Comment: Tax Evasion at the Top of the Income Distribution: Theory and Evidence

August 15, 2021

Gerald Auten, Office of Tax Analysis, U.S. Treasury Department
David Splinter, Joint Committee on Taxation, U.S. Congress

Abstract

Income underreporting is difficult to account for when measuring the distribution of income. Prior research has relied on special comprehensive IRS National Reporting Program (NRP) audit studies. Guyton, Langetieg, Reck, Risch, and Zucman (2021) bring attention to limitations of the NRP audits for detecting income from offshore assets and passthrough businesses. While underreporting is a serious issue, there are problems with the specific assumptions for allocating this income, such as inappropriate use of simple multipliers to account for undetected amounts and allocating misreported business income in proportion to reported business income. This comment examines methodological issues that potentially bias the results and suggests improvements that would be distributionally consistent. While uncertainty necessarily remains, there may be less additional underreporting and improved approaches suggest that underreported income may be less concentrated at the top. (*JEL* D31, H22, H26)

I. Background on Underreported Income

Guyton, Langetieg, Reck, Risch, and Zucman (2021, hereafter GLRRZ) argue there is substantially more underreporting of income than previously estimated and that it is found almost entirely in the top 1% of their "benchmark" distribution of income. GLRRZ make an important contribution by bringing attention to the issue that the IRS National Reporting Program (NRP) has limitations when detecting income from offshore assets and passthrough businesses. In this comment, we analyze GLRRZ's assumptions and allocation methods and suggest needed improvements. For example, estimates of unreported income from offshore wealth fail to account for recent offshore enforcement efforts. In addition, the distribution of underreporting is based on simple detection-controlled estimation (DCE) multipliers that are inappropriate for distributional analysis.[1] In some cases, we provide examples of alternative methods that conform with NRP estimates and are distributionally consistent when applied to micro data. Our analysis suggests that distributionally consistent methods would show that underreported income is less concentrated at the top of the distribution.

This paper comments on Guyton, Langetieg, Reck, Risch, and Zucman (March 2021, ungated link here). We recognize that GLRRZ is a work in progress and the authors plan to address some of our concerns, but given the attention this version has received, we are sharing our concerns on the current publicly available version. Reck, Risch, and Zucman (2021) responded to an earlier version of this comment. For helpful comments and discussions, we thank Tom Barthold, Brian Erard, John Guyton, Janet Holtzblatt, Wojciech Kopczuk, Patrick Langetieg, Emily Lin, Jamie McGuire, Jacob Mortenson, Daniel Reck, Matthew Smith, Steve Rosenthal, Alexander Yuskavage, Eric Zwick, and participants of the Tax Economists Forum. Auten: Views and opinions expressed are those of the authors and do not necessarily represent official Treasury positions or policy. Splinter: This paper embodies work undertaken for the staff of the Joint Committee on Taxation, but as members of both parties and both houses of Congress comprise the Joint Committee on Taxation, this work should not be construed to represent the position of any member of the Committee.

[1] A list of suggested methodological improvements is provided in Appendix C.

While tax data provide a valuable resource for estimating the distribution of income, an important limitation is that they suffer from the incentive to underreport income to avoid taxes. Fortunately, special IRS audit studies in the NRP shed light on the extent of such underreporting and the types of income where underreporting is most prevalent. These are not operational audits that typically focus on only a few identified issues, but comprehensive stratified random audits that theoretically examine all items on tax returns and oversample returns with high incomes or low-visibility business income. These special audit studies in the NRP have been the main source of estimates of underreported income and the tax gap, as well as providing the basis for the amounts included in national accounts.

GLRRZ use the 2006–2013 NRP studies in estimating both the total amount of underreported individual income and the effects of including this income on top income shares.[2] GLRRZ use the term "evasion," which implies the criminal offense of intentionally failing to report income. But the NRP data on which their paper is based includes all misreporting of income—including accidental errors, lack of adequate documentation, and issues where the application of rules are uncertain or in dispute. In this paper, we generally use the broader and more accurate terms *underreporting* and *misreporting* (i.e., underreporting less overreporting).

GLRRZ's analysis has four steps. First, it uses NRP audit micro data to add *detected* misreporting. Second, it imputes *undetected* underreporting using four simple DCE multipliers that scale up detected underreporting. While based on a sophisticated method, DCE multipliers were only designed for estimating total underreporting and give distorted results for distributional analysis. Third, GLRRZ adds imputations of additional entity-level underreporting from passthrough entities defined as partnerships, S corporations, estate and trusts and half of rental income on Schedule E. After subtracting estimated entity-level detected and undetected underreporting in the NRP, an assumed amount of total entity-level underreporting is allocated by reported income, such that almost all goes to the top of the distribution. Fourth, GLRRZ accounts for unreported income from offshore wealth. The assumed distribution and total unreported offshore income are based on Alstadsaeter, Johannesen, and Zucman (2018) and extend Langetieg, Reck, and Risch's prior work in Johannesen et al. (2020).

Combining the four components of estimated underreporting, GLRRZ argue that almost all of the underreported income not detected by the NRP represents evasion by the top 1% of "true" income. Including this income increases top 1% fiscal income shares by 1.5 percentage points (pp). This includes a 0.5 pp *decrease* from detected misreporting, 1.1 pp increase from undetected underreporting using simple DCE multipliers, 0.6 pp increase from additional passthrough business underreporting, and 0.3 pp increase from unreported income from offshore wealth.[3]

To account for undetected underreporting, GLRRZ multiply detected underreporting by the simple DCE multipliers. This adds an annual average of $974 billion over the 2006–2013 period. However, this approach causes exaggerated upward re-ranking of returns when applied to large amounts of detected underreporting, shifting underreporting from the bottom and middle to the top

[2] The first NRP study included a larger sample, but only covered tax year 2001. Previous comprehensive audit studies were under the Taxpayer Compliance Measurement Program (TCMP), last conducted in 1988. Brown and Johns (2007) compare the TCMP and NRP studies. The tax gap is the differences between estimated tax liabilities and the amount of taxes paid voluntarily and on time.

[3] GLRRZ do not break out passthrough and offshore effects in their Table 1 results but explain on page 39: "In the case where there is zero offshore evasion (only passthrough business income evasion), the top 1% income share rises by more than 0.6 points." And on pg. 29: "the pass-through adjustment…is about twice as large as the offshore adjustment."

of the income distribution. The use of simple DCE multipliers accounts for two-thirds of the GLRRZ increase in top 1% income shares.

Additional passthrough business underreporting is the second largest estimated amount of underreported income and increase in top shares. GLRRZ allocate this underreporting by reported income on individual tax returns. However, returns with business losses appear to be allocated little additional income despite accounting for over one third of detected misreporting (Auten and Langetieg, 2020). While offshore evasion has received considerable attention, it accounts for only a small portion of total added unreported income and effect on top income shares. This is not to say that overall underreporting is low or that it is not greater than that discovered in the NRP audit studies. Instead, the concern is that the GLRRZ assumptions likely overstate the top one percent's share of underreporting and may overestimate total underreporting.

Section II of this comment discusses the inappropriate use of simple DCE multipliers, the largest single methodological issue in GLRRZ. These multipliers were designed to estimate total misreported income and taxes but are inappropriate for application at the micro level. The DCE methodology is based on the idea that undetected underreporting can be estimated by the gap between auditors finding the least and most underreporting. These gaps are then used to compute average multipliers that scale up detected amounts to equal estimated total underreporting. The GLRRZ approach, however, applies the same multipliers to returns where large amounts of underreported income were discovered by the best auditors as to the small amounts found by less-skilled auditors, thereby misallocating the underreported income. When used to estimate income distributions, this tends to produce an upward bias in top shares.

Other researchers have previously noted that application of the simple DCE multipliers to micro data is not distributionally consistent, including Johns and Slemrod (2010), Bloomquist et al. (2012), and DeBacker et al. (2020). We suggest alternative approaches that are distributionally consistent. Relative to estimates using simple DCE multipliers, these approaches indicate that high-income underreporting rates are lower and that top income shares are reduced.[4]

Section III discusses the second largest issue: the estimation and allocation of additional passthrough underreporting. The GLRRZ "benchmark" approach allocates nearly all additional passthrough underreporting to the top 1%. We present evidence suggesting there is less entity-level passthrough underreporting than assumed in GLRRZ. Rather than relying on assumed amounts and distributions of underreporting, the paper's analysis should be based on (or supported by) the NRP and other IRS data and special studies.

Section IV discusses additional concerns. Most importantly, estimates of the amount of unreported income from offshore wealth fail to properly account for recent offshore enforcement efforts that increased disclosure rates. Other issues include failing to adequately account for overstated business losses and income reported on the wrong line with little or no net underreporting. In addition, we discuss concerns about some of the analysis in the Reck, Risch and Zucman (2021) response to an earlier version of this comment. Section V discusses implications of research on underreporting. Appendix C provides a summary of our suggestions for improving the GLRRZ methodology.

[4] These approaches lower top one percent underreporting rates by about half. This uses the gradient and level method described in Table 3 to allocate detected misreporting but with misreporting-to-reported-income ratio maximums set at the average of each income/ratio group. Also accounting for passthrough and offshore underreporting not in the NRP audits using GLRRZ's approach, top one percent underreporting rates in 2010 are estimated to be no more than 14%. This is two-thirds the GLRRZ estimate and close to the average underreporting rate across all income groups.

While the NRP audits are theoretically comprehensive, GLRRZ discusses types of underreporting that are likely missing. Unfortunately, GLRRZ makes clear that they are only interested in underreporting among high-income returns. This narrow focus disregards additional underreporting outside the top of the distribution. First, GLRRZ excludes non-filers from their analysis.[5] Second, GLRRZ allocates nearly all business-level passthrough underreporting to the top of the *reported* income distribution and suggests that only high-income individuals can engage in "sophisticated" evasion. However, successful underreporting moves individuals down the reported income distribution, where they may be less likely to be audited. Furthermore, undetected underreporting does not require sophisticated behavior—one can be paid in cash or in-kind for services provided, leaving no paper trail. GLRRZ argues that third-party reporting of income (e.g., Forms W-2 and 1099-MISC) should attenuate underreporting outside the top of the distribution. But third-party reporting cannot prevent all underreporting. For example, additional amounts of third-party reported income can be offset by claiming additional expenses or shifting them across time (Slemrod et al., 2017). Therefore, a note of caution seems appropriate: Looking for underreporting only in the top of the distribution, one will find it there. But looking for underreporting in the bottom and middle, one will also find it there.

