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Cash-on-Hand and College Enrollment: Evidence from Population Tax Data and the Earned Income Tax Credit†

By Day Manoli and Nicholas Turner*

We estimate causal effects of cash-on-hand on college enrollment decisions of students from low-income families. Using population-level, administrative data from US income tax returns, we exploit variation in tax refunds received in the spring of the high school senior year. The variation in tax refunds results from the kink point between the phase-in and maximum credit portions of the Earned Income Tax Credit schedule. The results suggest tax refunds received in the spring of the high school senior year have meaningful effects on college enrollment. (*JEL* D14, H24, I22, I23, I24)

How does cash-on-hand affect college enrollment decisions? A significant body of prior research has examined the impacts of family income on college enrollment (discussed more below), but it is unclear if cash transfers in the spring of the high school senior year, a time when many students are finalizing their college enrollment decisions, can also impact enrollment. On the one hand, students may face up-front, out-of-pocket costs that represent a barrier to entry for students from low-income families. In this case, additional cash-on-hand at the time of college enrollment decisions may have positive impacts on college attendance. On the other hand, college preparedness and preferences for college enrollment may be entirely determined prior to spring of the high school senior year. In this case, additional cash-on-hand at the time when students and families make college enrollment decisions may have no impact on these decisions. Quantifying the causal effects of cash-on-hand is challenging because cash-on-hand at the time of college enrollment decisions can be correlated with longer term disadvantages that leave students less academically qualified for college.

In this paper, we implement a novel research design to estimate the effects of cash-on-hand on college enrollment decisions. We exploit quasi-experimental variation in tax refunds received during the spring of the high school senior year. The

*Manoli: Department of Economics, University of Texas at Austin, 2225 Speedway Stop C3100, Austin, TX 78712 (email: dsmanoli@austin.utexas.edu); Turner: Federal Reserve Board of Governors, Federal Reserve Board, Research Division, Stop #83, 20th and C Streets, NW, Washington, DC 20551 (email: nick.turner@frb.gov). This research represents our private research efforts and does not reflect the views or opinions of the US Treasury. We thank Peter Bergman, Sandy Black, David Card, Raj Chetty, Hilary Hoynes, Larry Katz, Paco Martorell, Judy Scott-Clayton, Steve Trejo, Emmanuel Saez, Eric Zwick, and numerous seminar participants for valuable comments and suggestions. Day Manoli is grateful for funding from the Laura and John Arnold Foundation UTA15-000050. We also thank Randeep Kaur for excellent research assistance.
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quasi-experimental variation in tax refunds arises from policy nonlinearities in the tax code. These nonlinearities cause observationally similar households to receive different amounts of tax refunds at a time when many high school seniors are making college enrollment decisions. Using population-level administrative tax data, we implement a Regression Kink Design (RKD) to relate changes in tax refunds at kink points in the Earned Income Tax Credit (EITC) benefit schedule to changes in enrollment rates around these kink points. The analysis focuses on the first and third kink points in the EITC benefit schedule, which occur, respectively, at the transition between the phase-in and maximum credit portion of the benefit schedule and the transition between the phase-out and zero credit portion of the benefit schedule. As we discuss in more detail below, changes in the earnings measures used to compute EITC benefits at the second kink point in the EITC benefit schedule, which occurs at the transition between the maximum credit region to the phase-out portion of the benefit schedule, rule out an RKD analysis at this kink point.

The results indicate meaningful impacts of cash-on-hand on enrollment around the first EITC kink point but not at the third EITC kink point, which occurs at higher income levels. (For example, in 2011, for head-of-household filers with two qualifying children, the first EITC kink point occurs at $12,780 and the third EITC kink point occurs at $40,964.) For students in households around EITC Kink 1, we estimate that an additional $1,000 in cash-on-hand from tax refunds in the spring of the high school senior year increases college enrollment in the next year by 1.3 percentage points. In the EITC Kink 1 analysis sample, average pretax income is about $12,400, the average tax refund is about $5,300, and enrollment is about 30 percent. We examine the robustness of the estimates around EITC Kink 1 in multiple ways, and further analysis around EITC Kink 1 indicates some additional results. First, for households around EITC Kink 1, we find effects of cash-on-hand from tax refunds received in the spring of the high school senior year, but not from tax refunds received in spring of the high school junior year. This indicates that the timing of the additional income may be important. Second, we find evidence of enrollment effects that are persistent two years after the high school senior year. This indicates that additional cash-on-hand in the spring of the high school senior year leads to new enrollments as opposed to just earlier enrollments. We note that these effects may only apply locally around the first EITC kink point and may differ at different points in the income distribution or across different populations.

The research design and results offer multiple contributions to the prior literature. First, in the literature examining family income and college enrollment,[1] it is difficult to find credible quasi-experimental variation in income for low-income families, and even more difficult to find this variation in income at a time when students are making college enrollment decisions. Our research design contributes to this literature by overcoming these difficulties with quasi-experimental variation in tax refunds received during the spring of the high school senior year. This

[1] For evidence on the effects of long-run and short-run variation in family income on college enrollment, see Ellwood and Kane (2000), Shea (2000), Acemoglu and Pischke (2001), Carneiro and Heckman (2002), Keane and Wolpin (2001), Cameron and Taber (2004), Plug and Vijverberg (2005), Belley and Lochner (2007), Lovenheim (2011), Bailey and Dynarski (2011), Hilger (2016), Michelmore (2013), Bastian and Michelmore (2015), and Lovenheim and Reynolds (2013).

research design allows us to examine issues related to timing of additional income, persistence in effects of cash-on-hand from the senior year, and heterogeneity in cash-on-hand effects at different points in the income distribution.

Given that the research design exploits variation in cash-on-hand at the time of enrollment decisions, the analysis also relates to prior studies on excess sensitivity to predictable income changes. We discuss this literature in more detail in Section III. Theories related to excess sensitivity could explain why the RKD estimates around the first kink point in the EITC schedule exceed prior quasi-experiment estimates of the impacts of family income on college enrollment.

The research design and results also relate to the literature examining how tax policies and student aid policies affect college enrollment.[2] Previous studies generally focus on education tax credits and traditional student aid such as grants. Such aid policies typically operate through price effects, as they lower the relative price of college enrollment if a student enrolls in college. Building on this literature, our findings highlight income effects from the tax refunds. While tax refunds need not be spent on higher education, the lump-sum payments may help households cover any out-of-pocket college costs. Our findings support the idea that additional cash-on-hand increases college enrollment for families that benefit from the EITC, but we do not find similar effects at higher income levels, so similar cash-on-hand effects may not extend to other areas of the income distribution.[3] Beyond the student aid literature, this study also builds upon earlier studies documenting how tax policies affect child outcomes.[4]

Methodologically, this study contributes to a growing literature that develops and applies RKD as an empirical strategy to estimate causal effects based on policy nonlinearities.[5] The analysis is based on a large sample size: nearly all high school seniors in the United States between 2001 and 2011. This population-level administrative data allows us to implement a research design based on slope changes around tax kink points. Since it is unlikely that other factors change exactly at the tax kink points, this research design offers highly credible estimates of causal effects. As RKD relies on identifying kinks in the enrollment-income profile, it is important to distinguish between kinks and nonlinearities in the enrollment-income function. We consider multiple strategies to address this methodological concern. Specifically,

[2] For evidence on credit constraints, student aid, and college enrollment decisions, see van der Klaauw (2002); Dynarski (2003); Stinebrickner and Stinebrickner (2008); Nielsen, Sørensen, and Taber (2010); Lochner and Monge-Naranjo (2011); and Solis (2017). These papers primarily exploit quasi-experimental variation in enrollment-contingent student aid. Dynarski and Scott-Clayton (2013) summarize the student aid literature and indicate that additional $1,000 of student aid increases college enrollment by roughly 2 to 4 percentage points. For evidence on the effects of education tax credits on college enrollment, see Long (2004), Turner (2011a, b), LaLumia (2012), Bulman and Hoxby (2015), and Hoxby and Bulman (2015).

[3] Recent work by Bulman and Hoxby (2015a, b) examines the impact of tax-based federal student aid over a broader segment of the income distribution and finds no enrollment effects. Bulman and Hoxby (2015) and Hoxby and Bulman (2015) study the American Opportunity Tax Credit and the Tuition and Fees Deduction. Unlike the EITC from the high-school senior year, this credit provides students and their families with a benefit after they incur college costs. This timing difference may also account for the different patterns of enrollment.

[4] Hoynes, Miller, and Simon (2015) present estimates of the impacts of EITC benefits on birth weight. Dahl and Lochner (2012) present estimates of the impacts of EITC benefits on early age test scores. Michelmore (2013) studies the impacts of state and federal EITC benefits on college enrollment, though the identification strategies and treatment populations differ from those used in this study.

[5] See Calonico, Cattaneo, and Titiunik (2014); Landais (2015); Card, Lee, Pei, and Weber (2015); Ganong and Jäger (2014); Marx and Turner (2015); and Gelber, Moore, and Strand (2016).

in addition to looking at further analyses of timing, persistence, and other points in the income distribution, we also consider placebo analyses, controlling for income polynomials, and other strategies.

This paper is organized as follows. Section I describes the data, institutional background, and cross-sectional analysis. Section II presents the main empirical analysis to estimate causal effects of tax refunds on college enrollment. Section III discusses the magnitude of the RKD estimates in the context of the prior literature and evidence on liquidity and informational constraints. Section IV concludes.

I. Background and Data

A. *The Earned Income Tax Credit and the Child Tax Credit*

The EITC is a refundable tax credit that provides benefits to low-income working families. As the credit is refundable, taxpayers may benefit from the EITC even when they have no tax liability. The EITC amounts are primarily determined based on tax filing status, the number of qualifying children, and income.[6] Qualifying children for the EITC are relatives who are under age 19 or permanently disabled and who resided with the tax filers for at least half of the year.[7]

The EITC benefit schedule is characterized by three regions: the phase-in region where the credit is increasing in income, the maximum credit region where the credit value is constant in income, and the phase-out region where the credit is decreasing in income. The value of the credit is a function of earned income and adjusted gross income (AGI). Earned income includes wages, salaries and tips, and net earnings from self-employment. Benefits from unemployment insurance, workers compensation, food stamps, Medicaid, TANF, SSI, social security, disability, and child support do not count as earned income. AGI includes total gross income for the taxpayer minus adjustments for certain expenses.[8] As the name suggests, taxpayers must have positive earned income to claim the EITC. Starting from no earned income, the credit is phased in. As earned income continues to increase, taxpayers reach the maximum credit region of the EITC schedule.

