Behavioral Responses to Large, Complex Tax and Transfer Incentives: Evidence from College Financial Aid*

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Abstract

This paper makes two contributions to understanding behavioral responses to tax and transfer incentives and the welfare effects of tax complexity. First, we provide the first estimates of the elasticity of taxable income in the context of one of the largest means tested programs in the US: college financial aid. Using federal tax data and comparing households with college-going versus non-college-going children, we separately estimate substitution effects due to the implicit tax inherent in means testing aid and income effects due to college costs by exploiting variation in the timing of when these forces operate. We estimate an ETI of .07 and a marginal propensity to earn (MPE) of .44. Second, we develop a general sufficient statistics framework for measuring the impact of misperception of a complex tax on welfare and we conduct a survey to identify the model in the case of college aid. Among middle-income families with a child enrolled at a four-year college, misperception of the college aid implicit tax increases deadweight loss by 88% or $119 million annually, which highlights the cost of policy complexity. Notably, despite families underestimating their tax rate on average, the high variance of misperceptions across individuals drives the increase in deadweight loss. (JEL D90, H21, H24)

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I Introduction

Means-testing of college financial aid in the US creates an implicit tax on parent income so large that it frequently doubles the effective marginal tax rate (MTR) a family would face from income taxes alone. This implicit tax arises due to the phaseout of college aid over parent income: each additional dollar a family earns around the time a child is in college will typically reduce the child's college aid by some fraction of that dollar. Families earning anywhere from $40k to over $300k can see their effective MTR increase by 30 percentage points or more due to the implicit tax from college aid, in some cases pushing their all-in MTR to nearly 70%, dramatically reducing the incentive to earn income. At the same time, families also face college costs that can exceed $90k annually, creating competing pressures to increase income to pay these costs. Complicating all of this is the fact that financial aid schedules and college costs are complex and information on these factors is not easily attained. Despite the large tax and transfer incentives involved in college aid and the millions of families affected each year, to date there is scant empirical evidence studying parents' behavioral responses, and no evidence on the elasticity of parent income with respect to the large implicit tax in college aid which dictates the efficiency cost of means testing aid. Beyond the direct policy importance of understanding behavioral responses to these incentives for college aid design, the college aid setting offers unique variation to study fundamental substitution and income effect parameters, and the complexity of college aid represents a fertile ground to study the general question of how misperception of tax rates affects welfare.

This paper makes two primary contributions. First, we provide the first estimates of behavioral responses to the college aid implicit tax and simultaneously estimate responses to college costs. Second, we develop a general sufficient statistics framework for measuring deadweight loss when households misperceive a complex tax and we apply this framework in the college aid setting. In the first part of the paper, we estimate the elasticity of taxable income (ETI) with respect to the implicit tax on parent income from college aid phaseout and the marginal propensity to earn (MPE) with respect to college costs. The college aid implicit tax and college costs create competing incentives to reduce (substitition effect) and to increase (income effect) income. To separately estimate the ETI and the

MPE, we exploit the unique feature that parent income begins to affect college aid before college costs come due, which allows us to separate the substitution effect from the income effect and to produce estimates of both the ETI and the MPE. We estimate an ETI of .07 and an MPE of .44. In the second part of the paper, we consider the general question of how misperception of a complex tax affects deadweight loss. We develop a sufficient statistics framework that highlights several channels through which misperception may impact deadweight loss, and we show that each channel along with the net impact can be identified from survey data on tax rate perceptions that includes a secondary measure of perceptions that can be used as an instrument. Finally, we conduct a survey to measure perceptions of the college aid implicit tax. Using the survey data to identify our model, we estimate that misperception of the college aid implicit tax increases deadweight loss (DWL) by 88% or $119 million annually just among families with a child enrolled at a four-year in-state public or four-year private college. Notably, despite families underestimating their MTR from college aid on average, the substantial variance of misperceptions across individuals drives the increase in DWL.

We begin with an analysis of parent income responses to the college aid implicit tax and college costs. To study parent income responses to college incentives, we construct a novel panel dataset from administrative federal tax records, the ideal data source for this setting. The panel of tax data allows us to measure parent income over time and to identify whether, when, and where a dependent child attends college, which would be rare or impossible in other large administrative data sources. We link parents with their children through dependent claiming on the form 1040, and we merge in information from 1098-T forms to measure when and where a child is in college along with college characteristics including cost of attendance and the extent of aid phaseout over income from the Integrated Postsecondary Education Data System (IPEDS). We then study the trajectory of various sources of parent income reported on form 1040 and W-2 records around the time a child enrolls in college, as well as how these responses relate to household and college characteristics. The rich tax data allows us to investigate which types of households are more and less responsive to college incentives as well as which income sources account for most of the overall adjustments.

Our core empirical approach is to estimate an event study of parent income over child age with cohort and year fixed effects, which we fully interact with an indicator for whether the child enrolls

in college at age 18. We then focus on the college-specific child age effects to reveal parent income responses to college incentives. Parent income may be affected by two competing incentives: the implicit tax in college aid incentivizes parents to reduce income by lowering the marginal net-of-tax rate, while the introduction of college costs may cause parents to increase income due to liquidity constraints or limited forward planning. In order to separate these two effects, we leverage the fact that parent income begins to affect the child's college aid one year before college costs come due. The college-specific age effect in the first year that parent income affects college aid purely isolates the substitution response to the implicit tax. Then, the change in the college-specific age effect from this first year to the years in which college costs are due (and parent income still affects aid) isolates the response to college costs while differencing out the substitution response. In our main results, we focus on a treatment group in which the child enrolls at a 4-year in-state public or a 4-year private college, where the college aid implicit tax is largest, and we also show results for other groups.

Our empirical results show a striking pattern in which parent income falls precisely when it begins to affect a child's college aid and subsequently increases as college costs come due. In the first year in which parent income affects college aid, income is reduced by 1.0% on average. Scaling this by the average change in the net-of-tax rate of 14.5% produces an elasticity of .07. Responses are larger for households with a high share of flexible nonwage income, households with interest income above $500 reflecting relative high assets, and households in which the child attends a college with a higher rate of aid phaseout over income. Just over half of the total response to the implicit tax comes from reductions in W-2 wage income. The remaining half is largely accounted for by reductions in business income and capital gains. Turning to effects of college costs, we find that parent income increases by $1,898 on average by the third year of college. Scaling this by the average total cost of college produces a total wealth effect on annual income of .02, and scaling by the annuitized value of the average total cost of college produces an MPE of .44. Due to the intertemporal nature of the college setting, a useful benchmark for our ETI estimate comes from Martínez et al. (2021), who estimate the intertemporal elasticity of labor income by exploiting a "tax holiday" in which income is temporarily not subject to an income tax in Switzerland. Martínez et al. (2021) find an overall wage elasticity of .025. Our ETI of .07 is somewhat higher, which may be in part because we focus on the broader taxable income

elasticity measure that is relevant for welfare in our setting. Our MPE estimates are similar to recent estimates from lottery winners (Cesarini et al. 2017; Golosov et al. 2024) and student loan forgiveness (Dinerstein et al. 2025), which is of note because college costs can potentially be anticipated and smoothed in a lifecycle model. The fact that our MPE estimates are similar to estimates exploiting unanticipated wealth shocks suggests that college costs are not smoothed, perhaps due to liquidity constraints or limited forward planning.

We address two primary identification concerns. Regarding the ETI, one might argue that college enrollment is endogenous to preceding income shocks, which could impact or explain our results. Several pieces of evidence suggest this is not an important confounder. First, there is compelling evidence that exploits exogenous variation in parent income due to lottery wins (Bulman et al. 2021) and to layoffs (Hilger 2016) when a child is nearing college age, which finds little causal impact of shocks to parent income in this time period on a child's college enrollment. Moreover, to the extent that these effects exist, they would oppose the results we find: a positive (negative) shock to parent income when a child is nearing college age has a small positive (negative) effect on enrollment, which would produce a positive correlation between preceding parent income and a child's college enrollment, whereas we find a negative relationship. Second, if the negative relationship we find were importantly influenced by selection into college among those who experience a negative shock to income, this would produce an apparent increase of income among the remaining households in which the child does not select into college. However, in the tax data, we find very smooth trends of income in the period before a child turns 18 for households in which the child does not enroll in college, which both suggests that endogenous enrollment is not important in explaining our results and reinforces the findings of Bulman et al. (2021) and Hilger (2016) that shocks to parent income when a child is nearing college age do not have a large impact on college enrollment. Regarding the MPE, one might argue that changes in parent income when a child enrolls in college are due to the child moving out rather than college costs. The investigate this, we estimate the MPE separately for families with no younger sibling and families with a younger sibling that would still be in the home while the older child is in college. The estimates cannot be distinguished statistically, while the point estimates show that two-thirds of our MPE estimates remain even for households with a younger sibling still in the

home.

Next, we investigate the general question of how misperception of a complex tax may impact deadweight loss (DWL). We develop a sufficient statistics framework that reveals several channels through which misperception impacts DWL: DWL increases with the average misperception, the variance of misperceptions across individuals, and the covariances of (squared) misperceptions with an individual's income, elasticity, and tax rate. Each of these components along with the overall impact of misperception on DWL is potentially identifiable with survey data that directly measures the joint distribution of perceptions, income, elasticity, and tax rate; however, surveying individuals to obtain their perceived tax rate may produce a noisy measure of perceptions. We prove that each component and the total impact of misperception on DWL can still be identified if the survey includes a secondary measure of perceptions that can be used as an instrument and if the researcher accounts for identifiable bias correction terms. We additionally show that the structural elasticity with respect to the perceived tax, which determines welfare, can be identified by combining survey data on perceptions and the reduced form elasticity with respect to the true tax.

To apply our sufficient statistics framework for measuring the impact of tax rate misperception in the case of the college aid implicit tax, we administer a survey to parents of children who are college age and younger to measure their perceptions of the implicit tax rate they face from college aid. Our survey includes a direct measure of the respondent's perceived MTR from aid which we can compare against corresponding true statistics to measure individual-level misperception, and it also includes a secondary measure of perceptions that we use as an instrument.

We find evidence of substantial misperception of MTRs in college aid and that this misperception dramatically *increases* DWL. Notably, individuals underestimate their MTR from college aid on average, which alone would reduce DWL; however, despite the average underestimation of MTRs, on net misperception increases DWL due to the large variance of misperceptions across individuals. This pattern reinforces the emerging theme in behavioral public finance that variance in behavioral frictions can be more consequential for welfare than the average friction, as recently shown for sales tax salience (Taubinsky and Rees-Jones 2018) and nudges (Allcott et al. 2025). On average, individuals underestimate their MTR from college aid, which reduces DWL by 22%. However, the variance of

misperceptions across individuals overshadows the average underestimation of tax rates, so that on net misperception increases DWL substantially. Prior to accounting for covariances, the variance of misperceptions across individuals increases DWL by 140%. There is a negative covariance between (squared) misperception and tax rate that reduces DWL by 30%. On net, misperception increases DWL by 88%. We do not find statistical evidence of an impact on DWL due to correlations between misperception and income or elasticity. In total, combining our ETI estimates from tax data with our survey results on misperception, we find that the college aid implicit tax creates annual DWL of $254 million, of which $119 million is due to misperception, demonstrating the welfare cost of tax policy complexity. Our DWL estimates are restricted to middle-income families with a child enrolled at a four-year in-state public college or a four-year private college, and as such are almost certainly underestimates of total DWL due to the college aid implicit tax and the contribution from misperception when including lower and higher income families and those with a child enrolled at a two-year college or a four-year out-of-state public college.

This paper contributes to three main literatures. First, this paper builds on and bridges two strands of research related to college aid and prices: those studying the impact of implicit tax and cost components on income and savings decisions, and those studying the implications of complexity and opaqueness in college aid. It has been observed for decades that college aid is structured as an implicit tax on income and savings (Case and McPherson 1986; Edlin 1993; Dick and Edlin 1997). Several early papers focused on the elasticity of savings with respect to college aid incentives (Feldstein 1995a; Long 2004; Monks 2004). Two recent papers have studied whether students change their labor supply during college in response to similar aid incentives placed on student income (Darolia 2017; Park and Scott-Clayton 2018). Despite the long acknowledgement of these incentives, ours is the first paper to estimate the elasticity of parent income with respect to the college aid implicit tax. Parent income explains the majority of variation in college aid. Quasi-experimental estimates of the elasticity of parent income are therefore necessary for evaluating the tradeoff between efficiency and equity or insurance value inherent in means tested college aid and to build upon existing assessments of the optimal aid schedule (e.g. Colas et al. 2021; Fan et al. 2025). Our estimates of the income effect of college costs on parent income relate to two earlier papers (Handwerker 2011; Braga and Malkova

2024), where ours is the first to use large administrative data and to link children to the college they attend. Additionally, our analysis of misperception of the implicit tax in college aid establishes a connection between the literature on college aid incentives and the literature on implications of college aid complexity and opaqueness. Several papers have documented that information frictions related to college aid are important enough as to have considerable impacts on students' college application and enrollment choices (Dynarski and Scott-Clayton 2006; Bettinger et al. 2012; Hoxby and Avery 2012; Hoxby and Turner 2013; Dynarski et al. 2021). Our analysis applies the study of information frictions in college aid to the implicit tax context, finding consistent evidence of substantial misperceptions of the aid schedule with important welfare implications.

Second, this paper builds on the long literature studying substitution and income effects of tax and transfer systems (Mirrlees 1971; Ashenfelter and Plant 1990; Feldstein 1995b; Auerbach et al. 2000; Goolsbee 2000; Gruber and Saez 2002; Eissa and Hoynes 2006; Saez et al. 2012; Chetty et al. 2013; Kleven and Schultz 2014; Martínez et al. 2021). Much of this literature focuses on either very low income families (e.g. responses to social programs like the Earned Income Tax Credit (EITC)) or very high income families (e.g. executive compensation tax). Our paper provides one of few estimates of the ETI targeted to middle income households, as marginal implicit tax rates from college aid are concentrated among households earning between $50k and $150k. With the federal tax data, we are able to provide a detailed analysis of which types of income are more and less responsive to tax and transfer incentives. A further rare feature of our paper is the ability to separately estimate substitution effects (ETI) and income effects (MPE) using the same policy variation, where existing papers often isolate one or the other with reduced form variation.[1]

Third, this paper makes theoretical and empirical contributions to the growing literature on information frictions and behavioral responses to taxes. Many tax systems, especially implicit taxes in means tested social programs, are nonlinear and complex. If individuals misperceive their marginal

[1] Note that we use "income effect" to refer to a shock to wealth, as opposed to the effect of virtual income in a tax change that distinguishes compensated from uncompensated elasticities. Papers focused on substitution effects often difference out or assume away such income effects. Papers focused on income effects often isolate pure wealth shocks such as those from lottery wins or student loan forgiveness (Cesarini et al. 2017; Golosov et al. 2024; Dinerstein et al. 2025). Cesarini et al. (2017) provide estimates of income and substitution parameters by combining reduced form wealth shocks with a structural model, whereas our estimates exploit two levers of independent reduced form variation.

tax rate (MTR) and make income decisions based on their perception, this may have important effects on the efficiency cost of taxation and thereby alter optimal tax policy (Rees-Jones and Taubinsky 2020; Farhi and Gabaix 2020; Craig and Slemrod 2024).[2] Existing papers studying misperception fall into two camps: survey evidence directly measuring perceptions of taxes, which finds that individuals often misperceive their MTR, particularly in the context of a nonlinear tax schedule (Fujii and Hawley 1988; Rees-Jones and Taubinsky 2020); or reduced form evidence on behavior, which finds patterns consistent with misperception of MTRs (and marginal prices) impacting behavior, especially in nonlinear settings (de Bartolome 1995; Liebman and Zeckhauser 2004; Ito 2014; Liebman and Luttmer 2015; Feldman et al. 2016; Kostøl and Myhre 2021). These papers often impose structural forms of misperception – such as responding to one's average, as opposed to marginal, tax rate – and in most settings misperception is found to reduce DWL as individuals tend to underestimate their MTR. Our paper introduces a novel sufficient statistics framework that can be used to study the impact of misperception of any complex tax without imposing structure on misperceptions, demonstrates how to identify the impact of misperception on DWL by combining survey data on perceptions with reduced form ETI estimates, and applies these insights to the case of college aid with surprising results.[3] Our model reveals that DWL is not only impacted by the average misperception, but also by the variance of misperceptions and the covariance of squared misperceptions with income, elasticity, and tax rate. Our empirical results are the first to find and quantify a large *increase* in DWL due to misperception, counter to earlier results in which structural forms of misperception tend to reduce DWL. Particularly striking in this context is our finding that, despite individuals underestimating their MTR from college aid on average, the variance of misperceptions across individuals is so large as to produce a net increase in DWL of 88%. While recent evidence suggests that individuals exert costly effort to more accurately learn tax incentives when the stakes are higher (Morrison and Taubinsky 2023), our finding

[2] We make the distinction between *misperception* and *salience*. A tax is misperceived when some individuals think they face a marginal tax rate that is different from their true marginal tax rate. A tax is not fully salient when some individuals do not consider the tax when making choices. Several important papers in behavioral public finance have studied the role of salience, including Chetty et al. (2009), Goldin (2015), Taubinsky and Rees-Jones (2018), and Kroft et al. (2024) Our paper focuses on the issue of misperception in an empirical setting that is high-stakes and as a result likely to be salient to most, but in which the implicit tax rates are complex and information sources for learning one's marginal implicit tax rate are scarce, so that there is substantial scope for individuals to misperceive their tax rates.

[3] Our model and empirical approach to measuring the impact of misperception on DWL are precisely the kind of methodological advances advocated for in Rees-Jones and Taubinsky (2018) and Farhi and Gabaix (2020)

of substantial misperception in the high stakes setting of college aid demonstrates that policy complexity may significantly impact welfare even when individuals face strong incentives to overcome behavioral frictions.

The remainder of the paper proceeds as follows. Section II describes the large and complex implicit tax in college aid. Section III presents our empirical analysis of substitution and income effects in response to college incentives using federal tax data. Section IV presents a new sufficient statistics approach to measuring the impact of tax rate misperception on DWL along with results from implementing this method by combining our reduced form elasticity estimates from Section III with survey measures of misperceptions. Section V concludes.

II Background

II.A The Large Implicit Tax on Income from College Financial Aid in the US

Most students who attend college in the US do not pay the full "sticker cost". A college's sticker cost is the published annual cost to attend that college, which includes tuition and fees, housing, and other important costs like books, supplies, and transportation. Most students receive financial aid that lowers the effective cost they pay. Financial aid comprises grants or scholarships – money that covers college costs and does not need to be paid back – along with loans that are typically offered on favorable terms. Financial aid is available to students through sources including the federal government, state governments, individual colleges, and private entities. In the remainder of this paper, we will use the terms "financial aid" and "aid" to refer only to grants and scholarships offered by the federal government, state governments, and individual colleges, as this is the component that creates high-stakes incentives over income earned by the parents of college students.

The financial aid a student receives is heavily means-tested against the income of their parents, so that two students with parents earning different incomes can pay vastly different net costs to attend the same college. Figure I displays the total grant aid a student can expect to receive at the University of Pennsylvania (in red), a private four-year university, or at UC Berkeley (in blue), a public four-year

university, per year of enrollment based on their parents' pre-tax income. The information in Figure I is based on the Net Price Calculator (NPC) page on each university's financial aid website.[4] The annual total cost of attendance at the University of Pennsylvania (UC Berkeley) – including tuition and fees, housing, and other expenses – is approximately \$94k (\$47k). According to the respective NPC websites, students whose parent pre-tax income is below approximately \$50k can expect to receive approximately \$91k (\$37k) in grants per year, cutting their annual cost of attendance down to \$3k (\$10k). Beginning at incomes slightly above \$50k, aid is gradually phased out until it reaches zero for students whose parent income is above roughly \$350k (\$150k), in which case the family's annual net cost is equal to the full sticker cost of \$94k (\$47k).

Means testing of financial aid over parent income creates a strong incentive for middle- and higher-income families with a child in college to reduce reported income. Figure I labels the marginal implicit tax rates on parent income that occur due to the phaseout of college aid. In the case of the University of Pennsylvania (UC Berkeley), for families with income between \$50k and \$350k (\$150k), each additional dollar of reported income reduces the financial aid award (and thus increases the family's net cost) by 14 to 35 cents (16 to 33 cents). This phaseout of aid is akin to implicitly taxing income at the margin during each year of income reported on a student's Free Application for Federal Student Aid (FAFSA), with marginal implicit tax rates ranging from 14% to 35% (16% to 33%).

The implicit tax on income from college aid is large relative to other means-tested programs in terms of both the magnitude of marginal implicit tax rates and the wide range of income levels subject to them. For comparison, Figure I also shows the Earned Income Tax Credit (EITC) schedule for a family with one child. The EITC phases out at a marginal implicit tax rate of 16%. Phaseout of the EITC begins at incomes just below \$30k and extends up to approximately \$50k. In comparison, college aid phaseout applies larger marginal implicit tax rates to a range of income that can extend

[4] All colleges in the US that participate in Title IV financial aid programs – allowing their students to access federal grants, federal student loans, and federal work-study to assist in paying for college – are required to host a NPC website that allows students to enter several aspects of their family background and receive a tailored estimate of the financial aid they will receive and the net cost they will pay. As discussed in Section II.B, however, information on the broader aid schedule over income like that shown in Figure I can only be obtained by repeatedly entering different inputs to the NPC and recording the various aid estimates. Additionally, as discussed in Section II.B, college aid varies not only with income but also with family savings, family structure, and other family characteristics. The information in Figure I was produced by holding these other inputs fixed.

roughly three (UC Berkeley) to over ten (University of Pennsylvania) times as wide as that affected by EITC phaseout. While the impact of the phase in and phaseout regions of the EITC on labor supply and income has been the subject of a long literature (see e.g. Eissa and Hoynes 2006; Chetty et al. 2013), there are currently no estimates of the response of parents' labor supply or income to the implicit tax in college financial aid.

The marginal implicit tax from college financial aid adds on top of other marginal incentives a family faces on their income such as federal and state income taxes and FICA, so that middle- and higher-income families with a child in college can face an all-in marginal tax on income reaching up to 70%. Figure II shows the marginal net-of-tax rate for a family earning $125k around the time they send a child to college at UC Berkeley. In the years when parent income does not affect the child's college aid, the family keeps 64 cents out of the marginal dollar earned (or, their effective marginal tax rate is 36%). In the years when parent income affects college aid, the family keeps only 31 cents of the marginal dollar earned (or, their effective marginal tax rate is 69%), after accounting for a 33% marginal implicit tax from college aid phaseout. This dramatically reduces the incentive for families to earn income exactly in the years that affect a child's college aid. Figure II also demonstrates the intertemporal nature of the incentives created by college aid phaseout, as the incentive to reduce income occurs precisely during the years that affect college aid. As a result, the elasticity that dictates behavioral responses will be intertemporal in nature. Our estimates will therefore be comparable with recent estimates from "tax holiday" settings (Tortarolo et al. 2020; Martínez et al. 2021) in which brief periods were not subject to a federal income tax in countries including Argentina and Switzerland. College aid functions like an "anti- tax holiday" during which the effective marginal tax rate is temporarily elevated.

The large implicit tax from college aid applies nationwide and is steadily increasing at middle and higher incomes. Figure III shows trends in nationwide averages of the marginal implicit tax rate from college aid across income ranges and college types using data from IPEDS, which is published by the Department of Education.[5] For colleges across the US, IPEDS annually records the average financial

[5] Using the same data fixed for the 2019-20 academic year, Figure A.2 shows average aid and MTRs from aid across the US by institution type.

aid for students with total family income in each of five income bins: $0-30k, $30-48k, $48-75k, $75-110k, and above $110k. Using this data, we construct estimates of marginal implicit tax rates from college aid for adjacent income bins by dividing the change in average aid by the difference in medians of each income range.[6] We then compute averages of these MTRs by college type and year, weighted by total fall enrollment of full-time, first-time, first-year, degree-seeking undergraduates. From 2008 to 2022,[7] MTRs from college aid have decreased slightly at low incomes while remaining large at middle incomes and increasing considerably at higher incomes with no signs of reversing trend. Panel A shows a gradual decline in the average MTR from aid at low incomes from $0-30k to $30-48k, consistent with colleges extending the threshold below which students receive maximum aid and face no phaseout. As shown in panel B, for incomes from $30-48k to $48-75k where college aid begins to meaningfully phase out, average MTRs from aid have sat consistently around 15% for 4-year public and private[8] colleges and around 10% for 2-year public colleges. As college costs have increased over this period, so too have the range of incomes exposed to college aid phaseout expanded and the average MTR they face climbed upward. Panel C shows that, for income in the $48-75k to $75-110k range, the average MTR from college aid has jumped from 11% to 17% at 4-year privates, from 7% to 15% at 4-year publics, and from 3% to 8% at 2-year publics. Panel D shows that, for higher incomes from $75-110k to $110k and above, the average MTR from aid has doubled at 4-year privates and increased by even larger factors at 4-year and 2-year publics.[9] An important trend related to these patterns is the steady growth in college cost of attendance, as seen in Figure A.1. As college cost increases, the maximum aid offered at low incomes rises similarly, and in turn aid phases out over a wider range of income at potentially higher rates.[10] Given that the persistent increasing trend in college cost appears likely to continue, and following the steady increases in college aid MTRs at middle and higher incomes seen in Figure III, the college aid implicit tax is likely to continue growing

[6] For the top bin of $110k and above, we use $150k. The levels of the estimated implicit marginal tax rates will be sensitive to which value we choose. As a result, we focus only on trends in the college aid MTR for the range from $75-110k to $110k+.

[7] We refer to each academic year based on the fall term. For example, we refer to the 2008-2009 academic year as 2008. 2008 is the earliest year for which IPEDS records average grant aid separately by family income bins. At the time of writing, 2022 is the most recently available data.

[8] Throughout, we will use "4-year private" to refer only to 4-year nonprofit private colleges.

[9] We do not interpret the MTR levels for the $75-110k to $110k and above range. See footnote 6.

[10] Cook and Turner (2022) refer to this joint phenomenon as "high-tuition, high-aid pricing".

in prevalence.

Finally, the college aid implicit tax affects millions of families each year, with billions of dollars being allocated under this mechanism. As of fall 2021, there were 8 million students age 24 and under enrolled full-time as undergraduates at degree-granting institutions (National Center for Education Statistics 2024). Of these, 4.8 million attended a 4-year public, 1.9 million attended a 4-year (nonprofit) private, and 1.1 million attended a 2-year public college. As of 2022, a total of $117.2 billion in grant aid was awarded to undergraduates from federal ($38 billion), state ($14.7 billion), and institutional ($64.5 billion) sources (College Board 2024). As a reference point, in tax year 2022, 23 million workers and families received a total $57 billion from the EITC (Internal Revenue Service 2023).

II.B The College Aid Implicit Tax is Complex and Difficult to Learn

College financial aid is remarkably complex. The complexities involved in financial aid are well exemplified in the federal Expected Family Contribution (EFC) formula, which determines federal Pell grant eligibility and is an important reference point in state and institutional aid.[11] The EFC formula, and aid generally, is highly nonlinear, including interactions between parent income and other inputs like assets, student income, and more.[12] The EFC formula also includes several important kinks and notches, each of which varies by characteristics like parent age or number of people in the household and often changes from one year to the next.[13] Figure IV shows a brief excerpt from the

[11] Starting with the 2024 academic year, EFC was replaced with the Student Aid Index (SAI). SAI does not significantly alter the relationship between family income and college aid described in this section. In fact, one important aspect of the SAI will dramatically increase the MTR families face from college aid. When a family has multiple children in college at the same time, the EFC formula divided by number of children enrolled, which effectively meant that a family with one versus two children enrolled at the same college often faced the same overall MTR from aid, with some exceptions. The SAI no longer divides by number enrolled, meaning a family with multiple children in college simultaneously will face a total MTR from college aid that cumulates across children. In theory, it is not difficult for some families to face all-in MTRs over 100% after combining income tax rates and the cumulated MTR from college aid over multiple children under the new SAI formula.

[12] Figure I holds other inputs fixed and only varies parent income and associated income taxes.

[13] For example, parent income below the Income Protection Allowance (IPA) does not affect EFC, and the IPA varies by the size of the household. Parent assets below the Asset Protection Allowance are similarly excluded from the EFC, and this amount varies by the number of parents in the household and the age of the older parent. Families with total parent income below a certain threshold who also meet an additional set of criteria do not need to report any information on assets, creating a notch whereby these families escape the asset tax component of aid calculations altogether.

2015 EFC formula, reflecting the various inputs, allowance amounts calculated from tables not shown in the figure, conversion rates, and more that ultimately determine EFC and impact aid. Adding to the complexity of aid is the fact that state and institutional aid may deviate from the EFC formula in unclear ways and such differences may vary across institutions. As a result of the significant degree of complexity in aid, it is exceedingly difficult for an individual student or their family to determine precisely the marginal implicit tax they face on parent income or any other determinant of aid.

Information sources available for learning one's MTR from college aid are not transparent and may even add to confusion. One potentially common way for parents to learn their MTR from aid would be through online news articles. Figure V shows excerpts from two online news articles describing how families can maximize their financial aid. Both excerpts make it clear to understand that there is an incentive to temporarily reduce income with section headers reading "Postpone parental income" and "Minimize income in the base year." However, rather than list the marginal implicit tax rates that families in different income ranges might face, the first piece instead describes the average tax rate (in this context, the percent of income a family is expected to pay toward college, but not at the margin), while the second piece only lists what is roughly the maximum marginal implicit tax rate of 30%. For many families, these numbers will not accurately reflect the MTR they will face from college aid, so articles like these may even exacerbate misperception. Perhaps the most effective way for parents to learn the MTR they should expect to face from college aid is to use a college's Net Price Calculator (NPC) website, but this requires a degree of savvy. All colleges that receive Title IV federal funds must host an NPC, where families can enter information on their income, assets, and other aid inputs and receive an estimate of their financial aid and net price. Colleges do not typically post images or information directly conveying the content of the aid schedule over income as in Figure I on their financial aid websites; however, if a family is sufficiently savvy, they may think to navigate to a college's NPC, enter one set of income, assets, and other inputs and record the aid estimate that is produced, and then re-enter their inputs with a small change to income and record how predicted aid changes. [14] Even this method can prove difficult, as some colleges use a template for their NPC website that only allows the user to specify one of several income ranges, rather than inputting an

[14] In fact, this is how we produced Figure I.

exact income amount. In sum, due to the numerous barriers discussed above, it is unlikely that parents of college students know the marginal implicit tax rate from college aid that they face when making income decisions.

II.C Timing

Our empirical analysis of behavioral responses to college aid incentives in Section III relies on the timing at which parent income begins affecting a child's college aid to separate the substitution effect due to the implicit tax in college aid from the income effect due to paying college costs. Figure VI details the timeline features that allow us to isolate the substitution effect from the income effect. For each year when the oldest child is age 16 to 23, the figure shows three features of the corresponding calendar year: the matched academic year, spanning from spring of one academic year to fall of the next; whether parent income earned in that calendar year affects college aid; and whether college bills are due. Both in Figure VI and in the analysis of Section III, we define child age as of September 1 to align with academic years. The timeline in Figure VI focuses on cases in which the child enrolls in college immediately following high school and is enrolled for four years, consistent with our analysis in Section III that is based on college enrollment beginning at age 18.

As seen in Figure VI, the first calendar year in which parent income affects the child's college aid occurs when the child is age 17 (as of September 1), which spans from the spring of the child's third year in high school to the fall of the child's fourth year in high school.[15] Crucially, while income earned in this year affects the child's aid and is therefore subject to the implicit tax (substitution effect), the family has not yet started paying for college (income effect).[16] We have shaded this box red to

[15] Beginning with the 2017-18 academic year, the reference year of income used to determine college aid has shifted back by one year, a policy change known as "prior-prior". This means that the first year in which parent income affects a child's college aid, for a child who enrolls in college immediately after high school, is now the year in which the child is age 16. Our empirical analysis focuses on cohorts before prior-prior was introduced so that we can restrict to pre-Covid income measures. Our survey data described later in the paper includes a question on when respondents currently believe income affects college aid, i.e. several years after prior-prior has been in effect. Interestingly, only 5% of respondents identify the correct year, while 76% think it is the year that would have affected aid before prior-prior was implemented, and 18% think it is the year after that. This evidence indicates that the switch to prior-prior may have been efficient in the sense of reducing income responses to the college aid implicit tax by lowering salience.

[16] Moreover, the child has not yet moved out of the house to attend college, which could independently impact parent income.

indicate that this period is purely subject to the substitution effect. Our empirical estimates of the ETI with respect to the implicit tax in college aid in Section III focus on parent income when the child is age 17 to isolate the substitution response. College expenses first come due during the following year in which the child is 18. We have shaded this and subsequent boxes in Figure VI blue to indicate that these periods are also subject to the income effect. In Section III, we estimate the MPE by comparing parent income when the child is age 20 against when the child is age 17. Income earned in both of these years affects college aid and is therefore impacted by the substitution effect,[17] while only income earned when the child is age 20 is impacted by the income effect, so the difference is reflective of the MPE.[18]

III Empirical Analysis of Substitution and Income Effects due to College Aid

III.A Data

1 Linking Parents and Children

We begin by defining families and their associated children.[19] We define a family as the unique combination of a primary and, if applicable, secondary parent who claimed a child. We identify links between each child and their parent(s) by collating all linkages between primary and secondary filers and the dependents listed on all tax returns. The dataset consists of all children who (1) have a valid Social Security Number (SSN) or Individual Taxpayer Identification Number (ITIN), (2) were

[17] Students must re-apply for financial aid each year, with each year of aid being determined by a subsequent year of income. In Figure VI, income earned when the child is 17 affects aid in the first year of college, income earned when the child is 18 affects aid in the second year of college, income earned when the child is 19 affects aid in the third year of college, and income earned when the child is 20 affects aid in the fourth year of college.