II. DCE Multipliers: Inconsistent Allocation to Specific Returns & Exceed National Accounts

This section discusses issues with the DCE multipliers used by GLRRZ and presents estimates using alternative multipliers. The DCE method was originally developed to account for undetected underreporting using audit study data (Feinstein, 1990, 1991). While the underlying analysis uses a sophisticated procedure to assign total underreporting rates (detected and undetected) to each auditor based on detection rates of the best auditors, GLRRZ uses a simplified method with only four DCE multipliers that are applied regardless of auditor skill and success. The multipliers differ for each low/high reported income group and low/high visibility source of income. For example, wages are high visibility due to reporting on Form W-2 and sole proprietor income is low visibility.[6] The four simple multipliers are:

For returns with Total Positive Income < $100,000 and no Schedule C or F income:
Low-visibility income: 4.158 High-visibility income: 2.009

For returns with Total Positive Income >=$100,000 or with Schedule C or F income
Low-visibility income: 3.358 High-visibility income: 2.340

These simplified DCE multipliers were developed to estimate aggregate amounts of misreporting and are inappropriate for imputations at the tax-return level. This is because applying these simplified DCE multipliers at the return level loses the effect of varying auditor skill that is the basis for DCE analysis. As a result, this uniform scaling up of detected underreporting produces distributionally inconsistent results.[7] A second issue is that DCE multipliers can result in more total

[5] Erard and Ho (2003) estimates fill several gaps when only considering underreporting in the individual tax return audit studies. They augment the base data with special studies of non-filers, tip income, and informal suppliers. For a discussion of the tax gap and the informal economy, see Joint Committee on Taxation (2019).

[6] See Appendix A for a detailed discussion of the DCE approach used by Johns and Slemrod (2010) and GLRRZ.

[7] The IRS economists who use the NRP to estimate the official tax gap express the same concern, saying the simple multiplier method "was still primarily an aggregate approach." (Bloomquist et al., 2012, pg. 71) Current official tax gap estimates use a new methodology with parameters based on differences in auditor characteristics to estimate return-level undetected underreporting.

misreporting than in national accounts. Both issues can be exacerbated by line switching when an amount is added on the wrong line of a form and missing from another line. Thus, there are equal amounts of underreporting and overreporting. Line switches (or separately considering each line of tax returns) can exaggerate underreporting at the top of the distribution and overstate total underreporting. This is because DCE multipliers are only applied to underreporting. Note that GLRRZ do not control for line switching and apply multipliers on a line-by-line basis. Instead, line switching should be cancelled out or multipliers applied to total net misreporting (underreporting less overreporting across all lines of a tax return).[8]

An additional issue is that the simple multipliers are based on 2001 audits and may be outdated due to more recent developments. Since 2011, credit card and third-party network transactions (e.g., gig economy payments) are reported to the IRS with Form 1099-K and the cost basis of stocks purchased that year or later are reported on Form 1099-B. In addition, having annual NRP audits could have increased the audit efficiency since learning can be incorporated into subsequent-year audits and there may be some accumulation of skills by continuing auditors. Such changes could have resulted in lower multipliers for more recent years.

A. DCE multipliers applied at the micro level are not consistent with underlying method

The main concern with GLRRZ estimates is that DCE multipliers are not distributionally consistent. Applying uniform DCE multipliers to all income (within each income level/visibility type) treats all detected underreporting the same even though the basis of DCE analysis is the gap between rates of detection of the best and less skilled auditors (Feinstein, 1990, 1991; Erard and Feinstein, 2011). This exaggerates the amount of underreporting allocated to specific returns, which re-ranks them up the distribution and leads to overstated top 1% income shares.

Table 1 presents a simple example of how using uniform DCE multipliers can exaggerate top income shares. For reported income, return *a* is at the top of the distribution and has 40 percent of income. Adding detected underreporting ($1 for *a* and *b* and $4 for *c*) lowers *a*'s share to 36 percent. Hence, detected underreporting decreases the top share, as observed in the NRP. Finally, adding DCE adjustments by multiplying detected amounts by three (total underreporting of $3 for *a* and *b* and $12 for *c*) re-ranks taxpayer *c* to the top of the distribution with 42 percent of income. It also increases *c*'s detected underreporting rate from 33% to a post-DCE underreporting rate of 60%. Applying simple DCE multipliers to the NRP microdata also results in higher top income shares and underreporting rates. These increases result from inappropriately applying the same multiplier to *a* and *b*'s $1 of detected underreporting and *c*'s $4 of detected underreporting. Instead, these multipliers should differ based on auditor rates of detection.

Table 1: Example of adding detected underreporting and applying DCE multipliers

	Reported			**Reported + Detected**			**Reported + DCE**		
ID	Rank	Income	Share	Rank	Income	Share	Rank	Income	Share
a	1	$12	40%	1	$13	36%	3	$15	31%
b	2	$10	33%	3	$11	31%	2	$13	27%
c	3	$8	27%	2	$12	33%	1	$20	42%
Total		$30	100%		$36	100%		$48	100%

[8] Auten and Langetieg (2020) account for line-switching and find that this significantly reduces gross amounts of underreporting relative to a line-by-line basis.

Other researchers have recognized the problem of applying the simple multipliers to micro data for distributional analysis. DeBacker et al. (pg. 1106) write: "Because the published multipliers are applied to all auditors regardless of skill level, the biggest amounts of undetected misreporting will be attributed to the audits with the largest amounts of detected misreporting. This runs counter to the intended application of the adjustments and can exaggerate the true variation in misreporting." Johns and Slemrod (2010, pg. 400) write: "The use of the DCE multipliers will understate estimates of undetected income for some taxpayers, and almost certainly will do so for…audited returns where no income underreporting was detected, because no adjustment is made in these cases. Conversely, it may overstate estimates of undetected income for other taxpayers."

Using the same multipliers for the most and least effective auditors overstates underreporting rates of the top 1% ranked by post-DCE income. Returns with substantial detected underreporting are allocated the most undetected income. Some of these returns move from below to above the top 1% threshold. For example, a return with reported income of $50K and detected underreporting of $200K is still below the top 1% threshold. A low-visibility multiplier of about 4 increases total underreporting to $800K and pushes the return well over this threshold. This increases this return's underreporting rate to over 90 percent and re-ranks it into the top 1%, likely replacing a return with little or no underreporting. This example likely corresponds to a significant number of returns in the post-DCE top 1%. In recent NRP studies, Auten and Langetieg (2020) find that about 0.2 percent of returns in the middle quintile have *detected* underreporting almost four times their reported income. Assuming the average multiplier applies, adding undetected underreporting increases post-DCE income to about 13 times the amount reported. (Table A1: 0.19% of returns and average ratio of 4.91 in 4-8 ratio column). The upward re-ranking of these and other returns when adding undetected underreporting, which is exaggerated by using the same multipliers regardless of auditor skill, explains part of GLRRZ's estimated top 1% underreporting rate increase from 1 percent for detected misreporting to 21 percent post-DCE and other adjustments.[9]

B. More consistent options for allocating undetected underreporting

Undetected underreporting should be allocated with distributionally consistent approaches. While the more sophisticated methodology used for more recent tax gap estimates would be better than the simple multipliers used in GLRRZ, improved versions of the multiplier approach could also provide more consistent estimates at the micro level. The basic idea is that if information on auditors is available, one could apply smaller multipliers to the most effective auditors and larger multipliers to less effective auditors. In the absence of each auditor's effectiveness, a reasonable assumption is that taxpayers with high ratios of detected underreported income to reported income should have lower multipliers because they likely had a more effective auditor. To match aggregate totals, taxpayers with low detection ratios should have higher than average multipliers. Table 2 shows illustrative gradient multipliers that are higher for returns with low detected under-reporting rates and lower for returns with high detections rates.[10]

[9] DeBacker et al. (2020) show that adding detected misreporting causes little re-ranking of returns into the top 1%, hence the re-ranking effects in GLRRZ appear mostly due to scaling up of detected underreporting with DCE multipliers.

[10] The example gradient multipliers decline from 8 for returns with less than 10% detected underreporting to 1.1 for the very small share of returns where underreporting is more than 700% of the originally reported income. The ratio of 1.1 only applies to the 0.4 percent of returns in the bottom income quintile with positive income or small negative total incomes. The historical average DCE multiplier is about 3.3 (Brown and Johns, 2007)

Table 2: Alternative multipliers for undetected underreporting by ratio class

Ratio Class	0.5	1-1.1	1.1-1.2	1.2-1.5	1.5-2	2-4	4-8	8+
DCE multipliers (avg.)	3.3	3.3	3.3	3.3	3.3	3.3	3.3	3.3
Gradient	5.7	8.0	6.9	5.7	4.6	3.4	2.3	1.1
Flat gradient	3.0	4.0	3.5	3.0	2.6	2.1	1.6	1.1
Steep gradient	8.4	12.0	10.2	8.4	6.6	4.7	2.9	1.1

Notes: Multipliers times detected underreporting gives an estimate of detected plus undetected underreporting. Ratio class 0.5 multipliers only apply to tax returns with negative reported AGI.

This gradient multiplier approach can make use of the analysis of NRP audit data by Auten and Langetieg (2020). This paper develops a method of allocating detected misreporting by reported income that accounts for both a small share of tax returns having high underreporting rates and large shares of returns having modest misreporting rates or no detected misreporting. Returns in each reported income group are divided into bins of the ratio of underreporting to reported income of less than 10 percent, 10–20 percent, 20–50 percent, etc. (bins for overreporting are also included, see appendix Table A1). Selecting the appropriate number of returns in each income group and ratio cell and multiplying by the associated misreporting ratio closely approximates the results of the NRP audit data and correct distributions of detected misreporting.[11] The following section extends this approach to allocate undetected evasion using gradient multipliers instead of simple DCE multipliers.

C. Apply gradient multipliers and compare to GLRRZ estimates

To evaluate the GLRRZ estimates and the effect of gradient multipliers, this section extends the approach of Auten and Langetieg (2020) to allocating undetected income. As shown in the appendix, this approach replicates the estimates of Johns and Slemrod (2010). Table 3 shows the effect of adding misreported income on income shares. These estimates use 2010 representative tax return data (results are similar for nearby years) and GLRRZ annual average total misreported amounts indexed to 2010 dollars. As in the more recent tax gap estimates, the analysis in Table 3 is based on the averages of ten simulations (Bloomquist et al., 2012).