The income variable for determining the value of EITC benefits changes at the beginning of the phase-out portion of the schedule. The rules for determining benefits are as follows.[9] If earned income is equal to AGI, EITC benefits are computed using earned income. If earned income is not equal to AGI and AGI is below a phase-out earnings threshold, then EITC benefits are computed using earned income. If earned income is not equal to AGI and AGI is greater than or equal to the phase-out threshold, then EITC benefits are computed using the maximum of AGI and earned income. Online Appendix Table 1 lists the exact phase-out AGI thresholds.

[6] Eligibility for the EITC also includes a ceiling on investment income, from such sources as dividends, rental properties, etc. In 2014, the limit on investment income was $3,350.

[7] Children between ages 19 and 24 can also count as qualifying children if they were full-time students for any five months of the calendar year.

[8] The exact income sources and adjustments for computing AGI are listed on IRS Form 1040 (https://www.irs.gov/pub/irs-pdf/f1040.pdf). Examples of adjustments include moving expenses, alimony, and penalties for early withdrawal of savings.

[9] IRS Publication 596 provides the official documentation of the rules and eligibility criteria for this credit.

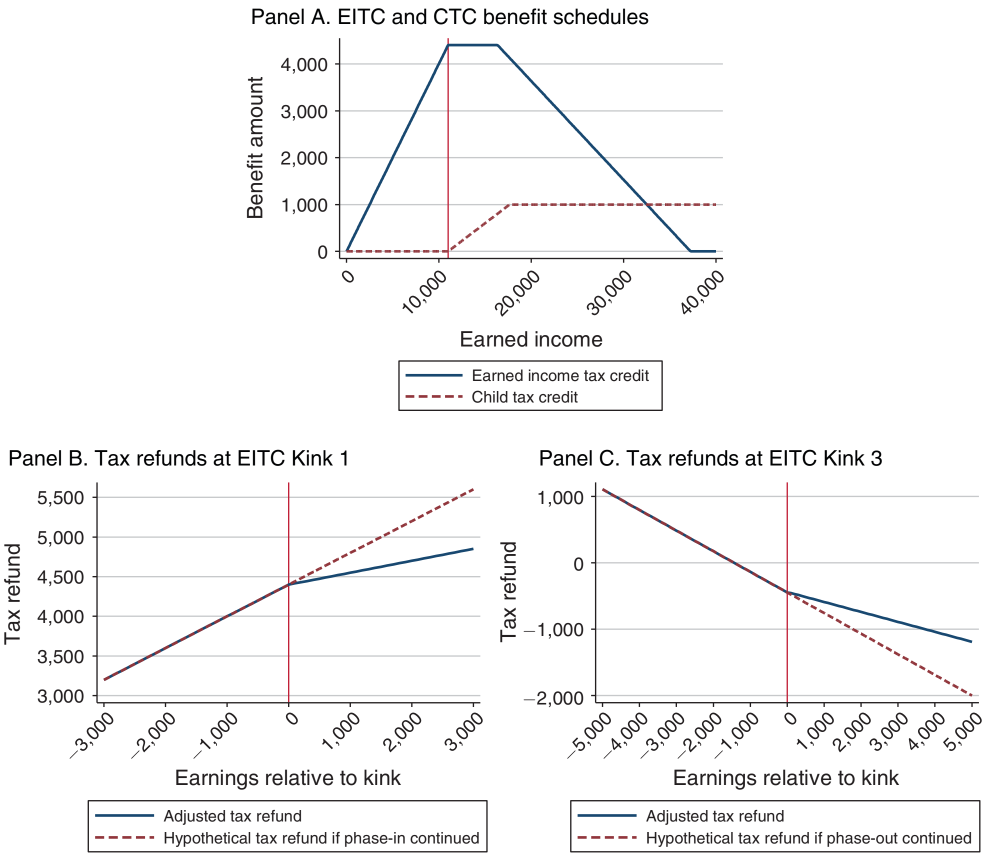

Figure 1. Institutional Background

Notes: These figures plot simulated federal tax amounts that are calculated using the NBER TAXSIM calculator. Panel A shows the EITC and CTC schedules for the case of a taxpayer with two children (one kid of age 14 and another of 17) with married filing jointly status for the year 2005. The solid line represents the EITC benefit schedule and the dashed line represents the CTC benefit schedule. Using the same case as panel A, panels B and C plot tax refunds as a function of earnings relative to the EITC kink points. For panel B, the EITC kink point corresponds to earned income of \$11,000, and for panel C, the kink point corresponds to earned income of \$37,263. For the tax calculations, earned income is from W-2 wages only, and earned income is equal to AGI. A negative tax refund means there is positive tax owed.

Following these rules, Figure 1, panel A illustrates a stylized EITC benefit schedule in the case where AGI equals earned income for a family with two dependents. The figure highlights that the EITC benefit schedule has three kink points: EITC Kink 1 where the phase-in portion meets the maximum benefit portion, Kink 2 where the maximum benefit begins to phase out, and Kink 3 where the credit becomes fully phased out.[10] Online Appendix Table 1 lists the exact values for EITC Kink 1, and online Appendix Table 2 lists the exact values for EITC Kinks 2 and 3.

[10] We recognize that there is also a kink point where the credit begins to phase in. However, empirically there are very few families with high school seniors at this low level of income.

In addition to the EITC, the Child Tax Credit (CTC) and Additional Child Tax Credit (ACTC) offer taxpayers benefits of up to $1,000 per qualifying child. These two tax credits are effectively a single tax credit with the CTC being the non-refundable portion and the ACTC being the refundable portion.[11] We refer to the combined CTC and ACTC as the CTC. Like the EITC, the CTC phases in with earnings and is phased out at higher levels of income. From 2001 through 2008, the CTC begins to phase in at EITC Kink 1 (either near the two qualifying children Kink 1 value in 2001–2007 or near the one qualifying child Kink 1 value in 2008; online Appendix Table 1 lists the income thresholds at which the CTC phases in). Unlike the EITC, qualifying children for the CTC must be younger than age 17. As the analysis sample consists of taxpayers with high school senior dependents who are age 17 or 18, the CTC applies only for those taxpayers in the analysis sample who have a younger child in addition to the high school senior dependent. Figure 1, panel A presents an example of the CTC benefit schedule for a married filing jointly family with two dependents in 2005.

B. *EITC Kink Points*

This section discusses the specific kink points in the EITC benefit schedule that we exploit in the empirical analysis below. Within a given year, the first EITC kink point, EITC Kink 1, varies only by the number of qualifying children. Online Appendix Table 1 lists the specific earnings thresholds for each tax year for the EITC as well as the earnings thresholds for the CTC. In the EITC phase-in portion, EITC benefits increase by $0.34, $0.40, and $0.45 per dollar of earned income based on one, two, or three or more qualifying children, respectively. In some years, the CTC begins to phase in at the first EITC kink point so that tax refunds continue to increase as earnings increase in the maximum credit region of the EITC benefit schedule.

To provide intuition for our RKD specification at the first EITC kink point, Figure 1, panel B plots a simulated example of the change in tax refunds at EITC Kink 1. The figure illustrates that tax refunds increase at a faster rate for earnings levels below the kink point because EITC benefits increase in the phase-in region. To the right of the kink point, EITC benefits no longer continue to phase-in since individuals are in the maximum credit region. Intuitively, the slope of tax refunds decreases as tax refunds no longer increase at the higher rate. In some cases, tax refunds still continue to increase to the right of the kink because the CTC begins to phase in at a rate of $0.10 per dollar of income at the first EITC kink point, though this credit does not apply to all families in the analysis sample. For families without a CTC qualifying dependent (a dependent child under age 17), the slope change in tax refunds at the first EITC kink point is roughly $0.34 because the slope changes from $0.34 (the EITC phase-in rate) to 0. For taxpayers in the analysis sample that have one high school senior dependent and at least one other younger child, the CTC does apply and the slope change in tax refunds at the first EITC kink point is roughly

[11] IRS Publication 972 provides the official documentation of the rules and eligibility criteria for this credit.

\$0.30 because the slope changes from about \$0.40 (the weighted average over the EITC phase-in rates based on the fractions of taxpayers with two, or three or more qualifying children) to roughly \$0.10 (the Child Tax Credit phase-in rate).

Given that the EITC and CTC kink points coincide in some cases, the regression kink design does not ignore the CTC variation and instead the research design around EITC Kink 1 is based on pooling variation from the EITC and CTC benefit schedules around EITC Kink 1. In particular, the research design around EITC Kink 1 exploits a slope change in tax refunds at EITC Kink 1 that is due to slope changes in both the EITC and CTC benefit schedules. In cases when the EITC and CTC kink points coincide, the slope change in tax refunds is driven by both the slope change in EITC benefits and the slope change in CTC benefits. In cases when the EITC and CTC kink points do not coincide, the slope change in tax refunds is driven only by the slope change in EITC benefits. We pool the variation in this way to have higher statistical power for the regression kink analysis.[12,13]

We also investigate kinks around EITC Kinks 2 and 3 (the transition from the maximum credit to the phase-out and from the phase-out to zero benefits respectively). Online Appendix Table 2 lists the income values for EITC Kink 2 and Kink 3. As mentioned in Section IA, the income measure used to determine EITC income benefits changes around EITC Kink 2. As a result, the definition of the running variable changes around EITC Kink 2, and this rules out implementing a traditional one-dimensional regression kink design around EITC Kink 2.[14]

Online Appendix Figures 1A and 1B illustrate the main issue at EITC Kink 2 in our setting. For these plots, we define EITC income as the income measure used to determine EITC benefits. Specifically, we follow the rules discussed in Section IA (and in IRS Publication 596 or EIC Worksheet B) so that EITC income is equal to earned income when AGI is less than the phase-out threshold, and EITC income is equal to the maximum of earned income and AGI if AGI is above the phase-out threshold. As illustrated in online Appendix Figure 3A, we are able to empirically reproduce the EITC benefit schedule.[15] However, even though the EITC benefit schedule can be reproduced with this running variable, online Appendix Figure 3B demonstrates that the frequencies change discontinuously at EITC Kink 2. Intuitively, taxpayers can only be on the maximum credit region close to EITC Kink 2 if their AGI is less

[12] When we try to obtain estimates based solely on EITC variation (for example by focusing only on households that do not have a younger child in addition to the high school senior dependent, which corresponds to 614,085 observations out of the 1,015,643 observations in the EITC Kink 1 analysis sample), we obtain consistent results with the full sample results presented in Section IIB.