[18] In theory, one could also exploit variation in exposure to the college aid implicit tax and to contemporaneous college bills in the following years. However, if either effect has persistence, for example due to frictions in labor market adjustments, then later year comparisons will not identify the ETI or MPE correctly. As a result, we do not pursue this variation, but instead focus on variation at the introduction of substitution and income effects.

[19] Technically, we measure tax units and their claimed dependents. For ease of reading and consistent with prior work using tax data to study filer-dependent relationships (e.g. Chetty et al. (2020)), we use the terms "family", "parent", and "child" throughout this paper.

claimed by only one family at age 14, (3) were the family's oldest child, and (4) were born between 1985 and 2004. There are approximately 29 million families whose oldest child meets the criteria in the balanced panel. The child is assigned to the family that claimed the child when the child was 14. We exclude any child claimed by multiple families at age 14, along with all associated families. This restriction lowers the number of families in the sample by 2.8 million, or 4%.

A family's oldest child is identified by selecting the oldest date of birth of any child assigned to the family. We exclude families whose oldest child is one of multiple children with the same date of birth (e.g., twins or triplets) or has a sibling born fewer than 12 months later. This restriction lowers the number of families in the sample by 0.8 million or 1.2%.[20]

2 College Attendance and College Characteristics

We measure college attendance based on the presence of the information return Form 1098-T, "Tuition Statement". The university files the 1098-T and includes such information as payments received, scholarships and grants, whether the student was at-least-part-time, and whether the student was a graduate student. We only count college attendance based on the presence of a 1098-T that records at-least-part-time undergraduate enrollment. If a student received at-least-part-time undergraduate 1098-Ts from multiple different institutions for the same tax year, we randomly select the institution to assign to the student.

A 1098-T is filed for almost every student, with some notable exceptions including individuals only taking non-credit courses and nonresident aliens. Additionally, schools are not required to file a 1098-T for any student whose tuition is paid entirely by a third-party, paid entirely by scholarships or grants, or was waived. Cronin and Gray-Hancuch (2024) find a majority of universities in 2017 file for every student, ignoring these exceptions. These institutions constitute 68% of all institutions and 65% of the undergraduate population with no clear differentiation between the institutions that file for every student and those that do not on cost-of-attendance, percent of undergraduates with scholarships or grants, average undergraduate scholarships or grants, and average Pell Grants.

[20] Due to computational restrictions, families with four or more children of the same age are also excluded from the dataset, even if these are not the oldest children in the family. This restriction lowers the number of families in the sample by approximately 1,000, or 0.002%.

Once the student is assigned to an institution for a given year, we use a crosswalk from Cronin and Gray-Hancuch (2024) to match the institution found on the 1098-T with the institution's observation in the Integrated Postsecondary Education Data System (IPEDS) from the National Center for Education Statistics for the 2017-18 academic year, which allows us to merge in college characteristics. From IPEDS, we measure the sector (2-year or 4-year, public or private, not-for-profit or for-profit) and state of the college along with information on total cost of attendance (COA) and average net price and financial aid among several family income ranges. We use sector and state to define enrollment types (for example, four-year in-state public). Regarding costs and financial aid, IPEDS records total COA and average net price and average financial aid for students with family income in each of five income ranges: \$0-30k, \$30-48k, \$48-75k, \$75-110k, and above \$110k. For our main analysis sample that has baseline income averaged over 2005 and 2006 between \$50k and \$150k, we assign a family-specific MTR from college aid for families with baseline income between \$50k and \$75k as the difference in average financial aid across the \$30-48k and \$48-75k income ranges divided by the difference in the median incomes of these ranges, and we assign an MTR for families with baseline income between \$75-150k as the difference in average aid across the \$48-75k and \$75-110k income ranges divided by the corresponding difference in median incomes. We assign a family-specific net price for families with baseline income between \$50k and \$75k using the average net price in the \$48-75k range; for families with baseline income between \$75 and \$110k using the average net price in the \$75-110k range; and for families with baseline income between \$110k and \$150k using the average net price for families with income above \$110k.

3 Income and Family Characteristics

We use information from Form 1040 and its related schedules from 2005 to 2018 as our income measures. We supplement this data with measures of wage income from the information return Form W-2. To avoid undue influence from outliers, we winsorize all income measures to their 99.9 and 0.1 percentiles. We also calculate ages for family members other than the oldest child, including parents and other associated children, to provide additional information about the family structure.

4 Main Analysis Sample Definition

We focus our analysis on families meeting the following criteria: baseline income, measured as average AGI in years 2005 and 2006, is between \$50k and \$150k, the range that is most exposed to the college aid implicit tax; taxes are filed each year from 2005 to 2018 with positive AGI[21]; the oldest child turns 18 between 2011 and 2016; the oldest child does not attend college at age 17; and the oldest child either attends no college at age 18 ("non-college families") or attends either a four-year in-state public college or a four-year private college at age 18, where we are able to measure average net price at the college for families in the \$48-75k, \$75-110k, and above \$110k income ranges and average aid for families in the \$30-48k, \$48-75k, and \$75-110k ranges in order to assign net price and MTR from college aid within our sample ("college families"). We relax each of these restrictions in heterogeneity and robustness analysis.

Table I shows descriptive statistics for our main analysis sample at the family level. 1.8 million families are included in our main analysis sample. Average income in the baseline years 2005 and 2006 is \$86k, of which the average income from W-2 wages accounts for \$78k. The baseline share of income from nonwage sources and the baseline taxable interest income are heavily skewed: the median share of nonwage income is 1% while the average share is 9%, and the median income from interest is \$40 while the average is \$479. The median age of the primary filer is 47 when the oldest child is 17. Among families with only one or two W-2 earners in both 2005 and 2006, 65% have two earners. 38% are treated, or "college", families. Among college families, the average MTR from college aid is 13% with a standard deviation of 8%, the average COA is \$37k, and the average net price is \$22k.

[21] Because we assign children to parents based on claiming on tax forms when the child is age 14, our sample necessarily conditions on filing when the child is age 14. As shown in robustness results below, when we do not condition the sample on filing in remaining years, there is differential mean reversion of filing patterns between college and non-college families, making it difficult to identify effects of college incentives. Nevertheless, we show that samples that do not restrict on filing behavior (beyond child claiming at age 14) or that restrict to persistent filing but without any restrictions on AGI produce similar patterns to our main results.

III.B Empirical Design

In our main specification, we estimate an event study of parent income over the oldest child's age around the time the child turns 18, including full interactions with an indicator for whether or not the child enrolls in college at age 18. This can be seen in Equation (1).

$$Y_{it} = \underbrace{(\alpha_{c(i)} + Col_i \cdot \gamma_{c(i)})}_{\text{Cohort (year child turns 18) effects}} + \underbrace{(\delta_t + Col_i \cdot \lambda_t)}_{\text{Year effects}} + \sum_{k=12}^{23} \underbrace{(\theta_k + Col_i \cdot \beta_k)}_{\text{Child age effects}} D_{it}^k + \varepsilon_{it} \quad (1)$$

In Equation (1), Y_{it} is income for family i in year t; Col_i is an indicator for a family in which the child attends college at age 18 (the "college" group, as opposed to the "non-college" group); $\alpha_{c(i)}$ and $\gamma_{c(i)}$ are cohort (year child turns 18) fixed effects, with $\gamma_{c(i)}$ reflecting any differential cohort effect for families in which the child attends college; δ_t and λ_t are year fixed effects, with λ_t reflecting any differential year effect for families in which the child attends college; D_{it}^k is an indicator for child age being equal to k; θ_k is the child age effect for the control group in which the child does not attend college; and β_k is the differential child age effect for the treated group in which the child does attend college. In our main estimates, we restrict the sample to families in which the child attends a 4-year in-state public college or a 4-year private college at age 18 or attends no college at age 18. We cluster standard errors by family.

When estimating Equation (1), we normalize all age effects at 12 and 15 to zero. Within each of the college and the non-college groups, cohort, year, and child age are collinear; as a result, in our fully interacted model, we must normalize against two values for one of these three variables within each group.[22] This approach additionally allows for the college and non-college groups to be on different linear pre-trends, with our estimates being relative to any differential linear pre-trend. With this context, the identifying assumption for Equation (1) is that, in the absence of college, the college group's income relative to their trajectory over child ages 12 to 15 would have evolved as the non-college group's income relative to their trajectory over child ages 12 to 15. In the following results, all

[22] See discussion of this issue in a "timing-based" data structure in Miller (2023) as well as Proposition 1 of Borusyak et al. (2024)

estimates based on Equation (1) that include college and non-college families should be interpreted as differences relative to the (differential) linear trend extrapolated from ages 12 to 15; all estimates that include only college or only non-college families should be interpreted as differences relative to the linear trend extrapolated from ages 12 to 15 within that group.

III.C Results

1 Income around College

We begin by documenting patterns of family income over child age in raw conditional means of the data. This serves to increase transparency for our main event study estimates, as the key patterns reflected in those estimates can also be observed directly in conditional means.

Separately for each cohort and within each of the non-college and the college groups, we estimate simple regressions of parent log AGI on indicators for each year, omitting 2008 as the reference year. These estimates capture conditional means of log AGI across years, relative to 2008, by cohort and college status. Figure VII shows the results.[23] Panel A shows estimates for the non-college group, and Panel B shows estimates for the college group. In each figure, a filled-in circle indicates the first year in which income would affect a child's college aid for each cohort.

Several patterns are apparent in the raw conditional means reported in Figure VII. Among the non-college group in Panel A, income is increasing on average after 2009, with a gap opening up across cohorts over time. The emergence of this gap is consistent with a lifecycle profile of earnings in which income grows faster (slower) for younger (older) parents: earlier cohorts have older parents on average, and they see slower income growth than later cohorts who have younger parents on average. For the non-college group in Panel A, income in the first year that would have affected college aid had they enrolled (represented by a filled-in circle for each cohort) is exactly in line with these broader trends, suggesting no systematic changes to income in the first aid base year for the control group. By contrast, the college group in Panel B shows clear evidence of income falling precisely when it begins to affect college aid. The same broad trends of increasing income after 2009 and a divergence

[23] Patterns with AGI as the outcome are shown in Figure A.3. Values are shown in Table B.1 and Table B.2.

across cohorts are apparent for the college group in Panel B. However, for the college group, there is a consistent pattern across all cohorts in which income in the first year that affects college aid (represented by a filled-in circle for each cohort) falls below these trends by about 1%. Additionally, for each cohort, income increases in the subsequent year (in which year equals cohort) and remains elevated thereafter. In sum, the conditional means in Figure VII suggest two key patterns that are unique to the college group: (i) income falls by about 1% relative to trend in the first year in which income affects college aid, and (ii) income then increases in the subsequent year, which is the first year in which college costs are due, and remains elevated thereafter.

We now turn to estimating the interacted event study in Equation (1), which simply collapses the information in Figure VII to a smaller set of digestible parameters. Figure VIII shows estimates of β_k, the college-specific child age effects, from Equation (1).[24] As described above, our estimates normalize against age 12 and age 15 effects, so that reported coefficients can be interpreted as being relative to the (differential) linear trend from age 12 to age 15. Figure VIII shows little evidence of nonlinear differential changes to income at ages 12 through 15, consistent with the fact that income is not yet affecting college aid and college expenses are not yet due. Income for the college group drops slightly at age 16, one year before income will affect college aid, which may reflect an anticipatory substitution response due to frictions in timing changes to income. Red highlighting in Figure VIII indicates the year in which household income is only subject to the substitution effect due to the implicit tax in college aid, but college costs are not yet due. Income for the college group drops by a net 1.0% in this year. Blue highlighting indicates the period when college costs are due, assuming four years of enrollment. Income increases during this period, leveling out around age 21 and remaining elevated thereafter.

The patterns presented in Figure VIII are striking in their consistency with simple economic theory. Parent income is reduced when it is purely subject to the substitution effect due to the implicit tax in college aid. Income then increases immediately as college costs are due, reflective of an income effect which may be due to liquidity constraints or limited forward planning. In the following analysis, we

[24] Values corresponding to Figure VIII along with subsequent heterogeneity analyses are shown in Table B.3. Values for the same but with AGI as the outcome are shown in Table B.4.

leverage this variation in timing of exposure to substitution and income effects to separately identify the ETI and the MPE.

2 Elasticity of Taxable Income (Substitution Effect)

To identify the ETI, we rely on the fact that income earned when the child is age 17 is subject to the substitution effect due to the implicit tax in college aid, but college costs are not yet due. As a result, the college-specific age 17 effect purely isolates the substitution response to the college aid implicit tax.

Table II shows our main ETI estimate. The first column of Table II reports the reduced form effect on parent log AGI, which is β_{17} from Equation (1). Parents reduce income by 1.0% in response to the college aid implicit tax. The second column shows the first stage effect on the log net-of-tax rate (NTR), which is β_{17} from Equation (1) when the outcome variable is the log NTR from college aid that applies to a given year's income. On average, the implicit tax from college aid reduces the NTR by 14.5%. Finally, the third column shows our main ETI estimate, which is the ratio of the reduced form to the first stage.[25] The elasticity of parent income with respect to the college aid implicit tax among families in which the child attends a four-year in-state public college or a four-year private college is .07.

Next, we consider how the ETI varies across groups. Figure IX shows heterogeneous ETI estimates by four characteristics: share of income from nonwage sources, level of interest income, parent age, and number of earners in the household.[26] First, we split our sample into households with a low or high share of income resulting from nonwage sources, where we define a high share to be at least 20% of income averaged over 2005 and 2006. Households with a high share of nonwage income have an elasticity of .137, more than twice that of households with a low share of nonwage income (.06), which may reflect lower frictions to adjusting nonwage income sources. Next, we split our sample

[25] To obtain standard errors, we estimate the ETI by two-stage least squares. Our two-stage least squares equations are similar to Equation (1) where the endogenous variable is the log NTR and the omitted instrument is the interaction between the indicator for the college group and the indicator for child age 17, or $Col_i \cdot D_{it}^{17}$. The first stage F-statistic for our main ETI estimate is over 1 million, and in all following subsample ETI estimates the F-statistic is at least 100,000, so we omit these from our tables.

[26] Values are shown in Table B.5.

into households with a low or high level of interest income, where high interest income is defined as taxable interest in 2005 and 2006 exceeding $500 on average.[27] This split captures different responses to the college aid implicit tax by wealth, as high interest income reflects high interest-bearing assets like savings. We find that households with high wealth (as proxied by high interest income) have an elasticity of .116, nearly twice as large as that for households with low wealth (.06). Then, we split the sample by whether the primary filer is above median age.[28] Households in which the primary filer is above median age have a higher elasticity of .087 compared with .052 among younger households, which may reflect less frictions to adjusting income or greater sophistication among older households. Finally, we split the sample into households that either have one or two wage earners in 2005 and 2006. Households with one wage earner have a somewhat larger elasticity of .085 compared with .057 among dual-earner households, suggesting that the patterns we observe are not driven by adjustments to secondary earner income.

Using the rich information on various sources of income available in the tax data, we investigate the mechanisms by which income responds to the implicit tax in college aid. Table B.6 shows the effect of the college aid implicit tax on various sources of income, in dollars, based on regressions of the form in Equation (1). The total effect on AGI is a reduction of $1,033 on average. Of this, W-2 wage income accounts for a $532 reduction, or 52% of the total effect. The remaining half of the total effect is due to nonwage factors impacting AGI. Of these, business income across S corporations, partnerships, and sole proprietorships account for a $166 reduction, or 16% of the total; taxable portions of IRA distributions account for a $61 reduction, or 6% of the total; and capital gains account for a $60 reduction, or 6% of the total. Interestingly, $15 of the total reduction is due to an increase in deductions from IRA contributions, even though IRA contributions generally should not impact college aid as the federal EFC formula adds this amount back to total income, which already suggests households do not accurately understand college aid incentives and this impacts their behavior.

Our primary W-2 wage income measure is the amount of W-2 wages listed on the 1040, which enters AGI. It is possible to lower this amount through actions such as contributing to one's 401(k) or

[27] Our cutoff of $500 is consistent with Hilger (2016), who additionally notes that $500 of interest income corresponds to $25,000 in savings at a 2% interest rate.

[28] In our sample, the median primary filer age is 47 when the child is 17.

taking deferred compensation. As a result, effects on our primary W-2 wage income measure may conflate effects on actual wage income along with effects on actions like 401(k) contributions or deferred compensation. To test this, we additionally estimate effects on Medicare wages, which is reported on the W-2 and includes total wage income subject to the Medicare tax. Because 401(k) contributions and deferred compensation are not excused from the Medicare tax, effects on Medicare wages are a purer measure of effects on true wage income. As seen in Table B.6, Medicare wages are reduced by \$454 when the child is age 17, which accounts for 85% of the \$532 reduction in our primary W-2 wage income measure. This suggests that the effect we find on W-2 wages reflects an effect on true wage income rather than other behaviors like 401(k) contributions or deferred compensation.[29]

Finally, we consider whether the patterns we observe may be attributable to factors other than a substitution response to the college aid implicit tax. To begin, we document that income reductions among the college group in the year that income first affects aid (i.e. β_{17}) are correlated with the MTR from college aid that a household faces. Figure X shows the effect of the college aid implicit tax on parent log AGI separately along three margins related to the MTR from college aid that a household will face: baseline household income, college type, and MTR from college aid at the college attended. First, we split the sample into low (\$10-50k), middle (\$50-150k, our main sample), and high (\$150-300k) baseline income groups based on average income in 2005 and 2006. In these estimates, we continue to restrict to households with persistently positive AGI and those in which the child enrolls in a four-year in-state public college or a four-year private college at age 18 or enrolls in no college at age 18. The response is greatest among middle income households, consistent with the fact that most aid phaseout occurs in this range.[30] Next, we split the sample based on the type of college a child attends. In these estimates, we continue to restrict to households with persistently positive AGI and those in which baseline household income is in the \$50-150k range. Responses are larger for

[29] Effects on our primary W-2 wage income measure and on Medicare wages show evidence of nonlinear pretends. If the true counterfactual extrapolates these nonlinear pretrends to the age 17 effect, then the contribution to the total effect on AGI from effects on wage income would be somewhat reduced.

[30] We observe nonzero effects for low and high income households. This may be for several reasons. Aid phaseout is not strictly confined to the \$50-150k range, though it is most consistent in that range. Income groups are defined based on 2005 and 2006 income, and as a result, a proportion of these households will have income in the aid phaseout range when their child attends college. High income households with income above the aid phaseout range may reduce income enough to be in the aid range. Finally, households may simply misperceive the income levels at which aid does and does not phaseout at the margin with income.

households in which the child attends a four-year college as opposed to a two-year college, consistent with four-year colleges having a greater MTR from college aid on average.[31] Finally, we split the sample based on whether the child enrolls in a college with a high (10%+) or low (0-10%) rate of aid phaseout, which we define based on IPEDS data as the aid phaseout rate from the $30-48k to $75-110k income ranges. The effect is more than twice as large for households in which the child attends a college with a high MTR from aid, at 1.3% compared with .5%. These patterns are consistent with the income reductions we observe being due to the college aid implicit tax as opposed to other factors.

One specific potential concern is whether the choice to enroll in college is endogenous to household income shocks. Two types of evidence suggest this is not an important factor in explaining our ETI estimates. First, existing studies find that effects of household income shocks when a child is in high school on college enrollment are generally small (Hilger 2016; Bulman et al. 2021). Moreover, these effects operate in the opposite direction to the patterns we observe: a positive shock to household income when a child is in high school tends to increase the likelihood of enrolling in college, which would induce a positive correlation between college enrollment and household income in the years immediately preceding college, whereas we observe that households in which the child enrolls in college tend to have reduced income immediately before college begins. We can further assess this selection margin in the tax data. To do this, we compare child age effects between households in which the child enrolls at any college at age 18 against households in which the child does not enroll in college at age 18. If the reduction of income among college households when the child is 17 is importantly due to selection into college based on a negative income shock, then we should observe an increase of income at age 17 relative to trend for the non-college group, as those with negative income shocks select out of this group. Figure XI shows these trends.[32] [33] While there is clear

[31] We observe effects at four-year public out-of-state colleges that are similar to four-year in-state public colleges and four-year private colleges. In most cases, the MTR from aid is much lower for out-of-state students at public colleges, as most state and institution level aid is reserved for in-state students at these institutions. Importantly, our effects are measured before the child has enrolled or even applied to colleges, so these households may be responding to an expected aid schedule that also includes four-year in-state public and four-year private colleges.

[32] Values for Figure XI along with the difference between treated and control groups, as well as all the same results for our main analysis sample, are shown in Table B.7. The same results with AGI as the outcome are shown in Table B.8.

[33] The coefficients in Figure XI are generally decreasing. Recall in Equation (1) we omit age effects at 12 and 15, and all effects are relative to the trend over these ages. As such, the downward trend in Figure XI does not reflect absolute declines in income, but a slowdown of income growth relative to the trend over ages 12 to 15.

evidence of a reduction of income at age 17 among the college group, the trend of income among the non-college group is smooth through this period and shows no evidence of an increase to income at age 17 that would reflect selection.[34]

3 Marginal Propensity to Earn (Income Effect)

In addition to large implicit tax rates, college exposes households to large costs. Figure VIII shows that college households begin increasing income relative to non-college households immediately as college costs are due. Here, we exploit the differential timing of when college costs are due versus when income affects college aid to identify the income effect separately from the substitution effect. Specifically, among households in which the child is in college for four consecutive years from ages 18 to 21, the implicit tax from college aid will apply to income earned from ages 17 to 20 while college costs are due from ages 18 to 22, as shown in Figure VI. We therefore use $\beta_{20} - \beta_{17}$ to identify the income effect operating at age 20 while differencing out the substitution effect that is operating in both years and can be identified by β_{17}.

We make a few additional sample restrictions for this analysis. Because we use β_{17} to difference out the substitution effect, we restrict our sample to households in which the child either attends a four-year in-state public or four-year private college at all ages from 18 to 21 or attends no college from ages 18 to 21, so that the relative substitution effect among the treated group is steady from ages 17 to 20. We drop a small number of households that are missing information on the primary filer's age, which is required to annuitize total college costs to produce MPE estimates. Finally, we restrict to cohorts in which the child turns 18 in years 2011 to 2014. This ensures that our measures of persistent college enrollment and non-enrollment through age 21 are defined using 1098-T information only through 2017, which is important because significant changes were made to 1098-T filing practices beginning in 2018 under the PATH Act of 2015. Specifically, beginning in 2018, Form 1098-T is required to only report the total amount paid toward qualified tuition and related expenses, whereas prior to 2018 institutions could report the amount paid or the amount billed. This changed 1098-T filing because

[34] As seen in Table B.7, sample sizes for control and treated groups are similar, so that if the decrease in income among the treated group were pure selection, there would be an offsetting increase in income among the control group of roughly the same magnitude.

some institutions only issue a 1098-T when the amount they would report is positive, which is less likely under the new payment based approach. Moreover, because having positive payments is a function of household income, conditioning our sample on the presence or absence of a 1098-T in 2018 and beyond would condition on trends in household income in ways that likely differ between treatment and control groups. Table B.9 shows event study estimates of college-specific child age effects on AGI from Equation (1) for the MPE sample along with subsamples, which can be compared with the same for the main ETI sample in Table B.4. Patterns are broadly similar between the two, with two exceptions: in the MPE sample, income falls by slightly more at age 17, and income increases less from age 17 to age 20. The first pattern is consistent with treated families in the MPE sample being more likely to overcome frictions to adjust income based on longer expected enrollment, and the second pattern is consistent with the relative substitution effect reducing over time in the ETI sample as some treated children leave college and some control children enter, which was the motivation for the MPE sample.

Table III shows our main MPE estimates. The first column shows the reduced form effect on income, which is $\beta_{20} - \beta_{17}$ from Equation (1) with AGI as the outcome. Parents increase income by \$1,898 on average in response to college costs when their child is age 20. Consistent with Golosov et al. (2024), the next four columns of Table III show two methods of scaling this reduced form effect by college costs to produce measures of the income effect: (1) the wealth effect, which scales the reduced form effect on annual earnings by the total cost of college; and (2) the MPE, which scales the reduced form effect on annual earnings by the annual cost of college that results from annuitizing the total cost of college over the remaining lifetime.[35] Our first stage effects are based on college costs incurred over the four years in which the child is age 18 to 21. The second column of Table III shows that the average cost of college over four years in our sample is \$91,439. The third column shows that scaling the reduced form effect on parent income when the child is age 20 by the average cost of college over four years yields a wealth effect of .021, which means that parent income is elevated in

[35] Also consistent with Golosov et al. (2024), we compute the annuitized cost of college by multiplying the total cost of college by $\frac{r}{1+r}(1-(\frac{1}{1+r})^{T-k+1})^{-1}$, where k is the primary filer's age when the child is 18, T is life expectancy which we set to 80, and r is the risk-free real interest rate which we set to .025.

the year their child is age 20 by 2 cents for every dollar of total college costs. The fourth column of Table III converts the total cost of college over four years into an equivalent annual cost spread over the remainder of the parents' lives, which equates to a $4,298 annual cost. The fifth column scales the reduced form effect on parent income when the child is age 20 by the annuitized cost of college, resulting in an MPE of .44, which means that parent income is elevated in the year their child is age 20 by 44 cents for every dollar of annuitized total college costs.

Figure XII shows how the MPE varies across the same groups for which we report heterogeneous ETI estimates above in Figure IX.[36] Across all subpopulations in Figure XII, confidence intervals are generally wide relative to differences in point estimates. We find suggestive evidence that households with high interest income (above $500) have a lower MPE than households with low interest income, which would be consistent with liquidity being an important factor. However, due to the wide confidence intervals, the evidence is only suggestive.

Table B.12 breaks down the total increase in AGI of $1,898 on average into contributions from various sources of income.[37] Of this, $1,247 comes from W-2 wages, accounting for 66% of the total increase. Medicare wages – which are not affected by actions such as contributions to 401(k) plans or deferred compensation and as such reflect a purer measure of true wage income – increase by $963, or 77% of the total increase in our primary W-2 wage income measure, suggesting most of the W-2 effect reflects an increase in true wage income. Business income makes up the overwhelming majority the remaining one-third of the total response accounted for by nonwage factors, with the total increase across income from sole proprietorships, S corporations, and partnerships accounting for a $536 increase, or 28% of the total increase. Notably, income from unemployment compensation falls by $49, suggesting that even among households with persistently positive AGI the extensive labor supply margin is relevant in response to college costs.

Figure XIII shows heterogeneity in the reduced form income effect.[38] The effect is larger among higher income households, which is consistent with the fact that college costs increase with income. The effect is largest among households in which the child attends a two-year in-state public college

[36] Values are shown in Table B.10.

[37] Values for full event study estimates are shown in Table B.11.

[38] Values for the difference between age 20 and 17 effects for various subgroups are shown in Table B.13.

and smallest among households in which the child attends a four-year public out-of-state college. Similarly, the effect is larger for households in which the child attends a college with a total cost of attendance below $25,000. These patterns may reflect that college choice is endogenous to a household's liquidity – households with less liquid assets may be more likely to enroll their child at a less expensive college.[39] Last, we split the sample based on whether there is a younger sibling in the family in order to test whether our measured effects solely reflect a response to no longer having children in the household.[40] The increase in income is somewhat less, but still present, for families with a younger child who do not face a sudden shift of having no children in the home when the focal child begins college. Taken at face value, the point estimates imply that roughly one-third of the total income effect may be due to the family no longer having a child in the house; however, we cannot reject that the effects are equal for families with and without a younger child, so our evidence remains consistent with college explaining all or nearly all of the observed increase in income.

By similar reasoning as described above for the ETI, Figure XI indicates that it is unlikely that college enrollment is endogenous to positive anticipated income shocks between ages 18 through 20. If this were the case, we would expect to see a sharp decline in income around ages 18 through 20 relative to trend for those who do not attend college as those who anticipate a positive income shock select out of the non-college group. However, the trend in income appears smooth through this period for the non-college group, so that this type of selection does not appear to be important in explaining our MPE results.

4 Including Nonfilers

Our preceding analysis restricts the sample to families that file taxes with positive AGI in all years from 2005 to 2018. The key reason for restricting to persistent filers is due to our assignment of chil-

[39] If college choice is endogenous to liquidity, this does not invalidate our research design. This type of endogeneity can be viewed as selection into treatment types based on treatment effects, rather than selection on counterfactual income. As a result, our design remains internally valid, though our main MPE estimates for those who select into four-year college enrollment may not generalize. Specifically, our estimates are likely to understate the MPE that would prevail at four-year colleges across the full population that did not necessarily select into four-year enrollment.

[40] For the purposes of this test, a family with a younger sibling is one in which the oldest claimed dependent aside from the focal child is between ages 1 and 14 when the focal child is age 18. This ensures that our estimates are not confounded either by substitution or income effects due to college for the second child.

dren to parents based on dependent claiming on tax forms when the child is age 14, which requires that the parents file taxes in that year. As a result, our sample necessarily conditions on parent tax filing in the year the child is age 14. When we do not further condition on filing in the remaining years of our sample window, we observe differential mean reversion patterns in filing between college and non-college families that confound estimates of effects on income (unconditional on filing). Figure A.4 demonstrates the differential mean reversion patterns in filing between college and non-college families.[41]

Nevertheless, the main effects documented within our restricted sample can also be seen clearly when not conditioning on filing behavior. Figure XIV shows effects on an indicator for filing with positive AGI and on AGI among the expanded sample that does not condition on filing behavior or on positive AGI. The impact of differential mean reversion in filing behavior is evident in the highly nonlinear pre-trend for effects on filing, which is the key issue that causes us to restrict our main analysis to persistent filers. Still, two important patterns are clear. First, the trend for filing behavior is smooth through the years impacted by the college aid implicit tax and by college costs, which both suggests that the extensive margin of filing and reporting positive AGI is not a primary margin of response to the college aid implicit tax or to college costs in its own right and moreover implies little room for our main estimates to be confounded by endogenous filing responses. Second, relative to its nonlinear pre-trend, AGI clearly falls at age 17 and subsequently increases over ages 18 through 20, exactly consistent with our preceding findings and in dollar amounts that also appear consistent with our main results. Comparing these patterns in AGI with the smooth trend through ages 17 to 20 for filing behavior reinforces the takeaway that AGI conditional on filing adjusts to college incentives while filing behavior does not.[42]

[41] Values are shown in Table B.14. We record a value of zero for AGI in instances of non-filing.

[42] The first column of Table B.14 also shows effects on AGI among persistent filers but without the restriction of positive AGI in all years. The effects are nearly identical to our main estimates of effects on AGI in the restricted sample, further suggesting that conditioning on positive AGI changes little after conditioning on persistent filing. Conditioning on positive AGI offers the benefit of making estimation of log specifications to estimate elasticities more straightforward.

IV Misperception and the Efficiency Cost of Taxation: A Sufficient Statistics Approach

Many taxes – both explicit and implicit – are complex. Existing evidence finds that individuals frequently misperceive their marginal tax rate or price when facing nonlinear schedules such as the federal income tax or tiered electricity pricing (Fujii and Hawley 1988; de Bartolome 1995; Ito 2014; Rees-Jones and Taubinsky 2020). Past studies of misperception have focused on settings in which there are natural candidates for structural forms of misperception – for example, misinterpreting the average tax rate as the marginal tax rate – and consequently have imposed this structure in their analysis. However, not all forms of taxation admit natural candidate forms for misperception – for example, means tested benefit programs like college aid or the EITC; moreover, even in cases that do have natural candidates, misperception may arise for any number of reasons including simple inattention, so that many individuals may bear misperceptions that diverge from the assumed structural form. If individuals misperceive the tax rate they face in a more general heterogeneous manner, how does this misperception impact the efficiency cost of the tax? Moreover, when individuals misperceive tax rates in this way, how can an econometrician measure the efficiency cost of the tax and the impact of misperception?

In this section, we detail a model of income choice in the presence of a tax that individuals may misperceive in a general heterogeneous manner with few restrictions. Using the model and a sufficient statistics approach, we derive an expression that reveals the channels through which misperception affects DWL, and we demonstrate how to identify each channel along with the total impact of misperception on DWL by combining reduced form ETI estimates with a survey-based measure of misperception. Finally, we apply these insights to the case of the college aid implicit tax to estimate the impact of misperception on DWL in this setting. Our theoretical results highlight several channels through which misperception may impact DWL, and our empirical results in the setting of the college aid implicit tax find that misperception dramatically increases DWL, driven by the large variance of misperception across individuals.

IV.A A Model of Income Response to a Temporary Tax and Deadweight Loss with Misperception

1 Model Setup and Solution

In the model, agents value consumption and they dislike producing income. There are two periods, with period 0 representing years in which agents face only a baseline tax and period 1 representing years in which agents face the baseline tax plus an additional temporary tax. Each agent chooses how much income to earn in period 0 and in period 1, and their consumption is then determined by their budget constraint. Agents can misperceive the period 1 tax rate in a heterogeneous manner.

Specifically, each agent solves the following two-period dynamic income problem:

$$\max_{C_i, Z_{i,0}, Z_{i,1}} U = C_i - \sum_{t=0}^{1} \frac{a_i}{1+\frac{1}{e(X_i)}} \left(\frac{Z_{i,t}}{a_i}\right)^{1+\frac{1}{e(X_i)}} \tag{2}$$

$$\text{s.t.} \quad C_i = (1-\tau_i^b)Z_{i,0} + [(1-(\tau_i^b+\tilde{\tau}_i)]Z_{i,1}$$

where C_i is agent i's total consumption; $Z_{i,0}$ is income earned in period 0; $Z_{i,1}$ is income earned in period 1; a_i is ability; e is an elasticity parameter that is allowed to vary based on observables in the vector X_i; τ_i^b is the baseline tax rate the agent faces in both periods; and $\tilde{\tau}_i$ is the additional tax rate the agent *perceives* to apply to their period 1 income. The true additional tax rate the agent faces in period 1 is τ_i. For simplicity, we assume utility is quasilinear in consumption, and as a result agents are indifferent to the allocation of consumption across the two periods, which emphasizes that the problem is one of dynamic income choice rather than of consumption allocation.

