Designed to Fail: Effects of the Default Option
and Information Complexity on Student Loan Repayment*

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Abstract: Within five years of leaving school, 25 percent of student loan borrowers default on required minimum payments. These defaults are costly: they add to interest and penalties on loans and lower credit scores, which limits access to future credit and can adversely affect job prospects. We ask why so few student loan borrowers enroll in Income Driven Repayment (IDR) plans, which insure against default caused by low earnings. To do so we run an incentivized laboratory experiment using a facsimile of the government's Student Loan Exit Counseling website. We test the roles information complexity, uncertainty about earnings, and the default option play. We find that switching the default option from the Standard plan with fixed minimum payments to an IDR with income-contingent minimum payments, and providing good information about earnings, can dramatically decrease choice of the risky Standard plan.

Keywords: Student Loans; Default Option; Income Driven Repayment; Experiment.
JEL: I22; I28; C91.

Forthcoming in Journal of Public Economics

1 Introduction

As of 2014, one-quarter of borrowers entering repayment in 2005, 2007 or 2009 had defaulted on their student loans. To put that in perspective, within five years of exiting school roughly 25 percent of borrowers from the most recent (2009) cohort had already gone through deferment, forbearance, and delinquency into default. This need not be the case. Borrowers with Direct Loans have the option to choose a repayment plan that adjusts with their earnings. Under these income driven repayment plans (IDRs), repayments are a percentage (typically 10-15 percent) of discretionary income above a threshold (usually 150 percent of the poverty line) with an added benefit of forgiveness, often after 25 years. Thus, if borrowers experience job loss or low income, small or nonexistent required repayments reflect this. In this sense IDR plans work as a form of low-cost *insurance* against default resulting from a borrower's inability to pay due to low earnings.

Yet, surprisingly few students choose one of these plans. According to the U.S. Government Accountability Office (GAO)[1] fewer than one-quarter of borrowers with Direct Loans are enrolled in some form of income driven repayment plan, with the vast majority enrolled in the Standard 10-year fixed repayment plan – the default option. The GAO further notes that in a 2012 study the U.S. Department of the Treasury found that 70 percent of defaulted borrowers met the criteria for enrolling in an IDR plan, which would have protected them against default. The default rate among those who did enroll in IDR, Treasury found, was less than one percent. Defaults that do occur, for whatever reason, are costly. For borrowers, a default adds to loan interest and penalties, affects credit scores which limits access to future credit, and can adversely affect job prospects, potentially leading to further debt at higher interest rates. For taxpayers, these defaults increase loan-servicing costs and directly detract from a taxpayer-subsidized program's solvency.

Since borrowers can pre-pay at any time under these plans, the cost of taking up this insurance by enrolling in an IDR is extremely low compared to the default risk borrowers are exposed to under the Standard plan. Why, then, are so few borrowers enrolling in an IDR, and why are so many in the most restrictive, Standard plan? Prior work in other contexts suggests that the way in which choices are presented can have meaningful effects on behavior (e.g., Chetty et al., 2014; Madrian and Shea, 2001; Thaler and Benartzi, 2004; Bhargava et al., 2017). We apply this line of thinking to the context of student loans, testing several hypotheses. First, we ask whether it is simply the power of the default option. All borrowers are automatically placed into the Standard plan and remain in that plan unless they take active steps to change.

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[1] GAO (2015, 2016).

A wealth of research on other topics suggests that the effects of default options are strong.[2] Second, we ask whether and to what degree borrowers' choices might also be driven by inaccurate information about their future earnings. Other studies suggest that students are overly optimistic about their employment and earnings prospects (Wiswal and Zafar, 2015; Betts, 1996), potentially leading them to undervalue the insurance against low earnings or unemployment provided by IDR plans. Third, we ask what role information complexity plays in suboptimal decision-making. At present borrowers are faced with no fewer than seven repayment options, each with its own complicated set of rules. Moreover, the Department of Education and allied non-profit student loan servicers present borrowers with an overwhelming amount of information, potentially obfuscating important protections afforded by IDR or other plans.

To test these hypotheses we run an incentivized laboratory experiment using a facsimile of the federal government's Student Loan Exit Counseling web interface. The government's Student Loan Exit Counseling is required of all students with federal loans upon exiting schooling or falling below half-time status and is their first point of contact for selecting a repayment plan. It provides student borrowers with information about their current loans, information about repayments and repayment options, and affords them an opportunity to select a repayment plan. Recreating this environment in the lab allows us to study the effect of varying program features, such as the default repayment plan option and the way information is presented, in a manner that will allow us to interpret results for the current policy environment. The experiment is run with a diverse and representative sample of 542 Georgia State University undergraduates who persisted through their first year of college and are eligible for federal student loans.

We incentivize our experiment by telling subjects they will receive a draw from a distribution of what recent college graduates might expect to earn over each of the next 25 years, and that after subtracting necessary living expenses and any payments due on their student loan in each period, what is left over is converted with a known exchange rate to U.S. dollars, meaning they keep some fixed percentage of their net earnings in the experiment. Subjects thus face a choice between repayment plans that require inflexible and possibly lower-cost repayment, and IDRs that provide protection against default and allow for a longer repayment horizon, potentially at the cost of increased interest capitalization. Importantly, if in any period earnings net of living expenses are less than what they are required to repay on their student loan according to their repayment plan, they will go into default, which both reduces their future earnings in the experiment and triggers an additional fixed monetary penalty.

As in current Department of Education policy, the Standard plan is the pre-selected default option in our baseline treatment. We then ask whether simply changing the default option can lead students to

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[2] For examples, see healthcare (Bhargava et al., 2017; Winter et al., 2006), retirement savings (Chetty et al., 2014; Madrian and Shea, 2001; Thaler and Benartzi, 2004), student aid (Bettinger et al., 2009; Castleman and Page, 2016), and organ donation (Johnson and Goldstein, 2003).

choose an IDR plan. We choose the REPAYE plan (a revised version of Pay As You Earn) as our alternative default repayment plan because it is the government's current and most highly publicized income driven repayment option, and hence the most policy-relevant candidate. In addition to changing the default option from Standard to REPAYE, we also include treatments that give students straightforward information about the distribution of earnings of recent college graduates, and treatments that reduce the complexity of information found on the Exit Counseling site by presenting subjects with a pairwise choice between Standard and REPAYE along with a plainspoken description of each plan and its benefits and drawbacks.

We find that the default option has a powerful effect. While about 63 percent of borrowers in our baseline treatment choose the Standard plan, nearly identical to the actual proportion among current borrowers, only 34 percent choose it when the default option is REPAYE. This suggests that the government has a policy lever if it wants to increase initial uptake of income driven repayment plans potentially reducing defaults caused by low earnings.

We contrast this with our second finding. Providing subjects with clear and concise information about the distribution of earnings among recent college graduates has a smaller effect on decision-making. Although this causes them to update reported beliefs about expected earnings, we observe only an 8-10 percentage point decrease in take-up of the Standard plan, and estimates are not statistically different from zero. On the other hand, the combination of providing earnings information *and* switching the default option to an income driven plan has the largest effect, reducing choice of the Standard plan to 16 percent from 63 percent in the baseline.[3] Further, we find that simplifying language and reducing complexity and the number of available choices has only a small effect, if any, on take-up of the standard plan.

Taken together this evidence provides insights both for behavioral economists and policy-makers alike. For the former, while the importance of the default option is documented in other contexts, we apply it to a new, policy-relevant environment uncovering novel subtleties in the choice of student loan repayment plans. Likewise, for policy-makers we demonstrate that the default option plays an outsized role in borrowers' choice of repayment plan, driving them in many cases to a suboptimal choice. Yet, many borrowers choose the Standard plan even when the default option is changed to an IDR option. Thus, while changing the default option may be in aggregate welfare enhancing, the optimal strategy may be to follow the policy lead in Australia, New Zealand, the UK, Canada, South Africa and others, and do away with the menu of choices, leaving IDR as the only option. This is especially appealing given that prepayment is always feasible and, hence, the increased interest cost of repayment deferral under IDR can be avoided by borrowers with the ability to make payments at or above those required under the Standard plan.

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[3] While this is a 60 percent larger reduction than changing the default alone, differences across the two treatments are marginally insignificant (p=.11).

2 Background

The U.S. Department of Education is the largest originator of student loans in the country. The majority of loans are distributed to students through the Direct Loan Program, which makes both unsubsidized and subsidized loans, the latter to students with demonstrated financial need.[4] The Department of Education provides the loans and maintains an informational website. The servicing of loans, including communicating with and counseling borrowers on plans and repayment options, is contracted out to four Title IV or six non-profit servicers.

Historically, take-up of income driven repayment plans (IDRs) has been low despite efforts by the Department of Education to promote them. These plans tie repayments to earnings, for the most part taking 10 -15 percent of adjusted gross income above 150 percent of the poverty line with forgiveness after 20-25 years. Most borrowers are eligible for these plans, in particular in the first years after exiting school, when earnings are low.[5]

It is unclear why take-up is so low. Two recent working papers by Abraham, et al. are complementary to our own in exploring this phenomenon. Abraham, et al. (2018b) explore the importance of the framing of IDRs versus fixed plans coupled with students' earnings expectations. The authors survey undergraduates and present respondents with scenarios in which they are assigned some level of borrowing and a set of choices between a fixed repayment plan and one that adjusts to income, with various framings. Students' responses suggest they would be more likely to choose a generic IDR plan when framing focuses on the insurance aspects, suggesting that the government's current framing, which emphasizes the potential for a longer repayment period and higher total payments is in part acting as a deterrent. Moreover, they find framing effects are larger for students expecting a higher probability of low or no earnings after graduation but, conditional on this probability, expected earnings do not significantly interact with framing.

In a companion paper (Abraham, et al. 2018a) the authors focus on the potential effects of IDR on career decisions in a laboratory experiment. While linking payments to earnings reduces returns to a high paying career, protection against default may allow students to pursue high value opportunities that carry more risk. They find that offering only IDR leads to the best outcomes – removing the fixed option makes risky and potentially lucrative careers more appealing, raising incomes. They also find offering only a subset of borrowers both options, as would be the case during a transition to an IDR-only policy, is not harmful. Taken together, results argue against the menu driven set of options currently presented in the existing policy framework.