[13] When aligning groups based on EITC kink points, we have verified that the composition of individuals also facing CTC slope changes or having CTC benefits does not change at EITC Kink 1. Specifically, using the regression kink specification described in the Empirical Analysis section below, we have tested for a kink in age of the student or number of other dependents, and we have found no evidence of kinks in these other outcomes.

[14] Weber (2016) discusses the changes in the EITC rules around EITC Kink 2 in detail and presents three-dimensional graphs of the EITC benefit schedule that illustrate the joint roles of earned income and AGI in determining EITC benefits around EITC Kink 2. These graphs suggest that it may be possible to implement a multi-dimensional regression kink analysis around EITC Kink 2, but such an analysis is beyond the scope of the current paper.

[15] We have also examined whether we could use just earned income or just AGI as the running variable. However, neither of these definitions of the running variable allows us to accurately reproduce the EITC benefit schedule around EITC Kink 2. Because of the measurement error, the results show a hump-shape instead of a kink at EITC Kink 2, and thus we do not have a clean first stage for the RKD analysis.

than the phase-out threshold and their earned income is less than EITC Kink 2. On the other hand, taxpayers can be on the phase-out portion of the benefit schedule just above EITC Kink 2 if their AGI is above the phase-out earnings threshold and either their AGI or earned income puts them in the phase-out income range. Thus, there is a discontinuous increase in the number of individuals who are in the phase-out income range as opposed to the maximum credit income range. The discontinuous change in the frequencies at EITC Kink 2 violates the RKD identifying assumptions and hence invalidates the RKD at EITC Kink 2.[16]

To provide intuition for our RKD specification at EITC Kink 3 (the point at which benefits are completely phased out), Figure 1, panel C plots a simulated example of the change in tax refunds around EITC Kink 3. The figure illustrates that, as adjusted gross income increases, tax refunds decrease at a faster rate for income levels below the kink point because EITC benefits are decreasing in the phase-out region. To the right of the kink point, EITC benefits no longer continue to phase-out since individuals are in the zero-credit region. Thus, as adjusted gross income increases to the right of the kink point, tax refunds decrease at a slower rate than the rate to the left of the kink point. Intuitively, the regression kink design aims to test whether the slope of the education-AGI relationship to the right of the kink point is higher than the slope to the left of the kink point.

A key identifying assumption of the regression kink design is that other variables do not have kinks at the tax kink points. Intuitively, if another variable in addition to tax refunds has a kink, then we cannot determine whether any kink in enrollment at the tax kink point is driven by the change in tax refunds, the other variable, or both. We discuss evidence on this identifying assumption in more detail below, but we note here that, based on institutional rules and aid formulas, federal student aid does not change around EITC Kink 1 or Kink 3.

C. *Data*

To analyze the effect of cash-on-hand on college enrollment we use information from the population of US tax returns and from the Social Security Administration (SSA). To focus on high school seniors, we create our sample by first pulling all social security numbers (SSNs) from the SSA data for individuals who are 17 or 18 during the years 2001 to 2011. For these observations, we assign high school cohorts based on the month and year of birth. In each year, the cohort of high school seniors is defined as individuals who were 18 as of December 31 and who were born in September through December, and individuals who were 17 as of December 31 and who were born in January through August. In aggregate, this approach matches well to the number of high school seniors reported by the Department of Education. For example, for 2007 the US Department of Education reports a total of 4.21 million

[16] We have also tried implementing an RKD at EITC Kink 2 using the restricted sample of individuals with earned income equal to AGI. However, even within this restricted sample, the frequencies are not smooth at EITC Kink 2. Some taxpayers who would have had AGI equal to earned income and been just above the phase-out earnings threshold report adjustments so that they have AGI below the phase-out earnings threshold and hence they qualify for maximum EITC benefits. Thus, within the restricted sample, the frequencies illustrate missing mass just above EITC Kink 2.

high school seniors, whereas we find 4.09 million in the tax data.[17] Next, we look for tax returns that claim these individuals as dependents during the sample period, retaining information on family structure (married, number of dependents) and income from the 1040 tax form. Given that EITC-claiming households cannot file as married filing separately, we restrict the sample to returns that file as either head of household or married filing jointly.

We measure college enrollment using data from the 1098-T tax form and data on the universe of federal student aid recipients, including all Pell grant and student loan recipients, from the US Department of Education. To remain eligible for Title IV federal student aid, schools are required to send a 1098-T form to nearly all students, and to the IRS.[18] We supplement the data from the 1098-T tax form with data on the universe of federal student aid recipients since some students who have educational expenses that are entirely covered by student aid may not receive a 1098-T. This situation is particularly relevant for low-income households around EITC Kink 1. Combining these data sources, we define an indicator for enrollment that is equal to one if a student has a 1098-T tax form or if an individual has any federal student aid. Recent work (Chetty et al. 2017) shows that the combination of these data sources creates a comprehensive measure of enrollment.

D. *Analysis Samples*

We construct the analysis samples using symmetric bandwidths around EITC Kink 1 and EITC Kink 3. We use symmetric bandwidths so that we can apply existing bandwidth selection programs following Card et al. (2015). We use a symmetric \$3,000 bandwidth so that we can avoid any interaction with EITC Kink 2 for the EITC Kink 1 sample, and we use the same bandwidth around EITC Kink 1 and EITC Kink 3 for symmetry in presentation. We also examine sensitivity to alternative bandwidths around each kink point. Given the rules for computing EITC benefits, we define earnings relative to the kink points using earned income for the EITC Kink 1 analysis sample and using AGI for the EITC Kink 3 analysis sample. Additionally, for both kink points, we exclude high school seniors who died at any time between 2001 and 2012, late-filed tax returns (so we can ensure that the tax refunds are received in the spring of the high school senior year and not later), taxpayers with any self-employment income, and taxpayers with more than a \$1,000

[17] This approach may misclassify some individuals. In particular, since we do not have data directly from schools on their senior students, their graduation, and subsequent enrollment, our ability to specifically identify high school seniors, on-time high school graduation, and college enrollment may be limited in the tax data. Chapman et al. (2011) discusses trends in on-time high school graduation rates. Heckman and Lafontaine (2010) also study trends in high school completion rates, and Elder and Lubotsky (2009) discuss early grade retention rates, particularly for disadvantaged students. However, we note that misclassification of high school seniors is not likely to be problematic for our specifications. In the RKD case, as long as this misclassification does not vary across the EITC kink point we examine, this will not have an effect on our estimates. Intuitively, measurement error in defining the senior year may impact the average enrollment rate but should not have a differential effect on the slope of the enrollment profile at the tax kink. Further, we find no evidence of a kink in student age at the kink points for EITC Kink 1 or EITC Kink 3.

[18] This form is used to verify educational expenses for certain tax-based aid programs. Exceptions to the 1098-T filing rule include: courses for which no credit is earned; nonresident alien students; and students whose qualified tuition is covered by a formal billing arrangement between the institution and the student's employer.

Table 1—Summary Statistics

	EITC Kink 1 sample		EITC Kink 3 sample	
	Mean	SD	Mean	SD
Enroll (1 year after HS)	30.39	45.99	43.52	49.58
Married filing jointly	0.19	0.39	0.36	0.48
Head of household	0.81	0.39	0.64	0.48
Child dependents	1.70	0.88	1.69	0.85
After-tax income	17,750.66	38,223.44	44,614.87	4,808.84
Balance due	−5,346.98	1,705.87	−2,642.32	2,162.06
Pretax income	12,403.68	38,068.65	41,972.55	3,879.44
Has refund	1.00	0.03	0.94	0.24
Observations	1,015,643		1,173,833	

Notes: Dollar values are CPI adjusted to 2015 dollars. Pretax income is total income, which is measured on line 22 of 1040 Form. This total income measure is the sum of all income listed on 1040 Form. Balance due captures the net amount due; it is negative if a net refund to the taxpayer is due. After-tax income is the sum of pretax income and the balance due.

difference between W2 earnings and total earned income. We impose the non-W2 income restrictions because many non-W2 income sources cannot be third party verified. Previous studies (Saez 2010 and Chetty, Friedman, and Saez 2012) highlight evidence of individuals with these income sources sorting along the tax schedule, which violates the identifying assumptions behind the RKD approach (Card et al. 2015). We also remove a small number of returns that are likely to have errors in key income measures.[19]

Table 1 shows summary statistics for the analysis samples. The EITC Kink 1 and Kink 3 samples, respectively, consist of roughly 1 million and 1.2 million high school seniors between 2001 and 2011. Mean enrollment for the EITC Kink 1 sample is roughly 30 percent, indicating that roughly seven out of ten children from these families do not attend college in the fall after their senior year. Mean enrollment for the EITC Kink 3 sample is slightly higher at roughly 44 percent. Roughly 20 percent of families are married for the EITC Kink 1 sample compared to roughly 36 percent in the EITC Kink 3 sample. The most striking difference across the two samples is in income. For the EITC Kink 1 and Kink 3 samples, respectively, after-tax income is roughly \$18,000 and \$45,000.[20] There are also large differences in tax refunds related to differences in EITC benefits. For the EITC Kink 1 sample, the average balance due indicates a tax refund of roughly \$5,300, and for the EITC Kink 3 sample, the summary statistics indicate an average tax refund of roughly \$2,600.

[19] We also exclude taxpayers with large differences between taxpayer entered values and computer verified values for total income, AGI, tax refunds, and EITC benefits. Specifically, taxpayers with differences of more than \$100 in total income, AGI, or tax refunds, and differences of more than \$10 in EITC benefits, are excluded.

[20] In Table 1, the large standard deviations in pre- and after-tax income for the EITC Kink 1 sample are driven by a few outliers. These outliers remained in the sample because the EITC Kink 1 sample is defined based on earned income, but pre- and after-tax income are defined using total income from IRS 1040 Form. After winsorizing the top and bottom 1 percent, the mean and standard deviation of pretax income for the EITC Kink 1 sample are \$12,931.34 and \$2,668.04 respectively, and the mean and standard deviation of after-tax income for the EITC Kink 1 sample are \$18,264.27 and \$3,928.80, respectively. These values do not affect the RKD estimates since the running variable for EITC Kink 1 is defined using earned income, and not these income measures.