We assume that each agent makes their income decisions *as if* $\tilde{\tau}_i$ is the true additional tax rate they face. If an agent misperceives this tax rate, so that $\tilde{\tau}_i \neq \tau_i$, we assume this is accounted for when the true budget constraint – which replaces $\tilde{\tau}_i$ with τ_i – binds, so that the agent's actual consumption might differ from their chosen consumption.

Taking the first order conditions and solving yields the solution for chosen income in each period

as:

$$
\begin{aligned}
Z_{i,0} &= a_i(1-\tau_i^b)^{e(X_i)} \\
Z_{i,1} &= a_i[1-(\tau_i^b+\tilde{\tau}_i)]^{e(X_i)}
\end{aligned} \tag{3}
$$

In period 0, when individuals face only the baseline tax, each agent produces income that is lower than their ability a_i and depends on the baseline net-of-tax rate $(1-\tau_i^b)$. In period 1, when the temporary tax is added, each agent produces income that is lower than their ability and depends on the total perceived net-of-tax rate $[1-(\tau_i^b+\tilde{\tau}_i)]$. $e(X_i)$ is the elasticity of earned income with respect to the total perceived net-of-tax rate for an individual with observables X_i. Below, we refer to e as the structural elasticity as it is the parameter that determines preferences over consumption and earning income.

2 Sufficient Statistics for Deadweight Loss and The Impact of Misperception

Following the seminal work of Feldstein (1999) demonstrating that the elasticity of taxable income (ETI) is a sufficient statistic to calculate the deadweight loss (DWL) arising from a tax, the field of empirical public finance has focused largely on estimating empirical ETIs with respect to various taxes. In this section, we present results showing that the standard reduced form ETI is no longer sufficient for computing DWL when individuals misperceive the marginal tax rate they face; instead, to compute DWL in the presence of misperception, one must combine the reduced form ETI with an individual-level measure of the perceived tax rate. Our results reveal the channels through which misperception affects DWL while laying the groundwork to identify and estimate both the total impact of misperception on DWL and the contributions from each channel.

We begin with the key result, which is a general expression for DWL of a tax in the presence of heterogeneous misperception of tax rates[43]:

[43] The expression is an approximation due to the standard use of a second order Taylor expansion as described in Appendix C.1

$$DWL \approx \underbrace{N \cdot \frac{1}{2} \cdot \Big\{ E\left[Z_{i,1} e(X_i) \tau_i^2\right]}_{\substack{\text{Correct Perceptions} \\ \text{(standard Harberger triangle)}}} \cdot \Big(E[\theta_i]^2 + Var(\theta_i) \Big) \; + \; E\left[e(X_i)\right] E\left[\tau_i^2\right] \cdot Cov\left(\theta_i^2, Z_{i,1}\right) \tag{4}$$

$$+ \; E\left[Z_{i,1}\right] E\left[\tau_i^2\right] \cdot Cov\left(\theta_i^2, e(X_i)\right) \; + \; E\left[Z_{i,1}\right] E\left[e(X_i)\right] \cdot Cov\left(\theta_i^2, \tau_i^2\right) \Big\}$$

where we define the individual-level misperception parameter θ_i such that $\tilde{\tau}_i = \theta_i \tau_i$, capturing the degree to which an individual over- or under-estimates their own MTR. The proof of Equation (4) can be found in Appendix C.1.

Equation (4) reveals several channels through which misperception of a tax impacts DWL. To begin, the expression nests the special case of perfectly accurate perceptions (i.e. $\theta_i = 1$ for all i), in which case DWL can be measured by the standard Harberger triangle observed in Equation (4). Relative to the baseline case of perfectly accurate perceptions, misperception affects DWL through the following channels:

1. **DWL increases with** $E[\theta_i]$: A higher perceived tax, on average, generates more distortionary behavior. DWL is not altered by the average perception when $E[\theta_i] = 1$. Below, we will define $Bias = E[\theta_i]^2 - 1$ so that positive Bias reflects an increase to DWL and negative Bias reflects a decrease to DWL.

2. **DWL increases with** $Var(\theta_i)$: Conditional on the average perception, greater variance of misperceptions increases DWL. This is due to the standard assumption of increasing marginal disutility of producing income, which means that larger reductions of income in response to a tax bear disproportionately greater impacts on DWL than do smaller reductions. As a consequence, in utility terms, it is worse to have some individuals reduce income by a large amount and others reduce income by a small amount than for all individuals to reduce income by the same amount, even if the average reduction is equal in both cases. This result is directly related to the standard public finance result that the DWL induced by a tax is proportional to the square of the tax rate, and it is consistent with the theoretical result in Taubinsky and Rees-Jones (2018) and Brusco

and Glass (2020) that DWL from a tax increases with both the mean and variance of the salience of the tax. Appendix D explains this with an example.

3. **DWL increases with** $Cov(\theta_i^2, Z_{i,1})$: DWL is greater when θ_i^2 – which accounts for both the average and variance channels listed above – is positively correlated with baseline income (i.e. income in the absence of the potentially-misperceived tax). Conditional on the elasticity, an individual with a higher baseline income will reduce income in response to a greater misperception as measured by θ_i^2 by more than an individual with a lower baseline income.

4. **DWL increases with** $Cov(\theta_i^2, e(X_i))$: DWL is greater when θ_i^2 is positive correlated with elasticity. Conditional on baseline income, an individual with a higher elasticity will reduce income in response to a greater misperception by more than an individual with a lower elasticity.

5. **DWL increases with** $Cov(\theta_i^2, \tau_i^2)$: DWL is greater when θ_i^2 is positively correlated with the square of the new tax. Conditional on baseline income and elasticity, an individual facing a higher tax will reduce income in response to a greater misperception by more than and individual facing a lower tax.

The net impact of misperception on DWL from a tax is ambiguous a priori – different combinations of the five channels above can produce a case in which misperception reduces, increases, or has no net impact on DWL. As a result, it remains an empirical question whether misperception of complex taxes reduces, increases, or has no net impact on DWL.

IV.B Identifying Deadweight Loss with Observational Data and Survey-based Measures of Misperception

1 Reduced Form vs. Structural Elasticity

We will distinguish between the reduced form elasticity and the structural elasticity. The reduced form elasticity corresponds to the standard approach for estimating the ETI in the public economics literature, which measures the change in income with respect to the true change in the marginal tax

rate. The structural elasticity instead measures the change in income with respect to the perceived change in the marginal tax rate. The structural elasticity is solely a function of the parameters of the utility function, but the reduced form elasticity depends additionally on the extent to which individuals misperceive the tax rate they face. Under the standard assumption of perfect perceptions, the reduced form elasticity will equal the structural elasticity; however, in the presence of misperception, the two are not necessarily equal. This distinction is essential for identifying DWL in the presence of misperception, as Equation (4) expresses DWL as a function of the structural elasticity $e(X_i)$ which is not identified by standard ETI estimation techniques when there is misperception. Here, we clarify the difference between the reduced form elasticity and the structural elasticity, and we demonstrate how to identify the structural elasticity – a necessary input to identify DWL – by combining the reduced form elasticity with a measure of perceptions.

Figure XV illustrates this distinction. The figure considers how income changes between period 0 and period 1 in the preceding model. The individual dislikes producing income at an increasing marginal disutility, but likes consumption.[44] $Z_{i,0}$ is income in period 0, which occurs where the marginal disutility of earning income equals the marginal utility of consumption, normalized to the log net-of-tax rate in period 0, $(1-\tau_i^b)$. In period 1, if the individual perceives her additional tax rate to be $\tilde{\tau} > 0$, she will choose a lower income level that equates her marginal disutility of earnings with her total perceived net-of-tax rate $[1-(\tau_i^b+\tilde{\tau}_i)]$. This lower income level is labeled as $Z_{i,1}$ in the figure, and the blue curve traces out how income will vary with the perceived net-of-tax rate. The slope of this curve depends on the individual's utility function, and in particular on the rate at which the marginal disutility of earning declines as income falls. This is the structural elasticity.

The econometrician typically doesn't observe the perceived tax rate, but only the actual rate, τ_i^b and τ_i, along with income in each period. In other words, the econometrician observes the points labeled "A" and "B" in the data. The ETI that the econometrician will estimate (the "reduced form ETI") is the change in log earnings divided by the change in the log net-of-tax rate between the two periods, which extrapolates the line through points A and B. The left panel of Figure 7 shows a case where $\tilde{\tau} > \tau$. Here, the econometrician will estimate a larger ETI than is implied by the utility function

[44] Figure XV assumes utility is quasilinear in consumption, so that the marginal utility of consumption curves are flat.

parameters alone. The right panel shows a different case, where the structural elasticity is the same but now $\tilde{\tau} < \tau$. Here, the econometrician's reduced form elasticity will be smaller than the structural elasticity.

The two panels in Figure XV provide intuition to support the following result: the relationship by which the structural elasticity and the perceived tax rate determine the reduced form elasticity can be inverted, so that knowledge of the reduced form elasticity and the percieved tax rate is sufficient to identify the structural elasticity. We now demonstrate this result formally.

In most empirical studies, the reduced form ETI estimand is an instrumental variables ratio of an average policy effect on log income scaled by the first stage average effect of the policy on the log net-of-tax rate. In such cases, the reduced form elasticity can be expressed following the notation in Figure XV as:

$$\beta^{ETI}(X_i) \coloneqq \frac{E[\ln(Z_{i,1}) - \ln(Z_{i,0}) \,|\, X_i]}{E[\ln(1-(\tau_i^b + \tau_i)) - \ln(1-\tau_i^b) \,|\, X_i]}$$

where we allow for heterogeneity by observables in vector X_i.

Combining this expression with the above model and assuming that the baseline tax is zero, which we will maintain in subsequent sections, we can identify the structural elasticity $e(X_i)$ as a function of the reduced form elasticity $\beta^{ETI}(X_i)$ and average perceptions of tax rates as:

$$e(X_i) \;=\; \beta^{ETI}(X_i) \cdot \frac{E[\ln(1-\tau_i) \,|\, X_i]}{E[\ln(1-\tilde{\tau}_i) \,|\, X_i]} \tag{5}$$

The proof of Equation (5) can be found in Appendix C.2.

2 Identification with Survey-based Measures of Misperception

Equation (4) involves several terms that depend on the distribution of misperceptions. Here, we demonstrate that survey data on individuals' tax rate perceptions can be used to identify these components.

In the survey, respondents provide a noisy signal of their perceived tax rate, $\hat{\tilde{\tau}}_{1i} = \tilde{\tau}_i + \varepsilon_{1i}$. We will assume that the error term ε_{1i} has conditional mean zero and is conditionally uncorrelated with the

percieved tax rate, with income, and with elasticity: $E[\varepsilon_{1i}|X_i,\tilde{\tau}_i] = E[\varepsilon_{1i}|Z_{i,1},\tau_i] = E[\varepsilon_{1i}|e(X_i),\tau_i] = 0$ and $Cov(\tilde{\tau}_i,\varepsilon_{1i}|Z_{i,1},\tau_i) = Cov(\tilde{\tau}_i,\varepsilon_{1i}|e(X_i),\tau_i) = Cov(\varepsilon_{1i},Z_{i,1}|\tau_i) = Cov(\varepsilon_{1i},e(X_i)|\tau_i) = 0$. Note these assumptions imply also that $E[\varepsilon_{1i}|\tau_i] = 0$ and $Cov(\tilde{\tau}_i,\varepsilon_{1i}|\tau_i) = 0$.

Crucially, respondents also provide a second signal that is related to their perceived tax rate, $\hat{\tilde{\tau}}_{2i} = \tilde{\tau}_i + \varepsilon_{2i}$, which can be thought of as an instrument. We will assume that the error term ε_{2i} has a constant conditional mean which may be different from zero, is conditionally uncorrelated with the perceived tax rate, and is conditionally uncorrelated with the error term ε_{1i}: $E[\varepsilon_{2i}|Z_{i,1},\tau_i] = E[\varepsilon_{2i}|e(X_i),\tau_i] = \alpha$, $Cov(\tilde{\tau}_i,\varepsilon_{2i}|Z_{i,1},\tau_i) = Cov(\tilde{\tau}_i,\varepsilon_{2i}|e(X_i),\tau_i) = 0$, and $Cov(\varepsilon_{i1},\varepsilon_{2i}|Z_{i,1},\tau_i) = Cov(\varepsilon_{i1},\varepsilon_{2i}|e(X_i),\tau_i) = 0$. Note these assumptions imply also that $E[\varepsilon_{2i}|\tau_i] = \alpha$, $Cov(\tilde{\tau}_i,\varepsilon_{2i}|\tau_i) = 0$, and $Cov(\varepsilon_{1i},\varepsilon_{2i}|\tau_i) = 0$.

Under these assumptions, and defining the survey-based measures of misperception $\hat{\theta}_{1i} = \dfrac{\hat{\tilde{\tau}}_{1i}}{\tau_i}$ and $\hat{\theta}_{2i} = \dfrac{\hat{\tilde{\tau}}_{2i}}{\tau_i}$, we have the following identification results[45]:

1. $E[\ln(1-\tilde{\tau}_i)|X_i] \approx E[\ln(1-\hat{\tilde{\tau}}_{1i})|X_i]$
2. $E[\theta_i] = E[\hat{\theta}_{1i}]$
3. $Var(\theta_i) = Cov(\hat{\theta}_{1i},\hat{\theta}_{2i}) - E[\hat{\tilde{\tau}}_{2i} - \hat{\tilde{\tau}}_{1i}] \cdot Cov\left(\dfrac{E[\tilde{\tau}_i|\tau_i]}{\tau_i}, \dfrac{1}{\tau_i}\right)$
4. $Cov(\theta_i^2, Z_{i,1}) = Cov(\hat{\theta}_{1i}\hat{\theta}_{2i}, Z_{i,1}) - E[\hat{\tilde{\tau}}_{2i} - \hat{\tilde{\tau}}_{1i}] \cdot E\left[\dfrac{1}{\tau_i^2} Cov\left(\hat{\tilde{\tau}}_{1i}, Z_{i,1}|\tau_i\right)\right]$
5. $Cov(\theta_i^2, e(X_i)) = Cov(\hat{\theta}_{1i}\hat{\theta}_{2i}, e(X_i)) - E[\hat{\tilde{\tau}}_{2i} - \hat{\tilde{\tau}}_{1i}] \cdot E\left[\dfrac{1}{\tau_i^2} Cov\left(\hat{\tilde{\tau}}_{1i}, e(X_i)|\tau_i\right)\right]$
6. $Cov(\theta_i^2, \tau_i^2) = Cov(\hat{\theta}_{1i}\hat{\theta}_{2i}, \tau_i^2)$

The proofs for these identification results can be found in Appendix C.3.

These identification results allow us to combine reduced form elasticity estimates with survey-based measures of misperceptions to estimate DWL in the presence of misperception, the impact of misperception on DWL, and the contributions to DWL from each of the separate channels revealed in Equation (4). The presence of the instrument $\hat{\tilde{\tau}}_{2i}$ is necessary for this approach, in particular for

[45] The results for $Var(\theta_i)$, $Cov(\theta_i^2, Z_{i,1})$, and $Cov(\theta_i^2, e(X_i))$ contain bias correction terms for when $\alpha \neq 0$. When $\alpha = 0$, the bias correction terms are zero and we have $Var(\theta_i) = Cov(\hat{\theta}_{1i},\hat{\theta}_{2i})$, $Cov(\theta_i^2, Z_{i,1}) = Cov(\hat{\theta}_{1i}\hat{\theta}_{2i}, Z_{i,1})$, and $Cov(\theta_i^2, e(X_i)) = Cov(\hat{\theta}_{1i}\hat{\theta}_{2i}, e(X_i))$.

identifying the variance and covariance components in Equation (4). Intuitively, without the instrument, one would be unable to distinguish variance of misperceptions from variance of the error term ε_{1i}. Similarly, one would be unable to distinguish correlation between squared misperceptions θ_i^2 and income, elasticity, or tax rate from correlation between the square of the error term ε_{1i}^2 and the same, which amounts to an inability to distinguish correlation between the conditional variance of misperceptions and income, elasticity, or tax rate from correlation between the conditional variance of the error term ε_{1i} and the same. Properly incorporating variation from the instrument allows us to identify the above variance and covariance terms even when the error term ε_{1i} is nondegenerate (i.e. when survey responses do not perfectly reflect each individual's underlying perception) and when the conditional variance of ε_{1i} may further be correlated with income, elasticity, or tax rate.

As is typical in settings regarding measurement error, most of our assumptions are untestable; however, combining the conditional mean assumptions does allow us to identify α (given the implied assumption above that ε_{1i} equals zero on average), and we can additionally test the implication that the difference in conditional means of the two error terms is constant across covariates. Specifically, we can identify[46] α as $\alpha = E[\hat{\tilde{\tau}}_{2i} - \hat{\tilde{\tau}}_{1i}]$. We can also identify conditional means of differences in the error terms $E[\varepsilon_{2i} - \varepsilon_{1i}|Z_{i,1}, \tau_i] = E[\hat{\tilde{\tau}}_{2i} - \hat{\tilde{\tau}}_{1i}|Z_{i,1}, \tau_i]$ and $E[\varepsilon_{2i} - \varepsilon_{1i}|e(X_i), \tau_i] = E[\hat{\tilde{\tau}}_{2i} - \hat{\tilde{\tau}}_{1i}|e(X_i), \tau_i]$, all of which equal α and are therefore constant under our assumptions. We estimate α and test whether the conditional average of the difference in error terms $\varepsilon_{2i} - \varepsilon_{1i}$ is constant over covariates in Section IV.C.

IV.C Combining the Reduced Form Elasticity with an Online Survey to Identify the Impact of Misperception on Deadweight Loss

1 Data

To identify the structural elasticity and the impact of misperception on DWL, we administer an online survey to directly measure parents' perceptions of the college aid implicit tax schedule. The survey was administered through Prolific Academic, a website that connects survey administrators with sur-

[46] More precisely, we identify the difference $E[\varepsilon_{2i} - \varepsilon_{1i}]$. Under our implied assumption that $E[\varepsilon_{1i}] = 0$, this equates to identifying α.

vey takers, between September 26, 2023 and October 19, 2023. The survey targeted parents with a child born between the years 2001 and 2023, so that the child would be between age 0 and 22 at the time the survey. The survey instructed the respondent to consider one child in particular and to answer all questions with regard to that child. Additional screeners for eligible participants included US nationality and fluency in English. The sample includes respondents from across the US and is not probability weighted. In total, 3,158 respondents completed the survey. Each respondent was paid between $3 and $4. All survey responses are anonymous.

The survey asks each respondent for their best guess of average financial aid provided to an in-state student attending a public four-year college in the respondent's current state with varying levels of family income, alongside various respondent demographics including previous year income, share of income from nonlabor sources, child age, and more. Crucially for the empirical design implemented below, the survey includes two separate elicitations of a respondent's perception of the college aid implicit tax schedule. First, the respondent is asked for their best guess of aid provided to an in-state student whose total parent income is $45k, $55k, $65k, or $75k. Then, beginning on a separate screen, the respondent is asked for their best guess of average aid provided to in-state students with family income in each of the following bins: $0-30k, $30-48k, $48-75k, $75-110k, and above $110k. These income bins correspond to the bins for which colleges report publicly available statistics on average aid through IPEDS – the same data used in Figure III and in Section III to measure the MTR from college aid and net price for individuals in the tax data – so that we can compare respondent's guesses against the true aid schedule in their state.

We construct our primary measure of the respondent's perceived MTR from college aid $\hat{\tilde{\tau}}_{1i}$ using their guesses for average aid provided to in-state students in their state by income bins. We begin by matching each respondent to the nearest of the five IPEDS income bins based on the respondent's previous year total household income. We then take the difference in the respondent's guess of average aid in their matched income bin and the income bin just below, and we divide this number by the difference in median incomes across the two income bins. For example, if a respondent's income is $60k, we match them to the $48-75k bin, compute the difference in their guesses of aid for the $30-48k and $48-75k bins, and divide this difference by $61.5k - $39k = $22.5k. This produces our measure

of a respondent's own perceived marginal implicit tax rate, $\hat{\tilde{\tau}}_{1i}$. We then measure the respondent's true marginal implicit tax rate τ_i by taking the difference in actual aid across the same two income bins as reported by IPEDS and scaling by the same difference in median incomes. Following Section 2, we then define the individual's misperception parameter as $\hat{\theta}_{1i} = \dfrac{\hat{\tilde{\tau}}_{1i}}{\tau_i}$.

We define our instrument $\hat{\tilde{\tau}}_{2i}$ by taking the difference between the respondent's guesses of aid for a student with parent income of \$75k versus \$45k and dividing by the income difference of \$30k. We similarly define the individual's instrument-based misperception $\hat{\theta}_{2i} = \dfrac{\hat{\tilde{\tau}}_{2i}}{\tau_i}$.

To improve the precision of our estimates, we winsorize both $\hat{\tilde{\tau}}_{1i}$ and $\hat{\tilde{\tau}}_{2i}$ to their respective 2.5 and 97.5 percentiles. We also winsorize $\hat{\theta}_{1i}$ and $\hat{\theta}_{2i}$ to their respective 2.5 and 97.5 percentiles after constructing these initially using the un-winsorized values of $\hat{\tilde{\tau}}_{1i}$ and $\hat{\tilde{\tau}}_{2i}$.

We make three restrictions for our analysis sample. First, to target individuals facing meaningful aid phaseout and for whom we can measure empirical aid MTRs in the IPEDS data, we restrict the analysis sample to respondents with income in the \$48k - \$110k range and who face a true MTR from aid as measured in IPEDS that is at least 5%, which reduces our sample size to 1,213.[47] Second, we restrict to respondents who identify one child who is age 0-22, which reduces our sample size to 1,180. Finally, to ensure data quality, we restrict to respondents who pass an attention check question at the end of the survey and who have a valid Prolific ID, which produces our final analysis sample with 1,167 respondents. Data quality is high, reflected in the fact that only 1% of respondents are screened out due to failing the attention check or not having a valid Prolific ID, and seen similarly in the median survey completion time of 13 minutes[48].

Finally, we weight all the following analysis to be representative of the US college population based on the respondent's state and whether their income is in the \$48-75k or \$75-110k range. To do this, we merge in total enrollment counts of full-time, first-time, degree-seeking students at four-year public colleges by state and income bin using IPEDS data. Our survey data successfully captures

[47] We cannot construct reliable aid MTRs for those with income above \$110k because the IPEDS data is censored above this point. In total, 1,543 observations are dropped due to having income either too low or too high to construct a MTR from IPEDS data, as our survey did not screen on this criterion.

[48] The survey contains several sections in addition to those used in this paper. The sections are as follows: survey introduction and background questions about family and child; baseline guesses of college costs and aid schedules; information treatment; post-treatment guesses of aid schedules; demographics, attention check, and survey feedback.

individuals across nearly all state and income bin combinations: of the 102 possible combinations of the 50 US states plus D.C. interacted with the two income bins of \$48-75k or \$75-110k, 96 are represented in our survey data; each of the 50 states plus D.C. is represented.[49]

Table IV reports descriptive statistics for our analysis sample. Average income is \$74k, 5% of respondents report that at least 20% of their income is from nonlabor sources, and 53% of respondents have completed a bachelor's degree. The average respondent is 40 years old, and the average age of their oldest child 22 or under – their next child who might attend college as a dependent student – is 11. The last three rows of Table IV preview aspects of the following DWL estimates. On average, respondents slightly underestimate their MTR from aid. The average MTR from aid is 15%, and the average respondent's guess of their MTR from aid is 12%. The average misperception parameter – the ratio of perceived MTR to true MTR – is 0.88. However, there is high (non-instrumented) variance to misperceptions. The standard deviation of MTR guesses is over five times as large as the standard deviation of true MTRs, at 0.22 compared with 0.04. The standard deviation of individual misperceptions is 1.51, which is similarly large compared with a mean of 0.88.

2 Results

We begin by documenting the first stage relationship between the survey instrument $\hat{\tilde{\tau}}_{2i}$ and the respondent's best guess of their own MTR from aid $\hat{\tilde{\tau}}_{1i}$. Figure XVI plots, for 20 bins of the instrument, the average value of the instrument and the average value of the respondent's best guess of their own MTR within that bin. The figure includes a quadratic fit. There is a clear positive relationship with a negative quadratic term. Column 1 of Table B.16 reports the result from regressing $\hat{\tilde{\tau}}_{1i}$ on linear and quadratic terms of $\hat{\tilde{\tau}}_{2i}$ in column 1. Both terms are significant at the 1% level, and the F-statistic for the regression is 57. The R^2 for the regression is 0.29, reflecting that the instrument successfully captures a considerable share of variation in $\hat{\tilde{\tau}}_{1i}$. We conclude that the instrument captures important variation in $\hat{\tilde{\tau}}_{1i}$, and as a result the instrument will allow us to consistently estimate the components of Equation (4) under the assumptions described in Section IV.B.

[49] The combinations not found in our survey data include the \$48-75k income bin for Rhode Island along with the \$75-110k income bin for Alaska, D.C., Mississippi, New York, and South Dakota.

As described in Section IV.B, we can identify α and test the implied assumption that the conditional average of the difference in error terms $\varepsilon_{2i} - \varepsilon_{1i}$ is constant over covariates. We estimate $\hat{\alpha} = \hat{E}[\hat{\tilde{\tau}}_{2i} - \hat{\tilde{\tau}}_{1i}]$ where $\hat{E}[\cdot]$ denotes the sample mean, and we test whether the difference in error terms is conditionally constant by regressing the difference $\hat{\tilde{\tau}}_{2i} - \hat{\tilde{\tau}}_{1i}$ on income $Z_{i,1}$, an indicator for nonlabor income accounting for at least 20% of total income (representing the elasticity-relevant covariate X_i), and their interactions with the true tax rate τ_i as well as the tax rate itself. Table B.15 shows the results. We estimate $\alpha = .09$ and soundly reject the null hypothesis $H_0 : \alpha = 0$. Otherwise, we cannot reject the nulls that the conditional average of the difference in error terms $\varepsilon_{2i} - \varepsilon_{1i}$ is constant over income, elasticity, their interactions with the true tax rate, and the tax rate itself. These results support our assumptions that the average of the error term on the instrument ε_{2i} is nonzero but is constant over covariates. The fact that $\alpha \neq 0$ may reflect differences in the conceptual content of the two variables $\hat{\tilde{\tau}}_{1i}$ and $\hat{\tilde{\tau}}_{2i}$, or it may reflect aspects of the survey conditions – for example, the questions that are used to construct $\hat{\tilde{\tau}}_{2i}$ are presented before the questions that are used to construct $\hat{\tilde{\tau}}_{1i}$, so respondents have additional time to think in between the two. What is key is that $\hat{\tilde{\tau}}_{1i}$ and the true tax rate τ_i are constructed identically and that $\hat{\tilde{\tau}}_{2i}$ has predictive power for $\hat{\tilde{\tau}}_{1i}$. Additionally, an insight from Section IV.B is that we can still use the instrument to identify the contribution of misperception and its corresponding channels to total DWL even when the instrument $\hat{\tilde{\tau}}_{2i}$ differs on average from our primary measure of the perceived tax rate $\hat{\tilde{\tau}}_{1i}$ by including the relevant bias correction terms.

Before moving to estimating the components of Equation (4), we first illustrate the key results graphically. Figure XVII captures the important patterns. For each of 10 bins of true MTR, the blue dots plot the average MTR guess against the average true MTR, with the red line showing the linear fit. The dotted black line identifies the 45-degree line. At true MTRs below approximately 12%, respondents on average overestimate their MTR, while at true MTRs above 12%, respondents on average underestimate their MTR. $\hat{\tilde{\tau}}_{1i}$ is nearly flat over true MTR; regressing $\hat{\tilde{\tau}}_{1i}$ on true MTR yields a coefficient of 0.13 with a robust standard error of 0.17, suggesting there may be a weakly positive relationship, but we cannot reject the null hypothesis that $\hat{\tilde{\tau}}_{1i}$ is unrelated to the true MTR on average. This suggests that the bias channel identified in Equation (4) will reduce DWL, as respondents on average underestimate their MTR. To capture the variance channel, the blue shaded region in Figure

XVII shows, for each bin of true MTR, the range from the 20th to the 80th percentile of fitted values of $\hat{\tilde{\tau}}_{1i}$ as predicted by the instrument $\hat{\tilde{\tau}}_{2i}$. Here, we use the instrument to predict the 20th to 80th percentile range of $\hat{\tilde{\tau}}_{1i}$ as opposed to directly computing this range for $\hat{\tilde{\tau}}_{1i}$ to avoid conflating variation due to noise in survey responses and to isolate only variation that is consistent across survey questions within a respondent. To construct the fitted values, we regress $\hat{\tilde{\tau}}_{1i}$ on linear and quadratic terms of the instrument $\hat{\tilde{\tau}}_{2i}$ and the interaction of the quadratic term with the true MTR to allow for variation in the relationship between $\hat{\tilde{\tau}}_{1i}$ and the instrument over the true MTR.[50] Even when we isolate only the variation in $\hat{\tilde{\tau}}_{1i}$ that can be explained by the instrument $\hat{\tilde{\tau}}_{2i}$, we find substantial variation in perceived tax rates conditional on the true MTR. At true MTRs below 10%, the instrumented 20th to 80th percentile range of perceived MTR spans approximately from 5% to 17%. At true MTRs around 20%, the range spans approximately from 7% to 25%. Because we report the 20th to 80th percentile range, 40% of perceived tax rates are still further away from the conditional average perceptions, raising the overall variation in perceptions beyond what is captured in Figure XVII.[51] In total, this demonstrates that there is substantial variation in misperceptions, and as a result the variance channel reflected in Equation (4) will considerably increase DWL. Finally, while not conclusive, Figure XVII suggests that the covariance of squared misperceptions with the tax rate may be negative and will thereby reduce DWL. Figure XVII shows that average misperception is decreasing in the true tax rate. However, because DWL is impacted by the covariance of squared misperceptions with the tax rate, the evidence in Figure XVII is only suggestive that this channel decreases DWL.

We now proceed to estimating the impact of each channel of misperception on DWL as reflected in Equation (4) and following the identification results in Section IV.B. The results are shown in Table V. The estimates in column 1 do not use the instrument and exclude bias correction terms – for example, $Var(\theta_i)$ is estimated with $\hat{Var}(\hat{\theta}_{1i})$; the estimates in column 2 make use of the instrument but still exclude bias correction terms – for example, $Var(\theta_i)$ is estimated with $\hat{Cov}(\hat{\theta}_{1i}, \hat{\theta}_{2i})$; and, finally, the estimates in column 3 – our preferred estimates – make use of the instrument and include bias

[50] Table B.16 shows the first stage regression results. In our preferred specification, we omit the interaction between the linear term of the instrument and the true MTR, as it is not significant when included.

[51] Moreover, this procedure only captures variation in $\hat{\tilde{\tau}}_{1i}$ that can be explained by linear and quadratic terms in $\hat{\tilde{\tau}}_{2i}$, which is likely to underestimate even the range from the 20th to the 80th percentile.

correction terms as in the identification results of Section IV.B. All standard errors in Table V are bootstrapped with 1,000 repetitions.

Our main estimates show that misperception dramatically impacts DWL, primarily through the channels of bias, variance, and covariance with tax rate. The first row shows the proportional impact of misperception on DWL, $(E[\theta_i]^2 - 1) + Var(\theta_i)$, prior to accounting for covariance channels.[52] If misperception were not correlated with income, elasticity, or tax rate, it would increase DWL by 118%. Breaking this effect down into bias ($E[\theta_i]^2$) and variance ($Var(\theta_i)$) channels, we see that the increase in DWL is more than fully accounted for by the large variance of misperceptions. Because individuals slightly underestimate their tax rate on average, DWL would be reduced by 22% if misperceptions were identical across the population; however, the substantial variance of misperceptions increases DWL by 140%. The last three rows of Table V show estimates of the covariance of misperception with income, elasticity, and tax rate. We proxy for elasticity using an indicator for whether nonlabor sources account for at least 20% of the respondent's household income,[53] consistent with our finding that individuals with more nonwage income reduce income by a greater extent in response to the college aid implicit tax as well as with the broader public economics literature finding similar results in other implicit and explicit tax settings (e.g. Chetty et al. 2013). We cannot reject the nulls that misperception does not covary with income or elasticity. However, we find that misperception is negatively correlated with the true tax rate. The estimated bias, variance, and covariance with tax rate channels are consistent with the suggestive evidence presented in Figure XVII: individuals slightly underestimate their tax rate on average, reducing DWL; there is substantial variance to misperceptions, increasing DWL; and misperception is negatively correlated with the true tax rate, consistent with greater underestimation on average at higher tax rates.

[52] Relative to Equation (4), here we rewrite $E[\theta_i]^2 + Var(\theta_i)$ as $1 + (E[\theta_i]^2 - 1) + Var(\theta_i)$. We refer to $(E[\theta_i]^2 - 1) + Var(\theta_i)$ as the "proportional impact", so that a positive (negative) value can be interpreted as an increase (decrease) in DWL. This also facilitates separating contributions into Bias $(E[\theta_i]^2 - 1)$ and Variance $(Var(\theta_i))$ channels.

[53] Specifically, the survey question for a respondent with a partner reads: "Roughly, what percent of your total household income comes from sources **other than** work (wages, salaries, tips, etc.), self-employment, or business ownership? Examples of other income sources include capital gains, interest, dividends, rental income, retirement income, or transfer payments. Consider your income and your partner's income." Respondents select one of the following options: 0% - 5%, 5% - 10%, 10% - 20%, 20% - 40%, 40% - 60%, or "More than 60%". We aim to target one dimension along which elasticity importantly varies, though is it possible that misperception may covary with elasticity along other dimensions, which is an important avenue for future work to consider.