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[4] Interest does not accrue on the subsidized version while borrowers are in school, during a 6-month post-schooling grace period, and during periods of deferment (for continued schooling or economic hardship). Borrowers also have the option to consolidate Direct Loans; consolidation affects their terms and repayment.
[5] Hershbein, et al. (2014) estimate that up to 99 percent of recent borrowers would have been eligible in their first year.

Borrowers' decisions about student loan repayments can have lasting consequences. There are no clear criteria for discharging student loan debt through bankruptcy.[6] Any unpaid student debt is discharged upon death or, in rare cases, permanent disability.[7] Negative credit reports or low credit scores from late payments or default on student loans can reduce access to credit or increase its cost. Gaulke and Reynolds (2018) show that just entering delinquency on student loan repayments leads to nearly a 50 point hit to credit scores.[8] It can also affect access to housing, and can reduce employment opportunities through employers' screening of credit histories.[9] Research has shown the effect of student loan debt on everything from occupational choice (Rothstein and Rouse, 2011) and small business formation (Ambrose, et al., 2015) to home ownership (Mezza, et al., 2016).

The benefits of IDRs are not limited to protection against default. They also afford flexibility and lower the burden of repayments for those with low incomes. Mueller and Yannelis (2018) compare borrowers who were and were not eligible for Income Based Repayment (IBR, an older IDR plan) after its introduction in the wake of the Great Recession.[10] They find enrolling in IBR reduced student loan defaults as well as the sensitivity of those defaults to home price fluctuations.[11] Similarly, Herbst (2018) estimates the effect of enrolling in IDR on a variety of outcomes. Using phone calls encouraging borrowers who are 10 or more days delinquent to sign up for an IDR plan, the author makes pre- versus post-intervention comparisons between those who sign up for IDR and those who do not. He finds IDR enrollees are less likely, by 21 percentage points, to fall 10 or more days delinquent. Enrollees also had higher credit scores and were more likely to hold a mortgage two years after. Similarly, IDR enrollees also paid down more of their debt each month, despite lower required average payments. Importantly, the author found that once borrowers were enrolled in IDR, many failed to recertify. This is in line with other work by Ericson (2016), which focuses on dynamic defaults (where consumers must recertify their choices over time), though primarily in a healthcare setting.

In a working paper, Muller and Yannelis (2019) show further evidence from a field trial conducted by student loan servicer Navient. In the experiment, Navient pre-populated IDR enrollment applications for

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[6] Legal decisions on this rule are in debate, and there is some evidence now of student loan cancelation in bankruptcy. See Iuliano (2012) for more.

[7] A recent paper by Yannelis (2017) suggests that reintroducing bankruptcy protection or eliminating wage garnishment would substantially increase default rates through "strategic" default by borrowers.

[8] Other recent papers attempt estimate the effect of flags on credit histories, liquidity and employment. See Bos, et al. (2018), Herkenhoff et al. (2016), and Dobbie et al. (2016).

[9] A Society for Human Resource Management survey found that up to 60 percent of employers conducted background checks of at least some of their employees, usually for jobs involving financial responsibility. See also National Public Radio (2012) and New York Times (2013).

[10] The study focused on Income Based Repayment because it was the leading non-fixed payment plan at the time.

[11] See Ionescu (2009) and Gary-Bobo and Trannoy (2015) for more theoretical work on student loan repayment schemes.

a randomly selected group of borrowers. The results were 34 percentage point increase in IDR enrollment, drop in monthly loan payments of over $350, and a seven percentage point decrease in new delinquencies.

2.1 The power of the default option

There is ample evidence that the default option – the pre-selected option that individuals must take active steps to override – can have substantive impacts on important decisions.[12] The student loan case fits this pattern precisely. First, students are pre-selected into the Standard 10-year fixed repayment plan. Second, the name of the plan itself, "Standard," may suggest that this is the recommended option, a hypothesis we test among a subset of our subjects. Third, the cost of switching out of the Standard plan is high: borrowers must contact their servicer, fill out a series of forms, and show proof that they are not able to make minimum payments in order to switch to an income-driven plan. A 2015 GAO report addressing low IDR uptake noted that while the Department of Education maintains information on IDR programs on its website, it has not established requirements for servicers to communicate clear information to borrowers about these plans. Their review of communications by servicers found "inconsistency" and lack of specificity in the information provided to borrowers. Borrowers were forced to seek out this information themselves, ostensibly from the government's student loan site (which we utilize in our experiment). The GAO concluded that in many cases borrowers are in fact unaware of these plans. A newer report from the Office of the Inspector General (2019) confirms these and other instances of non-compliance on the part of servicers.

We test the importance of the default option with treatments in which it is changed from Standard to REPAYE. This is done in both the complex information environment of the Department of Education website and in a simplified alternative environment.

2.2 The relevance of information about earnings

Standard human capital models of schooling often assume students have reasonably good, if not perfect, foresight into their future earnings. Thus, given perfect credit markets young adults can make optimizing decisions about college-going. Yet, empirical work (e.g. Betts, 1996) demonstrates that students' prior beliefs are not very accurate and that updating their beliefs with new information can have profound impacts on educational investments (including borrowing). For example, Wiswal and Zafar (2015) show that students revise their beliefs about future earnings when shown the population distribution they can expect to face. Current student loan exit counseling does not provide borrowers with information as to what recent

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[12] Johnson and Goldstein (2003) apply this to organ donation. Madrian and Shea (2001) apply this to 401k retirement plans. Chetty et al. (2014) show similar results from retirement savings policy changes.

graduates might expect to earn. If borrowers are overconfident, they may be less likely to take IDR plans that offer insurance against default caused by low earnings.

We test the importance of earnings information by including treatment cells where students are presented with simple graphical and text information of what recent college graduates earn in their first few years after college, including the share that earn less than what it would take to make the minimum payment on a $23,000 loan under the 10-year fixed repayment plan, which is what subjects face in the Standard plan in our experiment.

2.3 The role of complexity

While classical economics suggests that having more feasible options is better, the behavioral literature finds that increasing the number of choices and complexity of the decision environment can lead to sub-optimal decision-making, often referred to as "choice overload" or "comparison friction". Behavioral economists have reported that as the number of choice options increases (overload), and the availability of information becomes more difficult to access (friction), reliance on the default option increases.[13] This mechanism may be at work as students choose among loan repayment options. There are multiple options to choose from, including Standard, Graduated, Extended, Graduated-Extended, Income Based Repayment (IBR), Income Contingent Repayment (ICR), Pay as You Earn (PAYE) and Revised Pay As You Earn (REPAYE) plans. The full list and details are shown in Appendix 6.

Further, the precise details of each program, including eligibility, are difficult to access and comprehend. Students are making these decisions with a great deal of uncertainty about how interest compounds, the insurance nature of the offer, eligibility for each program, and about their future earnings. Some evidence suggests that students even have a poor grasp of the amount of their own borrowing (Akers and Chingos, 2014).[14] We ask whether this type of complexity reinforces the default option by including treatments where subjects are presented with only two choices (REPAYE and Standard) and are given a more straightforward description of each plan's costs and benefits, including plain-language wording about fixed versus income-driven repayments and clear language about the insurance aspect of REPAYE.[15]

3 The Experimental Environment

3.1 Student Loan Counseling

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[13] Cronqvist and Thaler (2004) show this with respect to Social Security options in Sweden. King, et al. (2012) report implications of choice friction in a field experiment on choice of Medicare Part D prescription drug plans in the U.S.

[14] Other recent work focused on healthcare but drawing on other examples as well (Ericson and Syndor, 2017) demonstrates that consumers' confusion may lead policymakers to incorrectly infer their preferences.

[15] We choose REPAYE as our alternative default option as it is the government's most recent IDR plan and the most likely candidate for an IDR default option.

For our experiment we create a facsimile of the federal student loan Exit Counseling site, which all students with federal loans are required to use upon exiting school or falling below half time, and which serves as their point of entry to loan repayment.[16] The site allows borrowers to look up their current loans and choose a repayment plan as well as obtain information about repayment plans, how interest accrues, the nature of deferment, forbearance, delinquency and default, and tips for repaying.

The site, StudentLoans.gov,[17] consists of multiple pages that are clicked through sequentially. First, borrowers log in by entering their Federal Student Aid identification number to look up their existing loans or enter their loan information manually. Subjects using our experimental site have a loan amount and interest rate pre-entered for them. Once this information is populated, borrowers using the government site or subjects using our laboratory facsimile click through the site's pages. The first page ("Understand Your Loans") presents information on loan basics, including loan types, and allows students to click on information describing key concepts such as interest accrual, capitalization, and acceleration. The second page ("Plan To Repay") allows borrowers to enter their projected annual income, to select any withholding, and to enter other monthly expenses such as rent, utilities and entertainment. We pre-select our subjects to have a family size of one and to live in Georgia (for purposes of calculating income tax). We enter monthly expenses to be 150 percent of the federal poverty line; the purpose of this is to match what we deduct from their gross earnings in our experiment as living expenses, which we tell them up front.[18]

Once projected earnings are entered, repayment plans appear along with descriptions and radio buttons to make a selection. In Appendix 2 we show a screen shot of this portion of the page with the loan parameters we use in the experiment (a $23,000 Direct loan) and enter $24,000 in expected earnings in the next year to provide an example for the reader.[19,20] It is here where borrowers or subjects can choose a repayment option by clicking on the radio button of the plan they want. If no action is taken, they remain in the pre-selected default plan. At the bottom of the second page there is a host of additional information including repayment incentives and tips on navigating repayment.[21] The third through fifth pages present no new choices, but do provide additional, possibly overwhelming, information on avoiding default (third page, "Avoid Default"), making finances a priority (fourth page, "Finances: A Priority"), and finally a

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[16] We make only minor changes to the site for the experiment: we do not allow subjects to enter personal information such as social security numbers and we pre-fill their state of residence as Georgia.

[17] The URL for the site is https://studentloans.gov/myDirectLoan/exitCounseling.action?execution=e1s1.

[18] In the experiment all earnings, repayments and poverty calculations are based on a single individual to abstract from dual income and dual loan issues within households.

[19] Borrowers and subjects can sequentially enter as many earnings values as they like. In the case shown in Appendix 2, which is relevant to our experiment, borrowers with loans below $30,000 are not eligible for Extended repayment plans.

[20] The government's web page extrapolates earnings at a 5 percent growth rate, which borrowers cannot change. The GAO has questioned this calculation and suggested that the Department of Education modify it to be more realistic.