The first step is to replicate the GLRRZ results from adding detected misreporting (Table 3, Panel A). Adding detected underreporting decreases top 1% income shares by 0.5 percentage points (pp), the same as in GLRRZ. Applying simple DCE multipliers decreases the top 1% share by only 0.1 pp relative to reported income, which is close to the Johns and Slemrod (2010) estimate of no change. In contrast, GLRRZ report a large increase in the top 1% income share when applying simple DCE multipliers as discussed below.

[11] When applying the approach to add detected underreporting to SOI cross-section or other non-NRP data, tax returns are assigned to ratio cells using a random number approach. This approach resembles that used in the recent official tax gap estimates. IRS (2019, pg. 18) states: "In order to simulate a realistic distribution of undetected income consistent with the predicted incidence of undetected income, a simulation process randomly allocates undetected income…by assigning a random number to each return and then assigning undetected income to that return if the random number was less than the probability of undetected income for that return."

Using gradient multipliers suggests that including total misreporting *decreases* the top 1% income share by 1.1 pp (Table 3, Panel B).[12] As discussed in more detail in the following section, applying the simple DCE multipliers leads to underreporting that exceeds amounts in the national accounts. When total estimated undetected underreporting is scaled to approximate the total amount of underreporting in the national accounts (about $340 billion less than in GLRRZ), the top 1% share decreases by 0.9 pp, slightly less than without scaling. The use of flatter and steeper gradients leaves the results essentially unchanged.

Table 3: Income Shares for Different Allocations of Undetected Underreporting, 2010 tax returns

	Income ($billions)				Income Shares (%)			Top 1% chg. from reported (pp)
	Total	P0-50	P50-99	Top 1%	P0-50	P50-99	Top 1%	
Panel A: Replicate GLRRZ approach								
reported income	7.9	0.8	5.5	1.6	10.1	70.1	19.9	---
after exam, no DCE	8.2	0.9	5.7	1.6	10.8	69.8	19.4	–0.5
after exam, with DCE	9.1	1.0	6.3	1.8	11.2	69.0	19.8	–0.1
Panel B: Distributionally consistent alternatives to simple DCE multipliers								
gradient	9.1	1.1	6.3	1.7	11.6	69.6	18.8	–1.1
gradient, NIPA	8.8	1.0	6.1	1.7	11.5	69.6	18.9	–0.9
flatter gradient, NIPA	8.8	1.0	6.1	1.7	11.5	69.6	18.9	–0.9
steeper gradient, NIPA	8.8	1.0	6.1	1.7	11.5	69.6	18.9	–0.9

Notes: Income is "fiscal" income on tax returns, defined as total income minus Social Security benefits, unemployment insurance benefits, alimony, and state refunds. Income after exam includes both underreported income and small amounts of overreported income. Amounts in 2010 dollars. The NIPA estimates scale underreporting amounts to match estimated totals in national income. Averages of ten simulations are shown.
Sources: Authors' calculations using 2010 INSOLE file and tables from Auten and Langetieg (2020).

There are significant differences between the results of GLRRZ and Johns and Slemrod (2010, JS). Relative to reported income, top 1% income shares after DCE do not change in JS but increase 0.6 pp in GLRRZ. In addition, the income share of the bottom decile decreases by 0.3 pp in JS but is unchanged in GLRRZ. Whereas JS remove about one fifth of observations due to data issues such as line switching, GLRRZ make no mention of addressing these issues. This may explain some of the difference because GLRRZ apply DCE multipliers to the detected underreporting of each line of a tax return, even if different lines offset one another. This line switching issue is why Bennett (2005, pg. 13) warns that "looking at individual line item results may not always give a clear picture of reporting accuracy." In contrast, the estimates used in Table 3 are for total misreporting across all lines and therefore not affected by line switching.

[12] Adding undetected underreporting more than triples the detected amounts. Therefore, we may expect gradient multipliers to result in a decrease of about 1.5 pp, but upward re-ranking of tax returns with undetected underreporting offsets a portion of this expected decrease. Note that the re-ranking effect from gradient multipliers is much less than from the distributionally inconsistent DCE multipliers (see Figure A1 in the appendix).

D. DCE multipliers in GLRRZ imply more misreporting than in national income

Another indication that simple DCE multipliers overstate total net underreporting is that GLRRZ misreporting appears to exceed the amount in national income by about one third. National income includes estimated underreporting based on NRP studies, including both detected and undetected amounts. But these amounts are far less than the total detected and DCE undetected amounts in GLRRZ.

National accounts explicitly break out proprietor misreporting (combined amount for sole proprietors and partnerships) of $561 billion and wage misreporting of $75 billion when averaging 2006–2013 (at 2012 dollars). The gaps between amounts in national accounts and reported on tax returns are about $44 billion for farms, $43 billion for rental income, and $80 billion for S corporations (Auten and Splinter 2019, online data Table T1). To compare with GLRRZ, amounts for dividends and interest misreporting should be added and non-filer portions should be removed from the values above. Under the assumption the former is $50 billion and the latter is 10 percent, this implies about $770 billion in filer underreporting in national income.

In comparison, GLRRZ add $1,304 billion in audit-based misreporting (Table A6, exam and DCE columns). But this amount includes sources that are not in national income. Excluding $70 billion in added misreported capital gains and $125 billion of excess loss carryovers from prior years[13] suggests that GLRRZ add audit-based filer misreporting comparable to national income definitions of about $1,110 versus about $770 billion in national income.[14] Therefore, the GLRRZ filer misreporting estimates appear to exceed amounts in national income by $340 billion, or more than one third and 2 percent of national income. Of course, to the extent that some underreported income is not currently included, it may be appropriate to increase national income.

III. Allocating Entity-Level Passthrough Underreporting

In addition to adding detected misreporting and undetected underreporting based on the special audit studies, GLRRZ discusses additional underreporting of passthrough income occurring on entity-level tax returns. The GLRRZ definition of passthrough entities includes partnerships, S corporations, estate and trust fiduciary income, and 50 percent of positive rental income on Schedule E assumed to be from partnerships.

The likelihood of at least some additional underreporting seems compelling, but the GLRRZ method of imputing it is not. Several of the steps adding this additional underreporting tend to allocate too much to the top of the distribution.[15] GLRRZ assumes the total passthrough entity-level underreporting rate is 20 percent of true income. But a large amount of entity-level passthrough underreporting is already in the baseline amount added for detected misreporting and DCE multiplier underreporting. For their benchmark estimates, GLRRZ (pg. 33) therefore

[13] These amounts are based on 5.3 percent and 13.8 percent of total misreporting being from capital gains and line 21 other income in GLRRZ Table A2, but only 70 percent of other income is estimated to be from overreporting of net operating loss carryovers. Although the NRP does not break out net operating losses from the rest of other income, the annual tax return data does. These show that between 2006 and 2013, when applying the Auten and Langetieg (2020) underreporting rates for positive and negative sources of other income, estimated net operating loss carryovers underreporting was 69 percent of total estimated other income underreporting.

[14] Note that GLRRZ's additional passthrough underreporting and income from offshore wealth are not included in national income and therefore not included in this discussion.

[15] Since the GLRRZ description of their imputation method is incomplete, our analysis reflects our current understanding based on information found in the paper and discussions with the authors.

"remove 57.6% of the DCE-adjusted estimate of partnership and S-corporation evasion in the NRP before adding" entity-level passthrough underreporting. This step throws out the distributional information in the NRP and replaces it with an allocation with no empirical basis (and contradicts evidence regarding returns with losses).

GLRRZ then allocate assumed total entity-level passthrough underreporting by reported passthrough income within income bins (i.e., deciles and top groups). Losses are netted out from gains within each income bin and the net losses in the bottom decile are ignored for this allocation. It appears that the "benchmark" estimates also include the excessive DCE-induced re-rankings. In addition, exclusively for this allocation, other income (the line on Form 1040) is removed from the income definition. The main effect is to remove net operating loss carryovers, further contributing to the re-ranking of reported business income to the top of the income distribution used for this allocation. These effects help explain why GLRRZ allocate 99 percent of additional passthrough underreporting to the top 1% when allocating by reported income.

There are problems with allocating underreporting by reported business income. It means that for two returns with the same *true* income, the return with less underreporting will be incorrectly allocated more underreporting (Splinter, 2020). GLRRZ make a similar point (pg. 12): "Ranked by reported income, top earners by construction tend to have low evasion (since they are selected on high declared income)." An additional issue is that reported passthrough business income is volatile over time (Splinter, 2012; Hines, 2020). GLRRZ uses this volatile income measure to allocate additional passthrough underreporting, exacerbating an underlying upward bias in annual top income shares relative to multi-year income shares.

The GLRRZ approach also allocates little additional underreporting to returns with business losses, which is inconsistent with the evidence that such returns account for a large share of underreported reported income (Auten and Langetieg, 2020). Returns with business losses should be treated as a separate group and allocated an appropriate share of the additional underreported income (possibly using the absolute value of reported business income). Allocating this underreporting almost entirely by positive reported income likely leads to distorted results. For example, nearly all additional passthrough underreporting goes to the top 1%. Essentially none is allocated to the bottom 90% of tax returns, despite returns of wealthy taxpayers with business losses or dramatically understated business income being in this group. If the sophisticated evasion schemes were successful, these taxpayers would often be found in lower income groups. While those at the top may engage in careful tax planning, GLRRZ's allocation assumption likely overstates the income share of top groups.

An additional concern is that GLLRZ remove 57.6% of detected and DCE amounts of misreported passthrough income to avoid double-counting entity-level misreporting already in these amounts. Instead, the detected entity-level underreporting should be retained for the returns where it was found and used to inform the allocation of undetected underreporting. Since only a small number of entity-level audits were performed in the NRP, allocations to other returns with passthrough income could be accomplished by selecting returns with similar characteristics as those subject to entity-level audits.[16] For example, a return's reported income, and industry (retail, construction, law firm, finance, etc.) or occupation (store owner, builder, lawyer, investor, etc.), and whether the business has only one or multiple owners could be considered for this purpose. Once selected, these returns would be allocated similar amounts of detected entity-level passthrough underreporting. It may be appropriate to consider evidence from operational audits.

[16] Official tax gap estimates also impute undetected underreporting to returns with no detected underreporting (IRS, 2019).