II. Empirical Analysis

A. *Regression Kink Design*

The goal of the empirical analysis is to estimate the causal impact of cash-on-hand, as measured by tax refunds in the spring of the high school senior year, on college enrollment, following the high school senior year. Because cash-on-hand may be correlated with other factors leaving students less academically prepared or qualified for college, it is necessary to develop a quasi-experimental research design to identify and estimate causal effects of cash-on-hand on enrollment. For example, if one were to start with an OLS regression of enrollment on cash-on-hand, one might find a positive correlation, but this may not represent a causal effect of cash-on-hand on college enrollment because students from households with more cash-on-hand at the time of college enrollment may have had prior advantages that better prepared them for college enrollment.

To overcome these challenges and estimate causal effects of cash-on-hand, we use a fuzzy regression kink design (RKD) around EITC Kink 1 and, separately, EITC Kink 3.[21] For a given tax kink point, this approach relates the change in the slope of the enrollment function at the kink point to the change in slope of tax refunds at the kink point. By exploiting the exogenous change in slopes around the tax kink points due to the policy changes, we are able to estimate causal effects of changes in cash-on-hand on changes in college enrollment. We implement the fuzzy RKD around a given kink point by estimating both the change in the enrollment-income profile and the change in the tax refund-income profile around the kink point. The estimate of the causal impact of cash-on-hand on college enrollment at that kink point is then the ratio of these slope changes. For the implementation around each kink point, we compute earnings relative to that kink point, denoted by *kinkdist*. As noted earlier, given the rules for computing EITC benefits, we define earnings relative to the kink points using earned income for the EITC Kink 1 analysis sample and using AGI for the EITC Kink 3 analysis sample. This measure allows us to pool the data across groups to estimate changes in the slopes of enrollment and after-tax income at each kink point.

The specific regression specifications are as follows. For each kink point, following Card et al. (2015) and Nielsen, Sørensen, and Taber (2010), we consider the following constant-effect, additive model to examine the effects of refunds on college enrollment:

$$enroll_i \;=\; \beta refund_i + g\left(kinkdist_i\right) + \varepsilon_i .$$

[21] Even though the tax refund function is deterministic, we use a fuzzy RKD rather than a sharp RKD. The fuzzy approach allows us to empirically estimate the change in slope of tax refunds and show that it matches the statutory slope change. The trade-off of using the fuzzy RKD in place of the sharp RKD is a potential loss of precision. As a result, implementing the fuzzy RKD should result in relatively more conservative inferences about the impact of tax refunds on enrollment, compared to using a sharp RKD specification. We also note that the fuzzy RKD accounts for incomplete take-up of tax benefits, though for taxpayers with children, EITC take-up rates exceed 95 percent (see Manoli and Turner 2017).

The subscript i refers to the individual who is a high school senior. The variable $enroll_i$ is an indicator equal to one if individual i enrolls in college in the year after his or her high school senior year. The variables $refund_i$ and $kinkdist_i$ are based on tax returns filed in the spring of individual i's senior year on which individual i is claimed as a dependent. The $refund_i$ variable measures the tax refund and $kinkdist_i$ measures the distance (in 2011 dollars) relative to the specified kink point. The function $g(\cdot)$ is a continuous function. The tax refund function, $refund_i = refund(kinkdist_i)$, is assumed to be a continuous and deterministic function of earnings (equivalently of earnings relative to the kink point) with a slope change at the kink point (i.e., at $kinkdist = 0$). If $g(\cdot)$ and $E(\varepsilon \mid kinkdist = k)$ have derivatives that are continuous in $kinkdist$ at $kinkdist = 0$, then the fuzzy RKD causal effect is given by

$$\beta = \frac{\lim\limits_{k\to 0^+} \frac{\partial E[enroll \mid kinkdist = k]}{\partial k}\Big|_{k=0} - \lim\limits_{k\to 0^-} \frac{\partial E[enroll \mid kinkdist = k]}{\partial k}\Big|_{k=0}}{\lim\limits_{k\to 0^+} \frac{\partial E[refund \mid kinkdist = k]}{\partial k}\Big|_{k=0} - \lim\limits_{k\to 0^-} \frac{\partial E[refund \mid kinkdist = k]}{\partial k}\Big|_{k=0}}.$$

The numerator of this expression captures the change in the slope of the conditional expectation of enrollment with respect to income at the kink point. The denominator reflects the change in the slope of tax refunds at the kink point. Identification with the RKD methodology requires that (i) taxpayers do not sort along the tax schedule and that (ii) other covariates do not have kinks at the tax kink point, and we present evidence consistent with both of these points below.

We estimate the changes in enrollment and after-tax income, for the above numerator and denominator respectively, using regressions of the following form:

$$enroll_i = \alpha kinkdist_i + \delta^{enroll} D_i \times kinkdist_i + \alpha_2 X_i + u_i,$$

$$refund_i = \gamma kinkdist_i + \delta^{refund} D_i \times kinkdist_i + \gamma_2 X_i + v_i,$$

where D_i is an indicator variable equal to one if earnings are above the kink point, i.e., $D_i = \mathbf{1}(kinkdist_i > 0)$. The variable X denotes a vector of covariates included in the regressions. The fuzzy RKD estimator is then given by

$$\hat{\beta} = \frac{\widehat{\delta^{enroll}}}{\widehat{\delta^{refund}}}.$$

The vector of covariates includes dummies for year, filing status, and number of kids. Intuitively, the coefficient $\hat{\beta}$ reflects the impacts on enrollment of additional cash-on-hand coming from increases in tax refunds, or equivalently from increases in after-tax income, in the spring of the high school senior year.

When estimating these enrollment and refund regressions around each kink point, we use the same bandwidths for both regressions. This allows us to estimate the fuzzy RKD using an instrumental variables regression in which we instrument for tax refunds using the slope change at the tax kink point, $D_i \times kinkdist_i$. Specifically, the first- and second-stage regressions are, respectively,

$$refund_i = \delta^{refund}[D_i \times kinkdist_i] + \gamma kinkdist_i + \gamma_2 X_i + v_i,$$

$$enroll_i = \beta refund_i + \alpha kinkdist_i + \alpha_2 X_i + u_i.$$

When estimating all of the RKD regression specifications, we cluster the standard errors based on \$100 bins of earnings relative to the kink. We have also verified that the statistical significance of the RKD point estimates does not change with alternative clustering strategies.

As the formula for the RKD estimator highlights, the RKD strategy yields estimates of causal effects that apply at the specific kink points being considered. Given that we implement the RKD strategy separately at EITC Kink 1 and EITC Kink 3, we note that, a priori, it is entirely possible for the estimates to be equal or different, and these estimates may be different from estimates at other kink points or parts of the income distribution.

B. *Regression Kink Estimates*

Figure 2 presents the graphical evidence corresponding to the RKD analysis around each kink point. Panels A and B correspond to EITC Kink 1, and panels C and D correspond to EITC Kink 3. For EITC Kink 1, Figure 2 panel A plots tax refunds against earnings relative to the kink point, and Figure 2 panel B plots enrollment against earnings relative to the kink point. We construct these plots by computing average tax refunds and enrollment rates within \$100 bins of income relative to the respective kink points. We generate the fitted values by using the individual-level data and regressing the enrollment indicator on $kinkdist_i$ and $kinkdist_i \times D_i$. We then plot the average of the fitted values in each \$100 bin of income relative to the kink. Note that while we bin the data in \$100 increments for the figures, the estimated slopes in the figures correspond to regression output from underlying individual-level data.[22] In addition, these stylized figures do not include controls used in our baseline specification. Figure 2, panels C and D, are constructed analogously for EITC Kink 3. For EITC Kink 1, Figure 2, panel A highlights a kink in tax refunds at the first EITC kink point and Figure 2, panel B shows a slope change in enrollment rates at the same kink point. For EITC Kink 3, Figure 2, panel C highlights a kink in tax refunds at the third EITC kink point, but Figure 2, panel D does not show any noticeable slope change in enrollment rates at the same kink point.

Table 2 presents the quantitative results estimated on individual-level data corresponding to the graphical evidence in Figure 2. For EITC Kink 1, the first-stage

[22] The choice of \$100 bins is arbitrary and used only for the graphical evidence. In online Appendix Figures 2 and 3, we show that the patterns in Figure 2, panels B and D hold when using alternative bin sizes. Because the RKD estimates are based on the individual-level data used for these plots, the choice of bin sizes does not affect the estimates at all (i.e., even with the different bin sizes in online Appendix Figures 2 and 3, the RKD estimates are still the same as those shown in Table 2).