Comparing our main estimates in column 3 with those in columns 1 and 2 demonstrates the value of instrumenting variation in misperceptions and properly accounting for a level difference in the instrument using bias correction terms, which are particularly important for estimating the impact of variance of misperceptions on DWL. Column 1 shows raw estimates that do not use the instrument at all – these are the estimates a researcher would obtain if they only collected one measure of survey respondents' perceived tax rate and did not have a second measure to use as an instrument. Without using the instrument, we would significantly overestimate the increase in DWL that is due to the variance of misperceptions at 227% as opposed to our preferred estimate of 140%. This reflects the fact that, without an instrument, a researcher would conflate variation due to noise in survey responses with true variation in perceptions; the instrument allows us to successfully isolate only variation in perceptions. Column 2 uses the instrument but excludes bias correction terms. Here, we estimate that variance of misperceptions increases DWL by 147%. This is much closer to our preferred estimate of 140%, reflecting that use of the instrument accounts for almost all of the overestimation in column 1, while accounting for the bias correction term has a smaller impact. In the last row, we find that we would slightly underestimate the covariance of misperception with the true tax rate if we did not have an instrument, though the estimated covariance is significant in all three columns. We cannot reject the nulls that misperception does not covary with income or elasticity in any of the three specifications.

For a more clear understanding of the net effect of misperception on DWL accounting for the channels of bias, variance, and covariance with the tax rate, we can apply the Law of Iterated Expectations to Equation (4) to compute DWL at each tax rate and then average across tax rates as:

$$DWL \approx \underbrace{N \cdot \frac{1}{2} \cdot E\left[\tau^2 E[Z_{i,1} e(X_i)|\tau]\right]}_{\text{Correct Perceptions}} \cdot (1 + \underbrace{(E[\theta_i|\tau]^2 - 1)}_{\text{Conditional Bias}} + \underbrace{Var(\theta_i|\tau))}_{\text{Conditional Variance}} \Big] \tag{6}$$

where we omit the terms from Equation (4) that depend on the covariance of misperception with income or elasticity as we do not find evidence that these channels are statistically different from zero, and the term capturing covariance of misperception with the tax rate drops out due to conditioning on the tax rate. Equation (6) shows that, at each tax rate, misperception impacts DWL proportionally

through the combination of conditional bias ($E[\theta_i|\tau]^2$) and variance ($Var(\theta_i|\tau)$) channels.

Following the result of Equation (6), Figure XVIII conditions on 10 bins of true tax rate, and for each bin computes the total proportional impact of misperception on DWL and the contributions due to bias and variance, along with a third order polynomial fit for each. The impact of misperception on DWL is greater at lower tax rates, leveling off for tax rates above roughly 10%. At tax rates below 10%, misperception increases DWL by 400%, due in nearly equal parts to overestimation of tax rates on average and high variance of misperceptions. For tax rates above 10%, misperception increases DWL by approximately 80-100%, with underestimation of tax rates on average reducing DWL by around a third and variance of misperceptions increasing DWL by over 100%.[54]

Finally, we combine our ETI estimates from Section III with our survey-based measures of misperception to fully identify DWL due to the college aid implicit tax and the contribution due to misperception and its various channels as outlined in Equation (4). Because we do not find statistical evidence that either of $Cov(\theta_i^2, e(X_i))$ or $Cov(\theta_i^2, Z_{i,1})$ is nonzero, we omit these terms from estimation. We use our estimates of the remaining terms involving $\theta_i - E[\theta_i]^2$, $Var(\theta_i)$, and $Cov(\theta_i^2, \tau_i^2)$ – as reported in column 3 of Table V. Recall the distinction above between the reduced form elasticity $\beta^{ETI}(X_i)$ and the structural elasticity $e(X_i)$. While our analysis in Section III estimates the reduced form elasticity, Equation (4) depends on the structural elasticity, so we must use the result of Equation (5) to convert the reduced form to the structural elasticity. Using the earlier identification result that $E[\ln(1-\tilde{\tau}_i)|X_i] \approx E[\ln(1-\hat{\tilde{\tau}}_{1i})|X_i]$, we estimate the factor that converts the reduced form to the structural elasticity $\frac{E[\ln(1-\tau_i)|X_i]}{E[\ln(1-\tilde{\tau}_i)|X_i]}$ to be 1.001 with a standard error of .054. Because we cannot reject the null that the conversion factor equals one – or, that the structural elasticity equals the reduced form elasticity – in our DWL calculations we use the reduced form elasticities directly.[55] To estimate the components of Equation (4), $E[Z_{i,1}e(X_i)\tau_i^2]$ and $E[Z_{i,1}]E[e(X_i)]$, we simply compute the corresponding sample averages directly in the tax data.[56] We restrict to households with baseline income averaged

[54] Figure A.5 shows similar estimates by child age. Perceptions appear to improve slightly with child age, but far from dramatically. Without accounting for the contribution from covariances, the variance of misperceptions still increases DWL by over 100% at nearly all ages, and the fitted proportional impact of misperception on DWL remains over 100% at all ages.

[55] We also cannot reject the null that the conversion factor equals one separately for those with high versus low nonlabor income, or that the conversion factor is equal for these two groups.

[56] We allow the elasticity to vary by high or low nonwage income share. $Z_{i,1}$ conceptually measures the individual's

over 2005 and 2006 between $50-150k, which filed taxes in 2015, and which have a child who turned 18 between 2011 and 2016 and who is enrolled in 2016 at a four-year in-state public college or a four-year private college for which we can measure average financial aid by family income bins. This leaves us with 713,885 households. Our estimates of DWL due to the college aid implicit tax and the contribution due to misperception will necessarily be underestimates as we focus on four-year college enrollment among middle-income households, omitting DWL produced among lower and higher income households as well as households with a child attending a two-year college; we find evidence in Section III that all of these omitted groups also reduce income in response to the college aid implicit tax.

Table VI reports our DWL estimates. The first column of Table VI reports average DWL at the household level, and the second column reports total DWL summed over all households, which we will focus on here. In total, the college aid implicit tax creates $253.7 million in DWL annually among middle-income households with a child attending a four-year in-state public or four-year private college. Of this, $118.9 million is due to misperception – or, misperception increases DWL by 88% relative to the $134.8 million that would prevail under accurate perceptions. The contribution to DWL from misperception is more than fully accounted for by the substantial variance of misperception across individuals, which alone increases DWL by $188.7 million. The slight underestimation of the MTR from college aid on average and the negative covariance between squared misperceptions and squared MTR reduce DWL by $29.7 million and $40.1 million, respectively. Because our DWL estimates restrict to middle-income families with a child enrolled at a four-year in-state public or four-year private college, they almost certainly underestimate the total DWL due to the college aid implicit tax and the contribution from misperception when accounting for lower- and higher-income families and children enrolled at two-year colleges or four-year out-of-state public colleges.

income in the taxed period if the tax were not in place. Therefore, to measure $Z_{i,1}$, we start with each family's observed income and then subtract the overall average effect on income due to the college aid implicit tax. In the small number of cases in which we measure a (small) negative MTR from IPEDS data, we replace τ_i^2 as zero.

V Conclusion

This paper makes two primary contributions. First, we provide the first estimates of the elasticity of taxable income for one of the largest means-tested programs in the US: college financial aid. We additionally estimate the marginal propensity to earn with respect to college costs. Using federal income tax data to compare families with a child who enrolls in college at age 18 against families with a child that does not enroll in college at 18, and exploiting the fact that income is subject to the college aid implicit tax one year before college costs are due, we estimate an ETI of .07 and an MPE of .44 for families with a child enrolled in a four-year in-state public college or a four-year private college. The ETI is approximately twice as large for families with a high share of nonwage income and families with interest income above $500, reflecting relatively high assets. Most of the ETI is due to changes in wage income, with business income and capital gains also accounting for substantial portions of the overall effect. The MPE is primarily due to changes in wage and business income.

Second, we develop a general sufficient statistics framework for studying the impact of misperception of a complex tax on deadweight loss, and we administer a survey to measure perceptions of the college aid implicit tax and identify the model in this setting. Counter to much of the existing literature on tax rate misperception, we find that misperception of the college aid implicit tax *increases* DWL by 88%, or $119 million annually just among middle-income families with a child enrolled at a four-year in-state public or four-year private college. This effect is driven by the substantial variance of misperceptions across individuals, a feature highlighted by the model. Because we focus on middle-income families with a child enrolled at a four-year in-state public or four-year private college, our DWL estimates almost certainly underestimate total DWL and the contribution from misperception across the full college population. Our model demonstrates that, to measure the welfare impact of misperceptions, it is not enough to know the average misperception; one must also know the variance of misperceptions as well as the covariance of squared misperceptions with income, elasticity, and tax rate. We show how to identify each of these channels and the total impact of misperception of a complex tax on DWL using survey data. The methods we introduce can be applied to quantify the welfare impact of misperception of any complex tax, including progressive income tax schedules or

other implicit taxes due to means-tested benefits such as the Social Security earnings test (Liebman and Luttmer 2015), the Child Tax Credit (Feldman et al. 2016), the EITC (Chetty et al. 2013), or others.

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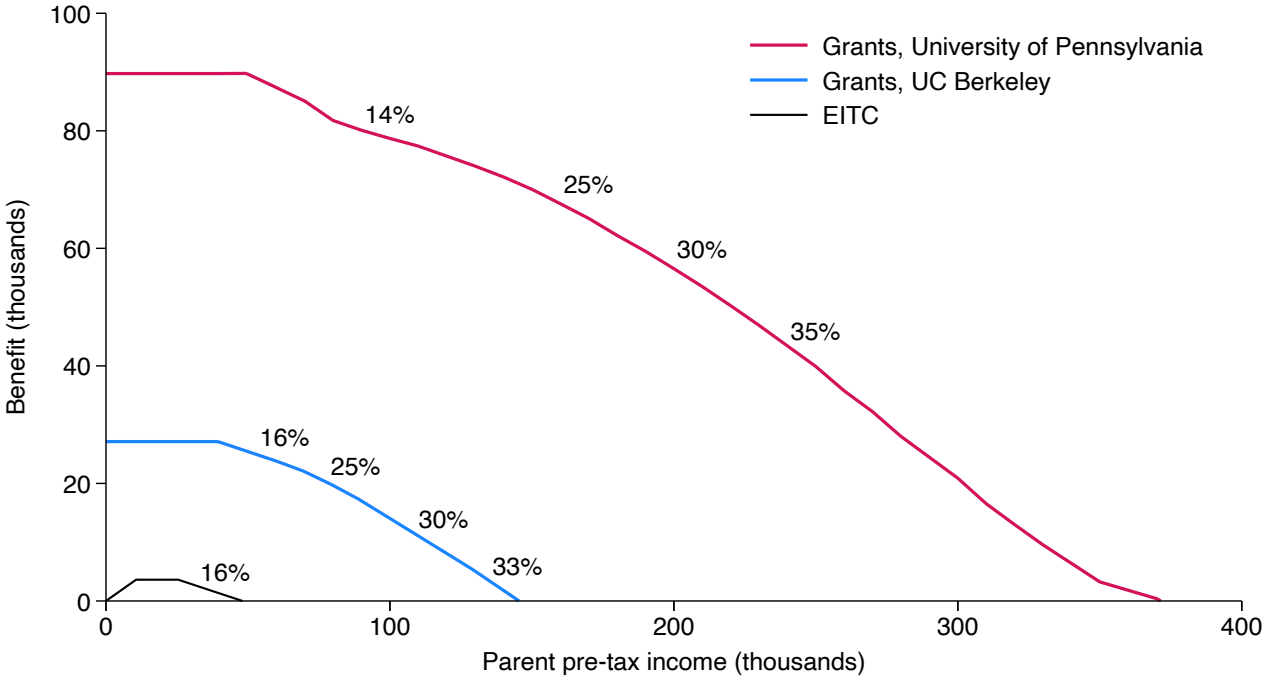
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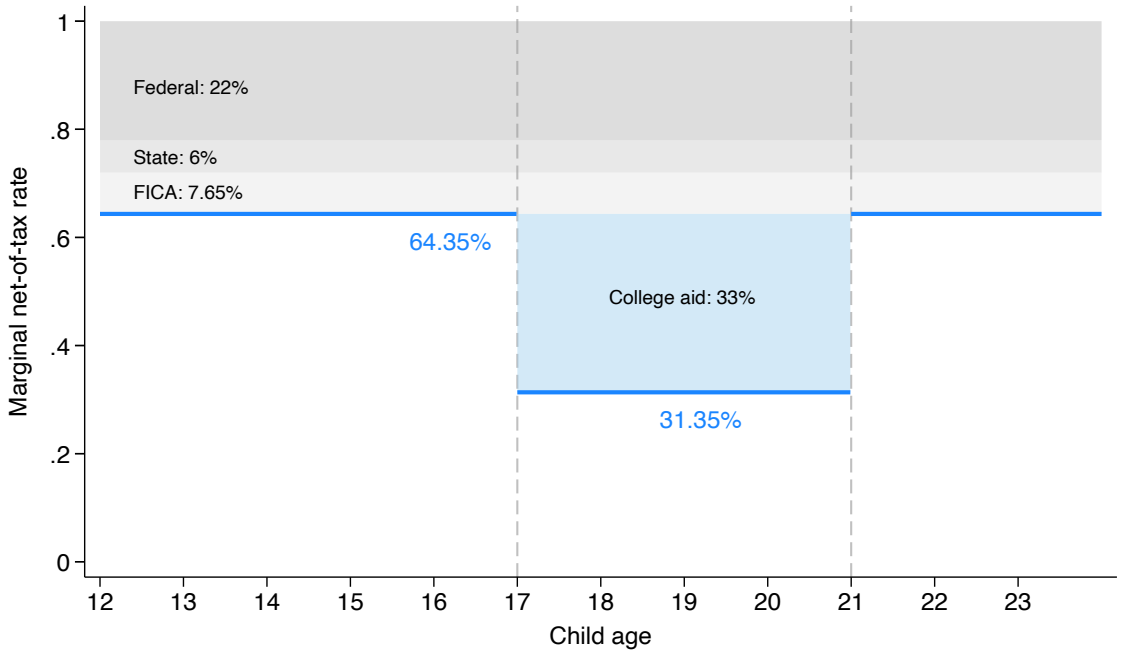
Figure I
The Size of the Implicit Tax on Income from College Aid



Notes: The top line in the figure shows the expected annual grant aid for one child enrolled at the University of Pennsylvania based on parent pre-tax income. The middle line is the expected annual grant aid for one child enrolled at UC Berkeley. As a reference point, the bottom line is the EITC schedule for a single parent with one child. Marginal implicit tax rates are labeled in the figure. Assets are fixed to zero when computing estimated college grant schedules via Net Price Calculator websites, and we adjust for estimated income taxes.

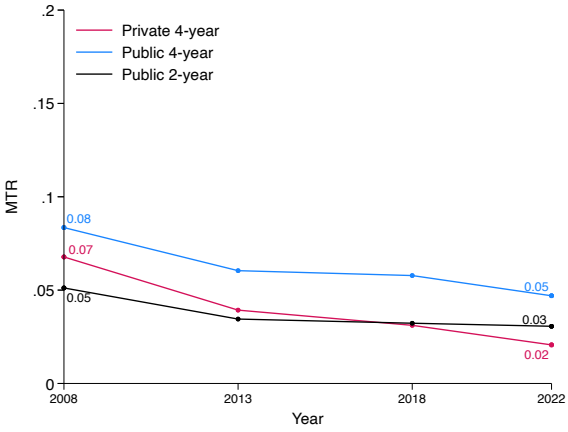
Figure II
Marginal Net-of-tax Rate for $125k Income Family around College

UC Berkeley, CA resident

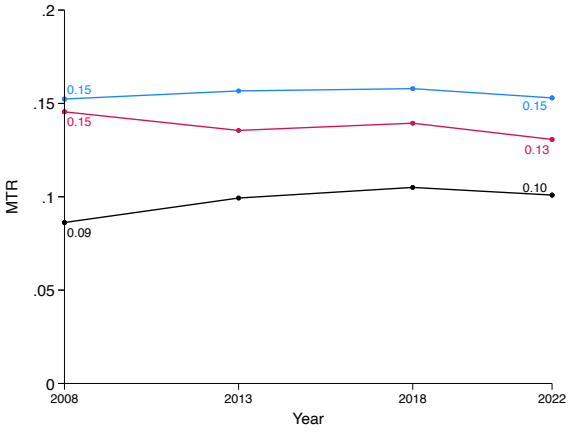


Notes: The figure plots the marginal net-of-tax rate on income faced by a family with income $125k who live in California and enroll a child at UC Berkeley around the years in which parent income is used to determine financial aid for the child. The marginal net-of-tax rate accounts for federal and state income taxes along with the implicit tax from college aid.

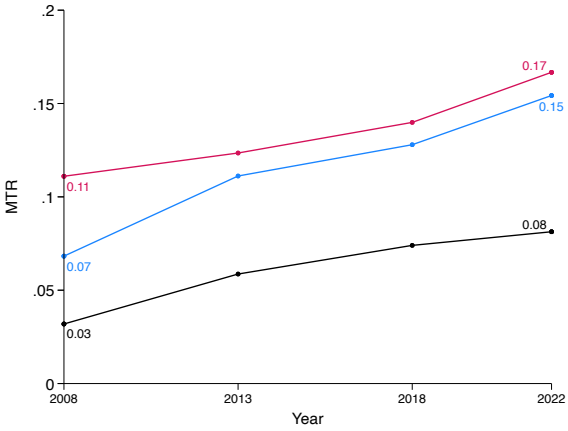
Figure III
Trends in the College Aid Implicit Tax



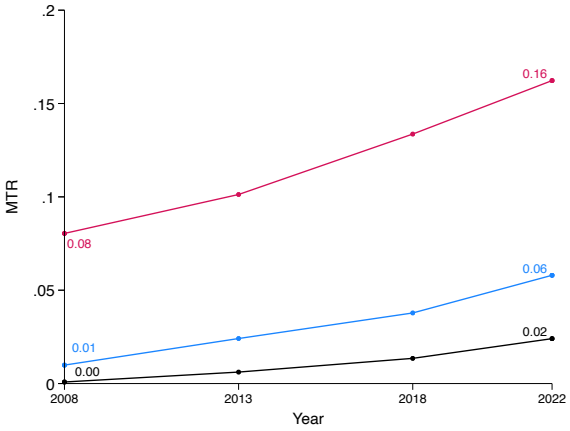
(A) \$0-30k to \$30-48k



(B) \$30-48k to \$48-75k



(C) \$48-75k to \$75-110k



(D) \$75-110k to \$110k+

Notes: The figure shows estimated MTRs from college aid based on IPEDS data. Averages are weighted by total enrollment.

Figure IV
Excerpt of 2015 Expected Family Contribution (EFC) Formula

2015–2016 EFC FORMULA A: DEPENDENT STUDENT

REGULAR WORKSHEET Page 1 **A**

PARENTS' INCOME IN 2014		
1. Parents' Adjusted Gross Income (FAFSA/SAR #85) If negative, enter zero.		
2. a. Parent 1 (father/mother/stepparent) income earned from work (FAFSA/SAR #88) _____		
2. b. Parent 2 (father/mother/stepparent) income earned from work (FAFSA/SAR #89) + _____		
Total parents' income earned from work	=	
3. Parents' Taxable Income (If tax filers, enter the amount from line 1 above. If non-tax filers, enter the amount from line 2.)*		
4. Total untaxed income and benefits: (Total of FAFSA/SAR #94a. through 94i.)	+	
5. Taxable and untaxed income (sum of line 3 and line 4)	=	
6. Total additional financial information (Total of FAFSA/SAR #93a. through 93f.)	–	
7. TOTAL INCOME (line 5 minus line 6) May be a negative number.	=	

ALLOWANCES AGAINST PARENTS' INCOME		
8. 2014 U.S. income tax paid (FAFSA/SAR #86) (tax filers only) If negative, enter zero.		
9. State and other tax allowance (Table A1) If negative, enter zero.	+	
10. Parent 1 (father/mother/stepparent) Social Security tax allowance (Table A2)	+	
11. Parent 2 (father/mother/stepparent) Social Security tax allowance (Table A2)	+	
12. Income protection allowance (Table A3)	+	
13. Employment expense allowance: • Two working parents (Parents' Marital Status is "married" or "unmarried and both parents living together"): 35% of the lesser of the earned incomes, or $4,000, whichever is less • One-parent families: 35% of earned income, or $4,000, whichever is less • Two-parent families, one working parent: enter zero	+	
14. TOTAL ALLOWANCES	=	

*STOP HERE if the following are true:

Line 3 is $24,000 or less **and**

- The parents are eligible to file a 2014 IRS Form 1040A or 1040EZ (they are not required to file a 2014 Form 1040) or they are not required to file any income tax return **or**
- Anyone included in the parents' household size (as defined on the FAFSA) received benefits during 2013 or 2014 from any of the designated means-tested federal benefit programs **or**
- Either of the parents is a dislocated worker.

If these circumstances are true, the Expected Family Contribution is automatically zero.

AVAILABLE INCOME		
Total income (from line 7)		
Total allowances (from line 14)	–	
15. AVAILABLE INCOME (AI) May be a negative number.	=	

PARENTS' CONTRIBUTION FROM ASSETS		
16. Cash, savings & checking (FAFSA/SAR #90)		
17. Net worth of investments** (FAFSA/SAR #91) If negative, enter zero.		
18. Net worth of business and/or investment farm (FAFSA/SAR #92) If negative, enter zero.		
19. Adjusted net worth of business/farm (Calculate using Table A4.)	+	
20. Net worth (sum of lines 16, 17, and 19)	=	
21. Education savings and asset protection allowance (Table A5)	–	
22. Discretionary net worth (line 20 minus line 21)	=	
23. Asset conversion rate	×	**.12**
24. CONTRIBUTION FROM ASSETS If negative, enter zero.	=	

PARENTS' CONTRIBUTION		
AVAILABLE INCOME (AI) (from line 15)		
CONTRIBUTION FROM ASSETS (from line 24)	+	
25. Adjusted Available Income (AAI) May be a negative number.	=	
26. Total parents' contribution from AAI (Calculate using Table A6.) If negative, enter zero.		
27. Number in college in 2015–2016 (Exclude parents) (FAFSA/SAR #74)	÷	
28. PARENTS' CONTRIBUTION (standard contribution for nine-month enrollment)*** If negative, enter zero.	=	

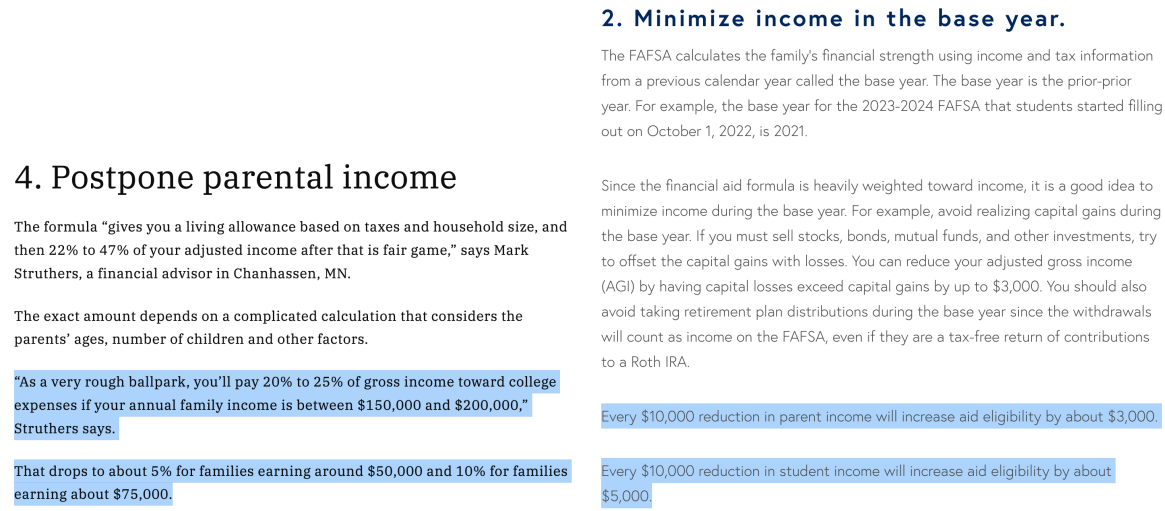
**Do *not* include the family's home.

***To calculate the parents' contribution for other than nine-month enrollment, see page 11.

continued on the next page

Notes: Excerpt from The EFC Formula, 2015-2016. Source: U.S. Department of Education, Federal Student Aid.

Figure V
News Descriptions of College Aid Implicit Tax



4. Postpone parental income

The formula "gives you a living allowance based on taxes and household size, and then 22% to 47% of your adjusted income after that is fair game," says Mark Struthers, a financial advisor in Chanhassen, MN.

The exact amount depends on a complicated calculation that considers the parents' ages, number of children and other factors.

"As a very rough ballpark, you'll pay 20% to 25% of gross income toward college expenses if your annual family income is between $150,000 and $200,000," Struthers says.

That drops to about 5% for families earning around $50,000 and 10% for families earning about $75,000.

2. Minimize income in the base year.

The FAFSA calculates the family's financial strength using income and tax information from a previous calendar year called the base year. The base year is the prior-prior year. For example, the base year for the 2023-2024 FAFSA that students started filling out on October 1, 2022, is 2021.

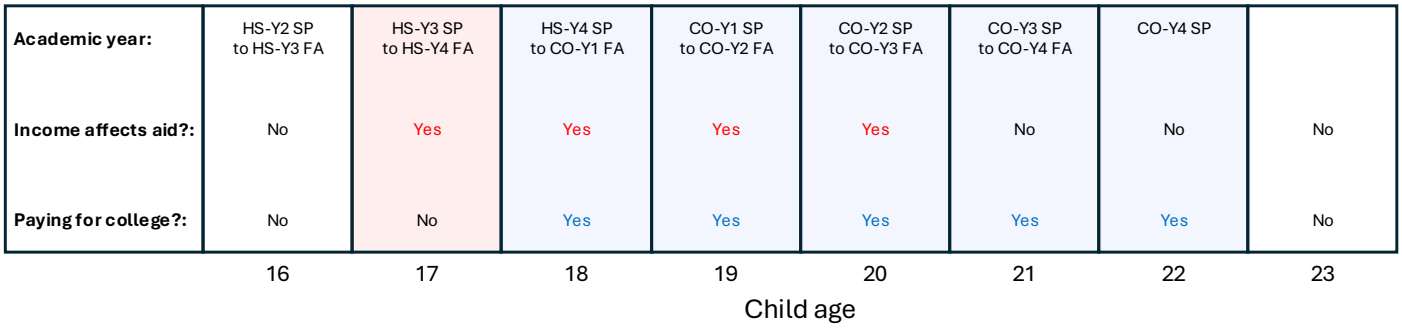
Since the financial aid formula is heavily weighted toward income, it is a good idea to minimize income during the base year. For example, avoid realizing capital gains during the base year. If you must sell stocks, bonds, mutual funds, and other investments, try to offset the capital gains with losses. You can reduce your adjusted gross income (AGI) by having capital losses exceed capital gains by up to $3,000. You should also avoid taking retirement plan distributions during the base year since the withdrawals will count as income on the FAFSA, even if they are a tax-free return of contributions to a Roth IRA.

Every $10,000 reduction in parent income will increase aid eligibility by about $3,000.

Every $10,000 reduction in student income will increase aid eligibility by about $5,000.

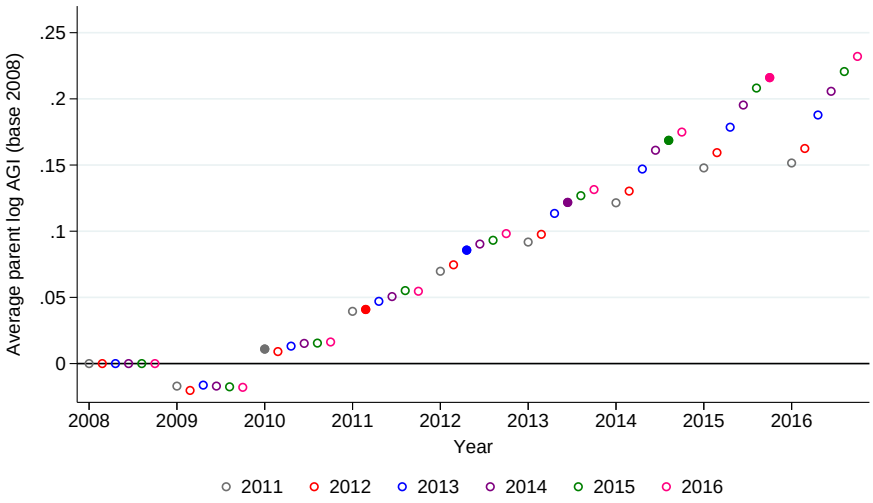
Notes: Sources: Money, "FAFSA Tips: These 7 Moves Could Help You Score More Financial Aid" (https://money.com/more-financial-aid-money-fafsa/) and College Ave, "File the FAFSA to Get More Money in College" (https://www.collegeavestudentloans.com/blog/file-the-fafsa-to-get-more-money-in-college).

Figure VI
Timeline of College-related Incentives

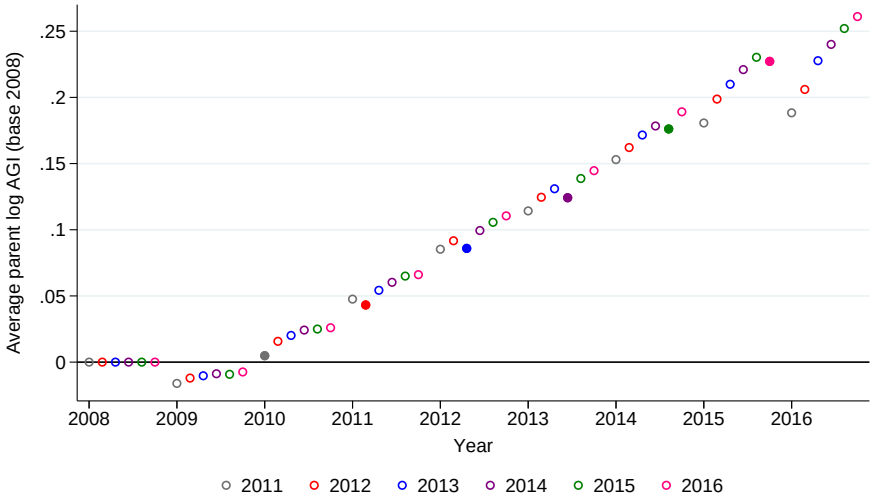


Notes: The figure shows, over child age as of September 1, the academic terms corresponding to the matched calendar year, whether income earned in that year will affect the child's college aid (for a child who begins college immediately after high school and is enrolled for four years), and whether college bills are due. At age 17, parent income affects college aid, but college costs are not due. College costs are due from ages 18 through 22. For ages 18 through 20, income affects aid and costs are due.

Figure VII
Income Patterns around Child Turning 18



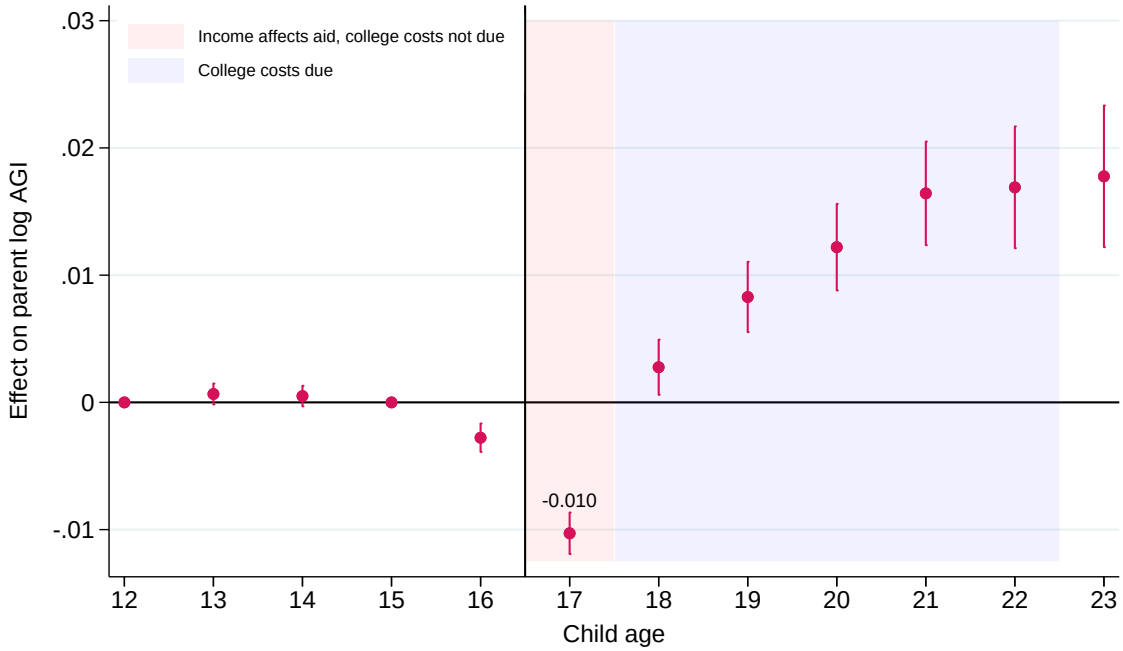
(A) Non-college



(B) College

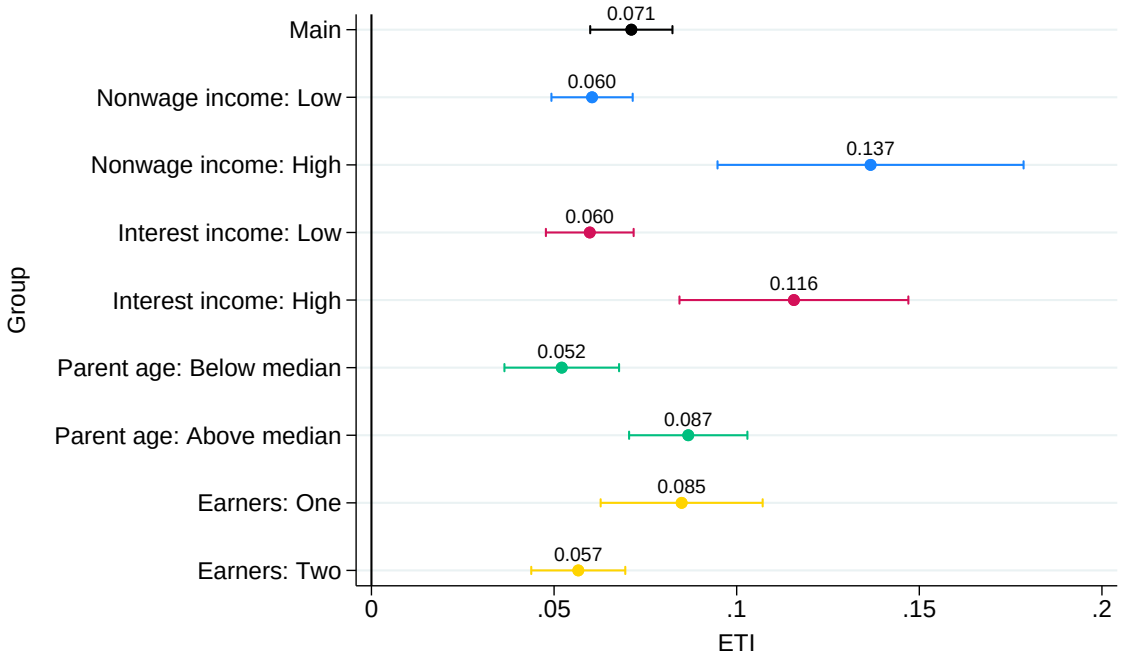
Notes: The figure shows patterns of average parent log AGI by child cohort (year the child turns 18) and year, relative to the base year 2008. Panel A shows patterns for non-college families. Panel B shows patterns for college families.

