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# The (Lack of) Anticipatory Effects of the Social Safety Net on Human Capital Investment†

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*How does the expectation that a child will receive government benefits in adulthood affect parental investments in the child's human capital? Most parents whose children receive Supplemental Security Income (SSI) benefits overestimate the likelihood that their child will receive SSI benefits in adulthood. We present randomly selected families with the predicted likelihood that their child will receive SSI benefits in adulthood. Reducing parents' expectations that children will receive benefits in adulthood does not increase investments in children's human capital. This zero effect is precisely estimated. Likely explanations include parents working more themselves, nonfinancial goals influencing investment, and families facing investment constraints.* (*JEL* G52, I26, I38, J13, J24, J31)

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There is increasing evidence that social safety net programs improve children's short- and long-term outcomes by providing financial resources to families in need (e.g., Akee et al. 2010; Dahl and Lochner 2012; Aizer et al. 2016; Hoynes, Schanzenbach, and Almond 2016; Bastian and Michelmore 2018; Braga, Blavin, and Gangopadhyaya 2020; Aizer, Hoynes, and Lleras-Muney 2022). This evidence demonstrates the positive *contemporaneous* effects of the social safety net, but the existence of the social safety net could also have *anticipatory* effects that are likely to be negative: does the anticipation of government benefits in adulthood reduce human capital investment in childhood? If so, such anticipatory behavior could cause irreversible harm to children and increase the cost of redistributing through the safety net.

In a simple economic model, the anticipation of benefits in adulthood decreases human capital investment through income and substitution effects. Through the income effect, expected government transfers reduce the child's expected marginal utility of earned income in the future, lowering the expected utility return to investment in human capital. That is, parents invest less in human capital because they do not expect their child to "need" money from working in the future. Through the substitution effect, parents invest less in human capital because, due to the phaseout rules for transfer programs, a child's adult benefits will be reduced if they work as an adult. That is, parents invest less in human capital because they anticipate a high effective marginal tax rate on their child's earnings in the future. We call these income and substitution effects collectively the "dynamic discouragement" effect of the social safety net (or, more broadly, the redistributive tax-and-transfer system) on human capital.

Several theoretical models of human capital investment feature this dynamic discouragement effect (e.g., Guvenen, Kuruscu, and Ozkan 2014; Heathcote, Storesletten, and Violante 2017; Stantcheva 2017). Macroeconomic models estimating the potential impact of expanding the social safety net through universal basic income find that it would have dynamic discouragement effects on human capital accumulation (Luduvice 2021; Daruich and Fernández forthcoming). In empirical work, human capital investments have been shown to be responsive to many types of dynamic or anticipatory considerations, such as life expectancy (Jayachandran and Lleras-Muney 2009; Oster, Shoulson, and Dorsey 2013) and adult earnings returns (Jensen 2010). Abramitzky and Lavy (2014) find that when Israeli kibbutzim move from equal wages to productivity-based wages, young people increase their educational achievement. Although direct evidence on the dynamic discouragement effect of the social safety net is limited, experts on child development think this effect is large, according to a survey we conducted.[1]

However, there are several reasons to believe that there might be less dynamic discouragement than the simple benchmark model predicts. Households may not be forward-looking enough to change behavior today in response to future government benefits (e.g., Ganong et al. 2022; Fang and Silverman 2009). Or, households may be constrained to invest less than they would like due to limited money, time, or

[1] We conducted this survey in 2022 through the Social Science Prediction Platform. We describe the survey in more detail in Section IIF.

bandwidth, which could mean that any dynamic discouragement is inframarginal to actual investment. Alternatively, parents may make decisions about human capital investment based on nonfinancial objectives (such as finding purpose in work) in addition to the financial objectives included in the benchmark model, which could dampen the response. Thus, the existence and magnitude of the dynamic discouragement effect are ultimately empirical questions.

In this paper, we test how beliefs about the availability of government benefits in adulthood affect human capital investment in childhood using an experiment that exogenously changes people's beliefs. Our context is the Supplemental Security Income (SSI) program—the largest cash welfare program in the United States, which, in 2022, spent \$51 billion on cash payments to 1.0 million children and 5.5 million adults with disabilities and low incomes. Most children receiving SSI qualify for the maximum annual benefit of around \$10,000 per year, which is about half of household income for the median family (Deshpande 2016a). The majority of children receiving SSI qualify on the basis of mental and behavioral conditions, such as ADHD.[2] Understanding human capital investment among children who receive SSI benefits is critical since they have very poor adult outcomes (Davies, Rupp, and Wittenburg 2009; Deshpande and Mueller-Smith 2022).

When children who receive SSI turn 18, they are reevaluated for SSI as adults. Because the SSI criteria are different for children and adults, many SSI children do not qualify for SSI benefits as adults and lose both the cash benefits and categorical Medicaid eligibility when they turn 18. In fact, nearly 40 percent of SSI children and 70 percent of those with mental and behavioral conditions are removed from benefits at age 18, as they do not qualify as adults (Hemmeter and Gilby 2009).

However, many parents whose children receive SSI are unaware that their children could lose benefits in adulthood. In our experimental sample, the average predicted likelihood that a child will be removed from SSI at age 18 (based on Social Security Administration data) is 70 percent, yet more than half of the parents in the sample believe there is *no chance* their child will stop receiving SSI benefits in the coming years. The average belief of the likelihood of removal is just 20 percent. These inaccurate beliefs about the likelihood of future benefits could lead parents to underinvest in their children's human capital. The income effect is the most obvious channel for underinvestment since the annual SSI benefit is about half of household income for this population. The substitution effect may also play a role since parents in our sample are aware that their child's SSI benefits will be reduced in adulthood if the child works.

We conducted a randomized controlled trial (RCT) with about 6,000 parents of a national sample of children (aged 14–17 years) who receive SSI. Our RCT randomly provided parents with information on their child's predicted likelihood of removal from SSI at age 18. We use this information shock as a source of exogenous variation in expectations to determine how expectations about government benefits in adulthood affect human capital investments in childhood. To deliver the information, we showed each parent in the treatment group a video that told them their

[2] These figures come from Social Security Administration (2022) and Social Security Administration (2021) (Tables 18 and 20).

child's specific likelihood of removal and the consequences of removal. For example, parents of children with a 70 percent removal probability (the median) watched a video telling them that 70 percent of children with similar characteristics to their child lose benefits at the age of 18. The video then emphasized that their child "will most likely not receive SSI benefits as an adult. If that happens, they will not receive any monthly payments from SSI … and they will need to find other sources of income to support themselves."

Our survey data show that treated parents understood the information and the gravity of the situation: they updated their perceived likelihood of removal for their child by 20 percentage points (pp) relative to the control group, they expressed 10 pp greater demand than the control group for a hypothetical insurance product to insure them against the loss of SSI benefits, and they were 9 pp more likely to make plans to work more themselves in the future if already employed. Despite these strong responses to the information, parents did not increase their take-up of human capital investments for their child—specifically, the resources we offered, including tutoring and job training services, which evidence suggests could increase children's future earnings. We estimate a treatment effect of information on average take-up of these human capital investments of virtually 0, just −0.2 pp. This effect is precisely estimated: we can rule out that information increased take-up of the investments by more than 1.5 pp, off of a base of roughly 30 percent.

We also find treatment effects that are close to 0 when we restrict to the 80 percent of the sample who underestimated the likelihood of removal at baseline or the 60 percent of the sample who thought there was no chance of removal at baseline. In fact, we cannot rule out a zero effect for any observable subgroup we look at, including parents who believe in a high return to human capital, parents who strongly believe the resources we offer would help their child succeed in school, and parents who feel they have the capacity to plan for the future.

This result is surprising relative to the benchmark model: most parents were unaware at baseline that their children might lose benefits, understood the information we provided and updated their beliefs, believed the loss of SSI would be a major income shock, and even changed their own plans to work in the future—yet they did not respond by investing more in the human capital of their child. The result is also a surprise relative to predictions by scholars with expertise in education and child development. In a survey we conducted, these experts predicted, on average, that the treatment effect would be a positive 14 percentage points. Our experiment strongly rejects this null hypothesis. We can also reject the null hypothesis generated from calibrating the Heathcote, Storesletten, and Violante (2017) model, designed to estimate the effect of taxes on skill investment, to the specific parameters of the SSI program.

We next turn to understanding why decreasing the perceived likelihood of future benefits does not increase human capital investment as the benchmark model would predict. We first present evidence of the validity of our finding of no dynamic discouragement. In particular, we show (i) that information leads to a large and persistent change in parents' beliefs about SSI removal (i.e., strong first stage), (ii) that the resources we offer capture parents' intentions to invest in human capital (i.e., good measurement of outcomes), and (iii) that alternative channels through which information could affect take-up are negligible. Regarding (ii), both stated and revealed preferences show that parents value the resources we offer.[3] Take-up rates of our education and training resources are around 30 percent in the control group despite nontrivial monetary and/or time costs to take up the offers, and nearly 70 percent of parents at baseline say these resources would be "extremely" helpful (versus "not" or "somewhat") for their child's success in school and career.[4]

We next show that parents update their beliefs about the need for income and the return to human capital—i.e., that income and substitution effects are at play. The majority of parents believe that losing SSI would be a major financial shock, and parents also understand that receiving SSI benefits as an adult decreases the financial returns to work. Parents also express confidence in their child's abilities to work in adulthood. Why then do these updated beliefs not translate into more human capital investment?

We evaluate several hypotheses and find varying levels of support for each. The strongest evidence is for the explanation that parents have alternative plans to recover the lost income. We find that, among parents who are already attached to the labor force, information leads to an increase in the parents' plans to work in the future and in actual parent employment and earnings in the year after our experiment. We also find some evidence for the explanation that parents make decisions about their child's education based not only on financial objectives but also nonfinancial ones, such as wanting their child to achieve their potential or to avoid the stigma of dropping out of high school. While this explanation would not explain why parents do not *also* respond to financial incentives, it could help explain reduced responsiveness, especially if parents are also nearing the limit of the investments they can make subject to time and resource constraints. Indeed, we find some suggestive evidence for the constraints explanation as well.

We also find some evidence for the explanation that the wealth effect—the reduction in permanent income due to the SSI loss—chokes off some types of human capital investment. Specifically, we find a small but statistically significant negative effect of removal information on the secondary outcome of college-going plans, which suggests that parents may believe they can no longer afford college without SSI. However, this explanation cannot account fully for the null effect since there is no treatment effect even for resources like job training that are not complementary to college-going. Finally, we investigate whether high discount rates dampen investment but find little support for this hypothesis.