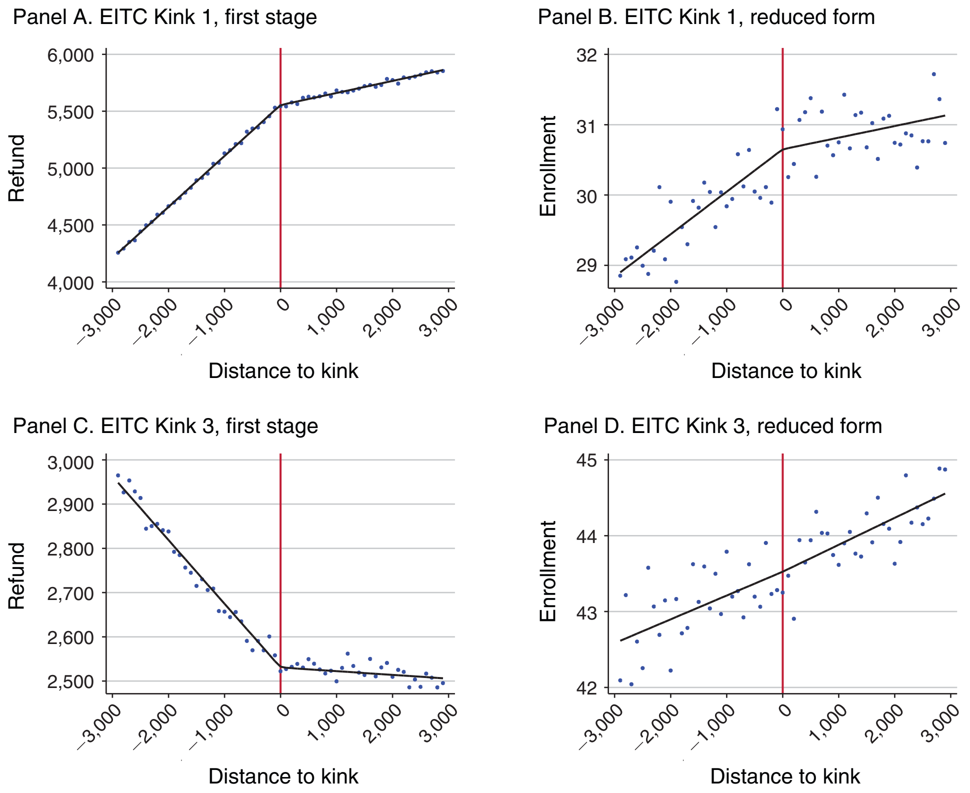

Figure 2. RKD Graphical Evidence

Notes: Panels A and B correspond to EITC Kink 1, and panels C and D correspond to EITC Kink 3. The circles show the mean tax refund or enrollment rate for each \$100 bin of earnings relative to the tax kink points. The solid lines show fitted values for each \$100 bin of earnings relative to the kink points. Fitted values are obtained from regressions using the individual-level data in which the tax refund or an enrollment indicator is regressed on a linear control for earnings relative to the kink point and a dummy for earnings less than the kink point interacted with the linear control. One hundred dollars bins are assigned based on rounding earnings relative to the kink point to the nearest \$100 amount.

change in the slope of after-tax income (tax refunds) is −0.34, and the estimated reduced-form slope change in enrollment is −0.44. Using the IV specification to estimate the ratio of these two coefficients, the RKD estimates indicate that a \$1,000 increase in after-tax income (tax refunds) causes roughly a 1.32 percentage point increase in college enrollment. We find similar results for one and two child households separately, though in each case the standard errors are sufficiently large that we cannot rule out zero effects. We note that the income point corresponding to EITC Kink 1 varies across one child and two child cases so that the separate results suggest that there is an enrollment kink for each group at the same location as the tax kink point but that the enrollment kink point occurs at different income levels. We do not find evidence of a kink in enrollment among one child families at the income level corresponding to the kink for two children families, and similarly, we do not find evidence of a kink in enrollment among two child families at the income level corresponding to the kink for one child families.

Table 2—RKD Estimates

	First stage dep. var. = refund	Reduced form dep. var. = enrollment	IV dep. var. = enrollment
Panel A. EITC Kink 1			
Earnings relative to kink (*kinkdist*)	0.440	0.564	−0.0175
	[0.00170]	[0.0598]	[0.105]
Slope change at kink ($D \times$ *kinkdist*)	−0.335	−0.443	
	[0.00298]	[0.119]	
Effect of $1,000 on enrollment (IV)			1.323
			[0.352]
Observations	1,015,643	1,015,643	1,015,643
Panel B. EITC Kink 3			
Earnings relative to kink (*kinkdist*)	−0.130	0.354	0.484
	[0.00297]	[0.0717]	[0.0508]
Slope change at kink ($D \times$ *kinkdist*)	0.131	0.131	
	[0.00487]	[0.112]	
Effect of $1,000 on enrollment (IV)			1.001
			[0.861]
Observations	1,173,833	1,173,833	1,173,833

Notes: Each regression includes dummy variables for senior year, number of children, and filing status. Standard errors are clustered based on $100 bins of earnings relative to the kink point and listed in brackets.

For EITC Kink 3, the first-stage change in the slope of after-tax income (tax refunds) is 0.13, and the estimated reduced-form slope change in enrollment is 0.13. Using the IV specification to estimate the ratio of these two coefficients, the RKD estimates indicate that a $1,000 increase in after-tax income (tax refunds) causes roughly a 1.00 percentage point increase in college enrollment. However, given that the reduced-form estimate is not statistically significant, the RKD estimate is also not statistically significant. Thus, given the standard errors of the estimates, we cannot rule out that the effects of cash-on-hand at EITC Kink 3 are similar to the effects at EITC Kink 1, or that they are statistically different from zero.

We have examined the differences between the estimates around EITC Kink 1 and EITC Kink 3 in multiple ways. For example, given that the EITC Kink 1 sample has a high fraction of households with head-of-household filing status, we have estimated the effects at EITC Kink 3 using the subsample of household around EITC Kink 3 with head-of-household filing status, but we do not find any evidence of a kink in enrollment even in this subsample around EITC Kink 3. Given the relatively large differences in income between the two samples, we conclude that the EITC Kink 1 estimates may apply for lower-income households, and the EITC Kink 3 estimates may apply to higher income households. Intuitively, cash-on-hand may affect college enrollment decisions for students from relatively low-income households but not have any impacts on college enrollment decisions for students from higher income households. We also acknowledge that the RKD estimates may only apply locally around each kink point, so the effects of cash-on-hand on college enrollment could be different at different points in the income distribution or for different populations.

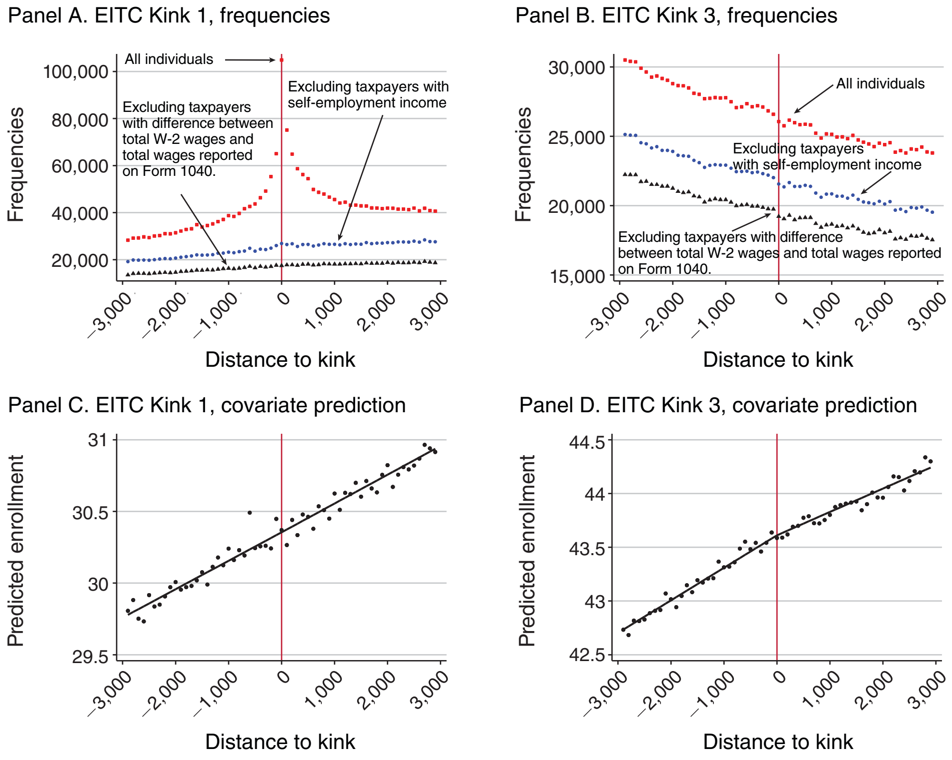

FIGURE 3. EVIDENCE ON THE IDENTIFYING ASSUMPTIONS

Notes: Panels A and C correspond to EITC Kink 1, and panels B and D correspond to EITC Kink 3. Panel A plots the number of tax returns in the following ways: frequencies including the self-employed, frequencies excluding the self-employed, and frequencies when excluding the self-employed and individuals with a difference between W-2 wages and wages reported on the 1040 form of more than $1,000. For panel C, the circles show mean predicted enrollment rates. The solid lines show fitted values of predicted enrollment rates computed by regressing an enrollment indicator on dummies for calendar year, filing status, and number of dependents and linear controls for senior year income. The plots for panels B and D are analogous to the corresponding figures in panels A and C.

C. *Evidence on the Identifying Assumptions*

Identification with the RKD methodology requires that (i) taxpayers do not sort along the tax schedule and that (ii) other covariates do not have kinks at the tax kink points. This section presents evidence that both of these key assumptions hold at each kink point. We study sorting along the tax schedule by examining frequencies of taxpayers around each kink point, and we study covariate changes around each kink point using covariate predicted enrollment rates.

Figure 3, panels A and B present plots of the frequencies of taxpayers around EITC Kink 1 and EITC Kink 3, respectively. For EITC Kink 1, prior to our sample restrictions (i.e., when we include all tax returns around the first EITC kink point), we find significant evidence of bunching around the kink point. This is consistent with previous evidence in the income tax literature (Saez 2010 and Chetty, Friedman, and Saez 2012). After excluding individuals with self-employment earnings or other non-third party verified income, as well as individuals with more than a $1,000 difference between earned income and AGI, we find no evidence of sorting along the tax schedule in the sample. Similarly, around EITC Kink 3, the frequencies do not show evidence of sorting along the tax schedule.[23]

To examine if any covariates change at the tax kink point, for each kink point, we regress enrollment on a set of covariates, obtain predicted enrollment values, and then test for a kink in predicted enrollment using the above RKD regression specifications. Intuitively, if the aggregate effect of the covariates in our specification has a kink at the tax kink point, then the predicted enrollment values will also have a kink.[24] However, as none of the covariates are expected to change at the tax kink point, we can verify that there is no evidence for a kink in the covariate predicted enrollment rates.

Figure 3, panels C and D present the graphical analysis of covariate predicted enrollment for EITC Kink 1 and EITC Kink 3, respectively. The plots show that there are no detectable changes in covariate predicted enrollment when predicting enrollment using a covariates from the tax data.

Related to the identifying assumptions, we present further analysis of federal student aid to examine how federal student aid eligibility varies around the EITC kink points. Specifically, using the data from the Department of Education, we compute percentiles of Expected Family Contribution (EFC) and Pell grants within each $100 bin of earnings relative to the kink and then examine whether there are any kinks in these percentiles. Intuitively, if additional cash-on-hand reduces federal student aid eligibility or the amount of aid a student is eligible for, then we would expect to observe a kink in the percentiles of federal student aid.