Another issue is that entity-level returns were not randomly selected for NRP audit. Our understanding is that entity-level business returns audits corresponded to the taxpayer having the business records. These were likely to be smaller, single-owner businesses where the owner could control both entity-level and individual reporting. For example, Joulfaian (2000) found a high correlation between underreporting by small corporations and on their executives' individual tax returns. Thus, some smaller businesses may have been selected for audit because of a greater likelihood of entity-level underreporting. In contrast, larger businesses are likely to have more professional management and may have lower underreporting rates, as found by the 2003/2004 S-corporation audit study (IRS, 2008b). This study found that underreporting rates were much higher among S corporations with fewer assets than for those with more assets: 28% underreporting for those with assets under $0.2 million vs. 11% for those with assets of $10 million or more. In addition, Steve Rosenthal pointed out to the authors that large investment partnerships have a strong incentive to report high earnings to attract investors. This and other audit information could help offset any bias in the sample of entity-level audits in the NRP.

In summary, GLRRZ remove entity-level passthrough underreporting and allocate a larger amount by reported income. Both approaches are questionable. Actual NRP entity-level detected under-reporting should be retained where found and simple DCE multipliers should not be applied to these cases. Then appropriate amounts should be imputed to similar taxpayers with passthrough income to account for undetected entity-level underreporting. Evidence that larger passthroughs (few of which were audited) have lower entity-level underreporting rates should be considered in this imputation, as well as other evidence from operational audits that could provide insights.

The main findings in the following subsections are that GLRRZ allocates nearly all additional passthrough under-reporting to the top 1%, evidence suggests there is less total additional passthrough misreporting than their baseline assumption, and there are issues with their comparison of operational audits and audit studies, which motivates the argument for additional passthrough underreporting.

A. GLRRZ allocate nearly all additional passthrough income to the top 1%

The data in GLRRZ can be used to back out the implied share of additional passthrough underreporting being allocated to the top 1% by "true" income in their "benchmark" estimate.. First, Table 4 considers the amounts of passthrough and offshore misreporting GLRRZ added before DCE (sophisticated after exam). Following GLRRZ, $54 billion of offshore underreporting is allocated to the top 1%. Removing this amount implies that 73 percent of total passthrough misreporting is allocated to the top 1% before DCE ($87 of $119 billion).[17]

Next, consider the additional passthrough underreporting GLRRZ add after DCE for their "benchmark" estimate. This results in a larger implied top 1% share of additional passthrough underreporting of 99 percent ($49 of $50 billion). This is larger than without DCE, likely because DCE re-ranks into the top 1% returns with more net positive reported passthrough income than the returns they replace, which generally have more wage or other capital income with little underreporting. GLRRZ Figure A3 re-ranking patterns fit this mechanism. While GLRRZ argues that their allocation results in a lower bound for top 1% shares, their "benchmark" approach is essentially equivalent to allocating all underreporting to the top 1%. Therefore, the results of allocating by reported income are likely already an upper bound.[18]

[17] To check this result, we examine the representative tax data for returns filed in 2012 (INSOLE file), grouping returns by fiscal income and estimate that 76 percent of reported net partnership and S corporation income is in the top 1%.

[18] GLRRZ sensitivity tests for passthrough misreporting allocations are based on pre-DCE income, but the results of sensitivity tests should be relative to "benchmark" estimates, which are after including undetected underreporting.

Table 4: Share of additional passthrough underreporting allocated to top 1%

Income Group	GLRRZ Table A6		Offshore underreporting removed			
	Sophisticated after exam ($)	Benchmark after DCE ($)	Total passthrough misrep. after exam ($)	Additional passthrough underrep. after DCE ($)	Total passthrough misrep. after exam (%)	Additional passthrough underrep. after DCE (%)
P0–90	8	1	7	0	6%	0%
P90–95	6	1	5	0	4%	0%
P95–99	25	5	21	1	17%	1%
Top 1%	141	103	87	49	73%	99%
Total	**180**	**110**	**119**	**50**	**100%**	**100%**

Notes: GLRRZ averages for 2006–2013, amounts in $2012 billions. As shown in GLRRZ Table A3, $54 billion of offshore income is allocated to the top 1%. Remaining amounts are allocated $1 billion to each of the bottom two groups and rest to the P95–99 group. *Source*: GLRRZ Table A6, offshore underreporting from Table A3, and authors' calculations.

B. Total business-level misreporting: Recent audits suggest lower underreporting rates

GLRRZ assume that the entity-level passthrough underreporting rate is 20 percent of "true" income (25 percent of reported income). This assumption is based largely on an estimated corporate tax underreporting rate of 19% from the 2008–2010 tax gap (taxes not paid on time divided by total estimated taxes due). But those were recessionary years and these initial estimates have been revised downward. The new tax gap measures revised this down to about 15% and for more recent years it fell to 14%.[19]

GLRRZ also cite estimated income misreporting rates for S corporations (12 to 14% for 2003–2004) and partnerships (26% for 1982).[20] Weighting these by their share of reported passthrough income (two-thirds is S corporation income), suggests an average misreporting rate of 18%. We agree with GLRRZ's suggestion that the partnership misreporting rate is likely lower in more recent decades due to new information reporting, especially with Schedule K-1, and the effect of the Tax Reform Act of 1986 shutting down many tax shelters. Based on the S corporation audit study, the IRS (2008a, pg. 14) wrote that the simple DCE average multipliers "likely account for more misreporting of S-Corporation income than was detected in the S-Corporation study. Based on these findings, no additional adjustment is presently recommended to the Schedule E partnership and S-Corporation tax gap estimate..." These results suggest that S corporations should be omitted from GLRRZ's allocation of additional passthrough underreporting. In addition, breaking out assumed partnership and S corporation misreporting would be helpful in understanding the analysis.

[19] These are one less the voluntary tax compliance rates in Table 3 of IRS (2019). However, *income* underreporting rates may be lower than *tax* underreporting rates. Corporate income underreporting rates are unavailable. But Johns and Slemrod (2010) show individual underreporting rates are much lower for income than for taxes: 11% vs 18%.

[20] The S corporation income underreporting rate is likely 15%. Table 2.1 from IRS (2008b) shows net misreporting of $40.9 billion. This represents 15% of the reported plus misreported amounts ($271.3 = $230.4 + $40.9). Note that higher S corporation misreporting rates may rely on double counting. If overreported officer compensation is added to entity-level income, those amounts should also be deducted from S corporation distributions reported on individual tax returns. GLRRZ's approach suggests the former addition but not the latter reduction, implying double counting of this compensation.

The combined evidence from C corporation and passthrough entity audits suggest an overall passthrough income misreporting rate of perhaps 15%. Decreasing the assumed total misreporting rate from 20% to 15% would reduce the additional passthrough underreporting added by almost two-thirds, implying the misreporting-induced increase in top 1% true income shares would fall from 0.6 to 0.2 percentage point.[21]

C. Issues with comparing operational audits and NRP audit studies

To argue that NRP studies miss underreporting at the top of the reported distribution, GLRRZ compare detected misreporting of taxes—not income—in operational audits and NRP audits for the top 0.01% of reported income. Underreported income amounts are not available in the operational audit data. However, using underreporting of taxes to draw conclusions about underreported income is problematic for several reasons.

First, the implied top effective tax rates on NRP audit study income are too low. GLRRZ Table A6 implies a DCE-based top 0.01% tax rate on underreported income of only 11 percent. For nearly all the years considered, the top tax rate was 35 percent and preferred rates were 15 percent. GLRRZ Table A1 shows that dividends and capital gains account for only one tenth of top 1% underreporting. This implies that tax rates on top 0.01% misreported income should be about 33 percent—three times the DCE-inclusive estimate. Even a slightly higher tax rate on underreported income would imply that the NRP studies capture more underreported taxes than operational audits for the top 0.01% (as well as other) reported income groups.

Second, GLRRZ compare 2006–2013 audit studies to 2010 operational data. For the top 0.01%, this year had an unusually large level of assessments as a fraction of tax liability: 1.7 percent compared to 0.7 percent, on average, for the other years shown in GLRRZ Figure A8. A multi-year average provides a more appropriate comparison. Using the multi-year average would put the operational audit tax assessments well below those of the NRP audits (even without the tax rate correction discussed above). In summary, while there likely is unidentified passthrough business underreporting in the NRP audit studies, the argument based on comparing top 0.01% operational audit assessment is unconvincing and the use of 2010 data is misleading.

IV. Short Comments

1. Offshore Evasion: Accounting for Recent Initiatives and Increases in Reporting. Estimating the effect of income from offshore wealth on income inequality is complicated: not all offshore wealth is owned by individuals, reporting of foreign bank accounts to the IRS has increased dramatically since 2009, and enforcement efforts have increased. GLRRZ estimates are based on the following key assumptions: total household offshore wealth of $1,058 billion (based on 2007 data) and that 95% of this wealth is undeclared.[22] The assumption that 95% of offshore wealth is undeclared seems too high for 2006–2013 and especially for years since 2009. Zucman (2015) states

[21] The total increase of 0.6 percentage point is noted by GLRRZ on page 39. Total additional passthrough under-reporting falls by 60 percent because based on GLRRZ Tables A2 and A6, the total business-level misreporting for a 20% misreporting rate is $120 ($180 sophisticated – $60 offshore) and the amount deducted from the NRP is $70 ($180 sophisticated – $110 benchmark), meaning a 15% misreporting rate adds only $120*0.15/0.20 – $70 = $20 and ($20 – $50)/$50 = –60% (all in $billions). This appears consistent with the GLRRZ Figure 7(b) sensitivity analysis.

[22] GLRRZ use estimates of total offshore wealth that are labelled as "household" wealth, but non-profit organizations make similar investments in private equity and hedge funds provided by U.S. investment banking firms. Estimates of offshore wealth show that non-profit organizations hold over $200 billion of offshore wealth (Auten et al., 2020).

that "a growing fraction of offshore wealth is duly declared, namely 20% in 2014, up from 10% in 2008." This implies the current GLRRZ benchmark assumption that 95% is undeclared should be lowered to 90% in earlier years and 80% in later years. Rosenthal (2021) expresses similar concerns. In addition, GLRRZ's "benchmark" estimates assume undisclosed offshore wealth is distributed equally weighted between the distribution observed in U.S. Foreign Bank Account Reports (FBARs) and leaked Nordic offshore assets. While U.S. taxpayers still hiding offshore income may have higher income than those who voluntarily disclosed, it is not clear that Nordic data is appropriate because it represents a different policy context and the top group is based on only ten observations.