[21] Note that some changes in details have taken place on StudentLoans.gov since our 2017 experiment and visiting the site now may present somewhat different information. We have preserved and provide links to our experimental facsimile. The subject instructions for treatments in our experiment are available at https://excen.gsu.edu/jccox/instructions .

summary page (fifth page, "Repayment Information") where they can enter contact information to facilitate contact with their servicer. In the experimental facsimile, we replace the fifth page with a summary page of their decision and do not allow them to enter personal information such as a phone number or address.

3.2 Incentivizing the experiment

We set up the experiment by telling subjects that they have a Direct Subsidized federal student loan of $23,000 at 4.6 percent interest – the maximum subsidized student loan amount at the time – and pre-enter this information for them.[22] In our instructions, subjects are told that they will be asked to go through student loan exit counseling and can choose a repayment plan. They are also told that they will be compensated for the experiment by receiving a random draw from the distribution of what actual recent college graduates with a Bachelor's degree might expect to earn over each of the next 25 years.[23] The incentive structure in the experiment revolves around the trade-off between repaying quickly, the risk of loan default, and the costs associated with defaulting. We take two steps to make these features salient in our experiment. First, we tell subjects that at the end of each period we subtract from their gross earnings federal payroll tax (FICA), and from the remainder of their earnings we subtract 150 percent of the federal poverty line to represent necessary living expenses. This benchmark corresponds with the earnings level at which students in income driven repayment plans are no longer required to make loan payments. Second, we tell subjects that from their earnings net of tax and living expenses, we will subtract any payments due on student loans according to their repayment plan. We inform them that if in any period their net earnings are less than what they are required to repay on their student loan, they will go into default and incur a penalty (they are afforded one period of forbearance, so default is not immediate).

For the penalty, we impose a two-fold cost of default. First, we decrease future earnings by 10 percent (which reduces their expected payout from the experiment). Second, at the beginning of the experiment we endow subjects with $8. If they default at any point, they forfeit this $8.[24] These penalties make the cost of default salient to participants,[25] and also structured to replicate the fact that defaulting on a student loan has a significant impact on credit scores, potentially increasing the cost of future borrowing, and possibly hurting future employment opportunities. The full set of instructions provided to subjects is shown in Appendix 1.[26]

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[22] This information is pre-entered for them on our facsimile of the student loan exit counseling website where they would normally enter their Federal Student Aid identification number which would fill in the form with their loan information.

[23] The exact procedure, as described to the subjects, is contained in the third paragraph of the Subject Instructions contained in Appendix A1.

[24] This $8 endowment is separate from the $5 show-up fee subjects receive that they cannot lose.

[25] Collection fees on defaulted student loans can amount to 20 percent or more of the loan balance: https://www.edvisors.com/college-loans/terms/default-federal/

[26] Subjects could read the instructions on their computers and were given a paper copy to reference throughout the experiment.

Loan payments in each period depend on the repayment plan subjects select. If a subject chooses a fixed repayment plan, for example the Standard plan, she faces an inflexible payment due each period. If she chooses an income driven plan, payments are a deterministic function of earnings, and she cannot default. Hence, by choosing one of the income driven repayment plans, subjects can completely insure themselves against default due to low earnings.

To generate earnings we use data from the 2012-13 (3-year) American Community Survey (ACS). We limit the sample to 461,802 individuals ages 24-49 with a four-year bachelor's degree. We estimate a pooled regression of earnings on a quartic in years since age 24. We use coefficients from this regression to create an empirical age-earnings profile which we then collapse into 20 representative ventiles. Deducting FICA (7.65 percent of earned income) gives us Adjusted Gross Income (AGI) as in federal calculations. We then calculate "discretionary income" as the difference between AGI and 150 percent of the one-person household poverty level, which is $17,505.

In Figure 1 we plot expected take-home pay from alternative repayment plans compared with the Standard repayment plan by ventile of the earnings distribution where the only penalty to default is a 10 percent reduction in future earnings as calculated from ACS data. The figure demonstrates that the income adjusting plans are dominant at the low end of the earnings distribution: 35 percent of 24-year-olds have annual earnings below 150 percent of the poverty line and thus default with the Standard plan under our conditions. We note that this is comparable to, though slightly higher, than estimates of actual default rates (which reflect some borrowers having IDR).

The benefit in net take-home pay we see between the Standard plan and the income driven plans is for the few borrowers just below the median of the distribution, where borrowers take home about 10,000 more in expectation from choosing Standard as opposed to REPAYE. Thus, if borrowers *knew with certainty* that they would be in the small part of the earnings distribution where they would earn enough to make all payments, but would not earn enough to be in the top end of the distribution where all plans are approximately the same because high earners' payments are similar to the Standard plan, they could see a small benefit from Standard. This also assumes they experience no unemployment or other negative income or earnings shocks. If, on the other hand, they face uncertainty as to where they will fall, the benefit of REPAYE (or any other IDR) over Standard is very large in expectation.

In our experiment, subjects do not know where they will fall in the earnings distribution, though in some treatments we tell them what the distribution looks like. IDR plans like REPAYE then provide full protection against large costs from repayment default caused by low earnings or a negative income shock, at a small cost of potential savings if it turns out the borrower receives high earnings and experiences no shock. In any calculation, the cost of insurance is small compared with the benefits even among non-risk averse borrowers. In other words, under the parameters of our experiment, assuming that subjects are risk-

neutral or risk-averse, IDR plans are (weakly) dominant. There are three caveats to this. First, outside of the lab borrowers can pre-pay. Allowing for this in the lab would require a dynamic experiment, though evidence suggests that prepayment is not common. More, borrowers can pre-pay under any plan, so the benefits of IDR compared with a fixed plan are not affected by this option; further, IDR's include the possibility of forgiveness. Second, our experiment does not include the cost of annual recertification. It might be that the recertification cost is a deterrent, but this is not true in the initial choice, which we model here as a viable policy option. Finally, borrowers may prefer a "commitment device" by choosing Standard. If borrowers face uncertainty over earnings, IDR plans offer inexpensive insurance against costly default. For standard to be preferable, it would have to be the case that the cost of future recertification and the benefit of the commitment mechanism would have to outweigh the value of insurance coupled with the option to pre-pay and the possibility for forgiveness under IDR. While for some borrowers this may be the case, given the high degree of earnings uncertainty after college (which our experiment includes), the high cost of default, low cost of insurance, and the option to pre-pay, the IDR plans are weakly dominant over fixed repayment.

Table 1 shows these differences in expectation over the entire earnings distribution assuming only a 10 percent reduction in earnings following default and not including the $8 fixed penalty. Not only do we show that borrowers under our scenario can expect to gain about 14,500 in lab currency in expectation by choosing REPAYE over the Standard plan but, importantly, the variance is far lower. Column 2 shows the difference in the standard deviation of take-home pay compared to the Standard plan. Not surprisingly, earnings variance is lower for IDR plans as default is not in play. The purpose of this exercise is to demonstrate that under reasonable assumptions, there are very few scenarios under which being tied to a fixed repayment plan is optimal, particularly if we assume borrowers are risk averse.

We convert subjects' lab earnings into U.S. dollars at a rate of $2.50 for every 100,000 they earn in the lab. Figure 2 shows U.S. dollar payout differences compared with the Standard repayment plan for all 20 ventiles of the lab earnings distribution, including the $8 penalty subjects pay if they default. It is important to keep in mind that only subjects in treatment arms that give information about earnings of recent graduates at age 24 have some, but not full, information about the earnings distribution.

3.3 Main Treatment Cells
The top of Table 2 lists the eight main treatment cells in our experiment, which will be the primary focus of our analysis. All of the treatments are described in full detail in an online appendix at https://excen.gsu.edu/jccox/instructions. These main treatments vary the three key policy parameters discussed above (the default option, information about earnings, and complexity). In treatments 1 to 4 we retain the full amount of complexity and choices borrowers face outside of the lab and either change only

the default option to REPAYE (treatment 2), add only information about earnings (treatment 3), or change both (treatment 4).

Treatment 1 is the Baseline treatment which presents the Exit Counseling web interface as borrowers experience it on the government site: Standard is the pre-selected default option, no information about earnings is provided, and subjects face the full complexity of information and choices. In treatments where REPAYE is the default, we pre-select REPAYE and reorder options such that REPAYE appears first (where Standard was), and Standard is then moved to the location where REPAYE was.

Some treatments present subjects with earnings information. In order to provide subjects with such information, we use data from the ACS and calculate deciles of the empirical earnings distribution of recent graduates at age 24 as of 2015. To make information easily understandable, we present subjects with bullet points describing what typical college graduates with a Bachelor's degree earn at that age. One of these bullet points indicates the share that earned less than 150 percent of the poverty line. Below this information we provide a simple table that displays deciles of the earnings distribution along with annual and monthly total and discretionary income. Last, we provide three bullet points relating discretionary income to total income and repayments. This information, displayed in Appendix 3, was designed to seamlessly fit the formatting of the government site. Subjects in these treatment cells are shown this information on the second page of Exit Counseling, just above where they make repayment decisions.

We then repeat this 2x2 treatment matrix under simplified information in treatments 5-8. While our "Complex" treatments described above reflect what is actually on the student loan exit interview site, our "Simple" treatments include only a pairwise choice between Standard and REPAYE and provide only one page with a straightforward description of each plan. For example, the Standard plan description informs subjects that if they make all scheduled payments they will accrue less interest, though payments do not adjust to income so they may have trouble repaying if earnings are low. For REPAYE, subjects are told that the plan provides insurance against unemployment or low earnings, but that this may extend repayment and increase the total amount paid. Appendix 4 shows a screen shot of the decision page (second page) for "Simple" treatments.

3.4 Additional treatment cells

In addition to our main treatments, the experiment included treatments listed in the bottom panel of Table 2. The first address implications of the lack of effective enforcement of the Department of Education's requirement for participation in Exit Counseling. To test for effects of skipping counseling, or for differential effects from diligent borrowers who would not skip counseling when given the opportunity, we include treatments 9 through 12 that are modifications of four of our main treatment cells to allow subjects to shirk participation in Exit Counseling and promptly leave the lab to receive their payment under the

default repayment plan in the treatment (which is Standard or REPAYE, depending on the treatment).[27] Other treatment cells test for whether the name itself, Standard plan, conveys a suggestion. Overwhelmed or confused borrowers might not want to choose a "non-standard" plan. To test this, treatments 13 and 14 differ, respectively, from treatment 1 (Baseline) and treatment 2 (REPAYE) only by changing the name of the Standard repayment plan to "Fixed."