Figure VIII
Parent Income over Child Age, College Minus Non-college



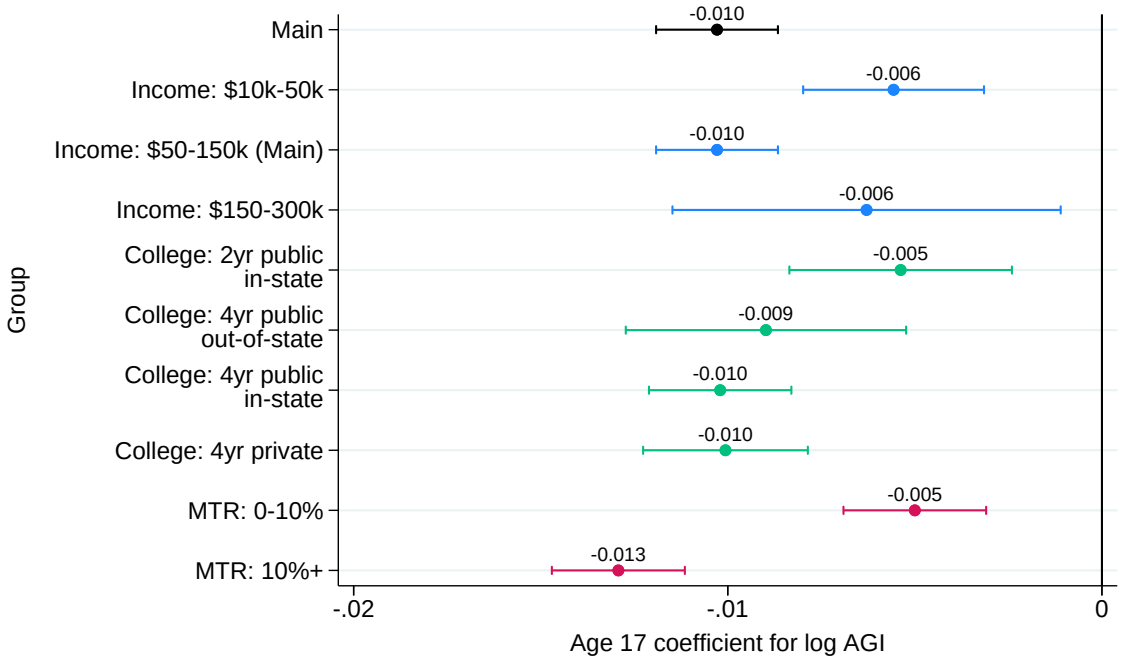
Notes: The figure reports estimates of interacted child age effects (β_k) from Equation (1) with log AGI as the outcome. Red shading at age 17 indicates when parent income affects college aid but college costs are not due, isolating the pure substitution effect. Blue shading at ages 18 through 22 indicates when college costs are due, assuming four years of enrollment. Standard errors are clustered at the family level.

Figure IX
ETI Heterogeneity



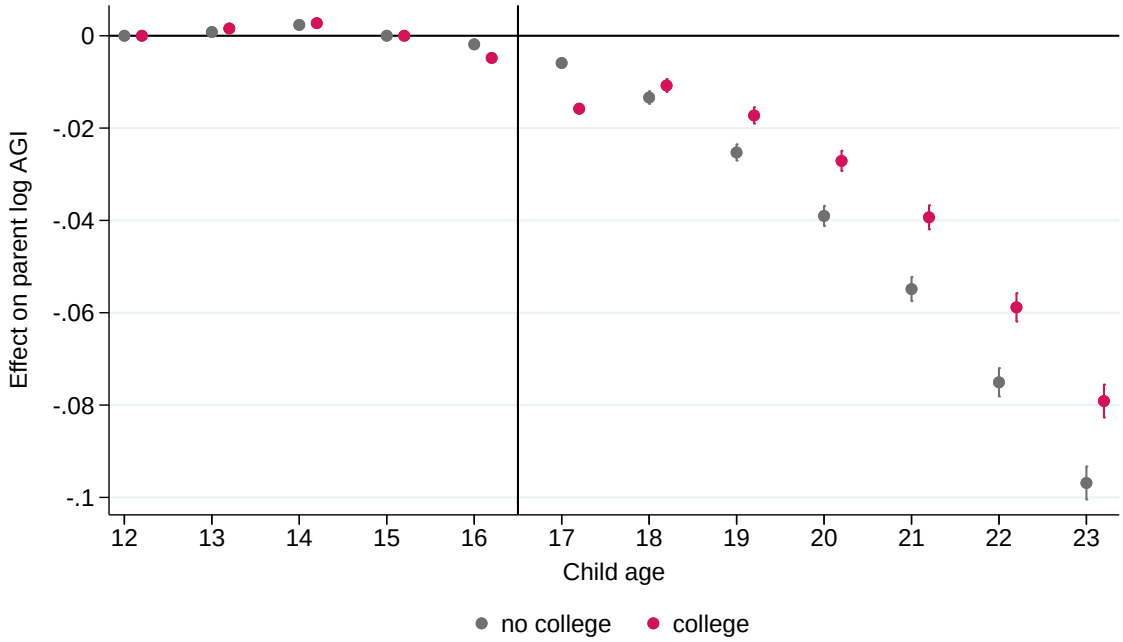
Notes: The figure shows ETI estimates by group. We estimate the ETI by 2SLS with equations similar to Equation (1), where the endogenous variable is the log net-of-tax rate and the omitted instrument is the interaction between the indicator for the college group and the indicator for child age 17. Standard errors are clustered at the family level.

Figure X
Reduced Form Substitution Effect Heterogeneity



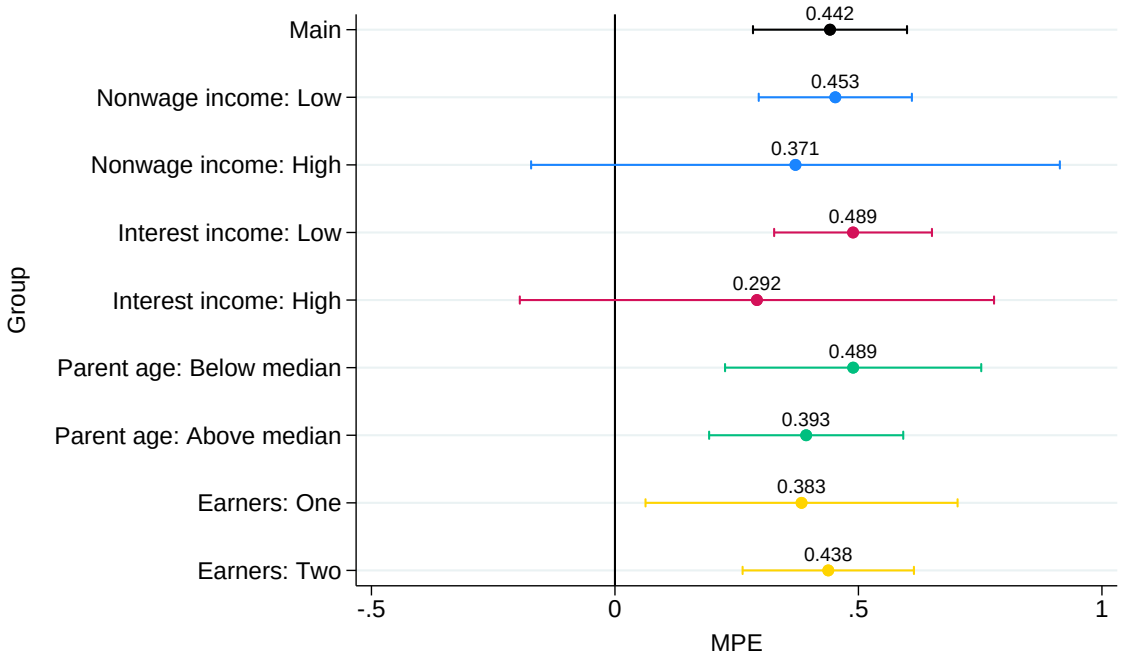
Notes: The figure shows estimates of β_{17} from Equation (1) by group with log AGI as the outcome. Standard errors are clustered at the family level.

Figure XI
(Lack of) Selection into College from Income Shocks



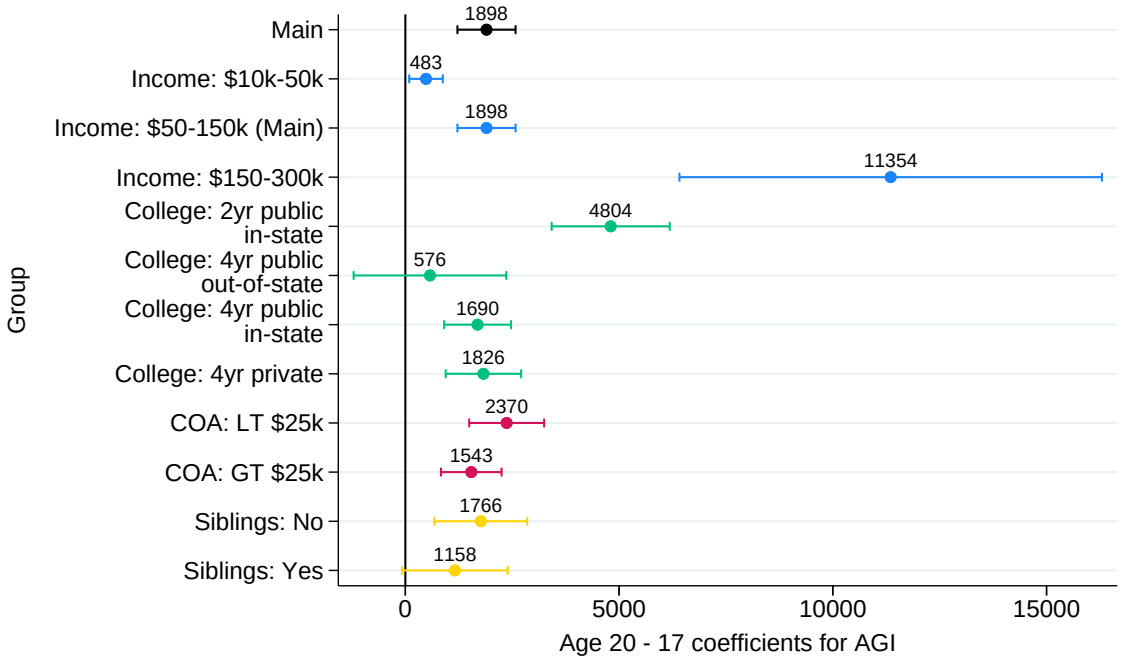
Notes: The figure shows estimated child age effects from a version of Equation (1) without interactions on log AGI separately for families in which the child goes to no college at age 18 and families in which the child goes to any college at age 18. Standard errors are clustered at the family level.

Figure XII
MPE Heterogeneity



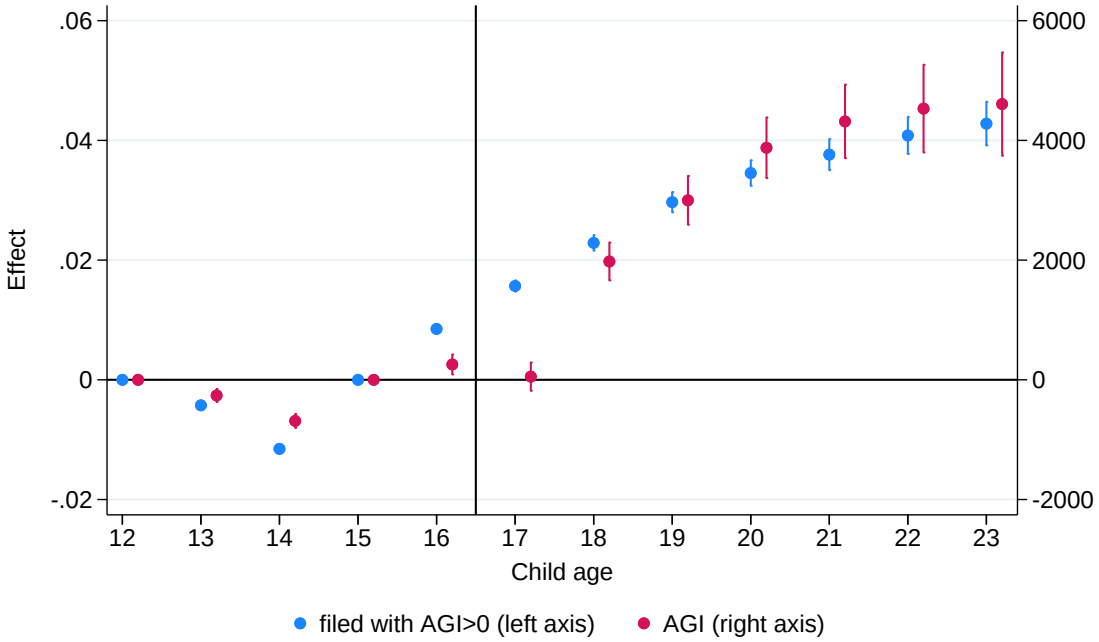
Notes: The figure shows MPE estimates by group. The MPE is the ratio of the reduced form effect of college costs on AGI, which is $\beta_{20} - \beta_{17}$ from Equation (1), to the first stage effect on annuitized total college costs. Standard errors are clustered at the family level.

Figure XIII
Reduced Form Income Effect Heterogeneity



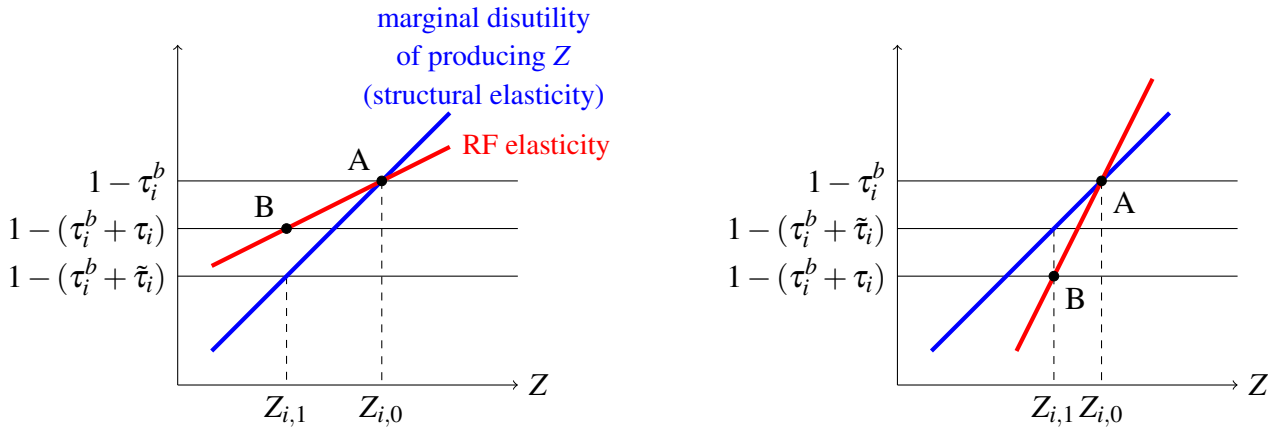
Notes: The figure shows estimates of $\beta_{20} - \beta_{17}$ from Equation (1) by group with AGI as the outcome. Standard errors are clustered at the family level.

Figure XIV
Effects on Filing and AGI without Persistent Filing Sample Restriction



Notes: The figure reports estimates of interacted child age effects (β) from Equation (1) with indicators for filing with positive AGI and on AGI as the outcome, among the expanded sample that includes nonfilers. Standard errors are clustered at the family level.

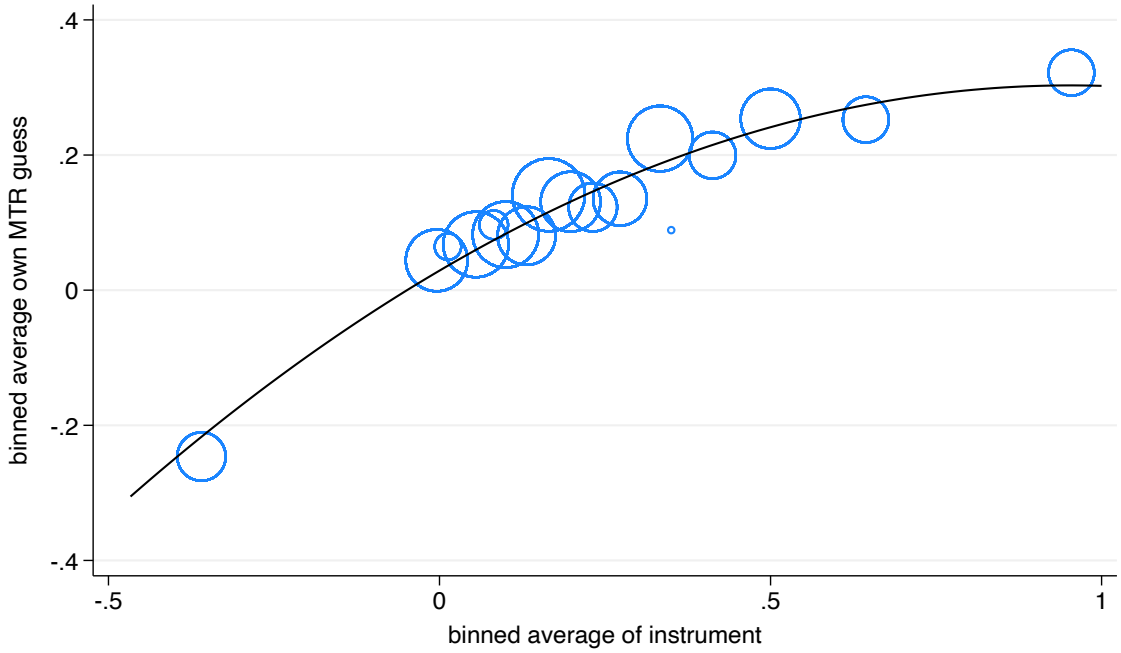
Figure XV
Reduced Form vs. Structural Elasticity



(A) $\tilde{\tau}_i > \tau_i$ (B) $\tilde{\tau}_i < \tau_i$

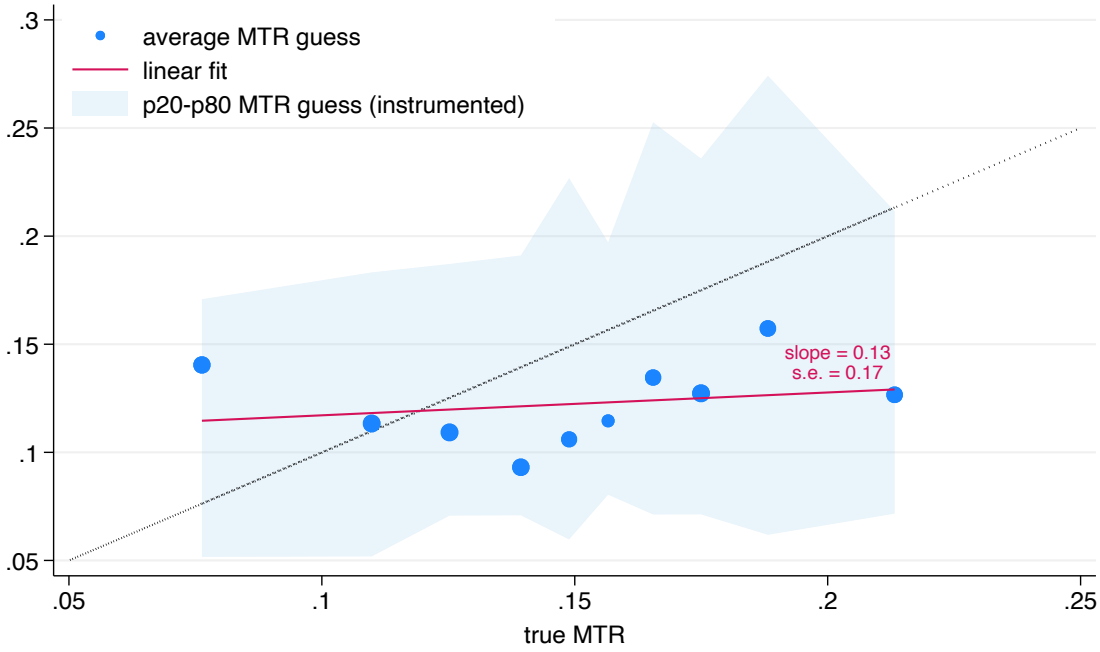
Notes: The figure details the relationship between the structural elasticity. In the left panel, the perceived tax rate is higher than the true tax rate. The blue curve is the structural relationship between income earned and the marginal disutility of earning income, and this curve along with the perceived tax determines income decisions. The econometrician observes data points A and B, and uses these to fit the reduced form relationship between income and the actual tax rate and thereby estimate the reduced form elasticity. In this case, the reduced form elasticity is larger than the underlying structural elasticity. The right panel shows the opposite case, where the perceive tax is lower than the true tax rate. In this case, the reduced form elasticity is smaller than the underlying structural elasticity.

Figure XVI
First Stage Relationship between Survey Instrument and Tax Perception



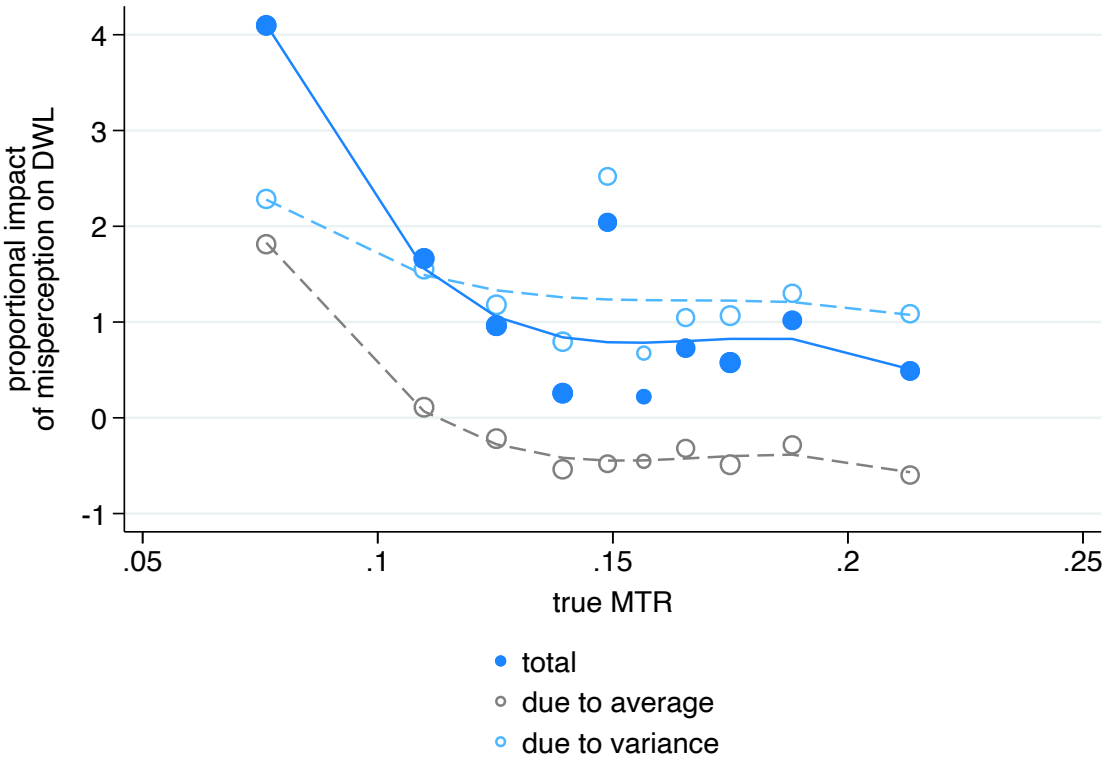
Notes: The figure shows, for 20 bins of the survey instrument, the average value of the respondent's own MTR guess against the average value of the instrument, along with a quadratic fit.

Figure XVII
Average and Spread of Misperception by True Tax Rate



Notes: For 10 bins of true MTR, the figure shows the average MTR guess with linear fit, as well as the range from the 20th to 80th percentiles of MTR guesses that are instrumented by a first stage regression on linear and quadratic terms of the instrument.

Figure XVIII
Impact of Misperception on Deadweight Loss by Tax Rate



Notes: For 10 bins of true MTR, the figure shows the proportional impact of misperception on DWL – i.e. $(E[\theta_i]^2 - 1) + Var(\theta_i)$ – along with the bias $(E[\theta_i]^2 - 1)$ and variance components, as well as a third order polynomial fit for each.

Table I
Tax Data Descriptive Statistics

	Mean	Std. dev.	Median	# of obs.
Household characteristics				
AGI (2005-06 average)	85,613	25,553	80,770	1,840,548
W-2 wages (2005-06 average)	78,058	29,113	74,870	1,840,548
Share nonwage income (2005-06 average)	0.09	0.21	0.01	1,840,548
Taxable interest (2005-06 average)	479	2,330	40	1,840,548
Primary filer age when child is 17	48	7	47	1,840,278
% two earners (2005 and 2006)	0.65	0.48	1.00	1,641,665
% treated	0.38	0.49	0.00	1,840,548
College characteristics, among treated				
MTR from college aid	0.13	0.08	0.12	700,508
Cost of attendance	37,040	16,129	29,530	698,495
Net price	21,939	8,211	20,709	700,508

Notes: Median values for AGI, W-2 wages, and taxable interest are rounded to the nearest $10 for disclosure purposes.

Table II
ETI due to the College Aid Implicit Tax

	Reduced form	First stage	ETI (IV)
	-0.010	-0.145	0.071
	(0.0008)	(0.0001)	(0.0057)
N	20,318,954	20,318,954	20,318,954

Notes: The first column shows the estimated reduced form effect of the college aid implicit tax on log AGI, which is β_{17} from Equation (1) The second column shows the estimated first stage effect on the log net-of-tax rate from college aid using the same specification. The third column shows the estimated ETI, which is the ratio of the reduced form to the first stage. The standard error for the ETI is computed by 2SLS where the endogenous variable is the log net-of-tax rate and the omitted instrument is the interaction between the indicator for the college group and the indicator for child age 17. Standard errors are clustered at the family level.

Table III
MPE due to College Costs

		Wealth effect		MPE	
	Reduced form	First stage	Wealth effect (IV)	First stage	MPE (IV)
	1,898	91,439	0.021	4,298	0.442
	(347)	(62)	(0.0038)	(4)	(0.0807)
N	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160

Notes: The first column shows the estimated reduced form effect of college costs on AGI, which is $\beta_{20} - \beta_{17}$ from Equation (1). The second column shows the estimated first stage effect on total college costs over ages 18 to 21. The third column shows the estimated wealth effect, which is the ratio of the reduced form to the first stage on total college costs. The fourth column shows the estimated first stage effect on annuitized total college costs. The fifth column shows the estimated MPE, which is the ratio of the reduced form to the first stage on annuitized total college costs. Standard errors are clustered at the family level.

Table IV
Survey Descriptive Statistics

	Mean	SD	Min	Max
Total income	$73,898	$17,529	$48,000	$110,000
Nonlabor income share >= 20%	0.05	0.22	0	1
Respondent BA	0.53	0.5	0	1
Respondent age	39.5	8.8	20	74
Age of oldest child 0-22	10.9	5.7	0	22
Own aid MTR, true	0.15	0.04	0.06	0.24
Own aid MTR, guess	0.12	0.22	-0.67	0.67
Ratio of MTR guess to true	0.88	1.51	-4.45	4.78
N	1,167			

Table V
Channels of Misperception's Impact on Deadweight Loss

	Raw	Instrumented	Instrumented and bias corrected
	(1)	(2)	(3)
Proportional impact	2.05	1.24	1.18
	(0.18)	(0.18)	(0.18)
due to Bias	-0.22	-0.22	-0.22
	(0.09)	(0.09)	(0.09)
due to Variance	2.27	1.47	1.40
	(0.19)	(0.18)	(0.18)
Cov with income (thousands)	-0.53	4.44	4.51
	(3.18)	(3.39)	(3.23)
Cov with elasticity (Nonlabor income share >= 20%)	-0.031	-0.058	-0.044
	(0.033)	(0.043)	(0.041)
Cov with tax rate	-0.009	-0.012	-0.012
	(0.002)	(0.002)	(0.002)
N	1,167	1,167	1,167

Notes: The table shows estimates of the components of Equation (4) involving θ_i. The first column does not use the instrument. The second column uses the instrument but ignores bias correction terms. The third column uses the instrument and includes bias correction terms. Standard errors are bootstrapped with 1,000 repetitions.

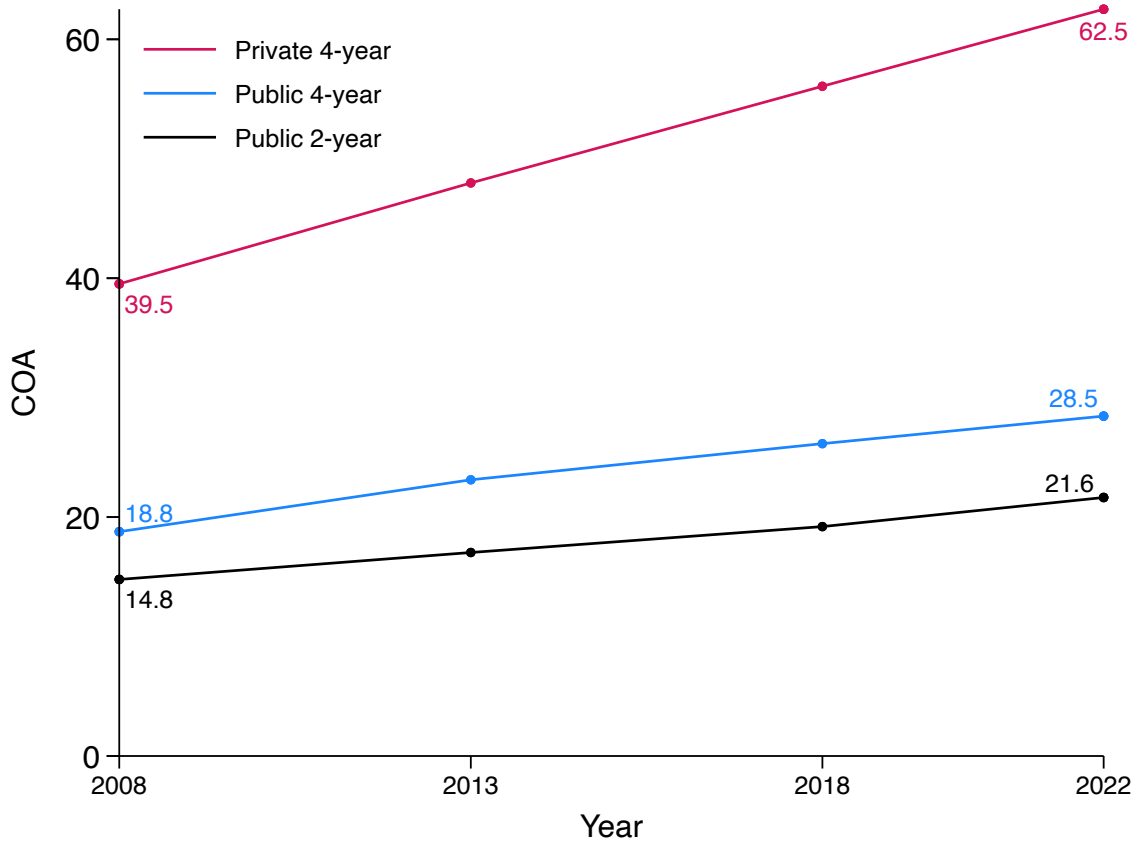
Table VI
Deadweight Loss from the College Aid Implicit Tax and the Role of Misperception

	Per family	Total (in millions)	Fraction of total	Fraction relative to no misperception
DWL	$355	$253.7	1.00	1.88
without misperception	$189	$134.8	0.53	1.00
due to misperception	$167	$118.9	0.47	0.88
proportional impact	$223	$159.1	0.63	1.18
bias	-$42	-$29.7	-0.12	-0.22
variance	$264	$188.7	0.74	1.40
covariance	-$56	-$40.1	-0.16	-0.30
N	713,885	713,885	713,885	713,885

Notes: The table shows total estimates of DWL due to the college aid implicit tax based on Equation (4) along with contributions from misperception and its channels. The first column shows estimates per family, and the second shows total DWL across all families (in millions).