Online Appendix Figures 4A and 4B present the plots of EFC and Pell grant percentiles around EITC Kink 1. Online Appendix Figure 4A highlights that almost all families around EITC Kink 1 have an EFC of 0 since they have sufficiently low incomes to qualify for maximum Pell grants.[25] Thus, around EITC Kink 1, additional cash-on-hand from larger tax refunds does not reduce the amount of aid an individual is eligible for. Online Appendix Figures 4C and 4D present the corresponding EFC and Pell grant plots around EITC Kink 3. The plots indicate that around EITC Kink 3, households have higher incomes so that they have higher EFCs than around EITC Kink 1, but there do not appear to be any kinks in EFC around the tax kink point. Thus, it does not appear that EFC or Pell grant amounts increase at a faster or slower rate once EITC benefits are fully phased-out. Overall, the plots

[23] We follow Card et al. (2015) and formally test for a kink in the frequencies by estimating a series of polynomial models using the binned frequencies. The polynomial models allow the first and higher order derivatives to change at the kink point, and we test for a kink in the frequencies based on whether or not there is a statistically significant change in the linear piece of the polynomial at the kink point. Overall, the frequencies appear smooth.

[24] Note that it is possible for the aggregate effect of the controls to be smooth through the kink while the individual controls have offsetting kinks. We do not find any evidence for this possibility. Instead, we find no evidence that each control has a kink. We use the predicted enrollment measure as a convenient way to summarize this overall finding.

[25] Because of the discrepancy between tax years and academic years, some households may have had higher incomes prior to the tax year of the high school senior year, and this income may have been during the academic year that corresponds to the EFC. As a result of the higher income, these households will have positive EFCs. This applies to only the top EFC percentiles around EITC Kink 1; more than 90 percent of the EITC Kink 1 sample has an EFC of 0.

indicate that the variation in cash-on-hand from changes in senior year tax refunds around EITC Kink 1 and EITC Kink 3 does not affect federal student aid eligibility.

D. *Sensitivity Analysis*

We present multiple pieces of further analysis to understand how sensitive the baseline RK estimates are to key modeling assumptions and sample restrictions. This further analysis helps to assess the credibility of the baseline results from the previous section.[26]

Nonlinearity in the Running Variable.—For each kink point, the baseline RK estimates are based on a regression specification with linear controls for the running variable, earnings relative to the kink point, on either side of the kink point. We have examined graphical and regression evidence to assess whether this baseline specification with linear controls is reasonable. Specifically, for each kink point, we estimate regressions of the following form for the first-stage and reduced-form (the IV specification is analogous):

$$enroll_i = \alpha g(kinkdist_i) + \delta^{enroll} D_i \times g(kinkdist_i) + \alpha_2 X_i + u_i,$$

$$refund_i = \gamma g(kinkdist_i) + \delta^{refund} D_i \times g(kinkdist_i) + \gamma_2 X_i + v_i,$$

where $g(\cdot)$ denotes a linear function (baseline), a quadratic function, or a cubic function. Online Appendix Table 3 presents the results for each kink point. Focusing first on the results for EITC Kink 1, the first-stage point estimates for the slope change coefficients appear highly stable across the alternative specifications. However, we note that the standard error of the point estimate increases from 0.003 to 0.011 and then to 0.02 as we move from the linear specification to the quadratic and then cubic specifications, even though the coefficients on the quadratic and cubic polynomial terms are statistically insignificant and there is relatively low variation in the outcome variable. This highlights that, in general, distinguishing statistically between a kink versus nonlinearity in the outcome-running variable relationship will require a relatively large slope change, relatively large sample sizes, and relatively low variation in the outcome variable.

Turning to the reduced form estimates at EITC Kink 1, as we move from the linear to quadratic specification, the coefficients on the quadratic polynomial terms are statistically insignificant. The estimated slope change at the kink point remains negative (-0.66), but the standard error increases from 0.12 in the linear specification to 0.46 in the quadratic specification (a roughly similar increase as in the first stage estimates), so the estimated slope change is no longer statistically significant. Thus, in the quadratic specification, it appears that there is not a sufficiently large kink in enrollment to be able to distinguish it statistically from nonlinearity in the

[26] While we focus on results in online Appendix Tables 3 and 4. In this section, we have also verified that the results are robust to alternative sample restrictions, including additional control variables, and alternative clustering of the standard errors. These results are available in online Appendix Tables 5 and 6.

enrollment-running variable relationship. In the cubic specification, the results indicate a statistically significant change at the kink point in the quadratic term, but the other coefficients are statistically insignificant. This suggests that the specification picks up on some change in enrollment at the kink point, but we cannot distinguish a kink from nonlinearity in the enrollment-running variable relationship. Overall, we see the results as highlighting relatively large standard errors as we consider alternative specifications with nonlinearity in the running variable, and the linear specification may not be unreasonable given that coefficients on the higher order polynomial terms are almost all statistically insignificant.

The regression results for EITC Kink 3 are similar to those from EITC Kink 1. The first-stage point estimates for the slope change are relatively stable across the alternative specifications, though the standard error increases from 0.005 in the linear specification to 0.019 and 0.051 in the quadratic and cubic specifications. The slope change in enrollment is not statistically significant in any of the specifications, and the coefficients on the higher order polynomial terns are also statistically insignificant.

We present graphical evidence to further assess kinks versus nonlinearities in the outcome-running variable relationships. Specifically, online Appendix Figure 5 presents plots of the means of the outcome variables by $100 bins of earnings relative to the kink, and we include means of the fitted values from regressions with linear, quadratic, and cubic polynomials in earnings relative to the kink on both sides of the kink point. Consistent with the regression results in online Appendix Table 3, for each kink point, the first-stage plot illustrates that the fitted values do not vary much across the different specifications. Turning to the reduced form plot for EITC Kink 1, to the left of the kink, the fitted values do not appear to vary much across the specifications. To the right of the kink point, the fitted values suggest that there is a slope change at the kink point, but the cubic in the running variable may be over fitting the data so that the slope change at the kink point goes from being negative in the linear and quadratic specifications to positive in the cubic specification. Similarly, in the reduced-form plot for EITC Kink 3, we see that the higher order polynomial specifications may be over-fitting the data on either side of the kink point so that the estimated slope change at the kink point becomes more positive with higher order polynomials.[27] Thus, we proceed by using the linear specifications in the relatively narrow bandwidths around the kink points, and we examine sensitivities to alternative bandwidths.

Alternative Bandwidths.—The baseline RKD estimates are based on symmetric bandwidths of $3,000 around each kink point. This is the maximum symmetric bandwidth around EITC Kink 1 that avoids overlapping with EITC Kink 2. To assess whether these bandwidths are reasonable for each kink point, we follow Card et al. (2015) and calculate recommended or rule-of-thumb FG (Fan and Gijbels 1992) bandwidths. The results indicate FG bandwidths of 2,622.12 and 3,280.93 for EITC

[27] Gelman and Imbens (2014) caution against using higher order polynomial specifications in regression discontinuity designs and recommend instead using local linear and quadratic specifications. These intuitions may apply in the regression kink design setting as well.

Kink 1 and EITC Kink 3, respectively. Online Appendix Figure 6 presents plots of the first-stage, reduced-form, and IV estimates when we vary the bandwidths over ranges that include values above and below these bandwidths. The figures for EITC Kink 1 illustrate that the RKD estimates are relatively stable and statistically significant for bandwidths larger than \$1,900. The plots for EITC Kink 3 do not indicate any evidence of statistically significant kinks in enrollment for any alternative bandwidths.

We also explore sensitivity to excluding observations just above and below the kink points. In particular, for each kink point, we consider a donut hole analysis in which we start with the sample in the \$3,000 bandwidth around the kink point, and then we exclude observations within a window around the kink point. Online Appendix Figure 7 presents plots of the estimates when we vary the donut exclusion threshold. (The baseline estimates correspond to the donut exclusion threshold equal to zero.) For EITC Kink 1, the estimates are stable across the exclusion thresholds, and they are statistically significant for exclusion thresholds below \$900. For EITC Kink 3, the estimates do not indicate any statistically significant results across any exclusion thresholds.

Placebo Analysis.—We present placebo analyses to assess whether the estimated slope changes in tax refunds and enrollment are actually occurring at the statutory kink points, or if the estimated changes result from nonlinearities and/or kink points close to the EITC kink points.[28] In the spirit of the permutation test suggested by Ganong and Jäger (2014), we implement a placebo test to address this concern. For this analysis, we vary a placebo kink around the true kink point and verify that the largest estimated kink in tax refunds and enrollment occurs at the true EITC kink point. We choose a distance from the true kink point p and define a placebo kink point based on this distance, $pkink = EITC\ kink + p$. Using this placebo kink point, we define earnings relative to the placebo kink point, $pkinkdist_i$ and an indicator $D_i^p = \mathbf{1}(pkinkdist_i < 0)$. We then estimate the slope changes in tax refunds and enrollment at the placebo kink using the following regressions:

$$enroll_i = \alpha_1 pkinkdist_i + \alpha_2 D_i^p + \delta^{enroll,p}\left[D_i^p \times pkinkdist_i\right] + \alpha_2 X_i + u_i,$$

$$refund_i = \gamma_1 pkinkdist_i + \gamma_2 D_i^p + \delta^{refund,p}\left[D_i^p \times pkinkdist_i\right] + \gamma_2 X_i + v_i.$$

We then plot the estimated slope changes, $\delta^{enroll,p}$ and $\delta^{refund,p}$, for each of the placebo kink points and verify that the largest estimated slope changes occurs at the true kink point.

Online Appendix Figure 8 presents the estimated slope changes when varying the placebo kink points in \$100 increments around the true EITC kink points. Plots A

[28] In addition to the placebo analysis presented in this section, for EITC Kink 1, we have also examined placebo tests based on data away from the EITC kink 1 points. Specifically, we had verified that we do not find a similar or statistically significant kink in enrollment for households with two or more children around the one child EITC kink point, and similarly, we do not find a similar or statistically significant kink in enrollment for households with one child around the two children EITC kink point. This type of placebo analysis is possible since the income thresholds for EITC kink 1 vary across one-child and two-children households.

and B correspond to the first-stage and reduced-form results for EITC Kink 1, and plots C and D correspond to the same plots for EITC Kink 3. The first-stage plots highlight that the largest slope changes seem to occur at the EITC kink points. The reduced-form plot for EITC Kink 1 indicates that the largest slope change also appears to occur near the true EITC point, and the reduced form plot for EITC Kink 3 does not show any statistically significant kinks around the true kink point.