2. Returns with Reported Business Losses are Crucial. Over one third of detected misreporting of business income in audit studies is among returns with reported losses. This is seen not only in the 1988 and 2001 audit studies, but also in the more recent studies (Auten and Langetieg, 2020). GLRRZ currently do not break out the effects of their imputations on returns with reported losses nor do they show the bottom decile in passthrough underreporting figures. Their allocation of additional passthrough underreporting gives little to those with significant business losses. Because of importance of misreported business losses, adequate allocation to returns with losses needs to be included in the main analysis and reported separately from other returns with small amounts of business income.

3. Line Switches and Income Shifting Across Years. It would be helpful if GLRRZ clarified how line switches and income shifting across years affect their estimates. These effects are important because only underreported amounts are scaled up with DCE multipliers, but overreported amounts are not. This makes intuitive sense but can lead to excess estimated underreporting due to line switching and income shifting across years. *Line switching* occurs when an amount is added on the wrong line of a form and missing from another line. But if only the underreported amount is scaled up by a DCE multiplier, total net underreporting would be overstated.[23] Instead, these amounts should be cancelled out. *Income shifting* from 2013 to 2012 tax returns to avoid tax rate increases was common among high-income taxpayers (Auten, Splinter, and Nelson, 2016). To the degree it's detected, the GLRRZ approach may incorrectly scale up the 2013 shifted income with DCE multipliers, exaggerating total misreporting and top income shares. These are potentially large issues. Johns and Slemrod (2010) removed almost a fifth of observations due to data issues such as line switching.

4. Underreporting among Non-Filers. GLRRZ write that, "high-income non-filers drive the bulk of the non-filer tax gap in recent years (TIGTA, 2020)." But this TIGTA report differs from the surrounding discussion in several ways. The TIGTA report discusses underpaid *taxes*—not underreported *income*. It is obvious the that underpaid taxes are more highly concentrated among those with higher incomes because many tax units have no federal individual income tax burden (Splinter, 2019) and most payroll taxes of non-filers are withheld by their employers. In addition, the definition of the term "high-income" also differs substantially. GLRRZ uses the term high-income to refer to the top 1% or 0.1% (fiscal incomes above $450,000 and about $2 million,

[23] Alternatively, two offsetting amounts may both be missing. "An example is one in which an examiner detects both gambling winnings and gambling losses for a taxpayer who reported neither. The net effect of these adjustments is often a minimal change in tax liability but a significant change in the accuracy of Form 1040, line 21, 'other income,' and Schedule A, line 27, 'other miscellaneous deductions.' Considered separately, the accuracy of each line item could raise concerns, considered together, maybe not as much concern." (Bennett, 2005, pg. 12)

respectively). In contrast, TIGTA uses a much lower threshold, explaining that "a high-income nonfiler is any nonfiler with a total income greater than or equal to $100,000." (TIGTA (2020, pg. 2). Finally, the definition of non-filer for tax gap estimates includes many late filers and is not comparable to income inequality estimates in PSZ or Auten and Splinter (2019), which use annual SOI tax return files that include some non-timely filed prior-year returns.

5. *Gradient multipliers are appropriate for total underreporting, not line-by-line amounts*
In response to an earlier version of our comment, Reck, Risch, and Zucman (2021) presented estimates that apply gradient multipliers on a line-by-line basis. Their approach raises two significant concerns. First, rather than controlling for the *overall* skill of each auditor, a line-by-line approach assumes each income source has a separate auditor skill distribution. This deviates from the intent of DCE and gradient multipliers to capture overall auditor skill. Second, their line-by-line estimates fail to account for line-switching errors, where NRP audits move income from one line to another, creating equal amounts of underreporting and overreporting. As discussed above, failing to cancel out these amounts results in overstatement of total underreported income because simple DCE multipliers are only applied to the underreported amounts. Our application of gradient multipliers to net underreporting of total income better captures the overall skill of auditors and are robust to line-switching errors.

V. Implications of Research on Estimated Underreporting and Evasion

Caution is needed if the results from studies such as GLRRZ are used to understand the impact of additional high-income audits. It may seem that the GLRRZ estimate of a high underreporting rate at the top of the "true" distribution implies the IRS can raise substantial revenue by targeting audits at returns with high reported incomes. But the IRS cannot observe those in the top of the "true" income distribution until after performing audits. As GLRRZ (pg. 13) explains: "The majority of evasion attributed to the top 0.01%...comes from individuals initially reporting income below the top 0.01% threshold who are re-ranked into the top 0.01% after DCE adjustment." This means that many returns with significant underreporting at the top of the *true* distribution are lower down in the *reported* distribution. Therefore, increasing audit rates of returns with the highest reported income is not necessarily the best use of IRS resources.[24]

The effectiveness of audits is generally evaluated in term of the return on investment (ROI), the expected additional revenue collected per dollar of enforcement activity costs. Holtzblatt and McGuire (2016) summarize these ROIs.[25] Allowing for some start-up time, the overall estimated ROI was 8.0 for increasing audit coverage.[26] Enforcement programs focused on the sources of

[24] Audits are already targeted at the top of the reported income distribution. TIGTA (2015) shows high audit rates at the top of the distribution, with audits of 20 percent of returns with adjusted gross income of at least $10 million (average of the three years shown). In addition, risk assessment targets audits within each income group (pg. 10): "The case is risk assessed to determine if an audit is warranted on the taxpayer's and his or her related entities' tax returns. During the risk assessment process, additional internal and external research is performed to identify large, unusual, or questionable items to determine the reasons for a low effective tax rate." For some programs, IRS also considers complexity when assessing risk of evasion. Sarin and Summers (2019, 2020) discuss how additional audits and technology investments could help better target high-income returns.

[25] ROIs from Treasury report (pg. 13), see https://home.treasury.gov/system/files/266/15.-IRS-FY-2016-BIB-Final.pdf

[26] CBO (2020) assumes a lower ROI of 6.4 (after three years). Using IRS data, Holtzblatt and McGuire (2020) estimate that additional funding would have a maximum ROI of 5.7 that decreases with additional funding. Sarin and Summers (2020) argue for a higher ROI. Belokowsky (2021) also discusses the likelihood of diminishing returns to ROIs.

high-income underreporting evaluated by GLRRZ had lower ROIs. Increasing audits of large partnerships had an ROI of 7.6, but a program to address international and offshore compliance had an ROI of only 3.7. These ROIs may not capture the full effect of audits on compliance to the extent of broader deterrence effects or future compliance of audited taxpayers. The latter appears particularly effective for compliance with the Earned Income Tax Credit (DeBacker et al., 2018a, 2018b).[27] In addition, ROIs for additional audits of large partnerships may change due to recent legislation. The Protecting Americans from Tax Hikes (PATH) Act of 2015 created a new partnership audit regime—instead of the IRS needing to pursue each partner separately, the IRS can audit large partnerships and assess underpaid taxes and penalties at the entity level.

Efforts to limit high-income tax evasion are not new. Troiano (2017) discusses policies introduced in the 1950s and 1960s that limited significant high-income underreporting: expansions of income tax withholding, third-party reporting, and intergovernmental agreements to coordinate audits. These policies caused large increases in reported top income shares, suggesting that prior efforts to limit evasion disproportionately affected those with high incomes.

These findings have implications for the best use of additional IRS funding for compliance. While some increase in audit rates of high-income returns may be appropriate, additional enforcement resources and the results of NRP audit studies could also help audit selection by better identifying returns with lower reported incomes but higher likelihood of underreporting. The combined efforts and increased cooperation of IRS divisions could lead to improved methods of selecting returns for audit and help maintain high ROIs for enforcement activities.

VI. Conclusion

While tax data are a valuable resource for measuring the distribution of income, one limitation is that underreported income is not included. The challenge confronted by researchers trying to allocate undetected underreported income is the lack of good empirical evidence. GLRRZ seek to identify and allocate high-income underreporting beyond that found in the detailed NRP audits. They provide evidence that a considerable share of income from offshore assets has not been reported. In addition, there may be additional entity-level underreporting of passthrough income beyond that already included in DCE corrections. We are impressed by their efforts and use of many sources of data to estimate the extent of this underreporting. However, the GLRRZ analysis would benefit from more appropriate methods and empirical support for particular assumptions. Most importantly, the simple DCE multipliers used are outdated and a distributionally inconsistent way to allocate undetected underreporting. Allocating passthrough misreporting in proportion to reported income is also distributionally inconsistent. We believe that the methodology and assumptions used tend to overestimate total underreporting, overstate true top incomes, and allocate too much underreporting to the top of the distribution. We point our areas where improvements are needed and suggest alternative methods that would be more distributionally consistent. These suggest less impact of underreported income on top income shares.

[27] DeBacker et al. (2018b, pg. 482) estimate that "operational audits of EITC claimants in 2014 resulted in a reduction of EITC claims of about $500 million in total, $310 million of which is due to reductions in claims post-audit." They conclude that audits are an "effective tool to better ensure that the benefits of an antipoverty program go to the intended beneficiaries of that program" Most EITC audits are low-cost correspondence audits, whose ROI was reported to be 14.9 in 2017 (Holtzblatt and McGuire, 2020).

References

Alstadsaeter, Annette, Niels Johannesen, and Gabriel Zucman. 2018. "Who Owns the Wealth in Tax Havens? Macro Evidence and Implications for Global Inequality." *Journal of Public Economics* 162: 89–100.

Auten, Gerald, Joseph Hancuch, and Paul Arnsberger. 2020. "Offshore Investments: The Case of US Non-Profits." Presentation at 113th Annual Conference of the National Tax Association.

Auten, Gerald, and Patrick Langetieg. 2020. "The Distribution of Underreported Income: What We Can Learn from the NRP." Working paper.

Auten, Gerald, and David Splinter. 2019. "Income Inequality in the United State: Using Tax Data to Measure Long-term Trends." Working paper.

Auten, Gerald, David Splinter, and Susan Nelson. 2016. "Reactions of High-Income Taxpayers to Major tax Reforms." *National Tax Journal* 69(4): 935–964.

Belokowsky, Simon. 2021. "New Research Suggests IRS Underestimates Tax Evasion by Wealthiest Filers." https://tax.unc.edu/index.php/news-media/new-research-suggests-irs-underestimates-tax-evasion-by-wealthiest-filers/

Bennett, Charles. 2005. "Preliminary Results of the National Research Program's Reporting Compliance Study of Tax Year 2001 Individuals Returns." IRS Research Bulletin: Proceedings of the 2005 IRS Research Conference: 3–14.