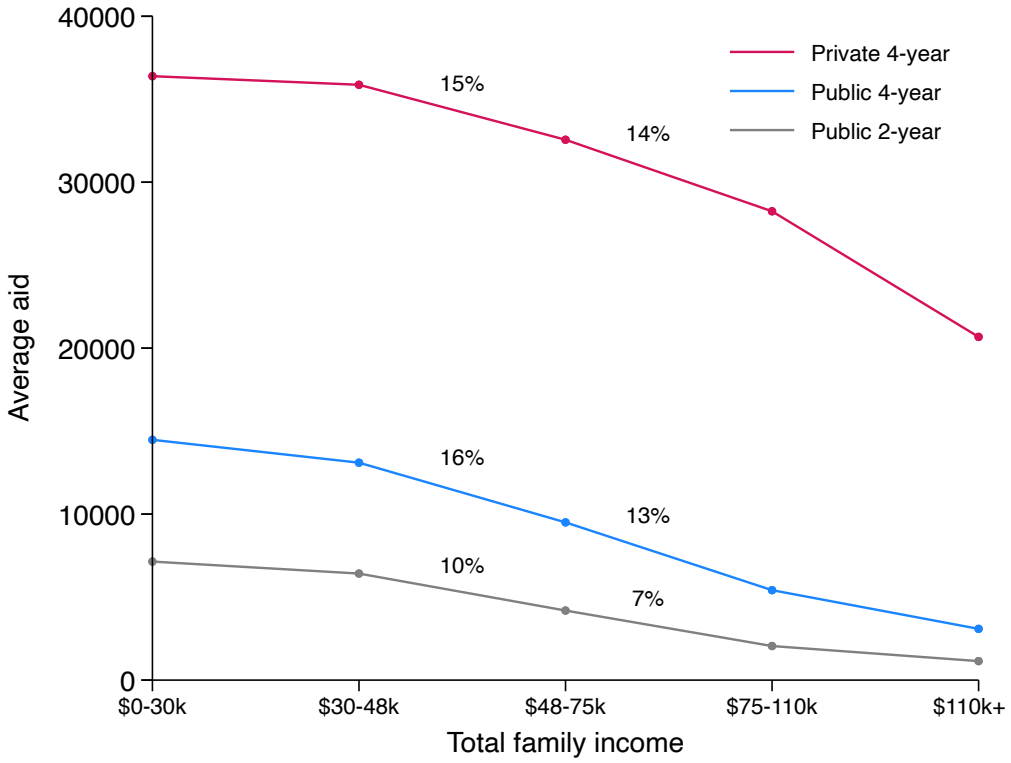
Appendix A Figures

Figure A.1
Trends in Cost of Attendance



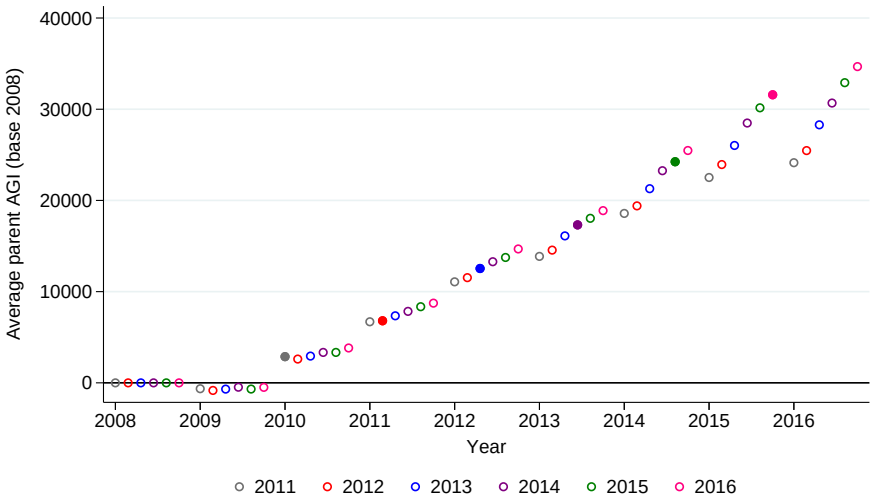
Notes: The figure shows trends in average cost of attendance using IPEDS data. Averages are weighted by total enrollment.

Figure A.2
Average Aid Schedules across the US

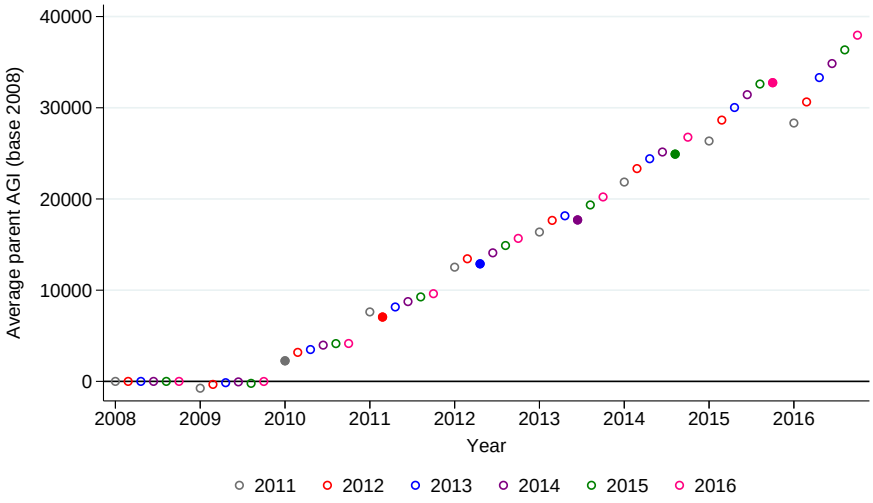


Notes: The figure uses IPEDS data to plot annual average grant and scholarship aid in each of five income bins, separately among private four-year colleges, public four-year colleges, and public two-year colleges. Averages are weighted by total enrollment. Estimated marginal implicit tax rates are listed in the figure, which are computed as the difference in average aid across adjacent income bins divided by the difference in the midpoints of each income bin.

Figure A.3
Income Patterns around Child Turning 18 – AGI



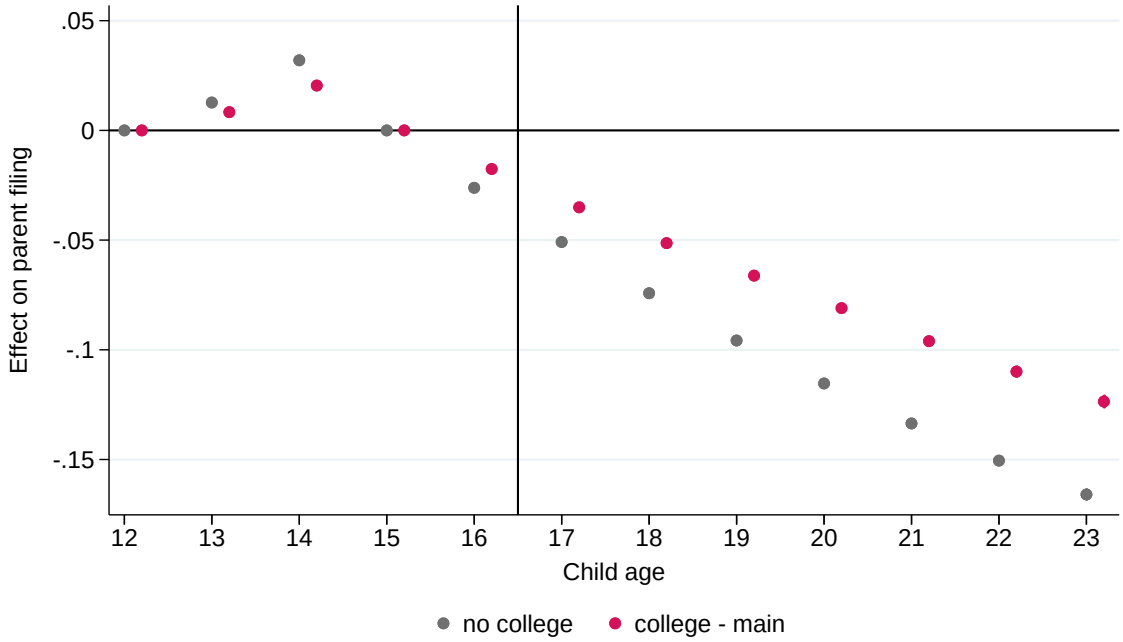
(A) Non-college



(B) College

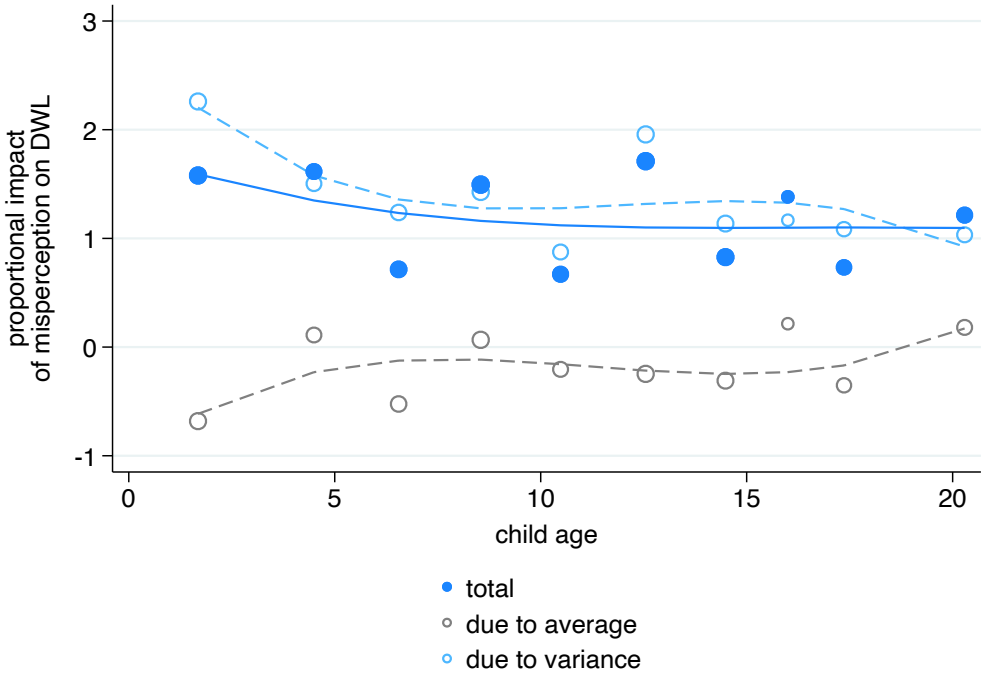
Notes: The figure shows patterns of average parent AGI by child cohort (year the child turns 18) and year, relative to the base year 2008. Panel A shows patterns for non-college families. Panel B shows patterns for college families.

Figure A.4
Filing among College and Non-college Families



Notes: The figure shows estimated child age effects from a version of Equation (1) without interactions on tax filing separately for families in which the child goes to no college at age 18 and families in which the child goes to a four-year in-state public or four-year private college at age 18. Standard errors are clustered at the family level.

Figure A.5
Impact of Misperception on Deadweight Loss by Child Age



Notes: For 10 bins of child age, the figure shows the proportional impact of misperception on DWL – i.e. $(E[\theta_i]^2 - 1) + Var(\theta_i)$ – along with the bias $(E[\theta_i]^2 - 1)$ and variance components, as well as a third order polynomial fit for each. The figure does not account for covariance channels of misperception's impact on DWL.

Appendix B Tables

Table B.1
Income Patterns around Child Turning 18, log AGI

	No college						College					
	2011	2012	2013	2014	2015	2016	2011	2012	2013	2014	2015	2016
2005	-0.122	-0.127	-0.127	-0.128	-0.131	-0.137	-0.126	-0.132	-0.135	-0.139	-0.143	-0.150
	(0.0009)	(0.0008)	(0.0008)	(0.0009)	(0.0009)	(0.0009)	(0.0012)	(0.0010)	(0.0010)	(0.0011)	(0.0011)	(0.0011)
2006	-0.058	-0.062	-0.061	-0.061	-0.063	-0.066	-0.066	-0.067	-0.071	-0.074	-0.075	-0.079
	(0.0008)	(0.0007)	(0.0008)	(0.0008)	(0.0008)	(0.0009)	(0.0011)	(0.0009)	(0.0009)	(0.0010)	(0.0010)	(0.0010)
2007	-0.017	-0.021	-0.020	-0.021	-0.021	-0.022	-0.015	-0.021	-0.023	-0.024	-0.024	-0.026
	(0.0007)	(0.0006)	(0.0007)	(0.0007)	(0.0007)	(0.0008)	(0.0010)	(0.0008)	(0.0008)	(0.0009)	(0.0009)	(0.0009)
2008	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
2009	-0.017	-0.020	-0.016	-0.017	-0.018	-0.018	-0.016	-0.012	-0.010	-0.009	-0.009	-0.007
	(0.0008)	(0.0007)	(0.0007)	(0.0007)	(0.0008)	(0.0008)	(0.0010)	(0.0009)	(0.0009)	(0.0009)	(0.0009)	(0.0009)
2010	0.011	0.009	0.013	0.015	0.015	0.016	0.005	0.016	0.020	0.024	0.025	0.026
	(0.0009)	(0.0007)	(0.0008)	(0.0008)	(0.0009)	(0.0009)	(0.0012)	(0.0010)	(0.0010)	(0.0010)	(0.0010)	(0.0010)
2011	0.039	0.041	0.047	0.051	0.055	0.055	0.048	0.043	0.054	0.060	0.065	0.066
	(0.0010)	(0.0008)	(0.0009)	(0.0009)	(0.0009)	(0.0010)	(0.0013)	(0.0011)	(0.0011)	(0.0011)	(0.0011)	(0.0011)
2012	0.070	0.075	0.086	0.090	0.093	0.098	0.085	0.092	0.086	0.099	0.106	0.110
	(0.0010)	(0.0009)	(0.0009)	(0.0010)	(0.0010)	(0.0010)	(0.0013)	(0.0011)	(0.0011)	(0.0011)	(0.0012)	(0.0012)
2013	0.092	0.098	0.113	0.122	0.127	0.131	0.114	0.125	0.131	0.124	0.139	0.145
	(0.0011)	(0.0009)	(0.0010)	(0.0010)	(0.0010)	(0.0011)	(0.0014)	(0.0012)	(0.0012)	(0.0012)	(0.0012)	(0.0012)
2014	0.121	0.130	0.147	0.161	0.169	0.175	0.153	0.162	0.172	0.178	0.176	0.189
	(0.0012)	(0.0010)	(0.0010)	(0.0011)	(0.0011)	(0.0011)	(0.0015)	(0.0012)	(0.0013)	(0.0013)	(0.0013)	(0.0013)
2015	0.148	0.159	0.179	0.195	0.208	0.216	0.181	0.199	0.210	0.221	0.230	0.227
	(0.0012)	(0.0010)	(0.0011)	(0.0011)	(0.0012)	(0.0012)	(0.0015)	(0.0013)	(0.0013)	(0.0013)	(0.0013)	(0.0013)
2016	0.152	0.163	0.188	0.206	0.221	0.232	0.188	0.206	0.228	0.240	0.252	0.261
	(0.0013)	(0.0011)	(0.0011)	(0.0012)	(0.0012)	(0.0013)	(0.0016)	(0.0013)	(0.0014)	(0.0014)	(0.0014)	(0.0014)
2017	0.170	0.184	0.210	0.233	0.249	0.263	0.211	0.232	0.254	0.273	0.288	0.300
	(0.0013)	(0.0011)	(0.0012)	(0.0012)	(0.0013)	(0.0013)	(0.0017)	(0.0014)	(0.0014)	(0.0014)	(0.0014)	(0.0014)
2018	0.189	0.202	0.233	0.258	0.277	0.294	0.233	0.256	0.281	0.303	0.324	0.339
	(0.0014)	(0.0012)	(0.0013)	(0.0013)	(0.0014)	(0.0014)	(0.0018)	(0.0015)	(0.0015)	(0.0015)	(0.0015)	(0.0015)
N	2,319,688	3,138,058	2,938,852	2,675,036	2,529,240	2,359,686	1,284,752	1,785,196	1,736,728	1,704,962	1,657,250	1,638,224

Notes: The table reports patterns of average parent log AGI by child cohort (year the child turns 18) and college status over years, relative to the base year 2008, estimated by regressing log AGI on year indicators with 2008 omitted. Standard errors are clustered at the family level.

Table B.2
Income Patterns around Child Turning 18, AGI

	No college						College					
	2011	2012	2013	2014	2015	2016	2011	2012	2013	2014	2015	2016
2005	-14,161	-14,618	-14,709	-14,944	-15,358	-15,775	-15,036	-15,519	-15,719	-16,160	-16,837	-17,405
	(99)	(90)	(94)	(99)	(102)	(103)	(140)	(119)	(113)	(116)	(129)	(123)
2006	-8,785	-9,196	-9,198	-9,293	-9,691	-9,839	-9,663	-9,773	-10,095	-10,367	-10,850	-11,172
	(93)	(84)	(89)	(93)	(96)	(96)	(132)	(112)	(105)	(109)	(122)	(115)
2007	-2,791	-2,927	-2,970	-3,077	-3,133	-3,202	-2,294	-2,950	-3,213	-3,186	-3,439	-3,594
	(93)	(92)	(93)	(93)	(96)	(97)	(149)	(117)	(106)	(111)	(130)	(118)
2008	0	0	0	0	0	0	0	0	0	0	0	0
	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)
2009	-643	-833	-686	-480	-680	-495	-748	-331	-145	-53	-214	-11
	(90)	(85)	(82)	(92)	(91)	(90)	(128)	(106)	(107)	(107)	(112)	(107)
2010	2,872	2,610	2,935	3,338	3,336	3,824	2,256	3,183	3,493	3,976	4,142	4,155
	(106)	(96)	(96)	(105)	(106)	(117)	(154)	(128)	(119)	(126)	(140)	(128)
2011	6,697	6,806	7,351	7,837	8,349	8,739	7,617	7,057	8,159	8,749	9,262	9,617
	(119)	(106)	(110)	(119)	(125)	(128)	(175)	(141)	(140)	(145)	(156)	(151)
2012	11,075	11,536	12,538	13,280	13,749	14,681	12,524	13,446	12,890	14,094	14,898	15,679
	(143)	(129)	(130)	(146)	(151)	(163)	(187)	(172)	(169)	(170)	(181)	(183)
2013	13,865	14,555	16,111	17,319	18,039	18,885	16,376	17,654	18,155	17,702	19,344	20,224
	(144)	(133)	(138)	(150)	(158)	(164)	(196)	(179)	(168)	(178)	(193)	(189)
2014	18,577	19,403	21,290	23,259	24,243	25,476	21,855	23,331	24,414	25,152	24,919	26,776
	(167)	(147)	(157)	(172)	(181)	(192)	(213)	(194)	(199)	(204)	(205)	(212)
2015	22,515	23,934	26,027	28,490	30,156	31,586	26,355	28,651	30,030	31,435	32,605	32,749
	(178)	(162)	(167)	(190)	(197)	(208)	(241)	(208)	(215)	(221)	(233)	(231)
2016	24,137	25,468	28,289	30,679	32,912	34,675	28,326	30,639	33,310	34,849	36,349	37,958
	(189)	(170)	(178)	(193)	(212)	(217)	(256)	(221)	(233)	(234)	(243)	(250)
2017	27,691	29,341	32,401	35,910	38,103	40,605	32,862	35,509	38,444	40,509	42,537	44,979
	(200)	(180)	(189)	(217)	(230)	(247)	(286)	(249)	(260)	(255)	(270)	(283)
2018	31,895	33,925	37,494	41,195	44,063	47,069	37,573	40,790	44,283	46,638	50,002	52,782
	(222)	(200)	(211)	(236)	(255)	(271)	(313)	(278)	(288)	(285)	(313)	(324)
N	2,319,688	3,138,058	2,938,852	2,675,036	2,529,240	2,359,686	1,284,752	1,785,196	1,736,728	1,704,962	1,657,250	1,638,224

Notes: The table reports patterns of average parent AGI by child cohort (year the child turns 18) and college status over years, relative to the base year 2008, estimated by regressing AGI on year indicators with 2008 omitted. Standard errors are clustered at the family level.

Table B.3
Event Study Estimates, log AGI

	Main sample									Expanded samples							
	Main	Nonwage income		Interest income		Parent age		Earners		By income		By college type					
		Low	High	Low	High	Below median	Above median	One	Two	Low	High	2yr public IS	4yr public OOS	4yr public IS	4yr private	MTR: 0-10%	MTR: 10%+
12	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
13	0.001	0.000	0.001	0.000	0.003	0.001	0.000	0.001	0.000	-0.001	-0.001	0.001	0.000	0.001	0.001	0.000	0.001
	(0.0004)	(0.0004)	(0.0017)	(0.0004)	(0.0013)	(0.0006)	(0.0006)	(0.0009)	(0.0005)	(0.0006)	(0.0014)	(0.0008)	(0.0010)	(0.0005)	(0.0006)	(0.0005)	(0.0005)
14	0.000	0.000	0.003	0.000	0.002	0.001	0.000	0.000	0.001	-0.002	0.001	-0.001	0.000	0.001	0.000	0.000	0.000
	(0.0004)	(0.0004)	(0.0017)	(0.0004)	(0.0013)	(0.0006)	(0.0006)	(0.0008)	(0.0005)	(0.0006)	(0.0013)	(0.0008)	(0.0010)	(0.0005)	(0.0006)	(0.0005)	(0.0005)
15	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
16	-0.003	-0.002	-0.006	-0.002	-0.008	-0.002	-0.003	-0.003	-0.002	0.000	-0.001	-0.002	-0.002	-0.003	-0.003	-0.002	-0.003
	(0.0006)	(0.0006)	(0.0022)	(0.0006)	(0.0017)	(0.0008)	(0.0008)	(0.0012)	(0.0006)	(0.0008)	(0.0019)	(0.0010)	(0.0013)	(0.0007)	(0.0008)	(0.0007)	(0.0006)
17	-0.010	-0.009	-0.020	-0.009	-0.018	-0.007	-0.013	-0.013	-0.008	-0.006	-0.006	-0.005	-0.009	-0.010	-0.010	-0.005	-0.013
	(0.0008)	(0.0008)	(0.0031)	(0.0009)	(0.0025)	(0.0011)	(0.0012)	(0.0017)	(0.0009)	(0.0012)	(0.0026)	(0.0015)	(0.0019)	(0.0010)	(0.0011)	(0.0010)	(0.0009)
18	0.003	0.003	-0.001	0.005	-0.005	0.006	0.000	0.000	0.005	0.006	0.007	0.008	0.004	0.002	0.004	0.007	0.000
	(0.0011)	(0.0011)	(0.0041)	(0.0012)	(0.0033)	(0.0015)	(0.0016)	(0.0023)	(0.0012)	(0.0016)	(0.0035)	(0.0020)	(0.0025)	(0.0013)	(0.0015)	(0.0013)	(0.0012)
19	0.008	0.008	0.006	0.010	-0.001	0.012	0.006	0.004	0.010	0.007	0.023	0.018	0.006	0.008	0.009	0.013	0.005
	(0.0014)	(0.0014)	(0.0053)	(0.0015)	(0.0042)	(0.0019)	(0.0021)	(0.0029)	(0.0016)	(0.0021)	(0.0044)	(0.0026)	(0.0032)	(0.0016)	(0.0019)	(0.0017)	(0.0015)
20	0.012	0.012	0.012	0.014	0.003	0.017	0.010	0.007	0.014	0.007	0.033	0.026	0.011	0.012	0.013	0.018	0.010
	(0.0017)	(0.0017)	(0.0064)	(0.0018)	(0.0051)	(0.0023)	(0.0026)	(0.0036)	(0.0019)	(0.0026)	(0.0054)	(0.0032)	(0.0039)	(0.0020)	(0.0023)	(0.0020)	(0.0019)
21	0.016	0.016	0.018	0.019	0.007	0.021	0.015	0.012	0.017	0.006	0.045	0.033	0.011	0.016	0.017	0.022	0.014
	(0.0021)	(0.0021)	(0.0077)	(0.0022)	(0.0061)	(0.0028)	(0.0031)	(0.0043)	(0.0023)	(0.0031)	(0.0064)	(0.0038)	(0.0047)	(0.0024)	(0.0028)	(0.0024)	(0.0023)
22	0.017	0.016	0.021	0.019	0.010	0.024	0.015	0.014	0.017	0.001	0.054	0.039	0.010	0.018	0.016	0.024	0.014
	(0.0024)	(0.0024)	(0.0090)	(0.0026)	(0.0072)	(0.0033)	(0.0036)	(0.0050)	(0.0027)	(0.0036)	(0.0075)	(0.0045)	(0.0055)	(0.0028)	(0.0033)	(0.0029)	(0.0027)
23	0.018	0.017	0.019	0.020	0.011	0.028	0.015	0.014	0.018	-0.005	0.064	0.049	0.009	0.019	0.016	0.026	0.015
	(0.0028)	(0.0028)	(0.0104)	(0.0030)	(0.0083)	(0.0038)	(0.0042)	(0.0058)	(0.0032)	(0.0042)	(0.0087)	(0.0052)	(0.0064)	(0.0033)	(0.0038)	(0.0033)	(0.0031)
N	20,318,954	17,483,928	2,835,027	17,227,118	3,091,836	10,575,825	9,740,206	6,329,934	11,802,742	20,096,844	3,319,558	14,137,463	13,655,072	17,336,704	15,863,397	17,189,380	18,651,728

Notes: The table reports estimates of interacted child age effects (β_k) from Equation (1) with log AGI as the outcome across samples. Standard errors are clustered at the family level.

Table B.4
Event Study Estimates, AGI

	Main sample									Expanded samples							
	Main	Nonwage income		Interest income		Parent age		Earners		By income		By college type					
		Low	High	Low	High	Below median	Above median	One	Two	Low	High	2yr public IS	4yr public OOS	4yr public IS	4yr private	MTR: 0-10%	MTR: 10%+
12	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)
13	91	102	-27	67	139	95	88	297	13	26	-81	-13	-241	35	176	-26	94
	(55)	(52)	(230)	(54)	(204)	(73)	(83)	(114)	(58)	(30)	(435)	(75)	(135)	(61)	(81)	(60)	(61)
14	138	88	427	176	-148	169	103	262	63	23	468	23	111	120	154	35	171
	(60)	(56)	(246)	(58)	(222)	(81)	(88)	(123)	(63)	(31)	(465)	(84)	(151)	(67)	(86)	(64)	(67)
15	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)
16	-281	-197	-777	-182	-694	-252	-317	-430	-119	-98	746	-87	-69	-244	-337	-151	-294
	(85)	(82)	(339)	(83)	(311)	(115)	(126)	(177)	(91)	(43)	(649)	(111)	(211)	(96)	(124)	(92)	(94)
17	-1,033	-878	-1,981	-871	-1,677	-801	-1,262	-1,310	-817	-459	95	-170	-476	-891	-1,211	-344	-1,269
	(119)	(114)	(475)	(116)	(429)	(163)	(174)	(245)	(127)	(62)	(910)	(163)	(295)	(134)	(171)	(128)	(132)
18	304	327	87	449	-187	599	9	-115	484	39	3,397	1,197	533	334	262	920	3
	(158)	(152)	(636)	(155)	(570)	(219)	(231)	(324)	(170)	(82)	(1200)	(218)	(390)	(178)	(228)	(171)	(176)
19	880	937	405	1,079	120	1,266	519	154	1,130	92	7,929	2,367	537	1,055	615	1,759	370
	(203)	(195)	(817)	(200)	(726)	(284)	(292)	(412)	(219)	(105)	(1524)	(284)	(500)	(228)	(292)	(220)	(225)
20	1,411	1,400	1,326	1,581	834	1,958	915	810	1,554	110	11,360	3,722	881	1,679	964	2,458	820
	(252)	(243)	(1002)	(249)	(898)	(355)	(359)	(513)	(272)	(131)	(1879)	(358)	(627)	(282)	(362)	(274)	(281)
21	1,727	1,647	1,992	1,843	1,415	2,274	1,282	885	1,886	38	15,353	4,915	684	2,190	993	3,082	973
	(308)	(299)	(1207)	(304)	(1091)	(436)	(436)	(624)	(334)	(161)	(2270)	(436)	(782)	(345)	(441)	(336)	(342)
22	1,836	1,765	2,007	1,928	1,918	2,665	1,197	1,238	1,976	-165	18,610	6,243	-99	2,542	792	3,714	729
	(368)	(360)	(1428)	(366)	(1293)	(528)	(515)	(745)	(402)	(192)	(2688)	(521)	(929)	(413)	(527)	(401)	(409)
23	2,076	1,924	2,570	1,957	3,404	3,240	1,173	1,260	2,255	-513	22,612	8,061	-288	2,977	781	4,399	850
	(437)	(428)	(1681)	(435)	(1523)	(632)	(605)	(885)	(479)	(228)	(3138)	(618)	(1106)	(490)	(628)	(478)	(486)
N	20,318,954	17,483,928	2,835,027	17,227,118	3,091,836	10,575,825	9,740,206	6,329,934	11,802,742	20,096,844	3,319,558	14,137,463	13,655,072	17,336,704	15,863,397	17,189,380	18,651,728

Notes: The table reports estimates of interacted child age effects (β_k) from Equation (1) with AGI as the outcome across samples. Standard errors are clustered at the family level.

Table B.5
ETI Estimates

	Reduced form	First stage	ETI (IV)
Main	-0.010	-0.145	0.071
	(0.0008)	(0.0001)	(0.0057)
N	20,318,954	20,318,954	20,318,954
Nonwage income: Low	-0.009	-0.144	0.060
	(0.0008)	(0.0001)	(0.0057)
N	17,483,928	17,483,928	17,483,928
Nonwage income: High	-0.020	-0.147	0.137
	(0.0031)	(0.0004)	(0.0214)
N	2,835,027	2,835,027	2,835,027
Interest income: Low	-0.009	-0.142	0.060
	(0.0009)	(0.0001)	(0.0061)
N	17,227,118	17,227,118	17,227,118
Interest income: High	-0.018	-0.154	0.116
	(0.0025)	(0.0003)	(0.0160)
N	3,091,836	3,091,836	3,091,836
Parent age: Below median	-0.007	-0.140	0.052
	(0.0011)	(0.0002)	(0.0080)
N	10,575,825	10,575,825	10,575,825
Parent age: Above median	-0.013	-0.149	0.087
	(0.0012)	(0.0002)	(0.0083)
N	9,740,206	9,740,206	9,740,206
Earners: One	-0.013	-0.151	0.085
	(0.0017)	(0.0003)	(0.0113)
N	6,329,934	6,329,934	6,329,934
Earners: Two	-0.008	-0.141	0.057
	(0.0009)	(0.0002)	(0.0066)
N	11,802,742	11,802,742	11,802,742

Notes: The first column shows the estimated reduced form effect of the college aid implicit tax on log AGI, which is β_{17} from Equation (1) The second column shows the estimated first stage effect on the log net-of-tax rate from college aid using the same specification. The third column shows the estimated ETI, which is the ratio of the reduced form to the first stage; the ETI is rounded after being computed from the ratio of the un-rounded reduced form to first stage. The standard error for the ETI is computed by 2SLS where the endogenous variable is the log net-of-tax rate and the omitted instrument is the interaction between the indicator for the college group and the indicator for child age 17. Standard errors are clustered at the family level.

Table B.6
Event Study Estimates, Mechanisms

	Wage income		Nonwage income (AGI - wages)																				
				Nonwage components																			
	Total wage	Wage, Medicare	Total nonwage	Capital gains (Schedule D)	Sole proprietorship (Schedule C)	S corp and partnership	Rents and royalties	Unemployment compensation	Tax exempt interest	Taxable interest	Qualified dividends	Ordinary dividends	Social Security, taxable	IRA distributions, taxable	Farm income (Schedule F)	Taxable refunds	Alimony	Other gains or losses	Other income	HSA deduction	Self-employed retirement deduction	IRA contribution deduction	Nonwage, other
12	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)
13	96	142	-5	16	-11	11	9	-2	-1	-2	-2	1	-4	-4	-1	1	0	3	-4	-1	0	1	-15
	(34)	(39)	(46)	(20)	(14)	(20)	(4)	(4)	(1)	(2)	(2)	(3)	(2)	(7)	(2)	(1)	(0)	(2)	(5)	(0)	(2)	(1)	(27)
14	89	119	50	15	-17	28	5	0	0	3	2	0	-3	-11	2	1	0	3	0	0	3	0	20
	(35)	(40)	(50)	(21)	(13)	(22)	(4)	(4)	(1)	(2)	(2)	(3)	(2)	(8)	(2)	(1)	(0)	(2)	(5)	(1)	(2)	(1)	(31)
15	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)
16	-251	-189	-30	16	-3	-53	3	4	0	-12	0	2	0	4	-1	2	0	-1	3	-1	2	-3	9
	(50)	(57)	(71)	(29)	(19)	(32)	(6)	(6)	(1)	(3)	(3)	(4)	(3)	(11)	(3)	(1)	(0)	(3)	(8)	(1)	(2)	(2)	(45)
17	-532	-454	-500	-60	-55	-111	-9	-3	0	-25	7	7	-2	-61	-6	0	0	-7	-10	-1	4	-15	-154
	(77)	(85)	(97)	(37)	(30)	(48)	(9)	(9)	(2)	(4)	(5)	(6)	(4)	(15)	(4)	(2)	(1)	(4)	(11)	(1)	(4)	(2)	(58)
18	140	-1	164	43	72	52	16	-28	1	-26	9	9	7	26	4	4	0	4	-8	0	0	3	-24
	(106)	(117)	(127)	(46)	(40)	(67)	(12)	(10)	(2)	(6)	(6)	(8)	(6)	(19)	(5)	(2)	(1)	(5)	(14)	(1)	(5)	(3)	(74)
19	681	512	199	11	131	168	24	-51	2	-36	19	17	12	17	2	6	0	-1	-21	-2	-5	4	-99
	(139)	(152)	(162)	(56)	(52)	(88)	(15)	(13)	(3)	(8)	(8)	(10)	(8)	(23)	(6)	(3)	(1)	(6)	(18)	(2)	(7)	(4)	(91)
20	1,019	777	391	37	185	214	44	-54	6	-48	27	24	15	44	-1	9	0	-1	-48	-3	-6	5	-58
	(176)	(192)	(199)	(67)	(65)	(111)	(19)	(15)	(4)	(10)	(10)	(13)	(11)	(28)	(7)	(4)	(1)	(7)	(22)	(2)	(8)	(5)	(110)
21	1,130	947	597	75	225	315	57	-61	7	-50	39	35	26	46	1	15	0	0	-53	-3	-15	5	-68
	(217)	(235)	(241)	(78)	(79)	(137)	(22)	(17)	(4)	(11)	(13)	(16)	(13)	(33)	(9)	(4)	(2)	(8)	(26)	(3)	(10)	(5)	(132)
22	1,034	799	802	72	239	416	68	-64	9	-59	47	43	39	57	4	16	0	-3	-69	-5	-26	-6	23
	(261)	(282)	(286)	(90)	(92)	(165)	(26)	(20)	(5)	(13)	(15)	(20)	(16)	(38)	(10)	(5)	(2)	(9)	(30)	(3)	(12)	(6)	(153)
23	974	695	1,101	189	225	545	66	-72	11	-64	62	61	59	24	-3	14	0	0	-78	-4	-32	-17	114
	(309)	(334)	(339)	(104)	(107)	(196)	(30)	(22)	(6)	(16)	(18)	(23)	(20)	(43)	(12)	(6)	(2)	(10)	(35)	(4)	(14)	(7)	(181)
N	20,318,954	20,318,954	20,318,954	20,318,954	20,318,954	20,318,954	20,318,954	20,318,954	20,318,954	20,318,954	20,318,954	20,318,954	20,318,954	20,318,954	20,318,954	20,318,954	20,318,954	20,318,954	20,318,954	20,318,954	20,318,954	20,318,954	20,318,954

Notes: The table reports estimates of interacted child age effects (β_k) from Equation (1) for various types of income. Standard errors are clustered at the family level.