Our finding that SSI has minimal dynamic discouragement effects on human capital investment has important policy implications. Dynamic discouragement impedes society's ability to redistribute income. Applied to the broader safety net, our finding of minimal dynamic discouragement implies that redistribution is less costly and that income can be redistributed more efficiently than previous models implied. When considered alongside the results of our expert survey, it also suggests

[3] Regarding (iii), we rule out the explanation that informing parents about the possibility of SSI removal could lead them to decrease human capital investments if they believe that these investments increase the likelihood of removal. We designed subtreatments specifically to pick up this "perverse incentives" channel and find no evidence of it.

[4] These resources also have high returns according to objective measures; for example, estimates suggest that job training services are worth \$960–\$4,700 annually in adult earnings (Dean, Dolan, and Schmidt 1999; Dean et al. 2015, 2017; Wilhelm and Robinson 2010).

that existing adult safety net programs may do less harm to children than researchers and policymakers had thought.

To our knowledge, this is the first paper to estimate the effect of expected future government benefits on current human capital investment using exogenous variation that isolates the anticipatory channel. Previous research has estimated the combined effect of contemporaneous and anticipatory changes. Abramitzky and Lavy (2014) estimate the effect of Israeli kibbutzim changing from equal wages to productivity-based wages, which could have affected both parents' current incomes and children's anticipated incomes. They find that children's educational achievement increases and provide suggestive evidence that the channel is anticipatory rather than contemporaneous. Dahl and Gielen (2021) estimate the effect of a parent losing disability benefits, which could have affected both children's current home environment and their anticipated benefits, and find a modest increase in educational achievement. A number of studies estimate the combined contemporaneous and anticipatory effects of the 1996 welfare reform law (which included many different provisions affecting current and future benefits) on employment, program participation, education, marriage, and fertility (Kaestner, Korenman, and O'Neill 2003; Dave, Corman, and Reichman 2012; Bastian, Bian, and Grogger 2021).[5]

This paper also builds on three existing strands of the empirical literature on the effects of the social safety net on behavior. The first is a reduced-form literature on the effect of the social safety net on labor supply and human capital, which focuses primarily on contemporaneous effects.[6] Another strand is a reduced-form literature on the dynamic effects of the social safety net, primarily in the context of savings behavior (e.g., Gruber and Yelowitz 1999; Dynarski 2004). A third strand consists of structurally estimated models of life cycle behavior, especially labor supply and retirement, accounting for the effects of social insurance and other policies (e.g., Haan and Prowse 2017; De Nardi et al. 2021; Borella, De Nardi, and Yang 2023). We build on all three strands by isolating the anticipatory effects of the social safety net on the important outcome of human capital investment using a randomized shock to expectations about future government benefits for identification. In doing so, we also relate to the theoretical literature modeling the human capital investment response to taxes and transfers (e.g., Golosov and Tsyvinski 2006; Guvenen, Kuruscu, and Ozkan 2014; Heathcote, Storesletten, and Violante 2017; Stantcheva 2017) and the structural literature estimating human capital production functions and the role of parental investment (e.g., Cunha and Heckman 2007; Cunha, Heckman, and Schennach 2010).

Finally, we contribute to a growing literature on the poor life outcomes of children receiving SSI benefits and interventions to improve those outcomes

[5] The time limits feature of the 1996 welfare reform law—that recipients could receive TANF for only five years over their lifetime and for only two consecutive years—has received particular attention, with papers finding evidence of anticipatory behavior in employment and welfare use among single mothers (Grogger 2002, 2003; Grogger and Michalopoulos 2003; Mazzolari 2007; Chan 2018; Low et al. 2020).

[6] Recent studies that examine the effect of disability programs on contemporaneous labor supply include Chen and van der Klaauw (2008); Von Wachter, Song, and Manchester (2011); Maestas, Mullen, and Strand (2013); French and Song (2014); Moore (2015); Deshpande (2016a,b); Autor et al. (2017); and Strand and Messel (2019). Garthwaite, Gross, and Notowidigdo (2014) study this question in the context of health insurance and Hoynes and Schanzenbach (2012) in the context of food stamps. Recent studies that examine contemporaneous effects on human capital include Akee et al. (2010); Dahl and Lochner (2012); and Riddell and Riddell (2014), among others.

(Davies, Rupp, and Wittenburg 2009; Hemmeter, Kauff, and Wittenburg 2009; Fraker et al. 2014; Mamun et al. 2019). Deshpande (2016a) and Deshpande and Mueller-Smith (2022) study the effect of removing youth from SSI at age 18 in a context in which removal was unexpected and find that SSI removal has negative consequences for the youth. This paper asks whether removed SSI youth would have better outcomes in adulthood if their families could anticipate their removal. We find that parents do not respond to information about SSI removal by increasing human capital investments. Information provision alone is therefore unlikely to counter the adverse effects of SSI removal.[7]

## I. Context: The SSI Program

SSI provides monthly cash payments to children (1.0 million) and adults (5.5 million) who have a qualifying disability and limited income and assets (Social Security Administration 2022). The maximum federal benefit amount for an individual is $841/month ($10,092/year) in 2022, and most states provide a small supplement. This amount is roughly equivalent to average parent earnings for households of child recipients (Deshpande 2016a). SSI provides categorical Medicaid eligibility in most states. Duggan, Kearney, and Rennane (2015) provide a comprehensive review of the SSI program and literature.

SSI children must requalify for the program under the adult criteria when they turn 18. About 40 percent of all children receiving SSI, and nearly 70 percent of children with certain behavioral conditions like ADHD, are removed from SSI at the age of 18 (Hemmeter and Gilby 2009). Children are removed from SSI at high rates at age 18 because the definition of disability changes between childhood and adulthood. For adults, disability is defined as an inability to work. Adults must demonstrate that they cannot earn more than the "substantial gainful activity" limit ($1,350/month for nonblind individuals in 2022) in order to qualify for disability benefits. In contrast, eligibility for children is based on age-appropriate activity. Children must have "marked and severe functional limitations" that limit their activities, which can include social interaction and school performance. Conditions like ADHD and speech and language delays may qualify a child for SSI because they limit age-appropriate activity, but they are less likely to qualify an adult unless they are severe enough to prevent work. Children who qualify on the basis of these conditions are thus highly likely to be removed at 18, resulting in the loss of SSI cash benefits and categorical Medicaid eligibility (though, in many states, they may qualify for Medicaid on the basis of low income). In 2015, the Social Security Administration began sending families of adolescents receiving SSI annual information about the age 18 redetermination and resources available to help with the transition, but it is unclear how many families read or understand this information.[8]

[7] Other recent evaluations of programs designed to improve the outcomes of children receiving SSI include the Youth Transition Demonstration (Fraker et al. 2014) and the Promoting the Readiness of Minors in SSI demonstration (Farid et al. 2022). Other recent examples of RCTs using Social Security Administration data include Hemmeter et al. (2020) and Zhang (2023).

[8] See https://www.ssa.gov/pubs/EN-05-11005.pdf for pamphlet. Another pamphlet, distributed when an SSI award is made, mentions that children who reach 18 will be reviewed (https://www.ssa.gov/pubs/EN-05-10153.pdf). See ssa.gov/youth for a full list of resources that the Social Security Administration makes available.

In the SSI children's program, the income and assets of the parents are used to determine both financial eligibility and monthly benefit amount. The SSI payment is made to the parent or representative payee of the child. Once a child turns 18, the child's own income and assets are considered, along with in-kind support from family. The monthly SSI benefit amount is reduced based on income. After a small exclusion, the monthly benefit is reduced by $1 for every $2 of earned income. Thus, there is effectively a 50 percent marginal tax rate on earned income. The benefit completely phases out at around $18,000 in earned income. SSI conducts periodic evaluations of both medical eligibility and nonmedical eligibility for adults.

Youth who receive SSI benefits are eligible for vocational rehabilitation services, provided by state vocational rehabilitation (VR) agencies, with the goal of preparing youth with disabilities for postsecondary education and/or employment. However, take-up of VR services among children receiving SSI has historically been low, with estimates around 10–15 percent (Honeycutt et al. 2015; Hoffman, Hemmeter, and Bailey 2017).

From the National Survey of SSI Children and Families, the high school completion rate among individuals who received SSI as children is 48 percent, meaning that high school completion and preparing for the labor market are the relevant margins of adjustment for this population. As a result, our resources are focused on tutoring and job training targeted for this population. Moreover, high school completion and job readiness are largely determined by behavior in adolescence, when parents still have some influence over their children. This increases the likelihood that our intervention, which provides information to parents, could lead to behavior change.

## II. Experimental Design

Our goal is to estimate the effect of beliefs about the availability of SSI benefits in adulthood on human capital investments in childhood. The key challenge in estimating the relationship between the expected future safety net and investment is identifying exogenous variation in expectations or beliefs. Our experiment generates exogenous variation by randomly delivering information to some households about the likelihood of removal from SSI at age 18. Since this information only concerns future adult benefits, not current childhood benefits, it allows us to isolate the anticipatory effect of future benefits from the contemporaneous effect of benefits. Our experiment first measures parents' beliefs about their child's likelihood of removal from SSI at age 18. It then delivers the information. Finally, it measures the effects of the information on beliefs and investment.

In this section, we begin by explaining how we draw our sample and generate predicted likelihoods of removal, with additional details in online Appendix B. We then describe our treatment groups and the information intervention. Next, we explain the logistics of how we implemented the experiment. We then describe our data sources and outcome variables. Finally, we present baseline summary statistics.

### A. *Predicted Likelihoods and Sample Selection*

We used administrative data provided by the Social Security Administration (SSA) to implement the experiment (SSA 1951–2020, 1974–2021, 1990–2021).

First, using historical SSA data on age 18 removal decisions for children receiving SSI, we created an OLS prediction of the likelihood of removal for any given individual child based on their observable characteristics.[9] We then applied that model to the universe of all current SSI recipients to generate individual-level predicted likelihoods of removal.

Next, we used the SSA data to select a sample of households with at least one child currently receiving SSI. The main criteria for inclusion in the sample were that the child had a predicted likelihood of removal at age 18 above 35 percent and below 95 percent (roughly two-thirds of all SSI child recipients) and that the child was aged 14–17 years.[10] We drew the sample nationally from across the United States, oversampling three states (Michigan, Wisconsin, and Massachusetts) where we had connections for later administrative data as well as six additional states where we had connections to state vocational rehabilitation offices. See online Appendix B for the complete set of sample restrictions.