Bloomquist, Kim, Ed Emblom, Drew Johns, and Patrick Langetieg. 2012. "Estimates of the Tax Year 2006 Individual Income Tax Underreporting Gap." IRS Research Bulletin: Proceedings of the 2012 IRS/TPC Research Conference: 70–78.

Brown, Bob, and Drew Johns. 2007. "National Research Program—Methods and Plans." IRS Research Bulletin: Proceedings of the 2007 IRS Research Conference: 125–143.

Congressional Budget Office. 2020. "Trends in the Internal Revenue Service's Funding and Enforcement."

DeBacker, Jason, Bradley Heim, Anh Tran, and Alexander Yuskavage. 2018a. "Once Bitten, Twice Shy? The Impact of IRS Audits on Filer Behavior." *Journal of Law and Economics* 61(1): 1–35.

DeBacker, Jason, Bradley Heim, Anh Tran, and Alexander Yuskavage. 2018b. "The Effects of IRS Audits on EITC Claimants." *National Tax Journal* 71(3): 451–484.

DeBacker, Jason, Bradley Heim, Anh Tran, and Alexander Yuskavage. 2020. "Tax Non-compliance and Measures of Income Inequality." *Tax Notes Federal*, February 17: 1103–1118.

Erard, Brian, and Chih-Chin Ho, 2003. "Explaining the U.S. Income Tax Continuum." *eJournal of Tax Research* 1(2): 93–109.

Erard, Brian, and Jonathan Feinstein, 2011. "The Individual Income Reporting Gap: What We See and What We Don't." IRS Research Bulletin: Proceedings of the 2011 IRS/TPC Research Conference: 129–142.

Feinstein, Jonathan S. 1990. "Detection Controlled Estimation." *Journal of Law and Economics* 33(1): 233–276.

Feinstein, Jonathan S. 1991. "An Econometric Analysis of Income Tax Evasion and Its Detection." *RAND Journal of Economics* 22(1): 14–35.

Guyton, John, Patrick Langetieg, Daniel Reck, Max Risch, and Gabriel Zucman. 2021. "Tax Evasion at the Top of the Income Distribution: Theory and Evidence." March. NBER working paper No. 28542.

Hines, James R., Jr.. 2020. "Corporate Taxation and the Distribution of Income." NBER Working Paper No. 27939.

Holtzblatt, Janet, and Jamie McGuire. 2016. "Factors Affecting Revenue Estimates of Tax Compliance Proposals." CBO working paper 2016-05, JCT working paper JCX-90-16.

Holtzblatt, Janet, and Jamie McGuire. 2020. "Effects of Recent Reductions in the Internal Revenue Service's Appropriations on Returns on Investment." IRS Research Bulletin: Proceedings of the 2019 IRS/TPC Research Conference: 128–146.

IRS. 2008a. “Preliminary Methodology for Estimating the TY2004 Tax Gap Associated with S-Corporations.” Draft version August 22, 2008.

IRS. 2008b. “Preliminary Results of the 2003/2004 National Research Program S-Corporation Underreporting Study.” Draft version August 22, 2008.

IRS. 2019. “Federal Tax Compliance Research: Tax Gap Estimates for Tax Years 2011–2013.” IRS Publication 1415 (Rev. 9-2019).

Johannesen, Niels, Patrick Langetieg, Daniel Reck, Max Risch, and Joel Slemrod. 2020. “Taxing Hidden Wealth: The Consequences of US Enforcement Initiatives on Evasive Foreign Accounts.” *American Economic Journal: Economic Policy* 12(3): 312–346.

Johns, Andrew, and Joel Slemrod. 2010. “The Distribution of Income Tax Noncompliance.” *National Tax Journal* 63(3): 397–418.

Johnston, David Cay. 2008. “Trust, But Verify.” *Tax Notes* 120 (August 4): 485-488.

Joint Committee on Taxation. 2019. “Overview of the Tax Gap.” JCX-19-19.

Joint Committee on Taxation. 2021. “Technical Explanation of the Modification of Exceptions of Reporting Third Party Network Transactions in New Section 9674 of Subtitle IX of the America Rescue Plan Act Of 2021, as Amended by the Proposed Manager’s Amendment.”

Joulfaian, David. 2000. “Corporate Income Tax Evasion and Managerial Preferences.” *Review of Economics and Statistics*, 82(4): 698-701.

Piketty, Thomas, Emmanuel Saez, and Gabriel Zucman. 2018. “Distributional National Accounts: Methods and Estimates for the United States.” *Quarterly Journal of Economics* 133(2): 553–609.

Reck Daniel, Max Risch, and Gabriel Zucman. 2021. “Response to a Comment by Auten and Splinter on ‘Tax Evasion at the Top of the Income Distribution: Theory and Evidence’.” July 21. Working paper.

Rosenthal, Steven M. 2021. “If Congress Wants the IRS to Collect More Tax from the Rich, It Needs to Pass Better Laws.” Tax Policy Center, Tax Vox. Available at www.taxpolicycenter.org/taxvox/if-congress-wants-irs-collect-more-tax-rich-it-needs-pass-better-laws

Sarin, Natasha, and Lawrence H. Summers. 2019. “Shrinking the Tax Gap: Approaches and Revenue Potential.” NBER working paper No. 26475.

Sarin, Natasha, and Lawrence H. Summers. 2020. “Understanding the Revenue Potential of Tax Compliance Investment.” NBER working paper No. 27571.

Slemrod, Joel, Brett Collins, Jeffrey Hoopes, Daniel Reck, and Michael Sebastiani. 2017. “Does Credit-Card Information Reporting Improve Small-Business Tax Compliance.” *Journal of Public Economics* 149: 1–19.

Splinter, David. 2012. “Income Variability: Effects on U.S. Income Inequality and Tax Progressivity.” Rice University, PhD Dissertation.

Splinter, David. 2019. “Who Pays No Tax? The Declining Fraction Paying Income Taxes and Increasing Tax Progressivity.” *Contemporary Economic Policy* 37 (3): 413–426.

Splinter, David. 2020. “Reply: Trends in US Income and Wealth Inequality: Revising After the Revisionists." Working Paper.

TIGTA. 2015. “Improvements are Needed in Resource Allocation and Management Controls for Audits of High-Income Taxpayers.” Treasury Inspector General for Tax Administration publication 2015-30-078.

TIGTA. 2020. “High-Income Nonfilers Owing Billions of Dollars are Not Being Worked by the Internal Revenue Service.” Treasury Inspector General for Tax Administration, 2020-30-015.

Troiano, Ugo. 2017. “Do Taxes Increase Economic Inequality? A Comparative Study Based on the State Personal Income Tax.” NBER working paper No. 24175.

Zucman, Gabriel. 2015. “Factual mistakes in “Robin Hood’s Very Merry Man.” Wall Street Journal. Available at https://gabriel-zucman.eu/files/press/201510WSJAnswer.pdf

Table A1: Percent of returns and misreporting ratios by ratio class, 2010–2011

Panel A: Percent of Returns by Ratio Class

Rank	-0.5	0.5	1	1-1.1	1.1-1.2	1.2-1.5	1.5-2	2-4	4-8	8+
<-$50k	4.06	37.01	34.09	3.07	3.50	11.42	1.90	4.15	0.61	---
< $0	6.98	20.09	29.11	2.80	2.31	5.50	6.28	10.46	9.26	7.23
$0-P20	---	5.12	66.40	8.62	3.29	5.62	3.43	3.73	1.78	1.89
20-40	---	4.70	70.54	10.57	3.34	5.07	2.97	2.22	0.50	---
40-60	---	4.18	72.52	11.80	3.40	4.48	2.03	1.28	0.19	---
60-80	---	3.59	70.95	17.15	3.38	3.45	0.93	0.53	0.01	---
80-90	---	3.75	74.39	16.42	2.48	2.15	0.63	0.17	0.01	---
90-95	---	3.38	75.20	15.47	3.27	2.12	0.43	0.12	0.01	---
95-99	---	4.58	72.80	18.19	2.73	1.32	0.25	0.13	---	---
99-99.5	---	4.84	74.74	17.74	1.72	0.46	0.40	0.09	---	---
Top 0.5%	---	3.79	77.73	15.40	2.02	0.82	0.18	0.07	---	---
All	0.12	4.47	70.63	12.95	3.21	4.10	1.97	1.61	0.52	0.44

Panel B: Average Ratio of Corrected to Reported Income by Ratio Class

Rank	-0.5	0.5	1	1-1.1	1.1-1.2	1.2-1.5	1.5-2	2-4	4-8	8+
<-$50k	-1.207	-0.770	1.000	1.078	1.174	1.357	1.667	2.918	4.639	---
< $0	-1.666	-0.594	1.000	1.029	1.116	1.362	1.863	2.850	6.242	24.082
$0-P20	---	0.560	1.000	1.039	1.145	1.338	1.716	2.743	5.628	17.410
20-40	---	0.514	1.000	1.038	1.145	1.322	1.677	2.753	5.294	---
40-60	---	0.763	1.000	1.039	1.145	1.328	1.700	2.548	4.906	---
60-80	---	0.940	1.000	1.032	1.141	1.314	1.701	2.591	5.158	---
80-90	---	0.942	1.000	1.031	1.138	1.316	1.705	2.463	5.172	---
90-95	---	0.964	1.000	1.034	1.152	1.306	1.636	2.586	4.848	---
95-99	---	0.954	1.000	1.028	1.134	1.314	1.688	2.660	---	---
99-99.5	---	0.931	1.000	1.027	1.136	1.363	1.683	2.034	---	---
Top 0.5%	---	0.952	1.000	1.028	1.132	1.313	1.661	3.082	---	---
All	0.103	0.725	1.000	1.035	1.143	1.326	1.702	2.707	5.603	17.834

Panel C: Standard Error for Ratio by Ratio Class

Rank	-0.5	0.5	1	1-1.1	1.1-1.2	1.2-1.5	1.5-2	2-4	4-8	8+
<-$50k	0.104	0.026	---	0.010	0.008	0.019	0.042	0.192	0.149	---
< $0	0.379	0.033	---	0.008	0.012	0.025	0.024	0.083	0.171	2.720
$0-P20	---	0.029	---	0.002	0.002	0.005	0.011	0.031	0.101	0.928
20-40	---	0.028	---	0.001	0.002	0.005	0.009	0.042	0.137	---
40-60	---	0.025	---	0.001	0.002	0.005	0.010	0.035	0.184	---
60-80	---	0.006	---	0.001	0.001	0.004	0.011	0.053	0.287	---
80-90	---	0.009	---	0.001	0.002	0.007	0.018	0.075	0.486	---
90-95	---	0.007	---	0.001	0.003	0.008	0.024	0.132	---	---
95-99	---	0.006	---	0.001	0.003	0.011	0.021	0.114	---	---
99-99.5	---	0.022	---	0.002	0.008	0.020	0.065	0.004	---	---
Top 0.5%	---	0.009	---	0.001	0.004	0.014	0.064	0.083	---	---

Notes: Ratio classes group returns by the ratio of reported income plus detected misreporting divided by reported income. For example, ratio class 1 has a detected misreporting rate of zero and ratio class 2–4 includes returns with detected misreporting rates of 50–75%. Detected misreporting is allocated by randomly assigning tax returns within each reported income group to a ratio class according to panel A. Other than those in the no-misreporting class, each return receives detected underreporting or overreporting by multiplying the absolute value of AGI by a draw from a distribution with a mean and standard error for the corresponding average ratio group (ratio class 8+ maximum is 1.25 times the mean). For ratio group 0.5, returns with positive income have overreported income, and returns with negative income have their losses reduced but the resulting income is still negative. *Source*: Auten and Langetieg (2020).