It may be concerning that the slope change in enrollment at EITC Kink 1 appears to be sustained at values just above the true kink point. However, we note that when interpreting these results, it is useful to consider the standard errors of the slope changes at the true kink point when assessing changes very near to the true kink point. For example, even in the first stage for EITC Kink 1, the plot highlights that it is necessary to move the placebo kink point by almost \$1,000 above or below the true kink point to obtain estimates that have confidence intervals that do not overlap with the confidence interval of the slope change at the true kink point. Thus, for the reduced form, it is plausible that within this range or even a slightly larger range around the true kink point, the placebo test may yield estimated slope changes in enrollment that are similar to the estimated slope change in enrollment at the true kink point. Furthermore, we note that the reduced-form estimates for EITC Kink 1 are robust to excluding observations within a \$1,000 window around the true kink point; that is, even when using data further away from the true kink point but still within the specified bandwidth, we obtain similar estimated changes in enrollment. Thus, the estimates for EITC Kink 1 appear to be occurring at the true kink point and do not appear to be rising above the true kink point.

Nonlinearity in the Enrollment-Income Relationship.—We use multiple strategies to examine whether the baseline estimates are driven by a nonlinear relationship between enrollment and earnings. However, we emphasize that the RKD estimates are based on variation in earnings relative to the EITC kink points and not just variation in earnings levels. The variation in the EITC kink points across households with different numbers of qualifying children and filing status allows us to control for nonlinear enrollment-earnings relationships that are not co-linear with the RKD running variable (earnings relative to the kink). Specifically, as shown in online Appendix Table 4, we demonstrate that the baseline results are robust to including fifth-order polynomials in earnings. (For EITC Kink 1, we include polynomials in earned income and AGI. For EITC Kink 3, earned income is not recorded for returns that do not receive EITC benefits, so we include polynomials in total income and AGI, which are both reported on IRS Form 1040. We have also verified that the results for EITC Kink 1 are robust to including polynomials in lagged income from the junior year.)

Overall, these findings indicate that the RKD estimates at EITC Kink 1 are not driven by nonlinearity in the enrollment-income relationship. We present graphical evidence in online Appendix Figure 9 to analyze this further. Using data on the population of high school seniors with family income between \$0 and \$100,000, as well as data from the analysis samples around EITC Kink 1 and EITC Kink 3, we create \$100 bins of CPI-adjusted income and compute mean enrollment in each bin. Online Appendix Figure 9 presents plots of enrollment by income bin for the different samples. The graphical evidence suggests a relatively linear relationship between enrollment and income across the different samples. Between \$0 and \$100,000, the estimated slope indicates a 0.39 percentage point increase in enrollment per \$1,000, and the estimates slopes in the EITC Kink 1 and EITC Kink 3 samples are, respectively, 0.31 and 0.48 percentage points per \$1,000. These estimates are slightly lower than the RKD estimates, and we discuss this comparison in more detail below. In regard to nonlinearity in the enrollment-income relationship, we conclude that, particularly for EITC Kink 1, the RKD estimates emerge only when earned income is re-centered relative to the tax kink point for each group.

E. *Timing of Income and Enrollment*

Given the statistical significance of the RKD estimates for EITC Kink 1, we focus on these results for further analysis on the relative importance of the timing of tax refund payments and persistence. First, in regard to the timing of tax refund payments, since the baseline results relate tax refunds in the spring of the high school senior year to college enrollment the next year, we examine whether tax refunds in the spring of the high school junior year also affect college enrollment. For this analysis, we replicate the RKD around EITC Kink 1 based on income in the high school junior year. In particular, we draw the sample of returns that are around the first EITC kink point in the high school junior year.[29] Thus, with this junior year sample, the first stage is based on the slope change in tax refunds at EITC Kink 1 in the junior year.

Table 3 presents regression kink estimates for both the junior and senior EITC Kink 1 samples.[30] In both samples, we test for kinks in tax refunds in the junior year and senior year. Thus, we verify that in the junior year EITC Kink 1 sample, we find the expected kink in tax refunds in the junior year and not in the senior year; and similarly for the senior year EITC Kink 1 sample, we find the expected kink in refunds in the senior year and not the junior year. The reduced form column highlights the main results for these samples: there does not appear to be any statistical evidence of a kink in enrollment based on junior year earnings relative to EITC Kink 1. Thus, the estimates indicate that changes in tax refunds in the high school senior year affect college enrollment, but we do not find statistically significant effects of changes in tax refunds in the high school junior year.

We next examine the persistence of the enrollment effects from tax refunds received in the spring of the high school senior year. We examine persistence by estimating the RKD specifications using two new outcome variables: (i) an indicator for having two consecutive years of enrollment and (ii) an indicator for being enrolled

[29] Similar to the analysis based on tax returns from the high school senior year, we examine evidence on the identifying assumptions for the regression kink analysis in the high school junior year, and we verify (i) that taxpayers do not sort along the tax schedule in the junior year and (ii) that there is no kink in covariate predicated enrollment. Online Appendix Figure 10 presents graphical evidence on the first stage, reduced form, frequencies, and covariate-predicted enrollment for the junior EITC Kink 1 sample.

[30] The senior year EITC Kink 1 sample used in Table 3 is not identical to the baseline EITC Kink 1 sample because it only includes cohorts 2002–2011, whereas the baseline sample includes the 2001 cohort. Because some data for parent taxpayers is only available from 2001 onward, we do not observe junior year earned income for the 2001 cohort.

Table 3—EITC Kink 1, RKD Estimates for Junior and Senior Samples

	First stage dep. var. = junior refund	Dep. var. = senior refund	Reduced form dep. var. = enrollment	IV dep. var. = enrollment
Panel A. Junior year K1 sample				
Earnings relative to kink	0.454	0.165	0.484	0.130
(*kinkdist*)	[0.00161]	[0.00330]	[0.0814]	[0.119]
Slope change at kink	−0.325	−0.0560	−0.254	
($D \times kinkdist$)	[0.00291]	[0.00573]	[0.136]	
Effect of $1,000 on enrollment				0.780
(IV)				[0.415]
Observations	875,108	875,108	875,108	875,108
	Dep. var. = junior refund	First stage dep. var. = senior refund	Reduced form dep. var. = enrollment	IV dep. var. = enrollment
Panel B. Senior year K1 sample				
Earnings relative to kink	0.139	0.439	0.538	0.00271
(*kinkdist*)	[0.00442]	[0.00185]	[0.0669]	[0.112]
Slope change at kink	−0.0250	−0.335	−0.408	
($D \times kinkdist$)	[0.00712]	[0.00331]	[0.130]	
Effect of $1,000 on enrollment				1.219
(IV)				[0.384]
Observations	885,070	885,070	885,070	885,070

Notes: Each regression includes dummy variables for senior year, number of children, and filing status. Standard errors are clustered based on $100 bins of earnings relative to the kink point and listed in brackets. The samples are based on $3,000 bandwidths around EITC Kink 1, and senior year cohorts 2002–2011.

two years after the high school senior year. The first measure relates to persistence in terms of continued enrollment. If individuals who are induced into college enrollment because of additional cash-on-hand do not continue in college, we may not see effects of tax refunds on consecutive enrollment. The second measure related to persistence in terms of intertemporal substitution. If individuals who do not enroll after the high school senior year because of limited cash-on-hand end up enrolling a year later, then we may not expect to see any effects of senior year tax refunds on enrollment two years of the high school senior year. Unfortunately, our estimates of longer run enrollment effects are only suggestive of continued enrollment effects beyond the two years after the high-school senior year. However, recent work that analyzes the same administrative data from IRS and the Department of Education used in this study (Chetty et al. 2017) shows that low-income students who enroll in college realize meaningful earnings effects later in life.[31] This suggests that the college entry margin we analyze in our baseline results is correlated with important later life outcomes such as labor market success.

[31] Specifically, this paper analyzes the 1980–1993 birth cohorts and shows that among 18–22-year-olds, low-income students perform nearly as well as high-income students in the labor market later in life (as measured by average earnings at ages 32–34) within each school. This within school pattern contrasts sharply with the general pattern, where children from low-income families are much more likely to be low-income as adults, relative to children from high-income families. One key reason for this pattern is the strong relationship between college enrollment and family income.

Table 4—Persistence

	Dep. var. = consecutive enrollment	Dep. var. = enrollment 2 years after senior year
Mean of dependent variable	24.84	34.74
Earnings relative to kink (*kinkdist*)	−0.0806	0.0212
	[0.110]	[0.135]
Effect of $1,000 on enrollment	1.372	1.288
	[0.365]	[0.432]
Observations	913,712	913,712

Notes: Each column reports RKD (IV) estimates around EITC Kink 1 with the dependent variables listed in the column headings. The sample size is restricted to high school senior cohorts 2001–2010 so that at least two years of potential enrollment could be observed for all individuals in the sample. Standard errors are clustered based on $100 bins of earnings relative to the kink point and listed in brackets.

Table 4 presents the RKD estimates using these two outcomes. The results for both outcomes are similar to the baseline EITC Kink 1 RKD estimates. These results indicate that individuals who are induced into college enrollment because of additional cash-on-hand appear to sustain their college enrollment over consecutive years, and individuals who do not appear to enroll after the high school senior year because of limited cash-on-hand do not appear differentially more likely to enroll later. Thus, the effects of cash-on-hand from tax refunds in the spring of the high school senior year appear persistent along both dimensions.[32]

III. Discussion

A natural starting point to interpret the magnitudes of the RKD estimates may be the cross-sectional (OLS) relationship between family income and college enrollment. Online Appendix Figure 9 illustrates this cross-sectional relationship broadly for households with income in the senior year between $0 and $100,000, and more specifically for the EITC Kink 1 and EITC Kink 3 analysis samples as well. From these samples, we estimate that a $1,000 increase in family income is associated with roughly a 0.30 to 0.50 percentage point increase in enrollment. These estimates are within the confidence intervals of the RKD estimates, but they are smaller than the RKD point estimates.