4 Results

The experiment was computerized and run in the laboratory of Georgia State's Experimental Economics Center (E*x*CEN). Subjects were recruited with email invitations from the list of about 2,500 Georgia State students who had previously volunteered to participate in experiments. We used a filter in the lab's online recruitment facility to limit participation to undergraduates beyond their freshman year who are U.S. citizens. These restrictions were put in place to focus on subjects who persisted through their first year of college, when dropout is highest, and to focus on those eligible for federal student loans. We recruited 542 subjects to participate in 15 sessions over the spring and fall of 2017 (the fall sessions were conducted for our additional treatment cells, T9 – T14). At the end of a session we collected subjects' demographic information and conducted an incentivized risk elicitation task (Eckel and Grossman, 2002; 2008). The characteristics of subjects who participated in the experiment are reported in Table 3. They were 38 percent male, 61 percent black, with average age 20.6 years. Of these, 49 percent were arts and sciences majors and 27 percent business majors; 73 percent of our subjects reported having student loans.

4.1 Benchmarking our Baseline treatment

We begin by benchmarking our Baseline specification to the distribution of repayment choices at the national level. This is to demonstrate that choices and incentives faced by subjects in the lab parallel those faced by student borrowers outside of the lab. Table 4 shows results from the distribution of repayments as reported by two recent GAO reports and the distribution of repayment choices selected by our subjects in the Baseline treatment.[28] The earlier (2015) GAO report, using data from the National Student Loan Data System (NLSDS) for borrowers with outstanding loan balances as of September 2014, finds 65 percent of borrowers in the Standard plan, 14 percent in Graduated, and 19 percent in one of the income driven plans. A similar (2016) study from the GAO shows that 24 percent of borrowers in Q3 of 2013 through Q3 of 2016 were in an IDR plan with the remaining 76 percent in the Standard or Graduated (fixed) plans.

The right-most column of Table 4 shows results from our Baseline treatment in which 62.5 percent of our subjects chose the Standard plan, 12.5 percent chose Graduated, and 25 percent chose one of the IDR

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[27] Subjects in these treatments are allowed to leave the lab and collect payment in a separate room as soon as they are finished.
[28] GAO (2015, 2016).

plans. If we take the more recent (2016) GAO report in column 1 and compare with our Baseline results in column 3, we find that 76 percent of our subjects chose a fixed repayment plan (Standard or Graduated) compared with 76 percent of actual borrowers, and that 25 percent of our subjects chose an IDR plan while 24 percent of national borrowers are in one of these plans. Comparing our results with the 2015 GAO report shows that we are within one percentage point of the share choosing Graduated repayment, and that our subjects were only two percentage points less likely to choose the Standard plan, and had a slightly higher likelihood of choosing IDR plans, by six percentage points, than actual borrowers.

We believe that these figures suggest not only that our experiment reflects to some degree the data generating process in actual repayment choices, lending credibility to the counterfactual policy simulations we run, but also that our subjects are taking the experiment seriously and are not choosing blindly (any more than they are outside of the lab).

4.2 Main treatment effects: the default option, earnings information, and the role of complexity

In Table 5 we show mean take-up rates of the Standard plan in our eight main treatment cells. As described above, 63 percent of subjects enroll in the Standard plan in the Baseline treatment. The table shows clear departures from this number for three treatments: switching the default option (REPAYE treatment), switching the default option and adding earnings information (REPAYE + Info treatment), and that combination with a simple description of only two available repayment plans (Simple + REPAYE + Info treatment) each significantly decreases the share of subjects choosing the Standard repayment plan. We do not detect statistical differences compared with the Baseline treatment in take-up of Standard for any other treatments.

As reported in column (1) of the upper part of Table 5, changing the default plan from Standard to REPAYE leads to a 28 percentage point reduction (44 percent) in enrollment in Standard (Treatment 2). Changing the default plan and providing earnings information leads to the largest effect, a 46 percentage point (73 percent) reduction in Standard (Treatment 4). While the effect of changing the default to REPAYE and adding earnings produces the largest departure from Standard, statistically this difference from only changing the default is marginally insignificant (discussed further in cross-treatment tests below).

Adding only earnings information (Treatment 3) leads to an insignificant though noisy 9 percentage point reduction in the Standard plan. This is not because this information is ignored. After completing the choice portion of the experiment, we asked subjects what they expect their own annual earnings to be post-graduation, and what they thought the typical college graduate would earn. Subjects project their own earnings will be about $10,000 more than what they believe the typical graduate will earn ($45,000 compared with $34,500). The latter remains larger than data suggests. Yet, those who received earnings information as part of their treatment had far more realistic expectations. Shown in Figure 3, earnings

information led to about a 15 percent decrease in these expectations. The same holds, though with a downward shift in expectations, when we ask subjects what a typical graduate will earn. These differences are statistically different from zero.

We also find no effect of simplifying information to a binary choice and straightforward language (Treatment 5); while the estimate is small (-0.046), standard errors do not allow us to rule out effects as large as a 27 percentage point reduction in Standard. More interesting is that changing the default option has little to no effect compared with the Baseline in the Simple environment (comparing Treatments 5 and 6).

As in the Complex environment, we further find that earnings information plays virtually no role in the Simple environment. While one might expect this information to be more impactful when there is less other information to distract from it, we find that this is not the case (Treatment 7). Yet, we again find that providing earnings coupled with a change in the default option produces a large and economically meaningful reduction in enrollment in the Standard plan in the Simple environment (Treatment 8), as in the Complex environment (Treatment 4).

In summary, we find that: (1) changing the default option has an impact in the Complex environment on choice of Standard but does not have such an effect in the Simple environment; and (2) while earnings information alone has little impact, information coupled with changing the default option has a large impact. Here we discuss whether we should consider these results surprising or not.

First, consider that earnings information has little impact on its own. That beliefs about earnings are affected while downstream effects on choices are not affected is not necessarily surprising. Ambuehl et al. (2014) conduct an experiment in which they provide various forms of financial literacy training and observe effects on financial literacy, and subsequent effects on financial decision-making. They show that an impact on the former need not lead to the latter. They explain this result by arguing that financial literacy does not necessarily improve decisions in complexly framed valuation tasks. In our case, this result seems to hold. The choice of repayment plan requires a more complex decision – including consideration of the insurance nature of REPAYE (or other IDR plans) and the costly long-run effects of defaulting – than simply understanding the probability one may have low earnings. Hence, we can point to the complexity of the decision students face, rather than a simple overestimation of their own (or others') earnings potential, as a prime candidate for the choices we observe.

Next, consider that treatments that combine changing the default to REPAYE with earnings information significantly reduce choice of Standard. This can be attributed to the availability heuristic, as interpreted by Schwarz, et al. (1991) to operate through ease of recall of relevant information, as follows. Some students in the experiment, and in the national student loan program, may quickly move through the

decision task of selecting a repayment plan and not carefully consider the full implications, including possible involuntary default, of ending up with Standard. Others, who consider actively overriding the default plan, would reasonably be expected to take choice of plan more seriously and use available information in making the decision. In a REPAYE + Information treatment (in Complex or Simple environment), a subject considering actively changing from the REPAYE default plan to the Standard plan would proximately observe a screen showing minimum required monthly payment of $239 in the Standard plan, as shown in Appendix 2 or 4, and a screen showing a 40 percent probability of receiving monthly disposable income less than $239, as shown in Appendix 3. Actively switching from the REPAYE default to Standard would create exposure to the 40% probability of disposable income less than minimum required payment.

Next, we consider why a larger share of subjects revert to Standard in the Simple environment than in the Complex environment in absence of earnings information. One explanation is that the Complex environment leads to choice overload, whereby subjects are less likely to make an active decision when presented with an overwhelming amount of information and choices. To produce the results we observe in the two environments, this would have to be coupled with a large share of subjects not understanding the decision at hand, which is one narrative consistent with our data and with data from the national student loan program (as discussed in the Introduction). A contributing factor may be that subjects who do make an active decision may focus on a key feature of the Education Department's web interface that is replicated in our experiment (see Appendix 2): the total *expected* repayment amount under REPAYE is portrayed as being larger than under Standard. Subjects in our experiment and borrowers in the national program may be misled about their future ability to repay a loan by the Education Department's unrealistic projection, also replicated in our experiment, of earnings growth of approximately five percent per year based on what subjects enter for their expected earnings (on page 2 of the choice platform, not in our survey afterwards). More, this feature in no way takes into account the likelihood of periods of unemployment or costs of default, rather it simply extrapolates a constant stream of earnings. While our data limit our ability to decisively identify which of these (or other) potential mechanisms generate the data we observe, it is relevant to ask whether other studies have found that simplifying choice environments does or does not seem to improve financial decision-making.

One obvious place to look is the literature on participation in retirement plans. Some employers have changed the default option from "opt in" to "opt out" of participation in defined contribution retirement plans (Madrian and Shea, 2001; Choi et al., 2002, 2004a,b; Beshears, et al., 2009). The adoption of opt out (or automatic enrollment) decreased transaction costs of filling out forms and, probably more importantly, decreased cognitive costs associated with choosing among alternative contribution rates and asset allocations. Automatic enrollment inherently involves default contribution rate and default asset allocation.

The complex opt in decision task is simplified to a "yes" or "no" decision with lower cognitive cost. Those studies found that automatic enrollment significantly increased participation. But it also anchored contribution rates and asset allocations on the *varying* defaults chosen in alternative implementations (Beshears, et al., 2009), which is inconsistent with optimal retirement planning.

Choice about retirement plan participation is, however, a different type of decision task than we explore with the student loan repayment experiment. In the student loan decision-making environment, whether in the field or in our lab, the student is not faced with an opt in or opt out situation; instead, the student is required to make a choice from among several (or between two) financial alternatives regardless of which loan repayment plan is the default option. In our Simple environment, the number of available plans is reduced to two out of the several plans in the Complex environment. There is no parallel to this treatment in the retirement plan literature. In the automatic enrollment environment, contemplation of contribution rate or asset allocation different from the defaults involves consideration of the full range of complex choice alternatives present in the opt in choice environment.