Appendix A: DCE Multiplier Background

GLRRZ replicates the simple DCE multiplier approach used by Johns and Slemrod (2010). The following discussion of the application and limitations of the four simple DCE multipliers is from Johns and Slemrod (2010, pp. 400–401):

> "The DCE analysis was done separately for two groups of returns. A return was allocated to one of the following groups: (1) Returns without reported Schedule C or Schedule F profit or loss, and with reported total positive income (TPI)[7] less than $100,000, or (2) Returns with reported Schedule C or Schedule F profit or loss, or with reported total positive income greater than or equal to $100,000. Within each of these two tax return groups, noncompliance equations were then estimated separately for total income and for "low-visibility" income subject to little or no information reporting, which included farm or nonfarm proprietor income, income from a partnership or S corporation, rental or royalty income, gains or losses reported on Form 4797, and income reported on the Form 1040 "other income" line. "High-visibility" income had at least some systematic information reporting and included wages and tips, interest and dividends, state and local tax refunds, alimony, capital gains, pensions, unemployment compensation, and Social Security income.
>
> The noncompliance equations that resulted from the DCE analysis were used to estimate the amount of total income underreporting (i.e., detected plus undetected) and the amount of low-visibility income underreporting. Unreported high-visibility income was then set to the difference between these two DCE estimates. Each DCE estimate for total underreported income was divided by the amount of underreporting actually detected. This procedure generates four separate "multipliers," one for each type of return and income-visibility category:
>
> Non-business returns with reported TPI < $100,000
> Low-visibility income: 4.158
> High-visibility income: 2.009
> Business returns (Schedule C or F) or returns with reported TPI > $100,000
> Low-visibility income: 3.358
> High-visibility income: 2.340.
>
> The DCE multipliers were then used to calculate, on a return-by-return basis, line-item net misreported amounts (NMAs) by multiplying the amount of underreported income detected during the NRP audit by the appropriate one of the four DCE multipliers. The multiplier was applied only to the detected underreporting of a line item if the sample return was selected for face-to-face audit and the examiner detected some underreported income. Note that this technique assumes that detection rates are similar across line items within each type of return and income-visibility category. The use of the DCE multipliers will understate estimates of undetected income for some taxpayers, and almost certainly will do so for the class of returns subject to correspondence audits and those audited returns where no income underreporting was detected, because no adjustment is made in these cases. Conversely, it may overstate estimates of undetected income for other taxpayers. Note specifically that the use of the multipliers implicitly allocates undetected income in proportion to the amount of income that was detected, within a given income visibility category. To the extent that certain types of low-visibility income are harder to detect than others, the use of the DCE multipliers may also overstate or understate the amount of noncompliance for some income sources.[8]
>
> Note finally that the individual underreporting gap estimates reported here focus only on misreporting on returns filed on a timely basis, and therefore do not take into account all noncompliance by individual taxpayers; the IRS estimates a separate tax gap for individual nonfilers, which includes late-filed returns. Nor do the estimates explicitly account for income derived from illegal activities. If the NRP examiner found income from illegal activities during the audit, that income is included but, as this would have been detected incidentally, it likely represents a very small portion of the whole."

Footnote 8 from Johns and Slemrod (2010): "The estimates based on the DCE-adjusted NRP subset do not come with standard errors, but we can infer something about the confidence surrounding estimates by looking at Table A1, which shows the number of tax returns, by income class, that comprise the sample.

Appendix B: Replicate estimates of Johns and Slemrod (2010)

Applying the NRP-based estimates of Auten and Langetieg (2020) to a sample of tax returns can replicate the estimates of Johns and Slemrod (2010). We use a representative sample of 2001 tax returns and define income as Adjusted Gross Income (AGI). Detected misreporting is estimated using the 2001 NRP results from Auten and Langetieg (2020). Undetected underreporting is estimated using the simple DCE multipliers described in appendix A. However, our estimates of detected underreporting do not differentiate between high and low visibility income sources. Therefore, after dividing tax returns into total positive income groups (and the presence of any Schedule C or F income), we select returns to have all detected underreporting treated as low or high visibility. Among returns with income from Schedules C, E, or F or with other income, 60 percent are randomly selected to have the low-visibility multiplier applied. Otherwise, the corresponding high-visibility multiplier is applied. To estimate both detected and undetected misreporting, we present the average of ten simulations, as done for the new tax gap estimates (Bloomquist et al., 2012).

Table A2 shows that this approach results in similar income shares as the 2001 NRP estimates in Johns and Slemrod (2010). One difference is that reported bottom 10% income shares are lower in the 2001 representative sample. Johns and Slemrod removed about a fifth of their observations due to data issues. If data issues are more common among returns with negative AGIs, this may explain why their bottom 10% share is nearly one percentage point higher. Income shares for reported income plus misreporting (after applying simple DCE multipliers) are all within 0.3 percentage points and usually exactly match.

Table A2: Misreporting using simple DCE multipliers has little effect on income shares in 2001

	Reported Income		**Reported + Misrep.**	
	Johns & Slemrod	Auten & Splinter	Johns & Slemrod	Auten & Splinter
Bottom 10%	0.1	-0.7	0.3	0.0
10%–20%	1.6	1.5	1.6	1.6
20%–30%	2.7	2.7	2.7	2.7
30%–40%	3.9	3.8	3.9	3.8
40%–50%	5.2	5.2	5.2	5.2
50%–60%	6.8	6.8	6.7	6.7
60%–70%	8.9	8.9	8.8	8.8
70%–80%	11.7	11.8	11.5	11.5
80%–90%	16.0	16.2	15.6	15.8
90%–95%	11.0	11.3	10.9	11.2
95%–99%	14.4	14.7	14.9	15.0
99.0%–99.5%	3.7	3.7	3.8	3.9
Top 0.5%	14.1	14.1	14.0	14.0
Total	**100.0**	**100.0**	**100.0**	**100.0**

Notes: Reported income is AGI. Reported+Misrep. is AGI plus estimated detected misreporting and undetected underreporting, which uses four simple DCE multipliers. Tax returns are ranked by corresponding income definition. Averages of ten simulations are shown.
Source: Johns and Slemrod (2010) estimates from 2001 NRP and authors' calculations using 2001 INSOLE tax return data and detected misreporting estimates from Auten and Langetieg (2020).

Table A3 extends these estimates to show the effect of replacing simple DCE multipliers with distributionally consistent multipliers. The first row shows the reported income amounts and shares for three income groups. The second row (after exam, no DCE) shows that adding detected misreporting lowers the top 1% income share by 0.3 percentage points. The third row (after exam, with DCE) includes detected misreporting and undetected underreporting, estimated using the simple DCE multipliers. The simple DCE multipliers offset all the top 1% income share decrease from detected underreporting. The next three rows replace the simple DCE multipliers with gradient multipliers. These decrease top 1% income shares by 0.5 percentage points. This decrease is larger than when only including detected misreporting and the level is lower than when applying DCE multipliers. As a sensitivity check, flatter and steeper gradients are applied and top 1% shares are relatively unchanged. The decrease in top 1% income shares is because gradient multipliers scale up detected underreporting, augmenting the top income share decrease from detected amounts, but gradient multipliers cause less upward re-ranking than DCE multipliers.