### B. *Treatment Groups and Intervention*

Figure 1 shows the experimental design. We first randomly divide our sample into two main groups:

*1. Treatment Group.*—Watch a video containing information about their child's predicted likelihood of removal (based on their diagnosis, severity, state, and other characteristics). Within the Treatment group, we randomized most individuals into the basic treatment group ("Information") and a small subset ("Information-Perverse") to receive an additional subtreatment discussed in detail below.

*2. Control Group.*—Watch a video with "placebo" (innocuous) information. Within the Control group, we randomized the type of placebo information (Geography or History), as discussed below.

We chose to convey the information through a video after extensive piloting of different modalities suggested that video was the most effective way to convey the information to parents. All videos, both information and control, had three sections:

(i) First Section (Same for All Groups): Introduction and brief overview of SSI, reminding parents of the basic structure of the program.

(ii) Second Section (Different across Groups): Information intervention for Treatment group, placebo information for Control group. We included the

[9] Alternative methods such as LASSO and causal forest yielded similar predictions. See online Appendix B for more details on the procedure for creating the prediction.

[10] We limited to those with an above 35 percent likelihood so as not to bother parents of children who are likely to continue on to adult SSI. We also did not include those with a likelihood above 95 percent because we did not want to tell anyone their child's removal was guaranteed (since our model is imperfect and reviews have some natural variation). We focus on children aged 14–17 years old based on guidance from counselors who work with this population that these are the key ages at which the age 18 review is close enough to be relevant but far enough to provide time for preparation.

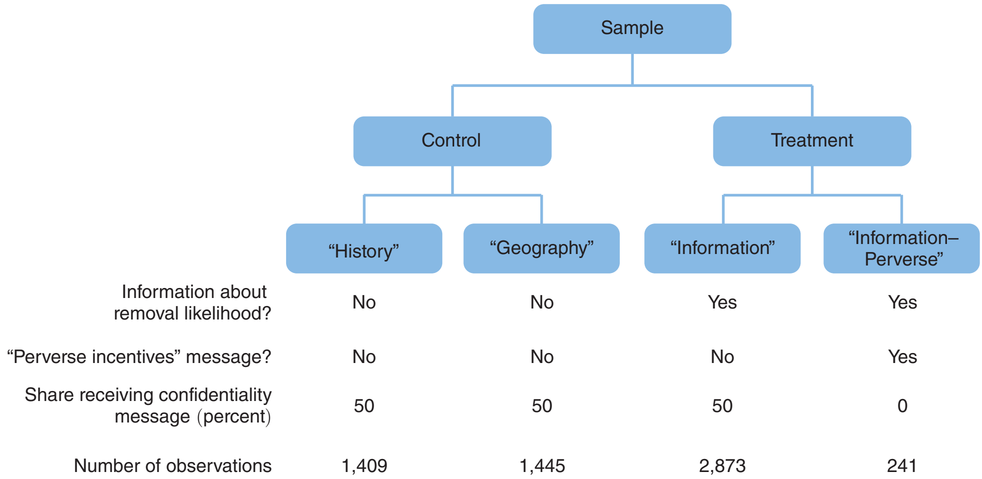


Figure 1. Experimental Design

*Notes:* The figure shows the design of the main experiment. "History" and "Geography" indicate the content of the placebo video for each subgroup. In addition to the Information video, "Information-Perverse" receives true information about what factors SSA considers in the age 18 removal decision. "Confidentiality message" indicates the randomly selected 50 percent of parents told that their take-up of resources will be kept confidential. See text for more details.

placebo to hold constant the length of the video and the salience of SSI. We discuss this content in detail below.

(iii) Third section (same for all groups): Overview of the resource center that we set up for the experiment, which parents could visit to take up free education and job training resources for their child. Section IIC has more information on the resource center.

*Information Intervention.*—The second section of the video shown to the Treatment group told parents the likelihood that their child would be removed from SSI. We began by explaining that the eligibility criteria for SSI benefits are different for adults than children and that their child would have to be reevaluated at the age of 18 to see if they still qualify for benefits. We then told parents the likelihood that their child would be removed from benefits at the age of 18 (rounded to the nearest 10 percent), explaining that the prediction was based on other children who had the same diagnosis, severity, age, and state of residence. We used several graphics, as well as qualitative descriptors, to ensure that even parents with a limited education level would understand the information. For example, the video for children with 80 percent removal rates says,

> We find that *almost all* of these children [who have the same characteristics as your child] lose SSI when they enter adulthood at the age of 18. In fact, 8 out of 10 of these children lose their SSI benefits as adults. That means that 80 percent of these children stop receiving SSI. Because these

> children have the same severe medical condition as your child, we think that your child also has an 80 percent chance of losing their SSI benefits when they turn 18. This means that your child will *most likely not receive* SSI benefits as an adult.

The specific numbers and qualitative statements were tailored to be accurate for the specific removal probability (e.g., "almost all" for 80 percent versus "most" for 60 percent). Online Appendix C contains the scripts and screenshots from the Treatment and Control videos, and the videos are available to view on YouTube.[11]

In the video, we also tried to make the consequences of removal from SSI concrete for parents. The video told parents that, if their child was removed from SSI at the age of 18, "they will not receive any monthly payments from SSI, they will not qualify for Medicaid through SSI, and they will need to find other sources of income to support themselves."

Note that the video's message is focused on the income effect (losing the income transfer) rather than the substitution effect (no longer being subject to SSI's 50 percent marginal tax rate). This is because the concept of marginal tax rates is complicated to explain, and we wanted the message of the video to be clear. That said, parents appear to be well informed about the high marginal tax rates associated with participation in SSI, and so we expect that they may have also made inferences about the substitution effect based on the video.[12]

*Control Group "Placebo" Treatment.*—For the Control group videos, the second section of the video contained "placebo" information. To verify that this information did not have an effect, we randomized control participants into one of two groups that received different information:

(i) *Geography:* Video with information about the geographic distribution of child SSI recipients across the United States

(ii) *History:* Video with information about the history of the SSI program (e.g., the year in which it was founded).

In online Appendix Table A.1, we test and find that there are no significant or meaningful differences in the effect of these two placebo videos on any outcomes, suggesting that they served their purpose of delivering innocuous information.

*Subtreatments to Assess Potential "Perverse Incentive" Effect.*—We are interested in the effect that the expectation of future benefits has on current human capital investment. However, in our setting, there is another channel through which information about the likelihood of removal from benefits could affect behavior.

[11] The Treatment video is available at https://youtu.be/57jvdStkhd4, the History video at https://youtu.be/gDxlLpTP-0o, and the Geography video at https://youtu.be/T3TgbzKGCkQ.

[12] According to our baseline survey, 62 percent of parents believe that in adulthood their child's SSI benefit will be reduced by at least 50 cents for every dollar the child earns (see online Appendix Table A.14).

Specifically, if parents believe that children with higher human capital are more likely to be removed, then increasing the perceived likelihood of removal could lead parents to decrease human capital investments in an attempt to prevent their child from being removed. The net effect of information will thus include this "perverse incentive" effect in addition to the dynamic discouragement (income and substitution) effect of interest.

In order to disentangle these effects, we implement two subtreatments. The first attempts to dampen the perverse incentives effect by telling a randomly selected 50 percent of parents that their take-up of resources will be kept confidential ("Confidentiality subtreatment"). The second attempts to amplify the perverse incentives effect by giving a small, randomly selected subset of the Treatment group (which we call the "Information-Perverse" group, shown in Figure 1) true information about what factors SSA considers in the age 18 removal decision (e.g., that the decision would depend upon whether their child is "able to earn a living as an adult" and that they would be asked to provide "information about [their] child's schooling"). Online Appendix Section B.4 provides more details on these subtreatments.

### C. *Logistics and Implementation*

We conducted our experiment from October 2021 to January 2022. We began by sampling 37,000 parents from SSA data using the criteria outlined in Section IIA. We then randomly assigned households to the groups outlined above (Information, Information-Perverse, History, and Geography), stratified based on state of residence and whether the child had an above-median removal probability for the sample selected from their state.

We then mailed letters to parents asking them to complete a web survey for a cash payment. We sent several reminder mailings during the nine weeks the survey was accepting responses (October–December 2021) and followed up by phone four weeks after the initial letter with nonrespondents. Note that the Treatment and Control groups were treated exactly the same (e.g., identical letters and phone calls) until they reached the video portion of the survey.[13] See online Appendix B for more details on implementation.

Among parents mailed letters, 18 percent started the web or phone survey. Among those who started the survey and were deemed eligible, 95 percent made it to the beginning of the treatment or placebo video, for an estimated 17 percent (= 18 percent × 95 percent) sample inclusion rate among eligibles (see Figure 2).[14] The vast majority (96 percent) of parents responded by web. Perhaps surprisingly, survey respondents do not differ meaningfully from nonrespondents on observable

[13] The only exception is that individuals assigned to the Information-Perverse group were asked one additional question in the baseline survey that the other groups did not receive: "If your child were to graduate from high school and excel academically, do you think that would make him/her more or less likely to remain eligible for SSI?" This question was part of the Information-Perverse subtreatment.

[14] Ninety-four percent of those who started the survey were deemed eligible, meaning they said that their child was receiving SSI. Our analysis sample includes all eligible parents who made it to the beginning of the treatment or placebo video. The 17 percent sample inclusion rate estimate assumes that there is the same rate of eligibility among nonrespondents as respondents. A more conservative estimate of the sample inclusion rate, which assumes that every nonrespondent is eligible, would be 16 percent.

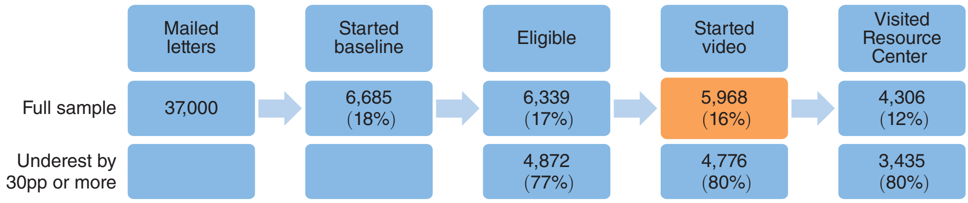


Figure 2. Response Rate at Each Experiment Phase

*Notes:* The figure shows sample size at each chronological step of the experiment. The orange box is what we use as our full experimental sample. "Mailed letters" includes every family whom we mailed a letter to. "Started baseline" is all who logged on to the web survey. "Eligible" is all who logged on to the web survey and reported that their child is currently receiving SSI. "Started video" is all who made it to the video intervention section of the web survey. "Visited Resource Center" is everyone who visited the resource center. The "Full sample" line is the full sample. In parentheses, we show the share of all who were mailed letters still in the sample at each phase. The "Underest by 30pp or more" is a count of those present at each phase who underestimated their child's removal probability at baseline by at least 30 pp. In parentheses, we show the percentage of the full sample present at that phase who underestimated their child's removal probability by at least 30 pp.

characteristics in the SSA administrative data (see online Appendix Table A.2). To further probe external validity, we estimate the treatment effects separately for earlier and later respondents in online Appendix Table A.3 and find no difference, suggesting that the treatment effects would also be similar if we had recruited more people into our sample.

