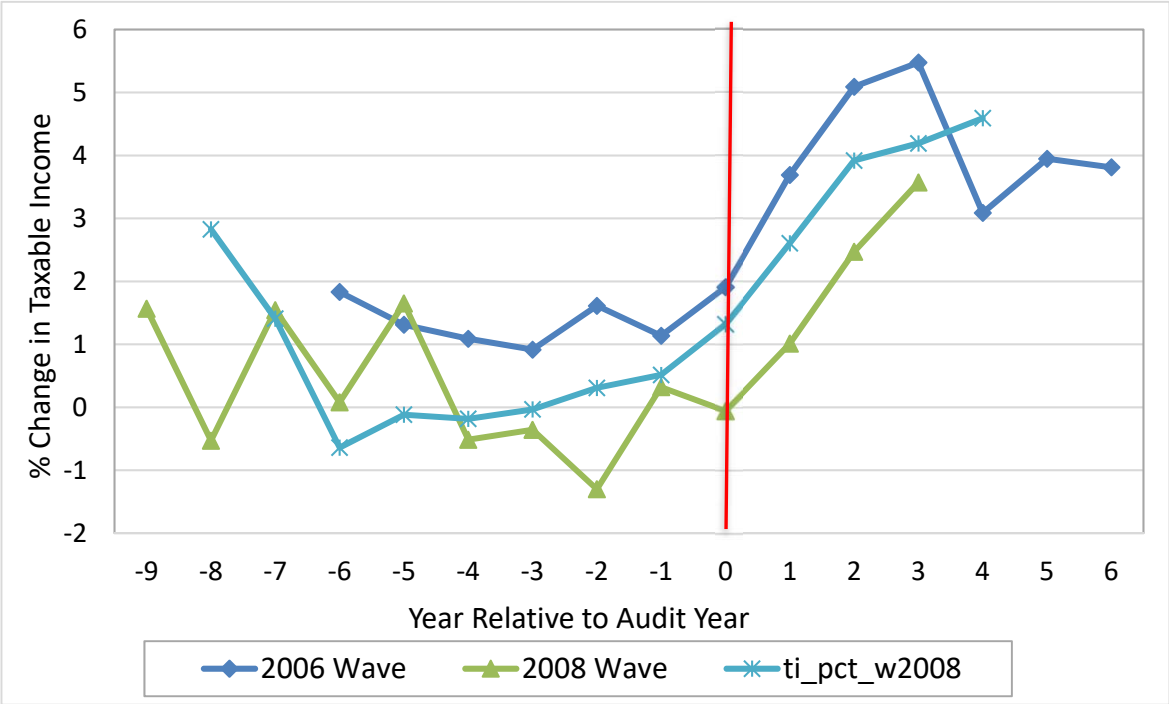


Figure A.2: Filers Who Used Paid Preparers vs. Others