Nonetheless, while they may seem like a natural benchmark, it has been widely recognized that OLS estimates may not reflect causal effects of family income on college enrollment. Unobserved abilities or preferences for education may be

[32] We have also examined heterogeneity in the EITC Kink 1 estimates based on geography and prior income. When examining effects across counties with higher and lower enrollment rates, we find that the IV point estimates are all positive and of similar magnitudes, and that the standard errors are sufficiently large that we cannot rule out that the estimates are equal across groups of counties with higher and lower enrollment rates. We conclude that cash-on-hand effects could be similar for students from low-income families in higher and lower enrollment counties. When examining effects across groups with higher and lower average AGI over the four years prior to the high school senior year, we find that the effects of cash-on-hand appear noisiest for the highest permanent income group. Thus, the cash-on-hand effects appear to be driven by groups with lower permanent income. These results are available in online Appendix Tables 7 and 8.

positively correlated with family income. As a result, the OLS estimates may reflect correlations between family income and these unobservables in addition to causal effects, so the OLS estimates would be larger than actual causal effects of family income on enrollment. A large body of prior research has implemented a variety of strategies to improve upon the OLS estimates and estimate causal effects of income on college enrollment.[33] In particular, similar to this study, two recent studies have developed quasi-experimental research designs and use administrative tax data for their empirical analyses. Bulman et al. (2016) exploit exogenous variation in the timing and size of lottery winnings to estimate the effects of additional income from winnings on enrollment. Hilger (2016) exploits exogenous variation in the timing of job loss to estimate the effects of reductions in income from job displacement on enrollment. Consistent with theories of positive correlations between family income and unobserved abilities, these two studies estimate smaller causal effects of family income on college enrollment than the OLS estimates. Thus, these estimates are smaller than the RKD point estimates as well, though the estimates are within the confidence intervals of the RKD estimates.

Given these earlier quasi-experimental estimates, it is useful to consider why the RKD estimates could exceed other quasi-experimental estimates. First, we note that the lack of effects around EITC Kink 1 in the junior year is consistent with earlier quasi-experimental evidence of relatively small income effects. However, the difference between the junior year and senior year results around EITC Kink 1 is suggestive that the effect of income in the senior year is different from the effect of income in the junior year. More specifically, we note that the senior year RKD estimates may reflect households' excess sensitivity to small anticipated income changes since these estimates are driven by variation in the amount of cash-on-hand in the spring of the high school senior year. In a standard lifecycle model without credit constraints, the permanent income hypothesis predicts that anticipated income changes, such as expected tax refunds, should have zero or minimal impacts on college enrollment decisions, so one would expect the RKD estimates to be close to zero and possibly smaller than other quasi-experimental estimates based on large unanticipated changes in income. However, in a setting with credit constraints, households may display excess sensitivity to small anticipated income changes at the time of college enrollment. Intuitively, if households are unable to borrow money, increases in cash-on-hand would allow them to overcome upfront, out-of-pocket costs associated with college enrollment. In this case, the RKD estimates could exceed earlier quasi-experimental estimates. Furthermore, in the junior year, low-income households may be myopic and not anticipate financial needs for college decisions in the senior year, so they may not save cash-on-hand from the

[33] Earlier quasi-experimental studies have exploited a variety of sources of variation in family income for identification. Examples of sources of variation include oil booms (Løken 2010), casino winnings (Akee et al. 2010), home price fluctuations (Lovenheim 2011), and job loss (Coelli 2011 and Pan and Ost 2014). Bastian and Michelmore (2015) exploit variation in EITC benefits from federal and state EITC expansions from the 1970s onward. Consistent with the current RKD estimates, they find that an additional $1,000 of EITC exposure between ages 13 and 18 increases the likelihood of high school completion (1.3 percent) and college completion (4.2 percent). While the RKD estimates may represent cash-on-hand effects from variation in tax refunds at the time of enrollment decisions, these estimates could be driven by similar effects plus additional effects from impacts of EITC benefits on maternal labor supply.

junior year to be used in the senior year. Thus, a theory involving credit constraints and some myopia could explain the effects we see in the senior year and the lack of clear evidence of similar effects in the junior year. However, we note that our estimates for the junior year are statistically imprecise, so we cannot definitely say that there are no effects of cash-on-hand in the junior year on college enrollment, or that the junior year effects are smaller than the senior year effects.

We present multiple pieces of evidence suggesting that credits constraints and excess sensitivity to cash-on-hand could explain why the RKD estimates at EITC Kink 1 exceed earlier quasi-experimental estimates. First, the current evidence based on excess sensitivity to tax refunds in the spring of the high school senior year is consistent with excess sensitivity to changes in transitory income in other settings with possible credit constraints. For example, Chetty (2008); Card, Chetty, and Weber (2007); and LaLumia (2013) find excess sensitivity to lump-sum cash payments during unemployment spells. Gross, Notowidigdo, and Wang (2014) find excess sensitivity to tax rebates in bankruptcy decisions; tax rebates appear to help possibly liquidity constrained households overcome upfront costs associated with filing for bankruptcy. Souleles (1999); Shapiro and Slemrod (2003); Johnson, Parker, and Souleles (2006); and Agarwal, Liu, and Souleles (2007) present evidence of excess sensitivity of consumption to income tax rebates and refunds. Hsieh (2003) finds no evidence of excess sensitivity of consumption to large anticipated payments from the Alaska Permanent Fund, but does find evidence of excess sensitivity to smaller anticipated tax refunds. Stephens (2003) finds excess sensitivity of consumption to the arrival of social security benefit payments. In addition, Gallagher and Muehlegger (2011) present related evidence based on excess sensitivity to waivers of upfront costs for hybrid vehicle purchases. Specifically, conditional on the value of the tax incentive, Gallagher and Muehlegger find that waivers for upfront sales tax costs lead to significantly larger increases in hybrid vehicle purchases than income tax credits. Each of these studies highlights evidence that individuals' decisions can be sensitive to transitory income in a variety of settings, so it is plausible that students from low-income households (around EITC Kink 1) may also display excess sensitivity to tax refunds in the college enrollment setting as well.

A second piece of evidence on possible credit constraints, particularly for students from low-income households, relates to information constraints and incomplete take-up of financial aid. While some students may be eligible for aid, if they are unaware of the aid or do not have the necessary information to complete the necessary forms, information constraints may leave students essentially credit constrained. Online Appendix Figure 11 presents evidence on the receipt of financial aid for enrolled students from households with incomes between \$0 and \$100,000.[34] The households around EITC Kink 1 are generally in the \$10,000 to \$15,000 income range, and while enrolled students from these households are almost all eligible for student aid, the evidence in this plot indicates that only 85 percent of the enrolled students from these households actually receive aid. Survey evidence from the

[34] We have verified that this evidence on student aid take-up from administrative tax and student aid data is consistent with survey evidence from the National Postsecondary Student Aid Study (NPSAS).

National Postsecondary Student Aid Study (NPSAS) on the reasons for not claiming aid indicates possible information constraints since many low-income students report thinking that they are ineligible for aid (see online Appendix Table 9).

As a last piece of evidence related to possible credit constraints for low-income households around EITC Kink 1, we note that tax refunds for these households are large enough to cover large portions, if not all, of the costs of college enrollment. Online Appendix Table 10 presents evidence on the costs of enrollment at different types of institutions, and the summary statistics in Table 1 indicate that households around EITC Kink 1 receive refunds of roughly \$5,300 on average. Moreover, tax refunds arrive in a lump sum at a time when many youths are making their enrollment decisions.

We note that the RKD estimates may also apply locally around each respective kink point, so excess sensitivity to predictable income changes could vary at different points in the income distribution across different populations. For example, the differences in the RKD estimates across EITC Kink 1 and EITC Kink 3 could indicate that the effects of cash-on-hand may only exist for lower income households that could be more likely to liquidity constrained at the time of college enrollment than higher income households.[35]

While comparisons to the prior literature on the impacts of family income on college enrollment may make the RKD estimates around EITC Kink 1 seem large, comparisons to the prior literature on the impacts of student aid on college enrollment may make the RKD estimates seem relatively small. Specifically, Dynarski and Scott-Clayton (2013) survey multiple studies and indicate that a \$1,000 increase in student aid increases college enrollment by roughly 4 percentage points. However, these student aid effects reflect price (or incentive) effects rather than income effects. Intuitively, student aid policy changes may make college enrollment particularly salient to students (or more so than tax refunds that do not arrive with any information about college enrollment), or students may be more responsive to enrollment incentives than liquidity. We also note that the RKD estimates are based on tax refunds that are direct, non-contingent income transfer to households. In particular, these income transfers are not earmarked for college. If households around EITC Kink 1 spend 25 percent of the tax refund on college enrollment costs,[36] then the estimated impacts of a \$1,000 reduction in college costs is four times larger than the impacts of a \$1,000 increase in income ($1.30 \times 4 = 5.2$ percentage points), and this would be comparable to estimates from the student aid literature.

[35] Aside from excess sensitivity to tax refunds, another reason why the RKD estimates may differ from earlier quasi-experimental estimates may be that the marginal propensity to consume education could vary by the source of additional income. For example, prior evidence on the uses of lottery winnings has highlighted purchases of new cars (Imbens, Rubin, and Sacerdote 2001) whereas prior evidence on the uses of tax refunds for low-income households has highlighted debt reduction, purchases of non-luxury durable goods, and educational expenses (Smeeding, Phillips, and O'Connor 2000).

[36] Households could spend the remaining income on other consumption. We have not found any evidence of a kink in earnings or labor force participation around EITC Kink 1 or EITC Kink 3, so it does not appear that these households consume more leisure because of additional cash-on-hand.

IV. Conclusions

We find evidence of meaningful effects of tax refunds received in the spring of the high school senior year on college enrollment decisions of students from low-income families. Regression results indicate that, for households around the first EITC kink point, an additional $1,000 of after-tax income in the spring of the high school senior year increases college enrollment by roughly 1.30 percentage points. We do not find evidence that cash-on-hand affects enrollment for students from households around the third EITC kink point which occurs at a higher income level. These results indicate that, along with traditional enrollment-contingent financial aid, policies that provide students from low-income households with liquidity to cover out-of-pocket costs may be effective at increasing college enrollment. More generally, studying the role of the federal income tax code in affecting higher education outcomes remains an interesting avenue for future work.

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