**Web Survey:** The web survey, hosted by NORC at the University of Chicago and available in both English and Spanish, consisted of four parts. Parents first completed a baseline survey. They then watched the videos. They then completed an endline survey. They were then provided with a link to the resource center. We grouped all of these activities into one survey to minimize attrition between the stages based on our experience from piloting. We discuss each activity in detail here. Online Appendix I contains the full text of the web survey.

*1. Baseline Survey.—* This brief survey gathered baseline data on parents' beliefs and attitudes about their child's education and SSI benefits. The most important section assessed parents' beliefs about the likelihood with which their child would be removed from SSI in the future, which we assessed with two questions:

(i) Do you think there's any chance [KID] will stop receiving SSI benefits over the next 10 years? [*No, there is no chance that* [*his/her*] *benefits will stop.* / *Yes, there is some chance that* [*his/her*] *benefits will stop.*]

(ii) (If "Yes") How likely do you think it is that [KID] will stop receiving benefits? [*10%* (*highly unlikely to lose benefits*) / *20%* (*unlikely*) / *30%* (*some chance*) / *40%* (*could very well*) / *50%* (*good chance*) / *60%* (*likely*) / *70%* (*probably*) / *80%* (*most likely*) / *90%* (*almost certainly*) / *100%* (*certainly will lose benefits*)]

We started with the "yes/no" questions to avoid asking a complicated probabilistic question to parents who were completely unaware of the removal possibility, as we thought that could potentially be a treatment in and of itself.[15]

*2. Videos and Intervention.*— We then showed the parents the appropriate video given their treatment group and, if applicable, child removal probability. The Information and Information-Perverse groups watched the video about SSI removal at age 18 tailored for their child's removal probability (rounded to the nearest 10 percent). The History and Geography control groups watched their respective placebo videos. For all groups, we conducted a "knowledge check" after the video to ensure the parent had watched the video. Nearly all respondents got the knowledge check correct.[16]

*3. Endline Survey.*— We then conducted a brief endline survey. We first assessed parents' beliefs about their own child's removal probability. To avoid "priming" the Control group by asking too many questions about SSI removal, we asked the endline beliefs questions to the Treatment group and to a 15 percent randomly chosen subset of the Control group.

Do you think that [KID] will lose SSI benefits as an adult?

(i) [*No, won't lose benefits / Will probably not lose benefits / May or may not lose benefits / Will probably lose benefits / Yes, will definitely lose benefits*]

(ii) (If not "No, won't lose benefits"): How likely do you think it is that [KID] will lose benefits?
[*10% (highly unlikely to lose benefits) / 20% (unlikely) / 30% (some chance) / 40% (could very well) / 50% (good chance) / 60% (likely) / 70% (probably) / 80% (most likely) / 90% (almost certainly) / 100% (certainly will lose benefits)*]

Next, the survey asked qualitative questions about plans for the future and parental attitudes. It then measured two of our primary outcomes, described below. After the endline survey, the web survey told parents they had completed the survey. We then provided parents with a link and log-on information for the resource center.

[15] The baseline survey also included questions about demographics, the child's schooling, the child's future (e.g., expected educational achievement), perceived returns to human capital, parental attitudes (e.g., whether too early to start planning), and how helpful various resources would be to help their child excel in school and/or career. Online Appendix D contains more information about the belief measures.

[16] For the Treatment group, we asked what fraction of children with their child's removal probability were removed at age 18. We asked the Geography subgroup what fraction of SSI recipients lived in their region and the History subgroup what year SSI was founded (both statistics from their respective videos). Most participants gave the correct answer: 75 percent for the Treatment group, 73 percent for Geography, and 88 percent for History. The participants who got the initial "knowledge check" wrong were shown text screens with the information from the videos before answering the "knowledge check" question a second time. Only 5 percent for Information, 6 percent for Geography, and 1 percent for History got the question wrong both times.

*4. Resource Center.*— We set up a resource center where parents could sign up for education and employment resources, such as job training, at no charge to them (described below). At the end of the resource center, to see whether the change in beliefs persisted over time, we also asked parents another beliefs question about their perceived likelihood of their child being removed from SSI.

*5. Resources Offered (Primary Outcomes).*—To capture parental investment following the treatment, we offered parents in both the Control and Treatment groups four human capital resources for their child, which we use as our primary outcomes. Since beliefs could affect multiple types of human capital investments, we offered different types of resources. In particular, investments can be temporal or financial in nature, and they can be targeted at either education or employment. In order to measure parental responses on as many margins as possible, we prespecified four primary outcomes that reflect different types of investments (temporal versus financial, education versus employment).

We offered two of the resources, job training and math lessons, in the resource center. These resources represent *temporal* investments since they required only the parent's time to sign up.

- *Resource 1: Job Training.*—The resource center invited parents to sign up their child for free job training services and provided them with a streamlined application process. Children and adults receiving SSI are eligible to receive free job training services provided by state vocational rehabilitation (VR) agencies, but many parents do not know about the services or have trouble navigating the sign-up process. Within the resource center environment, we presented parents with the correct form for their state, invited them to complete it, and then submitted the form to the state agency on their behalf. *We interpret completing the intake forms for job training services as a temporal investment in employment potential.*[17]
- *Resource 2: Math Lessons.*— The resource center also invited parents to sign their child up for an online education platform, which we set up for the purposes of this study, where children could complete lessons in math and computer skills tailored to their grade. *We interpret signing up for online math lessons as a temporal investment in education.*

Because we knew that not all parents would visit the resource center, we also measured two alternative investments for all parents at the end of the endline survey. Both of these resources represent *financial* investments.

[17] Residents of nine states where VR agencies agreed to participate (Arizona, Connecticut, Illinois, Massachusetts, Maryland, Michigan, New Jersey, Ohio, and Wisconsin) were able to complete the forms directly through our survey. Residents of the other states were sent an email with information about how to sign up for services in their state; in these cases, we use signing up for the information as the primary outcome variable. In online Appendix Table A.4, we show results separately for those who completed sign-up forms in the survey versus those who were sent an email with information. In addition to job training, parents could also sign up for education planning through the VR resource.

- *Resource 3: Tutoring.—* At the end of the survey, we told parents that, as a thank you for their time, they would be entered into a lottery where they could choose in advance whether their prize would be $50 cash or $300 of one-on-one tutoring for their child. *We interpret choosing tutoring over cash in the lottery as a financial investment in education.*
- *Resource 4: Career Book.—* At the end of the survey, we also told parents they had two choices for how to receive the payment that we had promised them for completing the survey. They could either receive $40 cash or "$35 cash plus a career guide book for teens (worth $16) with secrets to nailing job interviews and preparing for college entrance exams." The career book, entitled *What Color Is Your Parachute? For Teens*, is one of the best-selling career books for teens on Amazon and had a list price of $16 at the time of the study. *We interpret choosing the career book over cash as a financial investment in employment potential.*

We chose these resources based on (i) parent interest from our focus groups and pilots and (ii) evidence that these resources can improve the earnings potential of young people. Evidence suggests that job training can increase earnings potential by $960–$4,700 annually (Dean, Dolan, and Schmidt 1999; Dean et al. 2015, 2017; Wilhelm and Robinson 2010), math skills training by $600–$1,600 annually, and tutoring by $1,090–$2,540 annually (Guryan et al. 2023).[18] Consistent with this evidence on returns, the majority of parents say at baseline that the job training, math skills, and tutoring resources would be "extremely" (versus "somewhat" or "not") helpful for their child to excel in school and/or their career.

We also measure a few secondary outcomes. In the endline survey, we ask parents whether they intend for their child to attend college in the future and whether they intend for their child to work in young adulthood. In addition, we measure take-up of information about ABLE savings accounts in the resource center as a measure of parents' interest in saving for their child's future.[19] Using SSA administrative data, we also evaluate effects on parents' earnings and employment in 2022, the year after the experiment.

### D. *Data, Balance, and Summary Statistics*

The analysis uses several data sources: administrative data from the SSA on households at baseline, data from the survey (including baseline survey (Deshpande and Dizon-Ross 2022), video portion, and endline)(SSA 1951–2020, 1974–2021, 1990–2021), take-up data from the resource center we created, and SSA administrative data (Master Earnings File) on parents' earnings in 2022 to measure parent responses (SSA 1951–2022).

[18] For math skills, we calculate the earnings increases combining standardized math achievement test scores increases from Barrow, Markman, and Rouse (2009) with estimates from Hanushek and Woessmann (2008), who find that 1 standard deviation increase in test scores is associated with a 12 percent increase in earnings in adulthood.

[19] ABLE accounts are savings accounts that allow SSI recipients (or parents of children receiving SSI) to save in a way that is exempt from SSI's asset limits. As part of the resource center, we offered parents information about signing up for an ABLE savings account. We use requesting an email about how to sign up for the account as a secondary outcome capturing parents' interest in saving money.