Choi, et al. (2009) and Beshears, et al. (2013) report experiments in which individuals can choose to opt in to a retirement savings plan with a preselected contribution rate and asset allocation by simply checking a box on a card. This Quick Enrollment protocol greatly lowers the transaction and cognitive cost of opting in to a retirement plan. The result is increased participation. Another result is a high incidence of participants sticking with the default (a) contribution rate and (b) asset allocation in different experiments in which (a) and (b) are given different values. These anchored reported outcomes are inconsistent with participants choosing their optimal retirement plan participation. But these experiments do not conform to the difference between Complex and Simple environments in our experiment. An experiment with retirement plans that would conform to our experiment would be one in which different groups of individuals were offered menus containing two (simple) or many (complex) asset allocations and these treatments were crossed with selection of alternative default asset allocations. While subjects would still have to choose an asset allocation (the analog of choosing a loan repayment plan), they would not be able to opt out of this decision. This type of retirement plan experiment does not appear to have been reported in the literature.

The literature on retirement plan decision-making thus discusses three possible explanations of persistence of (or anchoring on) default options: (a) procrastination generated by complexity of the decision-making task, (b) procrastination generated by present-biased preferences, and (c) perception of the default as an endorsement. Explanations (a) and (b) have no relevance to our experiment in which procrastination is not possible. Explanation (c), possible perception of the default option as an endorsement does have relevance for interpreting our data. It is examined by one of the treatments reported in the following section.

4.3 Treatments with exit counseling skip option and Standard plan name change

Before turning to a discussion of magnitudes, we briefly consider the effect of changing the name of the Standard plan to Fixed plan because the designation as "standard" can imply a recommendation, particularly when it is the default plan. We also consider effects of the opportunity to skip exit counseling and avoid transaction and cognitive costs of participation. As shown in column (1) of the lower part of Table 5, by themselves neither the opportunity to skip counseling (Treatment 9) nor the name change from Standard to Fixed (Treatment 13) produces significant effects compared with the Baseline treatment, though standard errors in both cases do not allow us to rule out potentially meaningful effects. Changing the name from Standard to Fixed by itself leads to a 10 percentage point reduction in Standard, though it is not statistically significant and is the one result that is affected by including session fixed effects which reduces the effect to 2.8 percentage points. We do find an effect of changing the name when the default is REPAYE (Treatment 14). Thus, the name change works in preventing an active choice to revert to Standard ("Fixed"), but does not affect the likelihood of leaving the Standard ("Fixed") plan if defaulted into it.

Concerning skipping, of the 155 subjects in treatments 9 through 12, 19 percent skip the exit interview (see Table A1 in Appendix 7). Among skippers, nearly all skipped on or before the decision page (page 2 of counseling) in which they see all of the choices. We find that other than the mechanical effect of reinforcing the default for skippers, the option to skip does not alter our estimates of treatment effects, as shown in the bottom part of column (1) in Table 5. As evidence, in column (2) of Table 5 we drop the 30 subjects who skipped counseling and pool those who remained into their analogs in the main treatments with no effect on estimated treatment parameters. Column (3) confirms that adding subject demographics, such as gender, age, race, college within the university, and a measure of risk tolerance, along with experimental session fixed effects do not alter estimated treatment effects.

4.4 Effect Sizes and Robustness

Given this similarity, we use the larger sample in column (2) of Table 5, which includes non-skippers, to leverage sample size to add precision to our estimates. While many of the effects we estimate are economically meaningful, they are noisy, and we acknowledge here the limitations of what we can and cannot conclude. Our headline result is that changing the default option leads to a near 30 percentage point decline in the likelihood of taking the Standard Plan. We cannot rule out effects as small as about 16 percentage points, nor as large as 43, but the significant effect of the default option is a central finding.[29]

In Table 6 we report formal tests across treatment arms for our main results from the larger sample. The first three rows compare effects of changing the default option, adding earnings information, and the

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[29] We cluster standard errors on sessions, as is common in the experimental literature. This results in 15 clusters across 512 total observations, where treatments are randomized *within* sessions.

combination of the two, separately within the Simple and Complex environments. The next three rows compare across non-Baseline treatments. For example, row 4 shows that we can reject equality between changing only the default and only adding earnings information in the Complex environment (95 percent confidence intervals are provided with the difference across coefficients). Row 5 shows that we cannot conclude that the effect of changing the default and providing earnings information is statistically larger than just changing the default alone in the Complex environment, although estimates are only marginally insignificant (p=0.11). We do find that they are statistically significant in the Simple environment. Tests reported in row 6 show that we can reject equality of effects from only adding information and effects from adding information plus changing the default in both Complex and Simple environments.

The bottom of Table 6 compares treatments *across* the Simple and Complex environments. This part of the table shows that indeed the effect of changing the default option, and the effect of changing the default and adding earnings information, are statistically different depending on whether subjects face the Complex or Simple environment. In the case of only adding earnings information, we can largely rule out effects across the Simple and Complex environments. In the final two rows, we compare effects versus the Baseline across the Simple and Complex environments. Here we show that the effect of switching the default to REPAYE, compared with the Baseline, is larger in the Complex than Simple environment, but that we cannot say that the effect of REPAYE + Info is larger in the Complex than Simple, as shown in the last row.

4.5 Effects on the distribution of choices

In our final set of exercises we turn to the distribution of choices subjects make in the Complex environment. While limiting to a pairwise set of choices with plain language is illuminating, it may not be a viable policy alternative. Yet, altering the default option and/or providing earnings information in the current complex policy framework may be viable. In this analysis, we drop Simple treatment cells and observe how our main treatment arms affect the distribution of choices. While there are several options to choose from, we collapse these into four types: Standard, Graduated, REPAYE, and Other IDR (including PAYE, IBR, and new-IBR). Both among active borrowers and in our experiment, few choose Other IDR, hence collapsing them does not alter conclusions.

To facilitate interpretation and statistical tests given multiple potential outcomes, in Table 7 we show results from a multinomial logistic regression and report marginal effects. Columns show the effect of each treatment on the likelihood subjects choose each of the associated plans. Rows show treatment options with the Baseline (Standard as default, no earnings information) as the omitted category.

We find that the majority of subjects who are defaulted into REPAYE stay in that plan. There is little impact on switching to a different IDR plan, and no statistically meaningful increase in choosing

Graduated. We again find a noisy and statistically insignificant negative impact of adding earnings information on take-up of Standard, but show that it increased take-up of REPAYE, in part through a decrease in the use of all other plans coupled with a decrease in Standard. Finally, the combined effect of changing the default option and adding earnings information again produces the largest impact, increasing take-up of REPAYE.

4.6 Caveats and Policy Implications

We begin by considering whether it could be the case that borrowers are actually making the right decision by choosing Standard. We believe our results, combined with data on actual borrowers and results from prior work, show that is not the case. Earnings for many borrowers are most volatile and uncertain in the first year after exiting schooling. Protection against default by choice of an IDR would be most valuable here. Given initial choice of an IDR, in the event that earnings turn out to be high, a borrower can passively switch to Standard simply by taking no action to recertify IDR or choose to revert to the Standard plan. In other words, the cost to the borrower of switching to Standard from an IDR is meaningfully lower than the reverse, and, of course, a borrower always has the option to make larger than required payments.

Second, one might argue that borrowers believe they will actively switch plans if warranted, first enrolling in Standard and then switching to REPAYE, or another IDR, if they experience a negative income shock or find their earnings are below what they expected. We believe existing data on student loan defaults rules this out as a sufficient explanation. 70 percent of borrowers who default, not to mention the many more who experience costly delinquency, were eligible for an IDR plan at the time they defaulted (GAO, 2012). Hence one cannot credibly believe that most borrowers would switch away from an initial choice of Standard into IDR when and if needed. It may be that they think they will switch when and if low income warrants but do not, consistent with a time-inconsistency problem.

It could also be that borrowers prefer the commitment mechanism of a fixed payment plan to discipline the future self to avoid increasing near-term consumption by making the smaller payments allowed by IDR. But for this to be a sufficient explanation for behavior of the large proportion of borrowers who enroll in Standard and subsequently default on minimum required payments, one would have to believe borrowers are careful enough to make this choice in the first place but so careless when actually faced with low income that they do not switch plans when it is needed to avoid default.

We next address implications for policy and limitations. Results from our main specification suggest that changing the default option alone could reduce initial enrollment in the Standard plan by nearly half, with a comparable uptake in REPAYE. Further, we estimate that changing the default from Standard to REPAYE and adding earnings information could reduce take-up of Standard by nearly 46 percentage points, or 74 percent. These results are in line with recent field experiments, as in Herbst (2018), or Yannelis

and Muller (2019), who find a 34 percentage point increase in IDR enrollment when borrowers receive a pre-populated IDR application from their servicer. The key limitation to changing the default option at the outset as presented in our study is that borrowers will still need to verify earnings to remain in an IDR plan, and will have to recertify each year to continue to do so. Herbst (2018) shows that many fail to recertify after switching to IDR, even though the federal student loan website has a page dedicated to this which claims the process can be completed in about 10 minutes.[30]

In this sense, the purpose of our experiment is not to show that switching the default option will end student loan defaults, though it will certainly reduce them, particularly in the first year of repayment when they are very high. Rather, the purpose is to demonstrate that borrowers' decision-making behavior is inconsistent with the rationales that could be offered as support for the current policy's repayment options and the choice architecture through which these options are presented. In this light, our experiment is both demonstration of an easy change that can be made by the government, and serves as a "wind tunnel" study for a future field experiment that tests efficacy of this policy outside the lab.

Existing field work, taken together with our results, demonstrates the multitude of barriers borrowers face in leaving the Standard plan. Yet, existing work as cited above puts the onus on student loan servicers to enact these policies, which has its own set of limitations as these are independent actors and reassignments of borrowers to different loan servicers are common. Moreover, a large share of delinquencies and defaults happen quickly, and anecdotal evidence suggests that many borrowers are not in contact with servicers before default, despite requirements for servicers to contact borrowers directly.

Without downplaying the servicer's role, we offer a second alternative, whereby students are defaulted into IDR plans at the outset, providing a policy lever for government. Even taking conservative estimates from our experiment with the Complex environment suggests that a large majority of borrowers would not actively switch back to the Standard plan if they did not start in it. Yet the hurdle of providing proof of earnings would still exist. Muller and Yannelis (2019) demonstrate that these forms could be pre-populated with great effect, and Herbst (2018) demonstrates that maintenance efforts would be required to keep borrowers enrolled. Taken together, the body of evidence suggests that this combination of policy changes would be most effective: specifying IDR as the default option and offering pre-populated earnings records.