Table B.7
Event Study Estimates by Treated and Control for Main Sample and Any College, log AGI

	Four-year public in-state, four-year private			Any college		
	No college	College	Difference	No college	College	Difference
12	0.000	0.000	0.000	0.000	0.000	0.000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
13	0.001	0.001	0.001	0.001	0.002	0.001
	(0.0003)	(0.0003)	(0.0004)	(0.0003)	(0.0003)	(0.0004)
14	0.002	0.003	0.000	0.002	0.003	0.000
	(0.0003)	(0.0003)	(0.0004)	(0.0003)	(0.0003)	(0.0004)
15	0.000	0.000	0.000	0.000	0.000	0.000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
16	-0.002	-0.005	-0.003	-0.002	-0.005	-0.003
	(0.0004)	(0.0004)	(0.0006)	(0.0004)	(0.0004)	(0.0005)
17	-0.006	-0.016	-0.010	-0.006	-0.016	-0.010
	(0.0005)	(0.0006)	(0.0008)	(0.0005)	(0.0005)	(0.0008)
18	-0.013	-0.011	0.003	-0.013	-0.011	0.003
	(0.0007)	(0.0009)	(0.0011)	(0.0007)	(0.0007)	(0.0010)
19	-0.025	-0.017	0.008	-0.025	-0.017	0.008
	(0.0009)	(0.0011)	(0.0014)	(0.0009)	(0.0009)	(0.0013)
20	-0.039	-0.027	0.012	-0.039	-0.027	0.012
	(0.0011)	(0.0013)	(0.0017)	(0.0011)	(0.0011)	(0.0016)
21	-0.055	-0.038	0.016	-0.055	-0.039	0.016
	(0.0013)	(0.0016)	(0.0021)	(0.0013)	(0.0013)	(0.0019)
22	-0.075	-0.058	0.017	-0.075	-0.059	0.016
	(0.0016)	(0.0019)	(0.0024)	(0.0016)	(0.0016)	(0.0022)
23	-0.097	-0.079	0.018	-0.097	-0.079	0.018
	(0.0018)	(0.0022)	(0.0028)	(0.0018)	(0.0018)	(0.0026)
N	12,622,439	7,696,515	20,318,954	12,622,439	11,066,104	23,688,544

Notes: Columns one, two, four, and five report estimated child age effects from a version of Equation (1) without interactions on log AGI based on college status. Column three reports the difference between columns two and one, and column six reports the difference between columns five and four. Standard errors are clustered at the family level.

Table B.8
Event Study Estimates by Treated and Control for Main Sample and Any College, AGI

	Four-year public in-state, four-year private			Any college		
	No college	College	Difference	No college	College	Difference
12	0	0	0	0	0	0
	(0)	(0)	(0)	(0)	(0)	(0)
13	112	203	91	112	165	53
	(35)	(43)	(55)	(35)	(34)	(49)
14	189	327	138	189	310	121
	(37)	(47)	(60)	(37)	(38)	(53)
15	0	0	0	0	0	0
	(0)	(0)	(0)	(0)	(0)	(0)
16	-199	-480	-281	-199	-457	-258
	(53)	(67)	(85)	(53)	(54)	(75)
17	-658	-1,690	-1,033	-658	-1,561	-903
	(74)	(93)	(119)	(74)	(75)	(105)
18	-1,546	-1,243	304	-1,546	-1,222	324
	(99)	(123)	(158)	(99)	(100)	(141)
19	-2,912	-2,031	880	-2,912	-2,047	865
	(128)	(158)	(203)	(128)	(128)	(181)
20	-4,731	-3,320	1,411	-4,731	-3,350	1,380
	(160)	(195)	(252)	(160)	(159)	(225)
21	-6,764	-5,037	1,727	-6,764	-5,105	1,659
	(195)	(238)	(308)	(195)	(194)	(275)
22	-9,522	-7,687	1,836	-9,522	-7,774	1,748
	(234)	(285)	(368)	(234)	(232)	(329)
23	-12,473	-10,397	2,076	-12,473	-10,394	2,079
	(275)	(340)	(437)	(275)	(276)	(390)
N	12,622,439	7,696,515	20,318,954	12,622,439	11,066,104	23,688,544

Notes: Columns one, two, four, and five report estimated child age effects from a version of Equation (1) without interactions on AGI based on college status. Column three reports the difference between columns two and one, and column six reports the difference between columns five and four. Standard errors are clustered at the family level.

Table B.9
Event Study Estimates among MPE Sample, AGI

	Main sample											Expanded samples							
	Main	Nonwage income		Interest income		Parent age		Earners		Siblings		By income		By college type					
		Low	High	Low	High	Below median	Above median	One	Two	No	Yes	Low	High	2yr public IS	4yr public OOS	4yr public IS	4yr private	COA: LT $25k	COA: GT $25k
12	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)
13	100	172	-441	124	-181	177	13	355	118	141	112	21	-426	-165	-259	101	98	-97	119
	(84)	(77)	(358)	(82)	(309)	(112)	(127)	(173)	(87)	(146)	(150)	(56)	(648)	(149)	(222)	(94)	(110)	(104)	(87)
14	209	161	428	189	229	365	35	277	175	270	120	10	416	6	118	299	157	105	251
	(86)	(80)	(363)	(86)	(307)	(119)	(128)	(176)	(93)	(143)	(158)	(55)	(663)	(162)	(224)	(101)	(113)	(108)	(90)
15	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)
16	-310	-246	-576	-205	-736	-284	-379	-437	-189	-78	-436	-87	915	-213	-229	-333	-299	-178	-354
	(122)	(117)	(485)	(122)	(424)	(170)	(180)	(251)	(131)	(207)	(219)	(76)	(935)	(220)	(312)	(143)	(159)	(161)	(126)
17	-1,333	-1,086	-2,667	-1,122	-2,260	-1,246	-1,454	-1,491	-1,268	-1,135	-1,363	-563	430	-194	-1,230	-1,293	-1,415	-735	-1,504
	(188)	(179)	(758)	(188)	(638)	(264)	(273)	(384)	(203)	(311)	(334)	(119)	(1437)	(369)	(470)	(220)	(247)	(242)	(196)
18	-463	-320	-1,192	-171	-1,712	-387	-615	-739	-395	-357	-684	-253	3,191	877	-802	-668	-383	45	-729
	(272)	(259)	(1089)	(271)	(923)	(381)	(394)	(558)	(292)	(435)	(480)	(170)	(2048)	(533)	(684)	(315)	(356)	(345)	(283)
19	-93	87	-1,068	254	-1,517	-111	-181	-522	-55	215	-669	-157	6,844	2,326	-1,057	-345	-36	508	-440
	(365)	(349)	(1453)	(364)	(1249)	(516)	(525)	(743)	(395)	(584)	(653)	(221)	(2721)	(731)	(911)	(421)	(478)	(464)	(380)
20	565	817	-879	918	-856	550	456	161	601	631	-205	-80	11,784	4,610	-653	396	411	1,635	38
	(468)	(452)	(1834)	(468)	(1595)	(667)	(667)	(951)	(511)	(741)	(841)	(280)	(3444)	(962)	(1193)	(543)	(609)	(604)	(487)
21	1,005	1,251	-419	1,422	-811	647	1,264	314	1,240	1,308	435	60	16,610	6,844	-1,086	1,104	489	2,521	270
	(580)	(563)	(2245)	(581)	(1970)	(835)	(817)	(1172)	(636)	(911)	(1050)	(348)	(4237)	(1220)	(1481)	(669)	(754)	(756)	(602)
22	1,031	1,266	-332	1,490	-664	931	1,132	52	1,495	883	326	-260	21,965	8,911	-1,867	1,380	201	3,055	106
	(702)	(686)	(2682)	(705)	(2371)	(1022)	(978)	(1418)	(775)	(1100)	(1272)	(416)	(5074)	(1494)	(1790)	(811)	(914)	(918)	(728)
23	1,162	1,371	-223	1,474	395	1,265	1,135	-211	1,707	500	607	-668	26,323	11,741	-2,749	1,807	-171	3,995	-136
	(840)	(822)	(3182)	(846)	(2810)	(1234)	(1156)	(1700)	(929)	(1307)	(1526)	(494)	(5970)	(1781)	(2151)	(969)	(1095)	(1102)	(870)
N	9,595,160	8,247,450	1,347,710	8,104,054	1,491,106	5,026,308	4,568,852	2,963,711	5,611,660	2,859,069	2,772,715	9,726,213	1,611,614	6,500,841	6,589,477	8,147,851	7,922,829	7,550,483	9,353,576

Notes: The table reports estimates of interacted child age effects (β_k) from Equation (1) with AGI as the outcome across samples while meeting the MPE sample restrictions on persistent enrollment or non-enrollment from ages 18 to 21. Standard errors are clustered at the family level.

Table B.10
MPE Estimates

		Wealth effect		MPE	
	Reduced form	First stage	Wealth effect (IV)	First stage	MPE (IV)
Main	1,898	91,439	0.021	4,298	0.442
	(347)	(62)	(0.0038)	(4)	(0.0807)
N	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160
Nonwage income: Low	1,903	91,164	0.021	4,205	0.453
	(337)	(67)	(0.0037)	(4)	(0.0802)
N	8,247,450	8,247,450	8,247,450	8,247,450	8,247,450
Nonwage income: High	1,788	92,980	0.019	4,823	0.371
	(1336)	(164)	(0.0144)	(21)	(0.2770)
N	1,347,710	1,347,710	1,347,710	1,347,710	1,347,710
Interest income: Low	2,040	90,107	0.023	4,173	0.489
	(345)	(67)	(0.0038)	(4)	(0.0827)
N	8,104,054	8,104,054	8,104,054	8,104,054	8,104,054
Interest income: High	1,404	96,932	0.014	4,816	0.292
	(1196)	(151)	(0.0123)	(14)	(0.2484)
N	1,491,106	1,491,106	1,491,106	1,491,106	1,491,106
Parent age: Below median	1,796	88,400	0.020	3,671	0.489
	(493)	(85)	(0.0056)	(4)	(0.1342)
N	5,026,308	5,026,308	5,026,308	5,026,308	5,026,308
Parent age: Above median	1,910	94,177	0.020	4,864	0.393
	(495)	(89)	(0.0053)	(7)	(0.1017)
N	4,568,852	4,568,852	4,568,852	4,568,852	4,568,852
Earners: One	1,652	90,402	0.018	4,311	0.383
	(705)	(117)	(0.0078)	(9)	(0.1635)
N	2,963,711	2,963,711	2,963,711	2,963,711	2,963,711
Earners: Two	1,868	91,966	0.020	4,265	0.438
	(383)	(78)	(0.0042)	(5)	(0.0898)
N	5,611,660	5,611,660	5,611,660	5,611,660	5,611,660

Notes: The first column shows the estimated reduced form effect of college costs on AGI, which is $\beta_{20} - \beta_{17}$ from Equation (1). The second column shows the estimated first stage effect on total college costs over ages 18 to 21. The third column shows the estimated wealth effect, which is the ratio of the reduced form to the first stage on total college costs. The fourth column shows the estimated first stage effect on annuitized total college costs. The fifth column shows the estimated MPE, which is the ratio of the reduced form to the first stage on annuitized total college costs. Standard errors are clustered at the family level.

Table B.11
Event Study Estimates among MPE Sample, Mechanisms

	Wage income		Nonwage income (AGI - wages)																					
				Nonwage components																				
	Total wage	Wage, Medicare	Total nonwage	Capital gains (Schedule D)	Sole proprietorship (Schedule C)	S corp and partnership	Rents and royalties	Unemployment compensation	Tax exempt interest	Taxable interest	Qualified dividends	Ordinary dividends	Social Security, taxable	IRA distributions, taxable	Farm income (Schedule F)	Taxable refunds	Alimony	Other gains or losses	Other income	HSA deduction	Self-employed retirement deduction	IRA contribution deduction	Nonwage, other
12	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)
13	158	210	-58	-41	-10	61	12	-3	-2	-2	-2	1	-7	-3	-4	1	0	5	-9	-2	0	1	-54
	(52)	(62)	(69)	(32)	(22)	(30)	(7)	(5)	(1)	(4)	(3)	(4)	(3)	(11)	(3)	(2)	(0)	(3)	(8)	(1)	(3)	(2)	(40)
14	123	199	86	10	-16	38	15	1	0	8	0	-4	-6	0	1	1	0	2	-5	0	6	1	35
	(52)	(61)	(72)	(33)	(21)	(30)	(7)	(6)	(1)	(4)	(3)	(4)	(3)	(11)	(3)	(1)	(0)	(3)	(8)	(1)	(3)	(2)	(45)
15	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)
16	-351	-273	41	-3	-24	-39	9	16	0	-18	-6	-4	-4	6	-2	5	1	1	7	0	2	-2	97
	(76)	(87)	(101)	(42)	(30)	(46)	(10)	(10)	(2)	(5)	(5)	(6)	(4)	(17)	(4)	(2)	(1)	(5)	(12)	(1)	(4)	(3)	(62)
17	-868	-731	-465	-113	-63	-162	7	32	1	-40	-1	2	-5	-93	-7	4	1	-4	-20	-1	2	-18	12
	(130)	(143)	(150)	(58)	(51)	(77)	(15)	(15)	(3)	(8)	(8)	(10)	(7)	(25)	(6)	(3)	(1)	(6)	(18)	(2)	(6)	(4)	(88)
18	-523	-596	60	-59	35	-22	27	14	2	-49	4	7	1	-34	2	8	1	9	-29	-1	0	6	139
	(191)	(210)	(213)	(78)	(75)	(115)	(21)	(18)	(4)	(13)	(11)	(14)	(11)	(34)	(8)	(4)	(2)	(8)	(25)	(3)	(9)	(5)	(120)
19	-41	-148	-52	-128	125	57	44	-11	4	-71	9	10	6	-63	-1	14	0	-1	-32	-1	1	11	-26
	(260)	(284)	(285)	(99)	(100)	(159)	(28)	(21)	(6)	(17)	(15)	(19)	(16)	(42)	(11)	(6)	(2)	(9)	(33)	(4)	(13)	(7)	(156)
20	378	232	187	-106	222	89	70	-17	8	-93	20	27	12	-38	-7	20	0	1	-68	2	1	15	30
	(338)	(367)	(363)	(120)	(127)	(207)	(36)	(24)	(7)	(21)	(19)	(25)	(21)	(51)	(13)	(7)	(3)	(11)	(40)	(4)	(16)	(9)	(197)
21	690	627	315	-89	280	105	94	-23	12	-109	33	39	22	-30	-12	28	-1	8	-65	4	-5	21	1
	(422)	(458)	(447)	(142)	(156)	(259)	(43)	(27)	(9)	(26)	(24)	(31)	(27)	(61)	(16)	(8)	(4)	(13)	(48)	(5)	(20)	(11)	(238)
22	720	555	311	-94	269	116	106	-24	16	-130	40	46	35	-26	-14	30	-2	15	-100	2	-7	8	23
	(514)	(556)	(538)	(165)	(186)	(316)	(51)	(30)	(11)	(31)	(30)	(38)	(33)	(71)	(19)	(10)	(5)	(14)	(57)	(6)	(24)	(13)	(282)
23	671	548	491	64	222	121	108	-24	19	-147	55	63	61	-89	-25	27	-2	19	-110	2	-12	-4	144
	(614)	(662)	(642)	(190)	(217)	(377)	(60)	(34)	(13)	(35)	(36)	(46)	(40)	(80)	(22)	(11)	(5)	(16)	(65)	(7)	(28)	(15)	(332)
N	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160

Notes: The table reports estimates of interacted child age effects (β_k) from Equation (1) for various types of income while meeting the MPE sample restrictions on persistent enrollment or non-enrollment from ages 18 to 21. Standard errors are clustered at the family level.

Table B.12
Reduced Form Difference between Age 20 and Age 17 Effects among MPE Sample, Mechanisms

	Wage income		Nonwage income (AGI - wages)																				
				Nonwage components																			
	Total wage	Wage, Medicare	Total nonwage	Capital gains (Schedule D)	Sole proprietorship (Schedule C)	S corp and partnership	Rents and royalties	Unemployment compensation	Tax exempt interest	Taxable interest	Qualified dividends	Ordinary dividends	Social Security, taxable	IRA distributions, taxable	Farm income (Schedule F)	Taxable refunds	Alimony	Other gains or losses	Other income	HSA deduction	Self-employed retirement deduction	IRA contribution deduction	Nonwage, other
20 - 17	1,247	963	652	7	285	251	63	-49	8	-53	20	25	17	54	0	15	-1	4	-49	3	-1	33	18
	(247)	(266)	(271)	(85)	(91)	(154)	(25)	(15)	(5)	(14)	(14)	(18)	(16)	(36)	(9)	(5)	(2)	(7)	(28)	(3)	(12)	(7)	(150)
N	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160	9,595,160

Notes: The table reports estimates of $\beta_{20} - \beta_{17}$ from Equation (1) for various types of income while meeting the MPE sample restrictions on persistent enrollment or non-enrollment from ages 18 to 21. Standard errors are clustered at the family level.

Table B.13
Reduced Form Difference between Age 20 and Age 17 Effects among MPE Sample

	Main sample											Expanded samples							
	Main	Nonwage income		Interest income		Parent age		Earners		Siblings		By income		By college type					
		Low	High	Low	High	Below median	Above median	One	Two	No	Yes	Low	High	2yr public IS	4yr public OOS	4yr public IS	4yr private	COA: LT $25k	COA: GT $25k
20 - 17	1,898	1,903	1,788	2,040	1,404	1,796	1,910	1,652	1,868	1,766	1,158	483	11,354	4,804	576	1,690	1,826	2,370	1,543
	(347)	(337)	(1336)	(345)	(1196)	(493)	(495)	(705)	(383)	(554)	(630)	(200)	(2522)	(705)	(911)	(399)	(450)	(447)	(361)
N	9,595,160	8,247,450	1,347,710	8,104,054	1,491,106	5,026,308	4,568,852	2,963,711	5,611,660	2,859,069	2,772,715	9,726,213	1,611,614	6,500,841	6,589,477	8,147,851	7,922,829	7,550,483	9,353,576

Notes: The table reports estimates of $\beta_{20} - \beta_{17}$ from Equation (1) with AGI as the outcome across samples while meeting the MPE sample restrictions on persistent enrollment or non-enrollment from ages 18 to 21. Standard errors are clustered at the family level.

Table B.14
Effects without Filing or Positive AGI Sample Restrictions

Sample:	Including AGI <= 0	Including nonfilers			
	Difference	No college	College	Difference	
Outcome:	AGI	Filed	Filed	Filed and AGI > 0	AGI
12	0	0.000	0.000	0.000	0
	(0)	(0.0000)	(0.0000)	(0.0000)	(0)
13	68	0.013	0.008	-0.004	-261
	(62)	(0.0001)	(0.0001)	(0.0002)	(55)
14	120	0.032	0.020	-0.012	-686
	(66)	(0.0001)	(0.0001)	(0.0002)	(59)
15	0	0.000	0.000	0.000	0
	(0)	(0.0000)	(0.0000)	(0.0000)	(0)
16	-355	-0.026	-0.018	0.008	256
	(94)	(0.0002)	(0.0002)	(0.0003)	(85)
17	-1,166	-0.051	-0.035	0.016	53
	(131)	(0.0003)	(0.0003)	(0.0005)	(121)
18	295	-0.074	-0.051	0.023	1,978
	(174)	(0.0004)	(0.0005)	(0.0007)	(161)
19	881	-0.096	-0.066	0.030	2,999
	(223)	(0.0005)	(0.0006)	(0.0009)	(208)
20	1,455	-0.115	-0.081	0.035	3,877
	(275)	(0.0007)	(0.0008)	(0.0011)	(258)
21	1,747	-0.134	-0.096	0.038	4,317
	(333)	(0.0008)	(0.0010)	(0.0013)	(314)
22	1,905	-0.150	-0.110	0.041	4,531
	(397)	(0.0010)	(0.0012)	(0.0016)	(374)
23	2,164	-0.166	-0.124	0.043	4,608
	(467)	(0.0012)	(0.0014)	(0.0019)	(440)
N	20,837,456	18,953,594	10,199,473	29,153,068	29,153,068

Notes: The table reports estimates of child age effects as in Equation (1) for difference outcomes among expanded samples that do not restrict to persistent tax filing with positive AGI. Column one estimates interacted effects on AGI for the sample including families that always file but do not always have positive AGI. Columns two and three estimate non-interacted effects on tax filing for non-college and college samples including families that do not always file. Column four estimates interacted effects on filing with positive AGI and column five estimates interacted effects on AGI, both including families that do not always file. Standard errors are clustered at the family level.

Table B.15
Test of Identification Assumption Implications

	(1)	(2)	(3)	(4)
Constant	0.09	0.10	0.12	0.10
	(0.01)	(0.03)	(0.13)	(0.03)
True MTR		-0.06	-0.30	-0.05
		(0.17)	(0.79)	(0.18)
Income (thousands)			0.000	
			(0.002)	
True MTR X Income (thousands)			0.004	
			(0.010)	
High nonlabor income				0.00
				(0.10)
True MTR X high nonlabor income				-0.07
				(0.56)
N	1,167	1,167	1,167	1,167

Notes: The table reports regressions with the difference between the instrument and the respondent's own MTR guess as the outcome.

Table B.16
First Stage Fit between Survey Instrument and Tax Perception

	(1)	(2)	(3)
Instrument	0.60	0.76	0.61
	(0.07)	(0.21)	(0.07)
Instrument-squared	-0.34	-1.01	-0.82
	(0.09)	(0.35)	(0.19)
Instrument X true MTR		-0.98	
		(1.32)	
Instrument-squared X true MTR		4.32	3.07
		(2.15)	(0.98)
N	1,167	1,167	1,167
R-squared	0.29	0.30	0.30
F-stat	57	33	44

Notes: The table reports regressions with the respondent's own MTR guess as the outcome.

Appendix C Proofs

C.1 Proof of Equation (4)

To assess DWL, assume all tax revenue collected from each agent is returned to the agent as a lump sum transfer. To simplify exposition, define the function for the disutility of producing income $v_i(z) \coloneqq \frac{a_i}{1+\frac{1}{e(X_i)}}\left(\frac{z}{a_i}\right)^{1+\frac{1}{e(X_i)}}$. Then the welfare (utility) of agent i is:

$$W_i \;=\; \left\{(1-\tau_i^b)Z_{i,0} \;+\; [1-(\tau_i^b+\tau_i)]Z_{i,1} \;-\; v_i(Z_{i,0}) \;-\; v_i(Z_{i,1})\right\} \;+\; \underbrace{\tau_i^b Z_{i,0} + (\tau_i^b+\tau_i)Z_{i,1}}_{\text{Lump sum transfer}} \qquad \text{(C.7)}$$

We are interested in the change in welfare that results from introducing a tax τ_i relative to a baseline of no such tax, or our object of interest is $\Delta W_i \equiv W_i(\tau_i) - W_i(0)$ where we write $W_i(\cdot)$ to represent agent i's utility as a function of the period 1 added tax τ_i. As is standard in the public economics literature, we derive an expression for ΔW_i by approximating $W_i(\tau_i)$ using a second order Taylor expansion around a tax of zero:

$$W_i(\tau_i) \;\approx\; W_i(0) \;+\; \frac{dW_i}{d\tau_i}\Big|_{\tau_i=0}\tau_i \;+\; \frac{1}{2}\frac{d^2W_i}{d\tau_i^2}\Big|_{\tau_i=0}\tau_i^2$$

which implies:

$$\begin{aligned}\Delta W_i \;&=\; W_i(\tau_i) \;-\; W_i(0) \\ &\approx\; \frac{dW_i}{d\tau_i}\Big|_{\tau_i=0}\tau_i \;+\; \frac{1}{2}\frac{d^2W_i}{d\tau_i^2}\Big|_{\tau_i=0}\tau_i^2\end{aligned}$$

To simplify further, we must find expressions for the first and second derivatives of W_i with respect to τ_i. Taking the first derivative of Equation (C.7) with respect to τ_i yields:

$$\frac{dW_i}{d\tau_i} \;=\; [1-(\tau_i^b+\tau_i)]\frac{dZ_{i,1}}{d\tau_i} \;-\; Z_{i,1} \;-\; v_i'(Z_{i,1})\frac{dZ_{i,1}}{d\tau_i} \;+\; (\tau_i^b+\tau_i)\frac{dZ_{i,1}}{d\tau_i} \;+\; Z_{i,1}$$

which simplifies to:

$$\frac{dW_i}{d\tau_i} = \frac{dZ_{i,1}}{d\tau_i}(\tau_i^b + \tilde{\tau}_i)$$

where we make use of the first order conditions for Equation (2): $v_i'(Z_{i,0}) = (1 - \tau_i^b)$ and $v_i'(Z_{i,1}) = [1 - (\tau_i^b + \tilde{\tau}_i)]$.

The second derivative of welfare with respect to the tax is:

$$\frac{d^2W_i}{d\tau_i^2} = \frac{dZ_{i,1}}{d\tau_i}\frac{d\tilde{\tau}_i}{d\tau_i} + \frac{d^2Z_{i,1}}{d\tau_i^2}(\tau_i^b + \tilde{\tau}_i)$$

Plugging in the expressions for the first and second derivatives of W_i with respect to τ_i yields:

$$\begin{aligned}
\Delta W_i &\approx \frac{dW_i}{d\tau_i}\Big|_{\tau_i=0}\tau_i + \frac{1}{2}\frac{d^2W_i}{d\tau_i^2}\Big|_{\tau_i=0}\tau_i^2 \\
&= \left[\frac{dZ_{i,1}}{d\tau_i}(\tau_i^b + \tilde{\tau}_i)\right]\Big|_{\tau_i=0}\tau_i + \frac{1}{2}\left[\frac{dZ_{i,1}}{d\tau_i}\frac{d\tilde{\tau}_i}{d\tau_i} + \frac{d^2Z_{i,1}}{d\tau_i^2}(\tau_i^b + \tilde{\tau}_i)\right]\Big|_{\tau_i=0}\tau_i^2 \\
&= \frac{1}{2}\left[\frac{dZ_{i,1}}{d\tau_i}\frac{d\tilde{\tau}_i}{d\tau_i}\right]\Big|_{\tau_i=0}\tau_i^2
\end{aligned}$$

where the terms multiplied by $(\tau_i^b + \tilde{\tau}_i)$ drop out because (1) we assume the baseline tax τ_i^b is zero and (2) we evaluate the Taylor expansion beginning from $\tau_i = 0$ and we assume there is no misperception when the true tax is zero (i.e. $\tilde{\tau}_i\big|_{\tau_i=0} = 0$). In fact, individuals face a nonzero marginal tax on income at baseline due to federal and state income taxes along with incentives created by means testing in other social programs. Assuming a baseline tax of zero here focuses the analysis on DWL that is produced directly from the implicit tax in college aid regardless of the structure of other tax programs, while omitting the portion of DWL that depends on the interaction between college aid incentives and existing programs.

Now we define the individual misperception parameter θ_i such that $\tilde{\tau}_i = \theta_i\tau_i$, which captures the degree to which an individual over- or under-estimates their own marginal tax rate. Then we can

rewrite the preceding expression for the individual change in welfare as[57]:

$$\Delta W_i \approx \frac{1}{2}\frac{dZ_{i,1}}{d\tilde{\tau}_i}\Big|_{\tau_i=0}\tau_i^2\theta_i^2$$

We can then incorporate the elasticity $e(X_i)$ as:

$$\begin{aligned}\Delta W_i &\approx \frac{1}{2}\frac{dZ_{i,1}}{d\tilde{\tau}_i}\Big|_{\tau_i=0}\tau_i^2\theta_i^2 \\ &= -\frac{1}{2}\left[Z_{i,1}\frac{1}{1-(\tau_i^b+\tilde{\tau}_i)}\right]\Big|_{\tau_i=0} e(X_i)\tau_i^2\theta_i^2 \\ &= -\frac{1}{2}Z_{i,1}\Big|_{\tau_i=0} e(X_i)\tau_i^2\theta_i^2\end{aligned}$$

where, as above, the last step uses the fact that $(\tau_i^b+\tilde{\tau}_i)\big|_{\tau_i=0}=0$ under our assumptions. $Z_{i,1}$ appears in the final expression above as a weight on the elasticity $e(X_i)$, which accounts for the fact that, conditional on the elasticity $e(X_i)$, an individual with a higher baseline income $Z_{i,1}$ will reduce income by a greater amount. From here, we will suppress the notation indicating that $Z_{i,1}$ is to be evaluated at a tax of $\tau_i=0$, but this remains implicit.

The total deadweight loss in a population with N individuals is then:

$$DWL = N\cdot E[-\Delta W_i] \approx N\cdot\frac{1}{2}\cdot E\left[Z_{i,1}e(X_i)\tau_i^2\theta_i^2\right] \tag{C.8}$$

Equation (C.8) demonstrates that welfare can be computed using information on income, the structural elasticity of income with respect to a change in the perceived tax, the true tax, and misperception. In order to make sense of Equation (C.8) and to facilitate identification using survey data measuring

[57] Note θ_i enters this equation as $\frac{d\tilde{\tau}_i}{d\tau_i}\big|_{\tau_i=0}=\theta_i$.

perceptions, we rearrange terms to produce the following:

$$DWL \approx N \cdot \underbrace{\frac{1}{2} \cdot \Big\{ E\left[Z_{i,1} e(X_i) \tau_i^2\right]}_{\substack{\text{Correct Perceptions} \\ \text{(standard Harberger triangle)}}} \cdot \Big(E[\theta_i]^2 + Var(\theta_i) \Big) + E\left[e(X_i)\right] E\left[\tau_i^2\right] \cdot Cov\left(\theta_i^2, Z_{i,1}\right)$$

$$+ E\left[Z_{i,1}\right] E\left[\tau_i^2\right] \cdot Cov\left(\theta_i^2, e(X_i)\right) + E\left[Z_{i,1}\right] E\left[e(X_i)\right] \cdot Cov\left(\theta_i^2, \tau_i^2\right) \Big\}$$

which is the key result of Equation (4). This last step relies on the assumption that third and fourth central moments are zero, which rules out a higher order dependence structure. Specifically, the assumptions are that $E\left[\bar{Z}_{i,1} e(\bar{X}_i) \bar{\theta}_i^2\right] = 0$, $E\left[\bar{Z}_{i,1} \bar{\tau}_i^2 \bar{\theta}_i^2\right] = 0$, $E\left[e(\bar{X}_i) \bar{\tau}_i^2 \bar{\theta}_i^2\right] = 0$, and $E\left[\bar{Z}_{i,1} e(\bar{X}_i) \bar{\tau}_i^2 \bar{\theta}_i^2\right] = 0$, where here bars are used to denote deviations from the expectation, or generally $\bar{X} \equiv X - E(X)$.

C.2 Proof of Equation (5)

We can use the solution of the preceding model as written in Equation (3) to plug in for $Z_{i,1}$ and $Z_{i,0}$ to express the reduced form ETI β^{ETI} as a function of the structural elasticity e, conditional on observables X_i:

$$\begin{aligned}
\beta^{ETI}(X_i) &:= \frac{E[\ln(Z_{i,1}) - \ln(Z_{i,0}) \,|\, X_i]}{E[\ln(1-(\tau_i^b+\tau_i)) - \ln(1-\tau_i^b) \,|\, X_i]} \\
&= \frac{E[\ln(a_i[1-(\tau_i^b+\tilde{\tau}_i)]^{e(X_i)}) - \ln(a_i(1-\tau_i^b)^{e(X_i)}) \,|\, X_i]}{E[\ln(1-(\tau_i^b+\tau_i)) - \ln(1-\tau_i^b) \,|\, X_i]} \\
&= e(X_i) \cdot \frac{E[\ln(1-(\tau_i^b+\tilde{\tau}_i)) - \ln(1-\tau_i^b) \,|\, X_i]}{E[\ln(1-(\tau_i^b+\tau_i)) - \ln(1-\tau_i^b) \,|\, X_i]}
\end{aligned}$$

As a result, we can also solve for the structural elasticity as a function of the reduced form elasticity and a measure of both the perceived and actual tax:

$$e(X_i) = \beta^{ETI}(X_i) \cdot \frac{E[\ln(1-(\tau_i^b+\tau_i)) - \ln(1-\tau_i^b) \,|\, X_i]}{E[\ln(1-(\tau_i^b+\tilde{\tau}_i)) - \ln(1-\tau_i^b) \,|\, X_i]}$$

Under the assumption that the baseline tax is zero, which we will maintain in subsequent sections, this simplifies to:

$$e(X_i) = \beta^{ETI}(X_i) \cdot \frac{E[\ln(1-\tau_i) \,|\, X_i]}{E[\ln(1-\tilde{\tau}_i) \,|\, X_i]}$$

which is the result shown in Equation (5).