Table 1—Balance and Summary Statistics

| | Full sample | | Control vs. information | | | Control: History vs. geography | | | Treatment: Info vs. info-perverse | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | Mean (1) | SD (2) | Cntrl. Mean (3) | Info. Mean (4) | SD Diff. (5) | Hist. Mean (6) | Geo. Mean (7) | SD Diff. (8) | Info. perverse Mean (9) | Info. Mean (10) | SD Diff. (11) |
| *Panel A. Administrative data* | | | | | | | | | | | |
| Female child | 0.27 | 0.44 | 0.27 | 0.27 | −0.01 | 0.26 | 0.27 | −0.02 | 0.28 | 0.27 | 0.03 |
| Child's age | 15.58 | 0.88 | 15.58 | 15.58 | 0.00 | 15.56 | 15.60 | −0.04 | 15.53 | 15.58 | −0.05 |
| Single-parent household | 0.73 | 0.44 | 0.73 | 0.74 | −0.04 | 0.73 | 0.72 | 0.03 | 0.69 | 0.74 | −0.12 |
| Mother's age | 40.36 | 6.08 | 40.47 | 40.26 | 0.03 | 40.58 | 40.37 | 0.03 | 40.10 | 40.26 | −0.03 |
| Sibling on SSI | 0.26 | 0.44 | 0.26 | 0.24 | 0.05 | 0.27 | 0.26 | 0.01 | 0.29 | 0.24 | 0.11 |
| Months receiving SSI | 71.19 | 44.05 | 70.53 | 71.63 | −0.02 | 69.58 | 71.46 | −0.04 | 73.68 | 71.63 | 0.05 |
| Had a child medical review | 0.77 | 0.42 | 0.77 | 0.76 | 0.03 | 0.76 | 0.78 | −0.07 | 0.79 | 0.76 | 0.08 |
| Lost SSI from child medical review | 0.16 | 0.37 | 0.16 | 0.16 | 0.00 | 0.16 | 0.17 | −0.05 | 0.17 | 0.16 | 0.02 |
| Disability: Intellectual | 0.04 | 0.19 | 0.04 | 0.03 | 0.06 | 0.04 | 0.05 | −0.01 | 0.04 | 0.03 | 0.02 |
| Disability: Mental | 0.83 | 0.37 | 0.83 | 0.84 | −0.04 | 0.82 | 0.84 | −0.05 | 0.85 | 0.84 | 0.02 |
| Disability: Physical | 0.13 | 0.33 | 0.13 | 0.13 | 0.01 | 0.14 | 0.12 | 0.06 | 0.12 | 0.13 | −0.03 |
| *Panel B. Baseline survey* | | | | | | | | | | | |
| Female respondent | 0.89 | 0.31 | 0.88 | 0.90 | −0.05 | 0.88 | 0.89 | −0.01 | 0.90 | 0.90 | −0.01 |
| Parent respondent | 0.96 | 0.20 | 0.95 | 0.96 | −0.04 | 0.96 | 0.95 | 0.02 | 0.94 | 0.96 | −0.09 |
| Parent with disability | 0.41 | 0.49 | 0.42 | 0.41 | 0.02 | 0.42 | 0.42 | 0.00 | 0.41 | 0.41 | 0.01 |
| Parent did not graduate HS | 0.20 | 0.40 | 0.20 | 0.20 | 0.01 | 0.21 | 0.20 | 0.02 | 0.21 | 0.20 | 0.02 |
| Child receiving edu. accommodations | 0.76 | 0.43 | 0.75 | 0.77 | −0.03 | 0.75 | 0.75 | −0.00 | 0.78 | 0.77 | 0.04 |
| Child grade | 9.52 | 1.06 | 9.53 | 9.52 | 0.01 | 9.50 | 9.56 | −0.06 | 9.41 | 9.52 | −0.10 |
| Race: White | 0.41 | 0.49 | 0.42 | 0.39 | 0.06 | 0.42 | 0.43 | −0.02 | 0.39 | 0.39 | −0.01 |
| Race: Black | 0.44 | 0.50 | 0.43 | 0.45 | −0.04 | 0.45 | 0.42 | 0.06 | 0.42 | 0.45 | −0.07 |
| Race: Other | 0.04 | 0.20 | 0.04 | 0.04 | 0.01 | 0.05 | 0.04 | 0.06 | 0.07 | 0.04 | 0.11 |
| Ethnicity: Hispanic/Latino | 0.18 | 0.38 | 0.17 | 0.18 | −0.02 | 0.17 | 0.18 | −0.04 | 0.18 | 0.18 | 0.01 |
| *Panel C. Removal probability* | | | | | | | | | | | |
| Predicted likelihood of removal | 69.60 | 11.83 | 69.45 | 69.80 | −0.03 | 69.59 | 69.32 | 0.02 | 68.98 | 69.80 | −0.07 |
| Perceived likelihood of removal | 20.04 | 29.11 | 20.04 | 19.94 | 0.00 | 20.57 | 19.52 | 0.04 | 21.08 | 19.94 | 0.04 |
| Belief gap | −49.58 | 30.96 | −49.39 | −49.91 | 0.02 | −48.97 | −49.81 | 0.03 | −47.80 | −49.91 | 0.07 |
| Thought no chance of removal | 0.60 | 0.49 | 0.60 | 0.60 | 0.01 | 0.60 | 0.61 | −0.04 | 0.60 | 0.60 | −0.01 |
| *Test for joint orthogonality* | | | | | | | | | | | |
| $F$-stat | | | | | 0.82 | | | 0.80 | | | 0.98 |
| $p$-value | | | | | 0.92 | | | 0.93 | | | 0.55 |
| Number of individuals | 5,968 | | 2,854 | 2,873 | | 1,409 | 1,445 | | 241 | 2,873 | |
| Percent of sample | 100.0 | | 47.8 | 48.1 | | 23.6 | 24.2 | | 4.0 | 48.1 | |

*Notes:* The table shows summary statistics from SSA data in panel A and from our baseline survey in panels B and C. Different samples are shown in different columns. The SD columns display the difference between the means in the two previous columns, normalized by the square root of half the sum of the two group variances. "Had a child medical review" means the child previously received a regularly scheduled reevaluation of their medical condition to determine if they should continue to receive SSI benefits. "Predicted likelihood of removal" is the OLS prediction of the child's likelihood of removal at age 18 as specified in online Appendix B. "Belief gap" is the gap between "Perceived likelihood of removal" (parents' beliefs about their child's likelihood of removal, as measured through our baseline survey) minus the "Predicted likelihood of removal." Covariate balance test for the Confidentiality subtreatment (not shown) has $p$-value of 0.93.

Table 1 presents baseline summary statistics and tests for balance across groups. Sampled children were 16 years old on average, and 27 percent were female—reflecting the fact that mental and behavioral conditions are more likely to be diagnosed in boys than girls (Sciutto and Eisenberg 2007; Bitsko et al. 2022). Eighty-three percent of children have mental disability diagnoses.[20] Seventy-three percent came

[20]The five most common diagnoses are ADHD (43 percent), speech/language delays (15 percent), learning disorder (7 percent), autistic disorders/other pervasive development disorders (5 percent), and oppositional/defiant disorder (5 percent).

from single-parent households. Eighty-nine percent of respondents were female and 96 percent were the child's parent, with the remainder primarily relatives who live with the child. We refer to all respondents as "parents."

To assess balance across groups, we test for the joint orthogonality of all baseline variables with our various treatments: Treatment versus Control, History versus Geography, and Information versus Information-Perverse. We fail to reject the null of orthogonality in any of these tests. Moreover, the differences across groups are never large. Following Imbens and Rubin (2015), we assess the size of the differences in our various baseline measures. All of our normalized differences, presented in the "Std. Diff." columns, are far below the "cutoff" of 0.25 SD, which indicates good balance.

### E. *Mechanism Experiment*

We conducted a "mechanism experiment"—another study round designed to understand the mechanisms behind our main estimates—from January to April 2022. The mechanism experiment had a similar design to the main experiment but with a smaller sample (approximately 1,000 responses) and a few important changes to assess mechanisms:

- We asked a question about a hypothetical insurance product that would pay out after the child turns 18 *if* the child were not receiving SSI.
- We asked parents about their own work plans and (for Treatment group parents only) how they would handle the loss of SSI income if their child lost SSI at age 18.
- Instead of sending the link to the resource center immediately after parents had completed the survey, we waited a few days to send the link to allow for a "cooldown" period for emotions to settle and parents to process the removal information.[21]

### F. *Expert Prediction Survey*

In February 2022, we also conducted a survey through the Social Science Prediction Platform. The goal of the platform is to collect expert forecasts regarding the results of experiments in order to develop a more realistic null hypothesis to test. We sent the survey to all members of the NBER Children's and Education groups. Of the 243 individuals in the sample, 64 members completed the survey, a

[21] Due to logistical constraints, links were sent out once a week, and so the exact cooldown period length depended on the day of week the person received their survey. On average, participants who visited the resource center in the mechanism experiment visited it 11 days after they completed the endline survey, as compared with 30 hours after completion in the main experiment. Online Appendix Table A.15 shows summary statistics and tests for balance across Treatment and Control groups for the mechanism experiment, and online Appendix Figure A.1 shows the response rate at each survey phase. Unlike the main experiment, the mechanism experiment was nationally representative and did not oversample particular states. The mechanism experiment stratified on region of residence, rather than state of residence, because of the smaller sample size. In addition, due to limited sample size, the mechanism experiment did not include an Information-Perverse group. See online Appendix Table A.16 for estimates of the effect on primary outcomes in the mechanism experiment, which are all consistent with the effects in the main experiment.

26 percent response rate. The survey described to participants the study design, the size of the first-stage effect on beliefs, and the average in the Control group of a (relatively easy-to-describe) outcome: whether participants took up either resource in the resource center. The survey then asked participants to predict the take-up in the Treatment group of that same outcome (i.e., to predict the treatment effect). As recommended by the Social Science Prediction Platform, we use the mean from this survey as a null hypothesis that we test. The full text of the survey is in online Appendix H.

## III. Effects of SSI Removal Information on Beliefs and Human Capital Investment

We begin by briefly summarizing our hypotheses for how information about SSI removal might affect investment. We then proceed to analyze the actual effect of providing information on beliefs and investment.

### A. *Hypotheses for the Effect of Information*

In a simple economic model, providing information that decreases the perceived likelihood of future benefits could increase human capital investments through income and substitution effects. Via the income effect, the large loss in expected future unearned income increases the marginal utility of future earned income, thereby increasing the marginal utility returns to investment in human capital. Through the substitution effect, the decrease in the expected future tax rate increases the net financial returns to earned income, thereby increasing the financial return to investment in human capital.

The combined "dynamic discouragement" effect from the income and substitution effects could have a quantitatively meaningful effect on human capital investment in our context. For example, when we calibrate the Heathcote, Storesletten, and Violante (2017) model using an SSI-like transfer program, the model results imply an 11 percent increase in investments as a result of our information treatment (see online Appendix G for details). Separately, in our survey of experts, virtually all respondents (97 percent) predicted a positive treatment effect, and the average of experts' predictions of the treatment effect of information is a large 14 percentage point or 34 percent increase.[22]

However, there are other factors that could decrease the magnitude of the treatment effect. For example, households may not be as forward-looking as the simple model assumes, or they might face constraints on investment that dampen their responsiveness. In this section, we provide empirical evidence on whether the dynamic discouragement effect exists.

[22] We also conducted qualitative interviews with counselors and other individuals who work with families of children receiving SSI benefits, and they also thought that advance knowledge of SSI removal would lead to higher educational attainment.