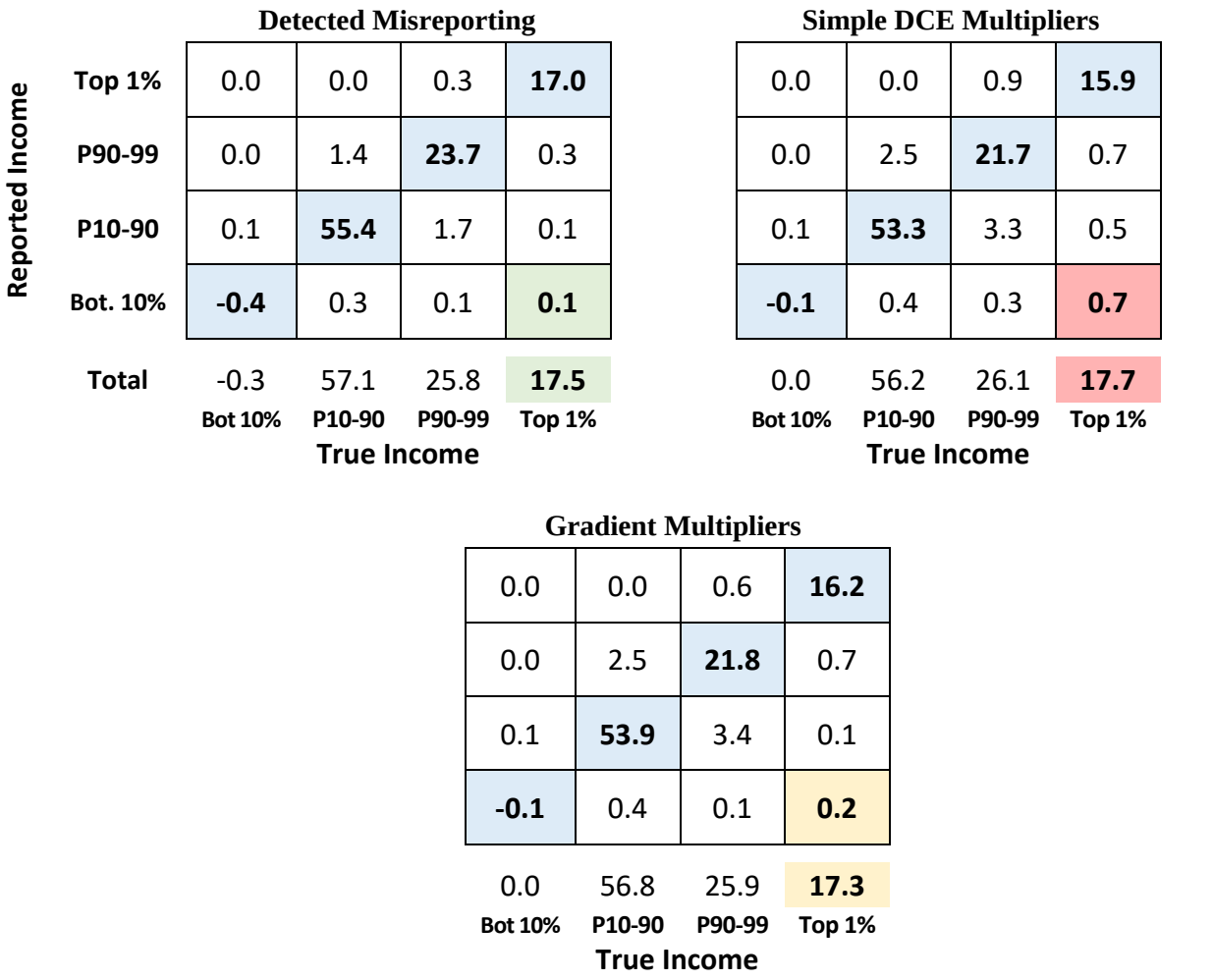
Table A3: Income Shares for Different Allocations of Undetected Underreporting, 2001 tax returns

	Income ($billions)				Income Shares (%)			Top 1% chg. from
	Total	P0-50	P50-99	Top 1%	P0-50	P50-99	Top 1%	reported (pp)
Panel A: Replicate Johns and Slemrod (2010) estimates								
Reported income	6.2	0.8	4.3	1.1	12.5	69.7	17.8	---
After exam, no DCE	6.4	0.8	4.5	1.1	13.2	69.4	17.5	–0.3
After exam, with DCE	6.8	0.9	4.7	1.2	13.2	68.9	17.8	0.0
Panel B: Distributionally consistent alternatives to DCE multipliers								
Gradient	6.8	0.9	4.7	1.2	13.5	69.3	17.3	–0.5
Flat gradient	6.8	0.9	4.7	1.2	13.5	69.3	17.3	–0.5
Steep gradient	6.8	0.9	4.7	1.2	13.4	69.3	17.3	–0.5

Notes: Income after exam includes both underreported income and overreported income. Amounts in 2001 dollars. Averages of ten simulations are shown.
Source: Authors' calculations using 2001 INSOLE file and estimates from Auten and Langetieg (2020).

Figure A1 shows how re-ranking shifts income shares from reported income groups to "true" (misreporting-inclusive) income groups. The top 1% income share of reported income is 17.8 percent. First, the top-left panel shows that detected misreporting decreases the top 1% income share to 17.5 percent. Also, little re-ranking is observed because most income is on the main diagonal (shaded in blue) and only 0.1 percent of income re-ranks from the bottom 10% of reported income to the top 1% of detected misreporting inclusive income (shaded in green). Second, the top-right panel shows that simple DCE multipliers result a higher top 1% income share. This is caused by substantial re-ranking, with 0.7 percent of income moving from the bottom 10% of reported income to the top 1% of post-DCE adjusted income (shaded in red). Third, the bottom-right panel shows that gradient multipliers result in lower top 1% income shares, with only 0.2 percent of income moving from the bottom 10% of reported income to the top 1% of post-gradient multiplier adjusted income (shaded in yellow). This decrease is because gradient multipliers cause a similar pattern of re-ranking into the top 1% as detected misreporting, although with additional re-ranking between the top 1% and the rest of the top 10%.

Figure A1: Reranking of income shares from detected misreporting and multipliers, 2001



Explanation of results: Adding misreported income using simple DCE multipliers increases the top 1% share from 17.5 to 17.7 percent due to the upward re-ranking of returns into the top 1% (bottom-right cell in red). Using the more appropriate gradient multipliers decreases the estimated top 1% share from 17.5 to 17.3 percent due to less re-ranking of returns into the top 1%.

Notes: Cell values sum to 100% and totals sum each column. Reported income groups are by adjusted gross income. True income groups are by reported income plus misreported income: detected only (top left), detected plus undetected using simple DCE multipliers (top right), and detected plus undetected using gradient multipliers (bottom). See discussion on prior page.

Source: Authors' calculations using 2001 INSOLE file and tables from Auten and Langetieg (2020).

Appendix C: Methodological Improvements for the GLRRZ paper

Improved DCE Methodology to replace simple DCE multipliers

- The most important need is replacing the use of four simple multipliers with an improved DCE methodology, such as one more similar to the current methodology used for tax gap estimates.
- Underreported amounts on one line that reflect income reported incorrectly on another line should be netted out, as discussed below. Income moved from one year to another presents a similar problem.
- Consideration should be given to allocating some of the total undetected underreporting to returns where none was detected.

Revise approach for estimating and allocating entity-level passthrough evasion

- Our suggestion: When entity-level audits were performed, retain any detected misreporting rather than replacing those amounts with arbitrary allocations. For any assumed underreporting in excess of these detected amounts, use a matching approach for other returns with passthrough income including those with no detected underreporting. Consider the entity industry (e.g., retail, rental, investment partnerships), entity ownership (e.g., single owner, few owners, large partnership) and apply appropriate underreporting ratios. Evidence from recent operational audits could be helpful, ideally by size or industry.
- Our understanding is that most entity-level audits were single-owner businesses where the taxpayer had possession of the records. These are likely to be smaller businesses with higher underreporting rates than, for example, investment partnerships.
- A matching approach could also be used to account for undetected underreporting on returns with no detected underreporting. Newly discovered businesses found by auditors could be useful for this purpose.
- Given the uncertainty of total entity-level underreporting, additional evidence is needed. Underreporting likely differs by entity type, size, and industry. Taxpayer occupation can also be informative.

Revise and provide documentation of approach for offshore income

- The current paper assumes 95% of offshore income goes unreported. But this ignores the effects of FATCA. Zucman (2015) wrote that "a growing fraction of offshore wealth is duly declared, namely 20% in 2014, up from 10% in 2008." The current percentages need to be revised, perhaps to 90% in earlier years and 80% in more recent years. If other data or studies have estimates, those could be cited and used.
- The total amount of offshore income and wealth of individuals should be reviewed and updated, with the data sources and assumptions documented and explained more clearly. The current total cites 2007 data, before FATCA and other enforcement efforts.
- The current paper seems to use the U.S. "household" category in Bank of International Settlements (BIS) data. But the only other category of non-financial institutions is "corporations." This raises possibility that the "household" category may include the offshore wealth of non-profits. This issue needs to be clarified and sources documented.

Ensure appropriate undetected underreporting is allocated to business losses

- Overstated business losses (Schedules C, E, and F) account for about one-third of detected underreported business income. But GLRRZ currently allocates little detected underreporting to business losses, and tables do not show this group separately. Appropriate allocations of undetected underreporting to business losses could significantly affect results. Appropriate

accounting for misreported business losses needs to be part of the basic methodology rather than just a sensitivity test.

- The current sensitivity test sets business losses to zero for 20% of the cases, an arbitrary allocation inconsistent with the NRP distribution of discovered overstating of losses.
- A related issue is excluding other income for one of the passthrough allocation steps. The current procedure may remove some net operating loss carryovers twice: once with the income definition for allocating underreported passthrough income and again with the adjustments for detected underreporting related to overstated net operating loss carryovers.

Adjust for wrong-line and wrong-year cases

- A significant portion of detected underreporting represents cases where taxpayers reported income on the wrong line. This results in offsetting positive and negative amounts that should be zeroed out.
- Line switching can result from accidental errors and efforts to avoid paying payroll taxes (e.g., classifying sole proprietor income as other income to avoid SECA taxes).
- Underreported amounts are multiplied by DCE multipliers, but not overreported amounts. Unless GLRRZ correct for line switching, this overstates total underreporting and results in excessive re-ranking.
- Income or losses reported in the wrong year is a similar problem. Wrong-year cases also need to be accounted for (i.e., zeroed out), as there are offsetting changes in another year. Significant changes in tax rates create strong incentives to report income and deductions in the wrong year. Significant shifting occurred between 2012 and 2013 due to the rate increases and base changes. There may be variables that indicate auditors identified shifted income. If this is not feasible an underreporting discount may be appropriate, especially for sophisticated taxpayers who are more likely to engage in this type of income shifting.
- Accounting for these issues could significantly affect the results. Johns and Slemrod (2010) removed nearly a fifth of NRP observations due to data issues, such as line switching.

More detailed discussion of sources of uncertainty in estimating underreporting of income would help readers understand the complexity of these issues. Some examples include the following:

- It would help readers and improve confidence if the methodology and sources were clearly explained in the paper or a technical appendix. For some key assumptions, GLRRZ just cites prior papers (which sometimes then cite other papers regarding underlying methods and data).
- Rather than reporting a single "benchmark" number and separately applying a few sensitivity tests, it would be helpful to provide a range of estimates that include all the imputations to emphasize the uncertainty of the "benchmark" results.
- It is possible that the IRS has evidence of additional forms of evasion or underreporting. If there is such evidence on the amounts of income involved, it would benefit readers to be aware of it.
- NRP "detected" underreporting may sometimes <u>*overstates*</u> true underreporting.
  - The application of complex provisions to particular cases is often uncertain and NRP auditors may take overly aggressive positions. Similarly, taxpayers may take overly cautious positions.
  - Lack of adequate records can result in denial of a deduction, even though in some cases the taxpayer had such expenses.
  - Some discussion of such issues would be useful. DCE multipliers could exacerbate overstated "detected" underreporting.
- The "timely filed and reported" standard for the tax gap estimates deserves some discussion. The effects of late-filers and non-filers on estimated misreporting also deserves discussion.