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[30] The IDR request site says, "The entire Income-Driven Repayment process must be completed in a single session. Most people complete the process in 10 minutes or less." Accessed at https://studentloans.gov/myDirectLoan/ibrInstructions.action on 02/20/2019.

5 Conclusions

Our experiment is motivated by the fact that one-quarter of borrowers default on their student loans within five years despite options that provide protection against involuntary default through income driven repayment plans. Worse yet, most borrowers pass up a plan that provides insurance against default from low earnings without requiring a sacrifice in consumption if earnings are high.

While standard models of economic behavior fail to explain this pattern, we find that behavioral economics does. In our incentivized laboratory experiment we find that the default option plays an outsized role in determining repayment plan enrollment, and that simply changing which plan is pre-selected reduces by nearly one-half the share of subjects enrolled in the Standard plan, the plan that most repayment defaulters are enrolled in.

In contrast, we find that while simply providing borrowers information about the distribution of earnings they might expect leads them to update their reported beliefs about their own and others' future earnings, it has a smaller and statistically insignificant effect on take-up of Standard. Yet, when we provide this information *and* change the default option to an income driven plan, behavior does change. This suggests that availability of earnings information may nudge behavior more effectively when choice of income-invariant repayment plan requires a decision to actively switch plans.

We consider other policy features as well. In particular, we test how complexity affects behavior and ask what happens if we limit the number of choice options and complexity of their description. We also test for effects of "shirking" behavior and of the Standard plan name itself as well as subjects' risk aversion. Our results support the conclusion that the default option plays a stronger role than any of these other features of the student loan policy environment, although they also support an effect from adding earnings information with change in the default plan. These results speak to three strands of research.

First, we reinforce prior work demonstrating that the default option influences choice and that a poor selection of that option can lead to suboptimal and even detrimental outcomes (Samuelson and Zeckhauser, 1988; Madrian and Shea, 2001; Thaler and Sunstein, 2003; de Haan and Linde, 2018). Second, we build on the literature addressing the causes (Abraham, et al., 2018b; Looney and Yannelis, 2015) and consequences (Baum, 2015; Bleemer, et al. 2014; Field, 2009) of student loan defaults. In particular, an experimental survey Abraham, et al. (2018b) finds that simply reframing a generic IDR type repayment plan such that insurance protection is emphasized over the possibly-increased interest cost leads to greater take up. This fits as well within a related literature on informational nudges targeting student borrowers at college entrance (Marx and Turner, 2018; Castleman and Page, 2016; Barr, Bird and Castleman, 2017).

Third, and in the broadest sense, we contribute to a discussion on returns to higher education. There is no shortage of public opinion that we are in the midst of a student loan "crisis." Yet, returns to college remain large. Dynarski and Kreisman (2013) speak directly to this apparent contradiction in the inability of

many college graduates to repay loans, despite increasing long run returns to schooling. Part of this is simply due to high variability of earnings in early careers, even for college graduates, and part of it is due to the fact that returns to schooling accrue over a lifetime, not immediately after graduation. Hence, offering flexible repayment plans that vary with earnings makes sense, both for borrowers and lenders. The problem is not that these programs are unavailable, but rather that they are underutilized.

If the federal government's rationale for defaulting students into the Standard plan is that borrowers will make optimizing decisions, we believe our results, in addition to statistics about repayment choice and default among borrowers, suggest that such a rationale is misguided. For example, having been offered the opportunity to choose a plan with income-contingent minimum payments, one would expect recent graduates with uncertain earnings to choose it in large numbers. One would also expect to see borrowers with known low earnings, putting them in jeopardy of default, shift toward income driven repayment plans. There is little evidence of either behavior in national student loan data. If fact, existing data suggests the opposite is true. We are not unique in observing this type of suboptimal behavior. Bhargava et al. (2017) show that a majority of employees in a large U.S. firm chose a health care plan that was clearly dominated by other options. Like us, they also show that neither risk preferences nor plan complexity drove these behaviors. Rather, it was simply employees' lack of understanding of health insurance. Ericson and Syndor (2017) reach similar conclusions. Chetty et al. (2014) go further, showing that in the case of retirement savings automatic contributions have a much larger effect on savings than price subsidies. Their takeaway is that policies relying on individuals to take active steps are likely to yield few effects compared with nudged contributions. Importantly, in our case, if borrowers are behaving optimally and with full information, then changing the default option should have little effect. We find the opposite.

Given these facts, it appears that there is little justification for making Standard the default plan. Findeisen and Sachs (2016) show not only that income driven loan contracts are optimal from a public investment standpoint, but also that these results can hold under a straightforward repayment schedule. Lochner and Monge-Naranjo (2016) make similar claims. Mueller and Yannelis (2018) demonstrate that this type of repayment scheme is effective in providing protection from negative shocks. Herbst (2018) shows that despite lower required contributions on average, IDR enrollees pay down more of their debt than other borrowers as a result of more timely and consistent repayment. These results, combined with our own, suggest that in case of the student loan program eliminating income-invariant repayment plans may be a policy improvement.

From a policy perspective, this need not be controversial. Australia and New Zealand have implemented fully income contingent repayment strategies. The UK, Canada and South Africa feature them prominently. Legislative efforts in the U.S. to shift to an income-adjusting repayment policy have gained little traction. Past efforts to move borrowers into income-driven repayment plans has seen limited

success. But none of these reform policies has shifted the default option. Results from our experiment indicate that changing the default option from Standard to an IDR plan, together with provision of easily comprehensible information about the distribution of earnings by college graduates, could significantly reduce the incidence of loan repayment default.

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Tables

Table 1: Simulated take-home pay differences and standard deviations vs. standard plan

	Difference from Standard Plan		
	Expected Earnings-Repayments	Difference of 1 s.d.	Prob(Default)
Standard	$0	$0	0.35
Graduated	-$375	-$1,272	0.35
REPAYE	$14,734	-$17,296	0
PAYE/IBR-New	$14,561	-$18,359	0
ICR	$8,916	-$12,456	0

Notes: Table shows expected difference in earnings less repayments compared with Standard repayment plan (column 1) and expected standard deviation differences (column 2) from Standard plan (this is sd(plan)-sd(standard)). Simulated earnings draws pre-tax income from work ventiles from distribution of four-year college graduates age 24 in 2015. Earnings then grow for 25 years assuming a quartic growth rate estimated from empirical cross-section in same year. Earnings are reduced by 10 percent in each year following repayment default (only possible for Standard and Graduated).

Table 2. Student loan experiment treatments

Treatment	Obs.	Default plan	Informational environment	Earnings Information	Skip option	Name change
Main Treatment Cells						
1 (Baseline)	40	Standard	Complex			
2	38	Standard	Complex	Yes		
3	39	REPAYE	Complex			
4	37	REPAYE	Complex	Yes		
5	38	Standard	Simple			
6	38	Standard	Simple	Yes		
7	38	REPAYE	Simple			
8	39	REPAYE	Simple	Yes		
Additional Treatment Cells						
9 (1)	39	Standard	Complex		Yes	
10 (3)	39	REPAYE	Complex		Yes	
11 (6)	39	Standard	Simple	Yes	Yes	
12 (8)	38	REPAYE	Simple	Yes	Yes	
13 (1)	40	Standard	Complex			"Fixed"
14 (3)	40	REPAYE	Complex			"Fixed"

Table shows all treatment cells.

Table 3. Subject summary statistics

	Mean	s.d.
Male	0.38	(0.48)
Age	20.6	(2.32)
White	0.10	(0.30)
Black	0.61	(0.49)
Hispanic	0.04	(0.21)
Asian	0.13	(0.34)
Other	0.11	(0.31)
Year 2-5	0.98	(0.14)
Has loans	0.73	(0.44)
School		
Business	0.27	(0.44)
Arts & Sci.	0.49	(0.50)
Policy	0.08	(0.28)
Health	0.08	(0.26)
Other	0.09	(0.28)
Obs.	542	

Notes: Table shows means (sd) for subjects in the experiment. All measures are self-reported.

Table 4: Experimental and actual take-up rates.

	GAO (2016)	GAO (2015)	Baseline
Standard	0.76	0.65	0.63
Graduated		0.14	0.13
IDR plans	0.24	0.19	0.25
Other	n/a	0.02	n/a

Notes: Columns 1 and 2 show the distribution of repayment plans for borrowers with outstanding balances as of 2013-2016 (column 1) and 2014 (column2) from two GAO reports (GAO 215, 2016). Column 3 shows results from our Baseline specification.

Table 5: Effects of Default Option, Earnings Information and Complexity on Take-up of the Standard Plan.

	All Subjects in Main or Other	"Non-skippers" pooled with Main Treatments	
	(1)	(2)	(3)
Main Treatment Cells			
T2. REPAYE	-0.283***	-0.296***	-0.282***
	(0.089)	(0.065)	(0.075)
T3. Info	-0.087	-0.099	-0.070
	(0.152)	(0.127)	(0.139)
T4. REPAYE+Info	-0.463**	-0.444***	-0.446***
	(0.159)	(0.115)	(0.127)
T5. Simple	-0.046	-0.023	-0.022
	(0.105)	(0.083)	(0.086)
T6. Simple+REPAYE	-0.072	-0.085	-0.065
	(0.100)	(0.070)	(0.089)
T7. Simple+Info	-0.020	-0.032	0.010
	(0.111)	(0.098)	(0.104)
T8. Simple+REPAYE+Info	-0.292***	-0.268***	-0.273***
	(0.082)	(0.063)	(0.070)
Treatments with Skip Option			
T9. Skip	0.119		
	(0.141)		
T10. Skip+REPAYE+Info	-0.446***		
	(0.139)		
T11. Skip+Simple	0.093		
	(0.146)		
T12. Skip+Simple+REPAYE+Info	-0.257**		
	(0.110)		
Name Changed to "Fixed"			
T13. Fixed	-0.100	-0.113	-0.028
	(0.105)	(0.071)	(0.068)
T14. Fixed+REPAYE	-0.450***	-0.463***	-0.376***
	(0.110)	(0.078)	(0.062)
Demographics and Session Fixed Effects			yes
Drop if skipped counseling		yes	yes
Observations	542	512	512

Notes: Results above are regression coefficients from least squares regression of choosing the Standard plan. Column 1 is all subjects. Columns 2 and 3 drop the 30 subjects from treatments 9-12 who skipped exit counseling at any point. In columns 2 and 3, subjects in treatments 9-12 who did not skip counseling are assigned to their corresponding main treatment cells as follows: T9=T1; T10=T4; T11=T5; T12=T8. REPAYE is changing the default option. Info is adding earnings information, and Simple is presenting a pairwise choice with plain language. Treatments with skip option allowed subjects to leave counseling at any time. Name change indicates treatments where the Standard treatment was renamed "Fixed".
Standard errors clustered on experimental sessions in parentheses. [*0.10, **0.05, ***0.01].