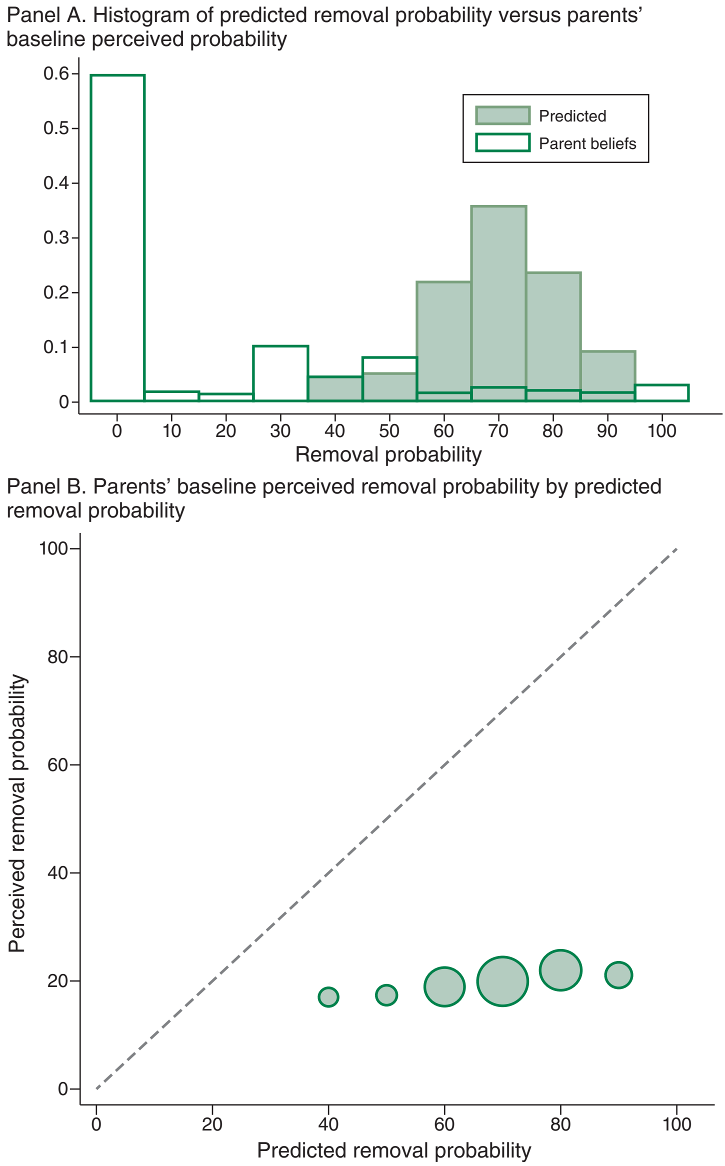


Figure 3. At Baseline, SSI Parents Underestimate the Chance That Their Child Will Lose Benefits

*Notes:* Panel A presents a histogram of predicted removal probability, as measured by our OLS prediction using SSA data, and parents' baseline perceived removal probability, as measured through our baseline survey. Panel B presents average perceived removal probability at baseline by predicted removal probability, where the size of the marker corresponds to relative group size. Predicted removal probability comes from our OLS prediction. Baseline beliefs are responses to the baseline question "How likely do you think it is that [KID] will stop receiving SSI benefits over the next 10 years?" This question is asked of respondents who respond to the preceding question "Do you think there's any chance [KID] will stop receiving SSI benefits over the next 10 years?" with "Yes, there is some chance that [his/her] benefits will stop." For those who respond "No, there is no chance that [his/her] benefits will stop," we code their perceived likelihood of removal as zero. Online Appendix Figure A.2 shows a histogram of the gap, at baseline, between predicted and perceived removal probabilities. Online Appendix Section D includes more information on the measurement of parent beliefs. Panel A sample size: observations = 5,968. Panel B sample size: observations = 5,968.

### B. *Beliefs about SSI Removal*

We use our experiment to generate exogenous variation in parents' beliefs about their child's SSI receipt in adulthood in order to identify the dynamic discouragement effect. This strategy relies on parents being misinformed at baseline about their child's likelihood of removal from SSI. Figure 3 shows that parents of children receiving SSI substantially underestimate their child's likelihood of removal at baseline. Panel A presents separate histograms of parent beliefs about their child's likelihood of removal, as measured through our baseline survey (see Section IIC for exact question), and their child's true predicted likelihood of removal, as measured by our prediction using SSA data.[23] About 80 percent of parents underestimate the likelihood that their child is removed. The median parent underestimates the likelihood by 60 pp, and the mean gap is 50 pp (see Table 1). About 60 percent of parents believe the likelihood of removal is 0, suggesting that the majority of parents are unaware of the age 18 review. Panel B of Figure 3 shows that average perceived removal probability does not increase with predicted removal probability. In other words, parent beliefs about removal are not even correlated with predicted removal probability, likely because many parents appear to be unaware of the existence of the age 18 review altogether.

Figure 4 shows that our information video caused parents to update their beliefs. The figure shows a histogram of the *endline* beliefs gap (endline perceived likelihood of removal minus predicted likelihood of removal) separately for the Treatment and Control groups. The Treatment group endline distribution is notably different from the Control distribution, with the Treatment group having a 20 pp smaller median beliefs gap. Roughly 22 percent of parents in the Treatment group have an endline gap of 0 (up from 3 percent at baseline): they fully update their beliefs based on the information.[24] Many other parents in the Treatment group update their beliefs to be somewhere between their baseline and the information delivered—i.e., the mass at smaller beliefs gaps increases relative to larger beliefs gaps.[25]

[23] Parents who answered "No" to the first baseline beliefs question ("Do you think there's any chance [KID] will stop receiving SSI benefits over the next 10 years?") are coded as having a perceived likelihood of zero. Because parents could only report their perceived likelihood of removal rounded to the nearest 10 percent, we calculate the beliefs gap from the predicted removal probability rounded to the nearest 10 percent as well. Using the unrounded predicted removal probability does not change the results. Whether using unrounded or rounded measures, 77 percent of parents underestimate their child's probability of removal. The average gap using both the unrounded and rounded predicted removal probability is 50 pp.

[24] Recall that the endline beliefs question is asked of only 15 percent of the Control group to avoid priming them to think about SSI removal. Note that the Control group beliefs also shift, with the median beliefs gap in the Control group falling from 60 pp to 40 pp. This could be the result of priming (that simply asking about the likelihood of removal causes Control group parents to update their beliefs to some extent) or of the baseline and endline questions being phrased differently. Panel A of online Appendix Figure A.5 shows the distribution of endline beliefs separately between the Treatment and Control groups, with consistent results.

[25] We do not expect all parents to fully update to the number we provide since parents could have private information about their child's condition or strong prior beliefs. For a random subset of parents whose endline perceived removal probability is more than 30 pp below the predicted removal probability, we ask an open-ended question of why they think their child's removal probability is different than that of children with similar observable characteristics. Most of these parents mention their child's diagnosis, while others say that they think their child's condition is particularly severe or permanent, or is deteriorating.

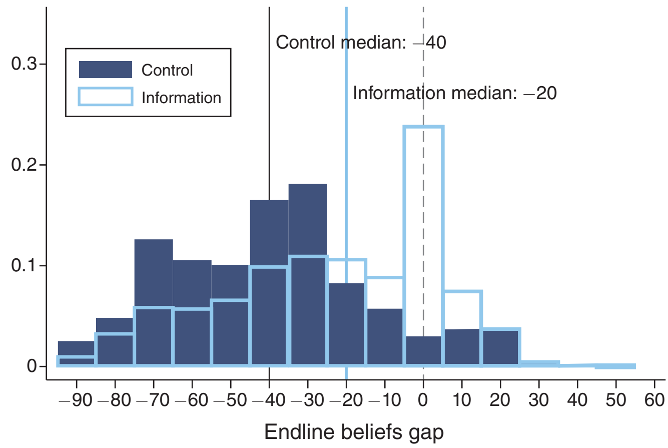


Figure 4. In Response to Information, Treated Parents Update Beliefs Relative to Control Parents

*Notes:* The figure shows histograms of the gap between a parent's beliefs about their child's likelihood of removal after the intervention, as measured through our endline survey, minus their child's true predicted likelihood of removal, as measured by our prediction using SSA data. Histograms are shown separately for the Control group members from whom we collected endline beliefs and the Information group. We exclude the Information-Perverse group. Endline beliefs are responses to the endline question "How likely do you think it is that [KID] will lose benefits?" This question is asked of respondents who respond to the preceding question "Do you think that [KID] will lose SSI benefits as an adult?" with anything other than "No, won't lose benefits." For those who respond "No, won't lose benefits," we code their endline beliefs as zero. See online Appendix Section D for more information on the measurement of endline beliefs. See online Appendix Figure A.3 for a version of this figure limited to the subsample that underestimates their child's likelihood of removal by 30 pp or more at baseline. Sample size: Control observations = 436; Information observations = 2,758.

We now test more formally whether information affected beliefs by estimating the following regression equation:

$$Y_i = \alpha + \beta Information_i + \gamma \mathbf{X}_i + \varepsilon_i, \tag{1}$$

where $Y_i$ here is parent endline beliefs and will later represent other outcome variables; $Information_i$ is an indicator for whether the child was assigned to the Information group; and, following our pre-analysis plan, $\mathbf{X}_i$ is a vector of controls selected using double-LASSO along with stratum fixed effects.[26] Again, following our pre-analysis plan, when estimating equation (1) (both here using endline beliefs as the outcome and later using our other outcome variables), we (i) exclude the Information-Perverse group and (ii) restrict to the 80 percent of individuals who underestimate the likelihood of removal by at least 30 pp at baseline ("underestimators") since we expected the first-stage effect of information on beliefs to be the most positive among individuals who underestimated their removal probability the most.[27]

[26] For consistency with our later results using our primary outcomes, we use here the controls selected by double-LASSO for all of our primary outcomes pooled. Using controls selected by double-LASSO for parent endline beliefs rather than those selected for our pooled primary outcomes does not meaningfully change our results. Online Appendix Table E.3 lists the controls selected by LASSO.

[27] Our data align with this hypothesis. The estimates in column 2 of Table 2 show that someone who underestimates more (and hence has a higher value of removal probability − baseline beliefs) would have a more positive first-stage effect on beliefs.