C.3 Proof of Identification Results with Survey-Based Measures of Misperception

1. $E[\ln(1-\tilde{\tau}_i)|X_i] \approx E[\ln(1-\hat{\tilde{\tau}}_{1i})|X_i]$

Using a first order Taylor approximation of $\ln(1-\hat{\tilde{\tau}}_i)$ starting from $\varepsilon_{1i}=0$, we have:

$$\begin{aligned}
E[\ln(1-\hat{\tilde{\tau}}_i)|X_i] &= E[\ln(1-(\tilde{\tau}_i+\varepsilon_{1i}))|X_i] \\
&\approx E\left[\ln(1-\tilde{\tau}_i) + \varepsilon_{1i}\frac{d}{d\varepsilon_{1i}}[\ln(1-(\tilde{\tau}_i+\varepsilon_{1i}))]|_{\varepsilon_{1i}=0}|X_i\right] \\
&= E\left[\ln(1-\tilde{\tau}_i) - \frac{\varepsilon_{1i}}{1-\tilde{\tau}_i}|X_i\right] \\
&= E[\ln(1-\tilde{\tau}_i)|X_i] - E\left[\frac{1}{1-\tilde{\tau}_i}\underbrace{E[\varepsilon_{1i}|X_i,\tilde{\tau}_i]}_{=0,\text{ by assumption}}\Big|X_i\right] \\
&= E[\ln(1-\tilde{\tau}_i)|X_i]
\end{aligned}$$

2. $E[\theta_i] = E[\hat{\theta}_{1i}]$

$$\begin{aligned}
E[\hat{\theta}_{1i}] &= E\left[\frac{\hat{\tilde{\tau}}_{1i}}{\tau_i}\right] \\
&= E\left[\frac{\tilde{\tau}_i+\varepsilon_{1i}}{\tau_i}\right] \\
&= E\left[\frac{\tilde{\tau}_i}{\tau_i}\right] + \underbrace{E\left[\frac{\varepsilon_{1i}}{\tau_i}\right]}_{=0,\text{ by } E[\varepsilon_{1i}|\tau_i]=0} \\
&= E[\theta_i]
\end{aligned}$$

3. $Var(\theta_i) = Cov(\hat{\theta}_{1i},\hat{\theta}_{2i}) - (E[\hat{\tilde{\tau}}_{2i}]-E[\hat{\tilde{\tau}}_{1i}])\cdot Cov\left(\frac{E[\tilde{\tau}_i|\tau_i]}{\tau_i},\frac{1}{\tau_i}\right)$

First we derive an expression for $Cov(\hat{\theta}_{1i}, \hat{\theta}_{2i})$:

$$\begin{aligned}
Cov(\hat{\theta}_{1i}, \hat{\theta}_{2i}) &= Cov\left(\frac{\hat{\tilde{\tau}}_{1i}}{\tau_i}, \frac{\hat{\tilde{\tau}}_{2i}}{\tau_i}\right) \\
&= Cov\left(\frac{\tilde{\tau}_i + \varepsilon_{1i}}{\tau_i}, \frac{\tilde{\tau}_i + \varepsilon_{2i}}{\tau_i}\right) \\
&= Cov\left(\frac{\tilde{\tau}_i}{\tau_i}, \frac{\tilde{\tau}_i}{\tau_i}\right) + Cov\left(\frac{\tilde{\tau}_i}{\tau_i}, \frac{\varepsilon_{1i}}{\tau_i}\right) + Cov\left(\frac{\tilde{\tau}_i}{\tau_i}, \frac{\varepsilon_{2i}}{\tau_i}\right) + Cov\left(\frac{\varepsilon_{1i}}{\tau_i}, \frac{\varepsilon_{2i}}{\tau_i}\right) \\
&= Var(\theta_i) + Cov\left(\frac{\tilde{\tau}_i}{\tau_i}, \frac{\varepsilon_{1i}}{\tau_i}\right) + Cov\left(\frac{\tilde{\tau}_i}{\tau_i}, \frac{\varepsilon_{2i}}{\tau_i}\right) + Cov\left(\frac{\varepsilon_{1i}}{\tau_i}, \frac{\varepsilon_{2i}}{\tau_i}\right)
\end{aligned}$$

Then we simplify the three covariance terms immediately above:

$$\begin{aligned}
Cov\left(\frac{\tilde{\tau}_i}{\tau_i}, \frac{\varepsilon_{1i}}{\tau_i}\right) &= \underbrace{E\left[Cov\left(\frac{\tilde{\tau}_i}{\tau_i}, \frac{\varepsilon_{1i}}{\tau_i}\middle|\tau_i\right)\right]}_{=0, \text{ by } Cov(\tilde{\tau}_i, \varepsilon_{1i}|\tau_i)=0} + Cov\left(E\left[\frac{\tilde{\tau}_i}{\tau_i}\middle|\tau_i\right], \underbrace{E\left[\frac{\varepsilon_{1i}}{\tau_i}\middle|\tau_i\right]}_{=0, \text{ by } E[\varepsilon_{1i}|\tau_i]=0}\right) \\
&= 0
\end{aligned}$$

And,

$$\begin{aligned}
Cov\left(\frac{\tilde{\tau}_i}{\tau_i}, \frac{\varepsilon_{2i}}{\tau_i}\right) &= \underbrace{E\left[Cov\left(\frac{\tilde{\tau}_i}{\tau_i}, \frac{\varepsilon_{2i}}{\tau_i}\middle|\tau_i\right)\right]}_{=0, \text{ by } Cov(\tilde{\tau}_i, \varepsilon_{2i}|\tau_i)=0} + Cov\left(E\left[\frac{\tilde{\tau}_i}{\tau_i}\middle|\tau_i\right], \underbrace{E\left[\frac{\varepsilon_{2i}}{\tau_i}\middle|\tau_i\right]}_{=\frac{\alpha}{\tau_i}, \text{ by } E[\varepsilon_{2i}|\tau_i]=\alpha}\right) \\
&= Cov\left(\frac{E[\tilde{\tau}_i|\tau_i]}{\tau_i}, \frac{\alpha}{\tau_i}\right)
\end{aligned}$$

And,

$$\begin{aligned}
Cov\left(\frac{\varepsilon_{1i}}{\tau_i}, \frac{\varepsilon_{2i}}{\tau_i}\right) &= \underbrace{E\left[Cov\left(\frac{\varepsilon_{1i}}{\tau_i}, \frac{\varepsilon_{2i}}{\tau_i}\middle|\tau_i\right)\right]}_{=0, \text{ by } Cov(\varepsilon_{1i}, \varepsilon_{2i}|\tau_i)=0} + Cov\left(\underbrace{E\left[\frac{\varepsilon_{1i}}{\tau_i}\middle|\tau_i\right]}_{=0, \text{ by } E[\varepsilon_{1i}|\tau_i]=0}, E\left[\frac{\varepsilon_{2i}}{\tau_i}\middle|\tau_i\right]\right) \\
&= 0
\end{aligned}$$

This leaves us with

$$Cov(\hat{\theta}_{1i}, \hat{\theta}_{2i}) \;=\; Var(\theta_i) \;+\; Cov\left(\frac{E[\tilde{\tau}_i|\tau_i]}{\tau_i}, \frac{\alpha}{\tau_i}\right)$$

We now show that $(E[\hat{\tilde{\tau}}_{2i}] - E[\hat{\tilde{\tau}}_{1i}]) \cdot Cov\left(\frac{E[\tilde{\tau}_i|\tau_i]}{\tau_i}, \frac{1}{\tau_i}\right) \;=\; Cov\left(\frac{E[\tilde{\tau}_i|\tau_i]}{\tau_i}, \frac{\alpha}{\tau_i}\right)$:

$$\begin{aligned}
(E[\hat{\tilde{\tau}}_{2i}] - E[\hat{\tilde{\tau}}_{1i}]) \cdot Cov\left(\frac{E[\tilde{\tau}_i|\tau_i]}{\tau_i}, \frac{1}{\tau_i}\right) \;&=\; (E[\tilde{\tau}_i + \varepsilon_{2i}] - E[\tilde{\tau}_i + \varepsilon_{1i}]) \cdot Cov\left(\frac{E[\tilde{\tau}_i|\tau_i]}{\tau_i}, \frac{1}{\tau_i}\right) \\
&= \Big(\underbrace{E[\varepsilon_{2i}]}_{=\alpha, \text{ by } E[\varepsilon_{2i}|\tau_i]=\alpha} - \underbrace{E[\varepsilon_{1i}]}_{=0, \text{ by } E[\varepsilon_{1i}|\tau_i]=0} \Big) \cdot Cov\left(\frac{E[\tilde{\tau}_i|\tau_i]}{\tau_i}, \frac{1}{\tau_i}\right) \\
&= Cov\left(\frac{E[\tilde{\tau}_i|\tau_i]}{\tau_i}, \frac{\alpha}{\tau_i}\right)
\end{aligned}$$

Therefore, combining the above we have:

$$\begin{aligned}
Cov(\hat{\theta}_{1i}, \hat{\theta}_{2i}) \;-\; (E[\hat{\tilde{\tau}}_{2i}] - E[\hat{\tilde{\tau}}_{1i}]) \cdot Cov\left(\frac{E[\tilde{\tau}_i|\tau_i]}{\tau_i}, \frac{1}{\tau_i}\right) \;&=\; Var(\theta_i) \;+\; Cov\left(\frac{E[\tilde{\tau}_i|\tau_i]}{\tau_i}, \frac{\alpha}{\tau_i}\right) \\
&\quad - Cov\left(\frac{E[\tilde{\tau}_i|\tau_i]}{\tau_i}, \frac{\alpha}{\tau_i}\right) \\
&= Var(\theta_i)
\end{aligned}$$

4. $Cov(\theta_i^2, Z_{i,1}) \;=\; Cov(\hat{\theta}_{1i}\hat{\theta}_{2i}, Z_{i,1}) \;-\; (E[\hat{\tilde{\tau}}_{2i}] - E[\hat{\tilde{\tau}}_{1i}]) \cdot E\left[\frac{1}{\tau_i^2} Cov\left(\hat{\tilde{\tau}_{1i}}, Z_{i,1}|\tau_i\right)\right]$

First we derive an expression for $Cov(\hat{\theta}_{1i}\hat{\theta}_{2i}, Z_{i,1})$:

$$\begin{aligned}
Cov(\hat{\theta}_{1i}\hat{\theta}_{2i}, Z_{i,1}) \;&=\; Cov\left(\frac{\tilde{\tau}_i + \varepsilon_{1i}}{\tau_i}\frac{\tilde{\tau}_i + \varepsilon_{2i}}{\tau_i}, Z_{i,1}\right) \\
&= Cov\left(\left(\frac{\tilde{\tau}_i}{\tau_i}\right)^2, Z_{i,1}\right) \;+\; Cov\left(\frac{\tilde{\tau}_i\varepsilon_{1i}}{\tau_i^2}, Z_{i,1}\right) \;+\; Cov\left(\frac{\tilde{\tau}_i\varepsilon_{2i}}{\tau_i^2}, Z_{i,1}\right) \\
&\quad + Cov\left(\frac{\varepsilon_{1i}\varepsilon_{2i}}{\tau_i^2}, Z_{i,1}\right) \\
&= Cov\left(\theta_i^2, Z_{i,1}\right) \;+\; Cov\left(\frac{\tilde{\tau}_i\varepsilon_{1i}}{\tau_i^2}, Z_{i,1}\right) \;+\; Cov\left(\frac{\tilde{\tau}_i\varepsilon_{2i}}{\tau_i^2}, Z_{i,1}\right) \;+\; Cov\left(\frac{\varepsilon_{1i}\varepsilon_{2i}}{\tau_i^2}, Z_{i,1}\right)
\end{aligned}$$

Then we simplify the three covariance terms immediately above that follow $Cov(\theta_i^2, Z_{i,1})$.

For the first term:

$$Cov\left(\frac{\tilde{\tau}_i\varepsilon_{1i}}{\tau_i^2},Z_{i,1}\right) = E\left[Cov\left(\frac{\tilde{\tau}_i\varepsilon_{1i}}{\tau_i^2},Z_{i,1}\middle|\tau_i\right)\right] + Cov\left(E\left[\frac{\tilde{\tau}_i\varepsilon_{1i}}{\tau_i^2}\middle|\tau_i\right],E\left[Z_{i,1}|\tau_i\right]\right)$$

Of the two terms involved, we have:

$$\begin{aligned}
E\left[Cov\left(\frac{\tilde{\tau}_i\varepsilon_{1i}}{\tau_i^2},Z_{i,1}\middle|\tau_i\right)\right] &= E\left[\frac{1}{\tau_i^2}Cov\left(\tilde{\tau}_i\varepsilon_{1i},Z_{i,1}|\tau_i\right)\right] \\
&= E\left[\frac{1}{\tau_i^2}\left(E\left[\tilde{\tau}_i\varepsilon_{1i}Z_{i,1}|\tau_i\right] - \underbrace{E[\tilde{\tau}_i\varepsilon_{1i}|\tau_i]}_{=0,\text{ by } Cov(\tilde{\tau}_i,\varepsilon_{1i}|\tau_i)=0,E[\varepsilon_{1i}|\tau_i]=0}E[Z_{i,1}|\tau_i]\right)\right] \\
&= E\left[\frac{1}{\tau_i^2}E\left[Z_{i,1}\underbrace{E[\tilde{\tau}_i\varepsilon_{1i}|Z_{i,1},\tau_i]}_{=0,\text{ by } Cov(\tilde{\tau}_i,\varepsilon_{1i}|Z_{i,1},\tau_i)=0,E[\varepsilon_{1i}|Z_{i,1},\tau_i]=0}|\tau_i\right]\right] \\
&= 0
\end{aligned}$$

And for the second term, observe that $E\left[\frac{\tilde{\tau}_i\varepsilon_{1i}}{\tau_i^2}|\tau_i\right] = \frac{1}{\tau_i^2}\left(Cov(\tilde{\tau}_i,\varepsilon_{1i}|\tau_i)+E[\tilde{\tau}_i|\tau_i]E[\varepsilon_{1i}|\tau_i]\right)$, which equals zero by the assumptions $Cov(\tilde{\tau}_i,\varepsilon_{1i}|\tau_i)=0$ and $E[\varepsilon_{1i}|\tau_i]=0$. Therefore, $Cov\left(E\left[\frac{\tilde{\tau}_i\varepsilon_{1i}}{\tau_i^2}\middle|\tau_i\right],E\left[Z_{i,1}|\tau_i\right]\right) = 0$. Then we have for the first of the three covariance terms:

$$\begin{aligned}
Cov\left(\frac{\tilde{\tau}_i\varepsilon_{1i}}{\tau_i^2},Z_{i,1}\right) &= E\left[Cov\left(\frac{\tilde{\tau}_i\varepsilon_{1i}}{\tau_i^2},Z_{i,1}\middle|\tau_i\right)\right] + Cov\left(E\left[\frac{\tilde{\tau}_i\varepsilon_{1i}}{\tau_i^2}\middle|\tau_i\right],E\left[Z_{i,1}|\tau_i\right]\right) \\
&= 0
\end{aligned}$$

For the second term:

$$\begin{aligned}
Cov\left(\frac{\tilde{\tau}_i\varepsilon_{2i}}{\tau_i^2},Z_{i,1}\right) &= E\left[\frac{\tilde{\tau}_i\varepsilon_{2i}}{\tau_i^2}Z_{i,1}\right] - E\left[\frac{\tilde{\tau}_i\varepsilon_{2i}}{\tau_i^2}\right]E\left[Z_{i,1}\right] \\
&= E\left[\frac{1}{\tau_i^2}\left(E[\tilde{\tau}_i\varepsilon_{2i}Z_{i,1}|\tau_i] - E[\tilde{\tau}_i\varepsilon_{2i}|\tau_i]E[Z_{i,1}|\tau_i]\right)\right] \\
&= E\left[\frac{1}{\tau_i^2}\left(E[Z_{i,1}E[\tilde{\tau}_i\varepsilon_{2i}|Z_{i,1},\tau_i]|\tau_i] - E[\tilde{\tau}_i\varepsilon_{2i}|\tau_i]E[Z_{i,1}|\tau_i]\right)\right]
\end{aligned}$$

Then we use the fact that $E[\tilde{\tau}_i\varepsilon_{2i}|Z_{i,1},\tau_i] = \alpha E[\tilde{\tau}_i|Z_{i,1},\tau_i]$ by the assumptions $Cov(\tilde{\tau}_i,\varepsilon_{2i}|Z_{i,1},\tau_i) = 0$ and $E[\varepsilon_{2i}|Z_{i,1},\tau_i] = \alpha$, as well as that $E[\tilde{\tau}_i\varepsilon_{2i}|\tau_i] = \alpha E[\tilde{\tau}_i|\tau_i]$ by the assumptions $Cov(\tilde{\tau}_i,\varepsilon_{2i}|\tau_i) = 0$ and $E[\varepsilon_{2i}|\tau_i] = \alpha$ to further simplify:

$$\begin{aligned}
Cov\left(\frac{\tilde{\tau}_i\varepsilon_{2i}}{\tau_i^2}, Z_{i,1}\right) &= E\left[\frac{1}{\tau_i^2}\left(E[Z_{i,1}E[\tilde{\tau}_i\varepsilon_{2i}|Z_{i,1},\tau_i]|\tau_i] - E[\tilde{\tau}_i\varepsilon_{2i}|\tau_i]E[Z_{i,1}|\tau_i]\right)\right] \\
&= E\left[\frac{1}{\tau_i^2}E[Z_{i,1}\alpha E[\tilde{\tau}_i|Z_{i,1},\tau_i]|\tau_i] - \alpha E[\tilde{\tau}_i|\tau_i]E[Z_{i,1}|\tau_i])\right] \\
&= \alpha E\left[\frac{1}{\tau_i^2}E[\tilde{\tau}_i Z_{i,1}|\tau_i] - E[\tilde{\tau}_i|\tau_i]E[Z_{i,1}|\tau_i])\right] \\
&= \alpha E\left[\frac{1}{\tau_i^2}Cov(\tilde{\tau}_i, Z_{i,1}|\tau_i)\right]
\end{aligned}$$

Finally, for the third term:

$$Cov\left(\frac{\varepsilon_{1i}\varepsilon_{2i}}{\tau_i^2}, Z_{i,1}\right) = E\left[Cov\left(\frac{\varepsilon_{1i}\varepsilon_{2i}}{\tau_i^2}, Z_{i,1}\middle|\tau_i\right)\right] + Cov\left(E\left[\frac{\varepsilon_{1i}\varepsilon_{2i}}{\tau_i^2}\middle|\tau_i\right], E[Z_{i,1}|\tau_i]\right)$$

Of these, the first term can be simplified as:

$$\begin{aligned}
E\left[Cov\left(\frac{\varepsilon_{1i}\varepsilon_{2i}}{\tau_i^2}, Z_{i,1}\middle|\tau_i\right)\right] &= E\left[\frac{1}{\tau_i^2}(E[\varepsilon_{1i}\varepsilon_{2i}Z_{i,1}|\tau_i] - \underbrace{E[\varepsilon_{1i}\varepsilon_{2i}|\tau_i]}_{=0, \text{ by } Cov(\varepsilon_{1i},\varepsilon_{2i}|\tau_i)=0, E[\varepsilon_{1i}|\tau_i]=0} E[Z_{i,1}|\tau_i])\right] \\
&= E[Z_{i,1} \underbrace{E[\varepsilon_{1i}\varepsilon_{2i}|Z_{i,1}\tau_i]}_{=0, \text{ by } Cov(\varepsilon_{1i},\varepsilon_{2i}|Z_{i,1},\tau_i)=0, E[\varepsilon_{1i}|\tau_i]=0} |\tau_i]
\end{aligned}$$

So we conclude the third term $Cov\left(\frac{\varepsilon_{1i}\varepsilon_{2i}}{\tau_i^2}, Z_{i,1}\right) = 0$.

This leaves us with:

$$Cov\left(\hat{\theta}_{1i}\hat{\theta}_{2i}, Z_{i,1}\right) = Cov\left(\theta_i^2, Z_{i,1}\right) + \alpha E\left[\frac{1}{\tau_i^2}Cov(\tilde{\tau}_i, Z_{i,1}|\tau_i)\right]$$

We now show that $(E[\hat{\tilde{\tau}}_{2i}] - E[\hat{\tilde{\tau}}_{1i}]) \cdot E\left[\frac{1}{\tau_i^2} Cov\left(\hat{\tau_{1i}}, Z_{i,1} | \tau_i\right)\right] = \alpha E\left[\frac{1}{\tau_i^2} Cov(\tilde{\tau}_i, Z_{i,1} | \tau_i)\right]$:

$$\begin{aligned}
(E[\hat{\tilde{\tau}}_{2i}] - E[\hat{\tilde{\tau}}_{1i}]) \cdot E\left[\frac{1}{\tau_i^2} Cov\left(\hat{\tau_{1i}}, Z_{i,1} | \tau_i\right)\right] &= (E[\tilde{\tau}_i + \varepsilon_{2i}] - E[\tilde{\tau}_i + \varepsilon_{1i}]) \cdot E\left[\frac{1}{\tau_i^2} Cov\left(\tilde{\tau}_i + \varepsilon_{1i}, Z_{i,1} | \tau_i\right)\right] \\
&= (\underbrace{E[\varepsilon_{2i}]}_{=\alpha, \text{ by } E[\varepsilon_{2i}|\tau_i]=\alpha} - \underbrace{E[\varepsilon_{1i}]}_{=0, \text{ by } E[\varepsilon_{1i}|\tau_i]=0}) \cdot E\left[\frac{1}{\tau_i}(Cov(\tilde{\tau}_i, Z_{i,1} | \tau_i) + \underbrace{Cov(\varepsilon_{1i}, Z_{i,1} | \tau_i)}_{=0, \text{ by assumption}})\right] \\
&= \alpha E\left[\frac{1}{\tau_i^2} Cov(\tilde{\tau}_i, Z_{i,1} | \tau_i)\right]
\end{aligned}$$

Therefore, combining we have:

$$\begin{aligned}
Cov(\hat{\theta}_{1i}\hat{\theta}_{2i}, Z_{i,1}) - (E[\hat{\tilde{\tau}}_{2i}] - E[\hat{\tilde{\tau}}_{1i}]) \cdot E\left[\frac{1}{\tau_i^2} Cov\left(\hat{\tau_{1i}}, Z_{i,1} | \tau_i\right)\right] &= Cov(\theta_i^2, Z_{i,1}) \\
&\quad + \alpha E\left[\frac{1}{\tau_i^2} Cov(\tilde{\tau}_i, Z_{i,1} | \tau_i)\right] - \alpha E\left[\frac{1}{\tau_i^2} Cov(\tilde{\tau}_i, Z_{i,1} | \tau_i)\right] \\
&= Cov(\theta_i^2, Z_{i,1})
\end{aligned}$$

5. $Cov(\theta_i^2, e(X_i)) = Cov(\hat{\theta}_{1i}\hat{\theta}_{2i}, e(X_i)) - (E[\hat{\tilde{\tau}}_{2i}] - E[\hat{\tilde{\tau}}_{1i}]) \cdot E\left[\frac{1}{\tau_i^2} Cov\left(\hat{\tau_{1i}}, e(X_i) | \tau_i\right)\right]$

The steps proceed identically to the preceding proof demonstrating that $Cov(\theta_i^2, Z_{i,1}) = Cov(\hat{\theta}_{1i}\hat{\theta}_{2i}, Z_{i,1}) - (E[\hat{\tilde{\tau}}_{2i}] - E[\hat{\tilde{\tau}}_{1i}]) \cdot E\left[\frac{1}{\tau_i^2} Cov\left(\hat{\tau_{1i}}, Z_{i,1} | \tau_i\right)\right]$, except with $e(X_i)$ in place of $Z_{i,1}$ and using the following assumptions: $Cov(\tilde{\tau}_i, \varepsilon_{1i} | e(X_i), \tau_i) = 0, E[\varepsilon_{1i} | e(X_i), \tau_i] = 0, Cov(\varepsilon_{1i}, \varepsilon_{2i} | e(X_i), \tau_i) = 0, Cov(\tilde{\tau}_i, \varepsilon_{2i} | e(X_i), \tau_i) = 0, E[\varepsilon_{2i} | e(X_i), \tau_i] = \alpha$, and $Cov(\varepsilon_{1i}, e(X_i) | \tau_i) = 0$.

6. $Cov(\theta_i^2, \tau_i^2) = Cov(\hat{\theta}_{1i}\hat{\theta}_{2i}, \tau_i^2)$

$$\begin{aligned}
Cov(\hat{\theta}_{1i}\hat{\theta}_{2i}, \tau_i^2) &= Cov\left(\frac{\tilde{\tau}_i + \varepsilon_{1i}}{\tau_i}\frac{\tilde{\tau}_i + \varepsilon_{2i}}{\tau_i}, \tau_i^2\right) \\
&= Cov\left(\left(\frac{\tilde{\tau}_i}{\tau_i}\right)^2, \tau_i^2\right) + Cov\left(\frac{\tilde{\tau}_i \varepsilon_{1i}}{\tau_i^2}, \tau_i^2\right) + Cov\left(\frac{\tilde{\tau}_i \varepsilon_{2i}}{\tau_i^2}, \tau_i^2\right) + Cov\left(\frac{\varepsilon_{1i}\varepsilon_{2i}}{\tau_i^2}, \tau_i^2\right) \\
&= Cov(\theta_i^2, \tau_i^2) + Cov\left(\frac{\tilde{\tau}_i \varepsilon_{1i}}{\tau_i^2}, \tau_i^2\right) + Cov\left(\frac{\tilde{\tau}_i \varepsilon_{2i}}{\tau_i^2}, \tau_i^2\right) + Cov\left(\frac{\varepsilon_{1i}\varepsilon_{2i}}{\tau_i^2}, \tau_i^2\right)
\end{aligned}$$

We now show that each of the three terms after $Cov(\theta_i^2, \tau_i^2)$ equals zero.

For the first term:

$$Cov\left(\frac{\tilde{\tau}_i \varepsilon_{1i}}{\tau_i^2}, \tau_i^2\right) = E\left[Cov\left(\frac{\tilde{\tau}_i \varepsilon_{1i}}{\tau_i^2}, \tau_i^2 \middle| \tau_i\right)\right] + Cov\left(E\left[\frac{\tilde{\tau}_i \varepsilon_{1i}}{\tau_i^2} \middle| \tau_i\right], E[\tau_i^2|\tau_i]\right)$$

Of these, for the first term we have:

$$\begin{aligned} E\left[Cov\left(\frac{\tilde{\tau}_i \varepsilon_{1i}}{\tau_i^2}, \tau_i^2 \middle| \tau_i\right)\right] &= E[Cov(\tilde{\tau}_i \varepsilon_{1i}, 1|\tau_i)] \\ &= 0 \end{aligned}$$

And for the second we have:

$$\begin{aligned} Cov\left(E\left[\frac{\tilde{\tau}_i \varepsilon_{1i}}{\tau_i^2} \middle| \tau_i\right], E[\tau_i^2|\tau_i]\right) &= Cov\left(\frac{1}{\tau_i^2} \underbrace{E[\tilde{\tau}_i \varepsilon_{1i}|\tau_i]}_{=0, \text{ by } Cov(\tilde{\tau}_i, \varepsilon_{1i}|\tau_i)=0, E[\varepsilon_{1i}|\tau_i]=0}, \tau_i^2\right) \\ &= 0 \end{aligned}$$

Therefore, we have $Cov\left(\frac{\tilde{\tau}_i \varepsilon_{1i}}{\tau_i^2}, \tau_i^2\right) = 0$.

For the second term:

$$\begin{aligned} Cov\left(\frac{\tilde{\tau}_i \varepsilon_{2i}}{\tau_i^2}, \tau_i^2\right) &= E\left[\frac{\tilde{\tau}_i \varepsilon_{2i}}{\tau_i^2} \tau_i^2\right] - E\left[\frac{\tilde{\tau}_i \varepsilon_{2i}}{\tau_i^2}\right] E[\tau_i^2] \\ &= E\left[\frac{1}{\tau_i^2}(E[\tilde{\tau}_i \varepsilon_{2i} \tau_i^2|\tau_i] - E[\tilde{\tau}_i \varepsilon_{2i}|\tau_i]\tau_i^2)\right] \\ &= E\left[\frac{1}{\tau_i^2}(\tau_i^2 E[\tilde{\tau}_i \varepsilon_{2i}|\tau_i] - \tau_i^2 E[\tilde{\tau}_i \varepsilon_{2i}|\tau_i])\right] \\ &= 0 \end{aligned}$$

For the third term:

$$Cov\left(\frac{\varepsilon_{1i}\varepsilon_{2i}}{\tau_i^2}, \tau_i^2\right) = E\left[Cov\left(\frac{\varepsilon_{1i}\varepsilon_{2i}}{\tau_i^2}, \tau_i^2 \middle| \tau_i\right)\right] + Cov\left(E\left[\frac{\varepsilon_{1i}\varepsilon_{2i}}{\tau_i^2} \middle| \tau_i\right], E[\tau_i^2|\tau_i]\right)$$

For the first term we have:

$$E\left[Cov\left(\frac{\varepsilon_{1i}\varepsilon_{2i}}{\tau_i^2}, \tau_i^2 \middle| \tau_i\right)\right] = E[Cov(\varepsilon_{1i}\varepsilon_{2i}, 1|\tau_i)]$$
$$= 0$$

For the second term we have:

$$Cov\left(E\left[\frac{\varepsilon_{1i}\varepsilon_{2i}}{\tau_i^2}\middle|\tau_i\right], E[\tau_i^2|\tau_i]\right) = Cov\left(\frac{1}{\tau_i^2}\underbrace{E[\varepsilon_{1i}\varepsilon_{2i}|\tau_i]}_{=0, \text{ by } Cov(\varepsilon_{1i},\varepsilon_{2i}|\tau_i)=0, E[\varepsilon_{1i}|\tau_i]=0}, \tau_i^2\right)$$
$$= 0$$

Therefore, $Cov\left(\frac{\varepsilon_{1i}\varepsilon_{2i}}{\tau_i^2}, \tau_i^2\right) = 0$.

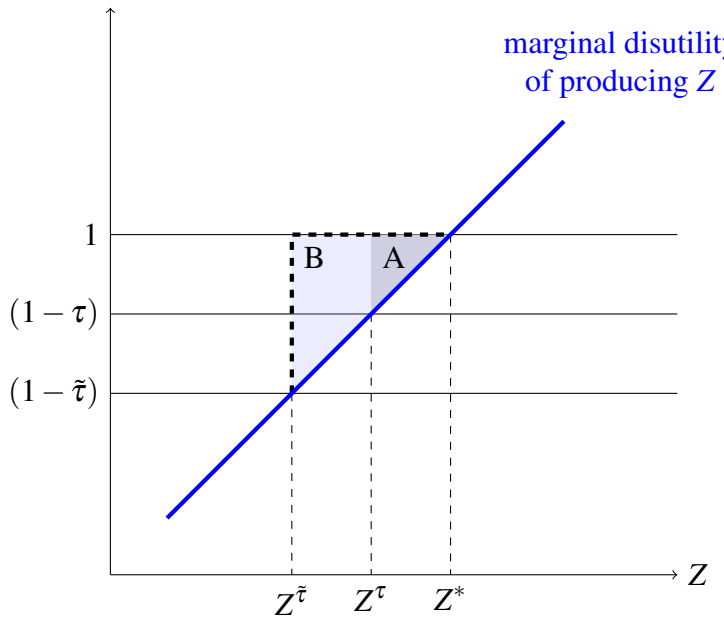
Combining, we have that $Cov(\theta_i^2, \tau_i^2) = Cov(\hat{\theta}_{1i}\hat{\theta}_{2i}, \tau_i^2)$.

Appendix D Why DWL Increases with the Variance of Tax Rate Misperceptions

To see why DWL increases with the variance of misperceptions, consider Figure D.1. Assume there are two agents with the same structural curve of marginal disutility of producing income Z who both face a true tax τ. In the first scenario, assume both agents correctly perceive the tax and choose income Z^{τ}. This produces total DWL of $2A$.[58] In the second scenario, assume one agent misperceives the tax as $\tilde{\tau} = 2\tau$, and the other agent misperceives the tax as zero. The first agent chooses income $Z^{\tilde{\tau}}$, contributing DWL equal to B, the area of the larger triangle. The second agent chooses income Z^{*} and contributes zero DWL, making the total DWL in this scenario equal to B. In both scenarios, the average perceived tax equals the true tax τ. However, DWL in the second scenario with higher variance of misperceptions is *twice* that of the first scenario. The intuition for this result can also be seen in Figure D.1. Increasing one agent's perceived tax above τ produces a first order increase in DWL (the square between 1 and $(1-\tau)$ on the vertical axis and between $Z^{\tilde{\tau}}$ and Z^{τ} on the horizontal axis), while reducing the other's perceived tax below τ does not produce an offsetting first order reduction in DWL. This relates to the common result that DWL increases with the square of the tax rate, where misperception can be viewed equivalently to an additional tax levied on top of the pre-existing true tax – it is the average of this square, not the square of the average, that determines DWL.

[58] Recall that under the maintained assumption of quasilinear utility, the marginal utility of consumption is constant, which we set equal to one.

Figure D.1
The Impact of Variance of Tax Rate Misperceptions on DWL



Notes: The figure depicts a hypothetical marginal disutility of earning income curve alongside choices of income and deadweight loss triangles that result when the agent perceives a tax rate of zero, the true tax τ, or a higher tax $\tilde{\tau}$.