Table 6. Significance tests across treatments. [Difference, 95% confidence interval]

	Treatment effect differences *within* complex or simple environments	
	Complex	Simple
REPAYE vs. Baseline	-0.296***	-0.062
	[-0.434, -0.157]	[-0.245, 0.122]
Info vs. Baseline	-0.099	-0.009
	[-0.371, 0.173]	[-0.257, 0.239]
REPAYE+Info vs. Baseline	-0.444***	-0.245**
	[-0.690, -0.196]	[-0.455, -0.034]
REPAYE vs. Info	-0.196*	-0.053
	[-0.401, 0.008]	[-0.302, 0.197]
REPAYE+Info vs. REPAYE	-0.148	-0.183*
	[-0.334, 0.038]	[-0.301, -0.065]
REPAYE+Info vs. Info	-0.344***	-0.235**
	[-0.578; -0.102]	[-0.463, -0.008]

	Treatment effect differences *across* complex and simple
REPAYE (complex vs. simple)	-0.211*
	[-0.349, -0.072]
Info (complex vs. simple)	-0.067
	[-0.341, -0.072]
REPAYE+Info (complex vs. simple)	-0.176**
	[-0.320, -0.032]
REPAYE vs. Baseline (complex vs. simple)	-0.234**
	[-0.459, -0.009]
REPAYE+Info vs. Baseline (complex vs. simple)	-0.199
	[-0.453, 0.055]
Observations	512

Notes: Sample is N=512 non-skippers from column 2 of Table 5. Statistics show differences across coefficients listed in each row. First panel shows difference across default option and information in the complex and simple frameworks. Second panel shows differences for each treatment between the complex and simple frameworks. 95% confidence interval of difference in coefficients in brackets. Standard errors are clustered on 15 experimental sessions, with treatments randomized within lab session.
[*0.10, **0.05, ***0.01].

Table 7: Effect of the Default Option and Earnings Information on Plan Choice

	Outcomes			
Treatment	Standard	REPAYE	Graduated	Other IDR
T2. REPAYE	-0.259***	0.271***	0.072	-0.084
	(0.065)	(0.103)	(0.056)	(0.087)
T3. Info	-0.097	0.190***	-0.035	-0.059
	(0.121)	(0.059)	(0.084)	(0.051)
T4. REPAYE+Info	-0.470***	0.337***	0.156	-0.024
	(0.147)	(0.065)	(0.107)	(0.084)
Observations		154		

Notes: Results are transformed marginal effects from multinomial logistic regression of choices on change of default option (REPAYE), adding earnings information (Info), and changing the default and adding earnings information (REPAYE+Info). Columns show effects compared with the baseline (omitted) treatment, which is Standard as default and no earnings information, or "as is" on the student loan website. Standard errors clustered on experimental sessions in parentheses. [*0.10, **0.05, ***0.01].
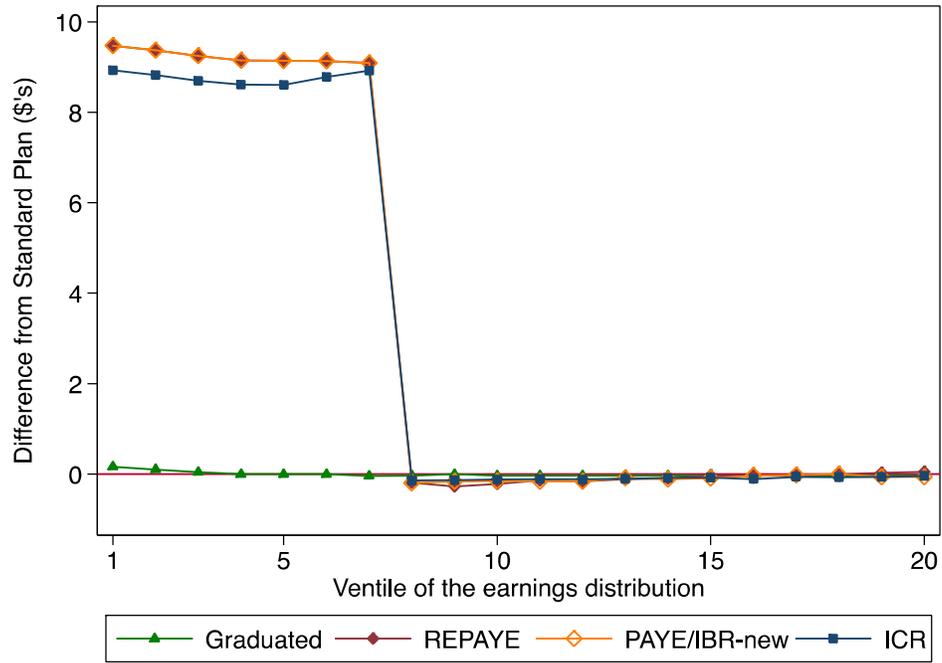
Figures

Figure 1. Simulated take-home pay differences vs. standard plan
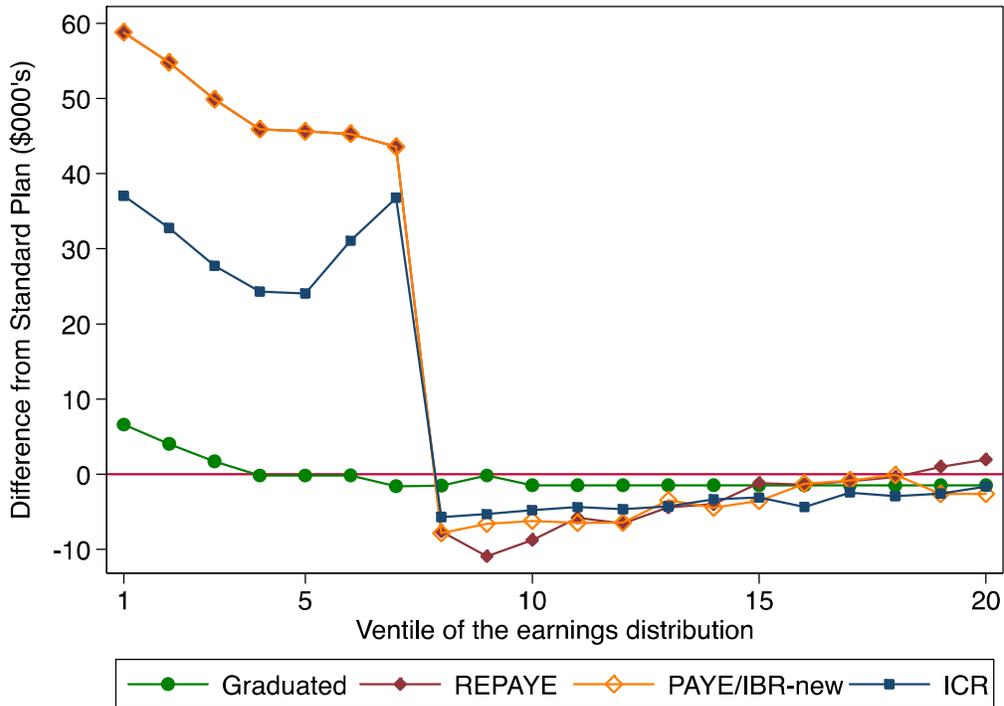

Notes: Figure plots expected difference in earnings less repayments compared with Standard repayment plan by ventile of the earnings distribution. Simulated earnings draws pre-tax income from work ventiles from distribution of four-year college graduates age 24 in 2015. Earnings then grow for 25 years assuming a quartic growth rate estimated from empirical cross-section in same year. Earnings are reduced by 10 percent in each year following repayment default (only possible for Standard and Graduated).

Figure 2. Experimental compensation vs. Standard plan

Notes: Figure plots expected difference in experimental compensation compared with Standard repayment plan by ventile of the earnings distribution. The exchange rate is $2.50 per 100,000 earned in simulated lab earnings. Subjects begin with an $8 U.S endowment which subjects lose if they default.

Figure 3. Effect of information about earnings on earnings expectations
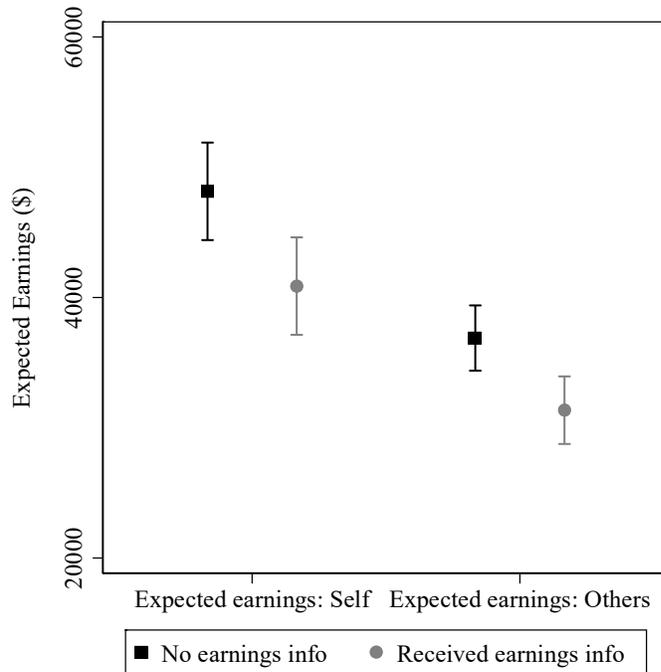
Notes: Figure shows mean responses to question what do you (others) expect to earn in the first year after graduation. Left two bars show means for subjects who did not receive information about the distribution of earnings. The right bars show means for those who received this information as part of treatment.

Appendices

Appendix 1. Subject instructions.