Table 2—Information about SSI Removal Affects Beliefs about SSI Removal

| Dependent variable: | Endline perceived probability of SSI removal | | | |
|---|---|---|---|---|
| | Underestimators | | Full beliefs sample | |
| | (1) | (2) | (3) | (4) |
| *Information* | 18.46 | −5.85 | 16.08 | −3.67 |
| | [1.24] | [6.11] | [1.34] | [5.42] |
| *Information* × *Removal Prob.* | | 0.35 | | 0.33 |
| | | [0.09] | | [0.08] |
| *Removal Prob.* | | −0.01 | | −0.01 |
| | | [0.11] | | [0.10] |
| *Information* × *Baseline Beliefs* | | 0.02 | | −0.13 |
| | | [0.07] | | [0.04] |
| *Baseline Beliefs* | | 0.42 | | 0.55 |
| | | [0.06] | | [0.04] |
| Control mean | 22.90 | 22.90 | 30.02 | 30.02 |
| *N* (Individuals) | 2,559 | 2,559 | 3,194 | 3,194 |
| *N* (Control) | 345 | 345 | 436 | 436 |
| *N* (Information) | 2,214 | 2,214 | 2,758 | 2,758 |
| *F*-stat | | 94.4 | | 95.9 |

*Notes:* The table shows OLS regressions where the dependent variable is the endline perceived probability of removal (on a scale from 0 to 100) as measured in our endline survey. The sample in regressions (1) and (2) is the Information group (i.e., Treatment group excluding Information-Perverse) and the subset of the Control group we collected endline beliefs from, with both groups limited to individuals who underestimated their child's likelihood of removal at baseline by at least 30 pp. The sample in regressions (3) and (4) is the Information group and the subset of the Control group we collected endline beliefs from. All regressions include controls selected by double-LASSO for a specification that pools all four of our primary outcomes, along with stratum fixed effects. See online Appendix Table E.3 for a list of selected controls.

Column 1 of Table 2 presents the results. Parents in the Information group believe that their child's removal probability is 18 pp higher than parents in the Control group, or a roughly 80 percent increase in the mean perceived removal probability relative to the Control group mean of 23 percent. Thus, our information treatment had a significant impact on beliefs, allowing us to use the treatment as a source of exogenous variation to estimate the effect of beliefs on investments. Column 3 of Table 2 shows similar results using the full sample.

Recall that we ask a final qualitative beliefs question at the end of the resource center, asking people to report the likelihood of removal on a five-point Likert scale. Since (as shown in online Appendix Table A.5) there is no selection based on treatment status in who visits the resource center, we can use this final beliefs question to test whether the effect on beliefs persisted or dissipated after the information delivery.[28] We find that the treatment effect on beliefs persisted. As shown in panel B of Figure 5, the Information group's beliefs remained significantly different than the Control group's, with the size of the treatment effect similar to that measured in the endline survey (panel A of Figure 5).

[28] Because not all respondents visited and/or reached the end of the resource center, only 50 percent of the respondents eligible to answer the question answered it. (Recall that we only asked the question of control group members who were also asked the endline beliefs question.)

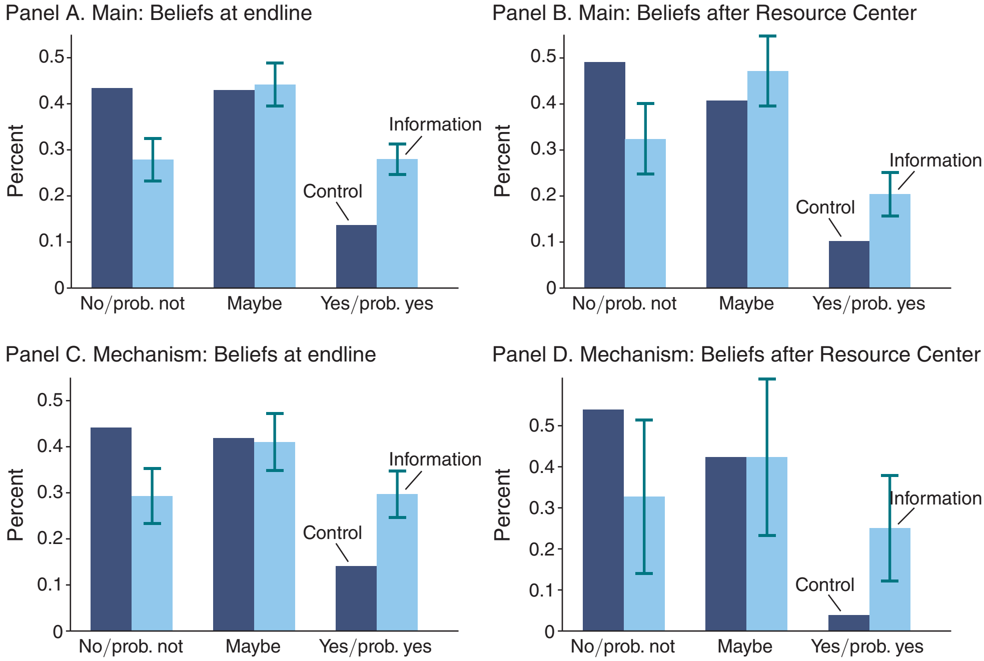


Figure 5. Effect of Information on Beliefs Persists beyond End of Survey

*Notes:* Panels A and C show responses in the main and mechanism experiments, respectively, to the beliefs question measured at endline "Do you think that [KID] will lose SSI benefits as an adult?" The response options were "No, will definitely not lose benefits" / "Will probably not lose benefits" / "May or may not lose benefits" / "Will probably lose benefits" / "Yes, will definitely lose benefits." Panels B and D show responses to the same question at the end of the resource center. Respondents were given a link to the resource center immediately after completing the endline survey in the main experiment but several days after in the mechanism experiment. The sample for panels A and C is the Information group and the random subset of the Control group from whom we gathered endline data; the sample for panels B and D additionally limits to those who completed the resource center and answered the question measured there. Thirty percent of the sample were asked and answered the resource center beliefs question in the main experiment compared to 11 percent in the mechanism experiment. Panel A sample size: Control observations = 438; Information observations = 2,766. Panel B sample size: Control observations = 167; Information observations = 1,509. Panel C sample size: Control observations = 442; Information observations = 451. Panel D sample size: Control observations = 52; Information observations = 52.

## C. *The Effect of Beliefs on Human Capital Investment*

Having shown that our information treatment shifted beliefs, we now analyze the effect on investments. Figure 6 shows the average take-up of our four main outcomes separately for underestimators in the Control group and the Treatment group. Take-up is very similar between the two groups. Note that take-up of each resource in the Control group is between 20 and 40 percent, indicating that a sizable fraction of parents find the resources valuable enough to make the meaningful financial or temporal investment to take up.

Table 3 presents regression estimates, which indicate a precise zero effect of information on investments. Columns 1–4 show estimates of equation (1), again for the underestimator sample, where the dependent variable is equal to take-up of each of the main outcome variables. Column 5 shows all outcomes pooled, with standard

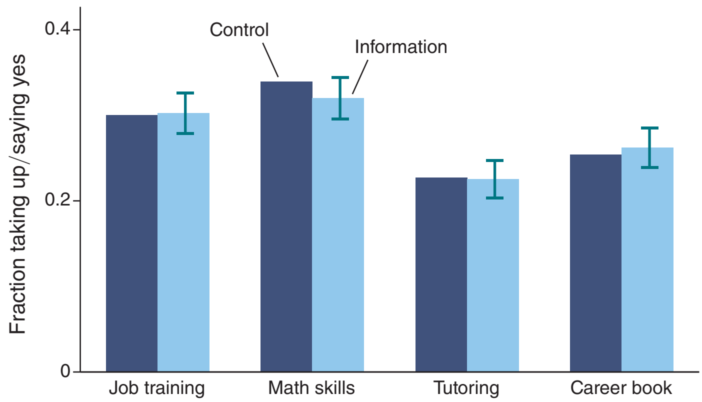


Figure 6. Providing Removal Information Does Not Increase Human Capital Investment

*Notes:* The figure shows the fraction of respondents taking up each resource and 95 percent confidence intervals. The sample limits to those who (i) underestimate the removal probability by at least 30 pp at baseline, and (ii) are not in the Information-Perverse group. "Job training" indicates completing an intake form for vocational rehabilitation services (in applicable states) or requesting information on how to sign up for those services. "Math skills" indicates signing up for the math/computer skills platform in the resource center. "Tutoring" indicates choosing the $300 in tutoring versus $50 in cash in the lottery or no response. "Career book" indicates choosing a $35 survey payment plus career book (worth $16) versus a $40 survey payment or no response. Sample sizes: Control group observations = 2,282; Information group observations = 2,307.

Table 3—Providing Removal Information Does Not Increase Human Capital Investment

| Dependent variable: | Job training (1) | Math skills (2) | Tutoring (3) | Career book (4) | Pooled (5) |
|---|---|---|---|---|---|
| *Information* | 0.002 | −0.019 | −0.002 | 0.008 | −0.002 |
| | [0.013] | [0.013] | [0.012] | [0.013] | [0.009] |
| Control mean | 0.30 | 0.34 | 0.23 | 0.25 | 0.28 |
| *N* (Individuals) | 4,589 | 4,589 | 4,589 | 4,589 | 4,589 |
| *N* (Control) | 2,282 | 2,282 | 2,282 | 2,282 | 2,282 |
| *N* (Information) | 2,307 | 2,307 | 2,307 | 2,307 | 2,307 |
| *N* (Observations) | 4,589 | 4,589 | 4,589 | 4,589 | 18,356 |

*Notes:* The table shows OLS regressions where the dependent variables are 0/1 indicators for the take-up of human capital investments. "Job training" indicates completing an intake form for vocational rehabilitation services (in applicable states) or requesting information on how to sign up for those services. "Math skills" indicates requesting log-on information for the math/computer skills platform in the resource center. "Tutoring" indicates choosing the $300 in tutoring versus $50 in cash in the lottery or no response. "Career book" indicates choosing a $35 survey payment plus career book (worth $16) versus a $40 survey payment or no response. Column 5 pools the outcomes from columns 1–4 into one regression. Robust standard errors are in brackets, except for regression (5), where standard errors are clustered at the individual level. In columns 1–4, each individual has one observation; in column 5, each individual has four observations—one observation for each resource. The sample limits to those who (i) underestimate the removal probability by at least 30 pp at baseline and (ii) are not in the Information-Perverse group. Each regression includes stratum fixed effects and controls selected by double-LASSO, shown in online Appendix Table E.3.

errors clustered at the individual level. There is no statistically significant increase in take-up of any of the outcomes. The null effect is precisely estimated. For example, the pooled specification in column 5 of Table 3 rules out (at 95 percent confidence) that information increased take-up of investments by a mere 1.5 pp relative to a control mean of 28 percent. Online Appendix Tables A.6, A.7, and A.8 show similar