Instructions

In the following you will experience a simulation of the federal government's Student Loan Exit Counseling in which students learn about student loan repayment options and can choose a re-payment plan. For our purposes, we will assume you have a **direct subsidized student loan of $23,000 at an interest rate of 4.6%**.

Your payment for participation in this study will come from "lab earnings" taken over 25 "lab years." These "lab earnings" are designed to reflect a random draw from the distribution of what actual recent college graduates with a Bachelor's degree might expect to earn over each of the next 25 years.

To create your "lab earnings", we begin by dividing the distribution of pre-tax earnings from work for 24 year olds in the U.S. with a Bachelor's degree into 20 representative individuals. You are then randomly assigned to be one of these individuals – thus you have an equal chance of being in the bottom, middle or top of the earnings distribution. You will then receive what a typical individual at that place in the distribution might expect to earn each year for each of 25 "lab years."

At the end of each "lab year" we first calculate your "discretionary earnings" by subtracting from your "lab earnings" basic living expenses, which we set equal to 150% of the current federal poverty line, and withholding for Social Security and Medicare. From this we then subtract payments due on your student loan until the loan is either paid off or, in some cases, forgiven. The remainder (your "discretionary earnings" less any loan repayment) you will keep. These remaining "lab earnings" will be converted into real U.S. dollars at a rate of $2.50 per 100,000 "lab dollars" earned.

In addition, you will start the study with a gift of $8 real U.S. dollars.

If in any "lab year" your "discretionary earnings" are less than what you are required to repay on your student loan, your loan will go into default. If you default on your loan, you will receive a penalty in the loss of your $8 U.S. dollar gift in addition to a 10% penalty on all future "lab earnings" thereafter.

Summary

- You begin with a direct subsidized loan of $23,000 at 4.6% interest.
- You then complete the Student Loan Exit Counseling.
- You are then randomly assigned a stream of "lab earnings" drawn from the distribution of what 24 year old college graduates might expect to earn over each of the next 25 years.
- At the end of each "lab year" basic living expenses and federal withholdings are deducted from your "lab earnings" first, then payments on student loans are deducted if any balance remains. The remaining "lab earnings" you keep.
- These are converted into real U.S. dollars at an exchange rate of $2.50 for every 100,000 "lab dollars" you have left over in each year.
- If in any "lab year" you default on your student loan repayment because you cannot make your payment, you forfeit your $8 gift and all future "lab earnings" are reduced by 10%.
- You will learn your "earnings" draws after completing the Exit Counseling.

If you have any questions, please raise your hand and someone will approach you and answer your question. When you're ready to begin, please click on the Begin button at the bottom of this page. You may take as much time as you want to obtain information from the Exit Counseling site.

Appendix 2. Repayment options on facsimile exit counseling website.

Your repayment plan is the Standard plan unless you change your plan by clicking on the other radio buttons below.

	Initial Monthly Payment	Total Amount Paid	Repayment Period	
⊙ Standard	$239	$28,737	10 years	See Payment Guidelines
● Graduated	$135	$30,194	10 years	See Payment Guidelines
● Extended, Fixed	Extended only available for amounts greater than $30,000.	Extended only available for amounts greater than $30,000.	25 years	See Payment Guidelines
● Extended, Graduated	Extended only available for amounts greater than $30,000.	Extended only available for amounts greater than $30,000.	25 years	See Payment Guidelines
● Revised Pay As You Earn	$52	$36,060	20 years	See Payment Guidelines
● Pay As You Earn	$52	$36,060	20 years	See Payment Guidelines
● IBR for New Borrowers	$52	$36,060	20 years	See Payment Guidelines
● Income-Contingent Repayment	$136	$34,749	25 years	See Payment Guidelines

•These estimated monthly payments are based on your inputs and may not be exact. Contact your loan servicer to see if you qualify for an income-driven repayment plan.

Notes: Image shows repayment options as seen by subjects. A value of $24,000 was entered as expected earnings after college in order to create initial monthly and total payments.

Appendix 3. Earnings information on facsimile website.

Estimate What You Will Owe, Spend, and Earn

In the sections below, enter what you expect to earn annually once you graduate.

How much did recent college graduates with a Bachelor's Degree (age 24) earn in 2015?

- On average, recent graduates with a Bachelor's Degree earned about **$22,000 annually** or **$1,850 per month** last year in wages.
- More than 30% earned less than 150% of the federal poverty line.
- **Over half** of recent college graduates earned less than **$24,000** annually or **2,000 per month**.
- **Over 30%** of recent college graduates had **no discretionary income.**

Annual income for recent college graduates in 2015.

	Annual Income	Annual Discretionary Income	Monthly Income	Monthly Discretionary Income
Bottom 10%	$700	$0	$58	$0
Bottom 20%	$5,800	$0	$483	$0
Bottom 30%	$11,700	$0	$975	$0
Bottom 40%	$17,650	$200	$1,471	$17
Bottom 50%	$23,150	$3,850	$1,929	$321
Top 50%	$28,700	$9,000	$2,392	$750
Top 40%	$33,550	$13,500	$2,796	$1,125
Top 30%	$39,800	$19,250	$3,317	$1,604
Top 20%	$47,350	$26,250	$3,946	$2,188
Top 10%	$70,850	$47,900	$5,904	$3,992

How are income, discretionary income and repayment related?

- Your **discretionary income** is what you have left after basic living expenses have been taken out.
- **Repayment** under some plans is only required if your discretionary income is greater than 0.
- **About 9 out of 20 college graduates (45%)** had monthly discretionary income **less than $240** at age 24.

Notes: Image shows screen shot of information provided to subjects in treatment cells that provided information about the distribution of earnings for recent college graduates.

Appendix 4. Simplified Information on facsimile website

Your repayment plan is the Standard plan unless you change your plan by clicking on the other radio buttons below.

	Initial Monthly Payment	Total Amount Paid	Repayment Period
⊙ Standard	$239	$28,737	10 years

Under the **Standard Repayment Plan**, the total amount you owe on your loan is divided into monthly payments that you make for 10 years.
- The amount due is the same each month and depends on how much you borrowed regardless of what you earn.

Benefits:
- If you make all scheduled payments you will pay less in total under this plan because you accrue less interest by paying back quickly.
- Payment are predictable and there is no need to show proof of income.

Drawbacks:
- Payments do not adjust to your income, so you may have trouble repaying if your earnings are low.
- If you fail to make payments due to unemployment or low earnings, your loan will go into default.
- There is no loan forgiveness under the Standard Plan

	Initial Monthly Payment	Total Amount Paid	Repayment Period
◯ Revised Pay As You Earn	$0	$0	20 years

Under **REPAYE**, your monthly payment is equal to 10% of your Discretionary Income. If you earn less than 150% of the Federal Poverty Line, you do not owe any payment at that time. Any remaining unpaid balance after 25 years is forgiven.

Benefits:
- REPAYE provides insurance against unemployment or low earnings.
- When you cannot afford to pay, no payments are due.
- Your loan is forgiven if you cannot pay it off in 25 years.

Drawbacks:
- Making little or no payment in some years will add to the total amount of interest.
- You may make payments for longer under this plan unless you choose to make more than required minimum payments.

-These estimated monthly payments are based on your inputs and may not be exact. Contact your loan servicer to see if you qualify for an income-driven repayment plan.

Appendix 5. Alternate subject instructions.

You now have the option to participate in the Student Loan Exit Counseling.

If you decide to **participate:**
- You will have an opportunity to study student loan repayment plans and select a plan to pay off your loan.

You may also **skip** the Student Loan Exit Counseling. If you **skip**:
- You will be assigned a preselected repayment plan to pay off your loan.

At any time you can click on the "Skip Exit Counseling" button below or on any other page during the Exit Counseling to conclude student loan portion of the experiment:
- You will learn your "earnings" draws immediately after completing the Student loan Exit Counseling or clicking on "Skip Exit Counseling" button.

You can leave the experimental lab as soon as you see your final earnings on your screen.

Appendix 6. List of available student loan plans and descriptions.

PLAN TYPE	WHO QUALIFIES?	YEARS TO REPAY	MONTHLY PAYMENT	PUBLIC SERVICE LOAN FORGIVENESS AFTER 10 YEARS?	OUTSTANDING DEBT CANCELLED AT THE END OF REPAYMENT?
Standard	All	10	Same throughout repayment.	No[4]	No[5]
Graduated	All	10	Increases every 2 years.	No	No
Extended	Direct[1] and FFEL[2] loans from after Oct. 7, 1998, greater than $30,000 balance.	25	10% or 15% of discretionary income. Changes with income.	No	No
Income-Based Repayment	All who have a partial financial hardship.[3]	20 or 25	10% or 15% of discretionary income. Changes with income. Will never be more than under Standard plan.	Yes	Yes
Pay As You Earn	Direct loans from after Oct. 1, 2007. Must have a partial financial hardship.	20	10% of discretionary income. Changes with income. Will never be more than under Standard plan.	Yes	Yes
Revised Pay As You Earn	All Direct loans.	20 or 25	10% of discretionary income. Changes with income.	Yes	Yes
Income-Contingent Repayment	All Direct loans.	25	The lesser of 20% of discretionary income or what you'd pay on a 12-year fixed payment plan. Changes with income.	Yes	Yes
Income-Sensitive Repayment	FFEL program loans.	10	Based on annual income. Each lender's formula for determining amount varies.	No	No

Source: Studentdebtcrisis.org (https://studentdebtcrisis.org/all-of-your-federal-student-loan-repayment-options-in-one-chart/).
Retreived November, 2019.

Appendix 7. Distribution of Subjects' Exit Counseling Skipping Choices

Table A1

	Complex		Simple		
	T9. Baseline Can Skip	T10. REPAYE Can Skip	T11. Baseline + Earnings Can Skip	T12. REPAYE + Earnings Can Skip	Total
Did not skip	0.74	0.77	0.82	0.89	0.81
Skipped before counseling	0.10	0.03	0.03	0.00	0.04
Skipped on pages 1 or 2	0.08	0.21	0.15	0.11	0.14
Skipped on pages 3 or 4	0.08	0	0	0	0.02
Obs.	39	39	39	38	155

Notes: Table shows patterns of skipping exit counseling under treatments where subjects were given the opportunity to skip and could leave when finished. Page 2 of exit counseling is where subjects/borrowers see repayment options and can select a plan.