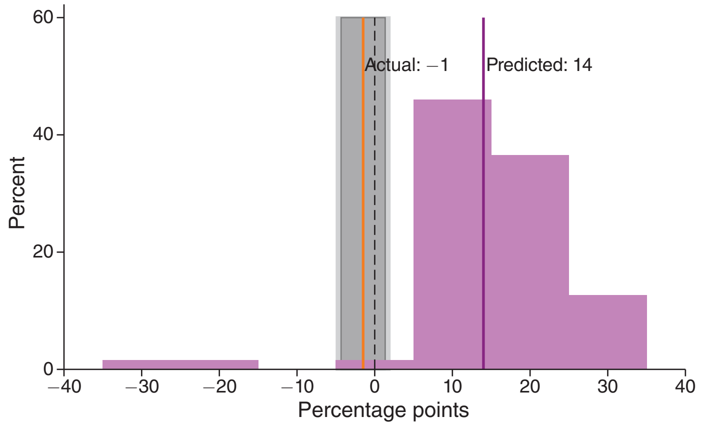


Figure 7. We Strongly Reject Expert Predictions of a Positive Treatment Effect

*Notes:* The figure shows data from our survey through the Social Science Prediction Platform and from our experiment. The purple histogram represents expert predictions of the treatment effect of information on the outcome of whether respondents who underestimated the probability of removal would take up either of the resources in the resource center. The sample for the expert predictions is 64 experts (out of 243 contacted) from the NBER Children's and Education groups. The x-axis shows the treatment effect on that outcome. The purple line denotes the mean of the expert predictions (14 pp). The orange line denotes the actual treatment effect point estimate (−1 pp). The control group mean of the outcome is 41.8 percent. The gray region represents the 95 percent CI around the actual treatment effect, with the inner CI being the 95 percent CI for rejecting any fixed null and the lighter (slightly larger) CI being the 95 percent CI for rejecting the mean of the predicted treatment effect distribution, accounting for the fact that the mean is a noisy estimate of an underlying population parameter. The full text of the survey is in online Appendix H.

estimates using the full sample (i.e., not restricting to underestimators), excluding control group members who received endline beliefs questions (in case there are priming effects), and excluding controls, respectively.

The effect of 1.5 pp is small relative to other differences in take-up: 8 pp between parents who do and do not believe college has a high return, 11 pp between parents who are above median and below median for thinking it is too early to plan for their child's future, and 17 pp between parents who believe the resources are "extremely" useful and those who do not (online Appendix Table A.9).

In addition to testing the null hypothesis of zero, we also test and reject the null hypotheses generated both by our calibration of Heathcote, Storesletten, and Violante (2017) and by our expert survey. Turning first to the Heathcote, Storesletten, and Violante (2017) model, the "base case" model and calibration assumptions imply a treatment effect of 11 percent (see online Appendix G for details). The top of our confidence interval rules out an effect of 5 percent (1.5 pp on a control mean of 28 percent). Even if the baseline assumptions used in the Heathcote, Storesletten, and Violante (2017) model overestimate the effect by a factor of 2, our confidence intervals would still rule out its predicted effect.

We can also rule out the null hypothesis generated by our expert survey, the results of which are shown in the purple histogram in Figure 7. The average of the predicted treatment effects is 14 pp, whereas our actual treatment effect outcome is

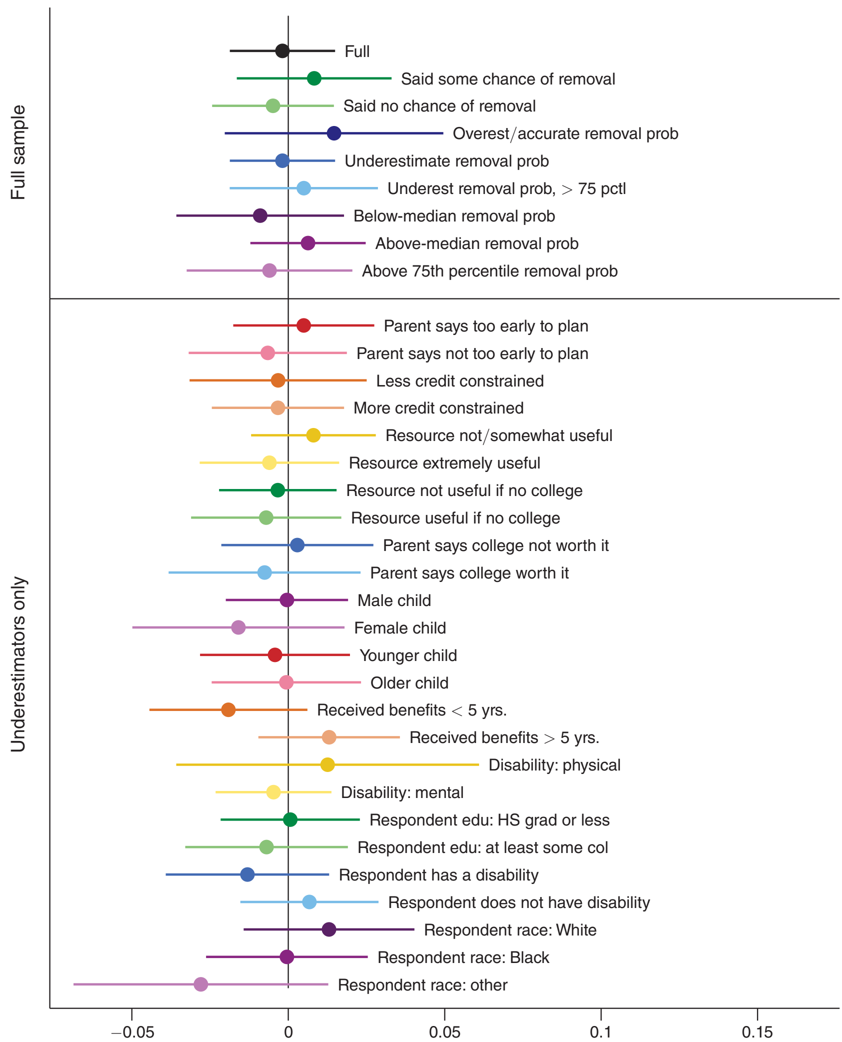


FIGURE 8. NO SIGNIFICANT EFFECTS OF INFORMATION ON HUMAN CAPITAL IN ANY SUBSAMPLE

*Notes:* The figure shows the treatment effect coefficient and 95 percent confidence interval from estimating the pooled specification from column 5 of Table 3 in different subsamples. All subsamples limit to those who are not in the Information-Perverse group. The samples below the horizontal line additionally limit to those who underestimate the removal probability by at least 30 pp at baseline. The row label indicates the specific subsample. "Said some (no) chance of removal": said some (no) chance of removal at baseline. "Overest/accurate removal prob": overestimate likelihood of removal at baseline or within 30 pp of predicted removal probability. "Underestimate removal prob": underestimate at baseline by at least 30 pp. "Underest removal prob, > 75 pctl": difference between predicted baseline removal probability and perceived removal probability above the seventy-fifth percentile. "Below-median removal prob," "Above-median removal prob," and "Above seventy-fifth percentile removal prob": below-median, above-median, or above seventy-fifth percentile value of predicted removal probability, respectively. "Parent says too early to plan" and "Parent says not too early to plan": below-median and above-median responses to baseline question regarding whether too early to plan for child's future. "Less credit constrained" and "more credit constrained": below-median and above-median responses to baseline question about likelihood of affording college. "Resource extremely useful" and "Resource not/somewhat useful": defined at respondent × resource level, indicators for saying resource "extremely useful for the child's future income and career" (versus something less than that). "Resource not useful if no college" and "Resource useful if no college": defined at respondent × resource level, indicators for saying resource would be helpful even if child does not attend college. "Parent says college worth it" and "Parent says college not worth it": indicators for parent saying they think increase in earnings from attending a four-year college enough to cover the cost. "Younger child" and "Older child": below-median and above-median child age. "Disability: physical/mental": child disability type. "Respondent edu/disability/race": self-reported. See online Appendix Table A.9 for estimates.

Table 4—Providing Information Does Not Increase College, Work, or Savings Plans

| Dependent variable: | Thinks child will go to college (1) | Thinks child will work (2) | Savings account (3) |
|---|---|---|---|
| *Information* | −0.057 | −0.002 | −0.014 |
| | [0.013] | [0.014] | [0.012] |
| Control mean | 0.51 | 0.58 | 0.21 |
| *N* (Individuals) | 4,394 | 4,379 | 4,589 |
| *N* (Control) | 2,186 | 2,183 | 2,282 |
| *N* (Information) | 2,208 | 2,196 | 2,307 |

*Notes:* The table shows OLS regressions where the dependent variables are 0/1 indicators for if the parent thinks the child will go to college or if the child will work as an adult, as measured in our endline survey, and if the parent requests information about the ABLE savings account in the resource center. All regressions include a vector of controls selected by double-LASSO, shown in online Appendix Table E.3, as well as stratum fixed effects. Robust standard errors are in brackets. The sample limits to those who (i) underestimate the removal probability by at least 30 pp at baseline and (ii) are not in the Information-Perverse group.

–1 pp (shown in orange). This means that we can strongly reject the null of the mean from the prediction survey, as well as 95 percent of expert predictions.[29]

The theory of dynamic discouragement effects predicts that the underestimators should have increased their investments and the overestimators should have decreased their investments. However, Figure 8 shows that treatment effects are not statistically different from zero in either group. We cannot rule out a zero effect in any other observable subgroup that we test, including parents of children with a high removal probability, parents who say it's not too early to plan for their child's future, parents who are less credit constrained, parents who say our resources are helpful to their child's success, and households with different demographic characteristics (e.g., child sex, child age, child disability, parent race).

Turning to our secondary outcomes, shown in Table 4, we do not find increases in parents' expectations about their child going to college or their child working in adulthood. If anything, we estimate a statistically significant *decrease* in parents' expectations about college going. This negative effect could be a chance result, or it could reflect wealth effects combined with credit constraints: that the prospect of losing SSI income makes parents think that college tuition will no longer be affordable or that the child will need to forgo college to work instead (though again, we find no effects on work plans). We also do not find any effect of the information on parents' interest in signing up for savings accounts for their child.

*IV Specification.*—The fact that we have no treatment effect of information among samples that updated their beliefs suggests that those beliefs likely did not affect investments. Still, to formally verify this conclusion, we run an instrumental variables analysis for the following second-stage equation:

$$Y_i = \alpha + \beta EndlineBeliefs_i + \gamma \mathbf{X}_i + \varepsilon_i, \tag{2}$$

[29] The inner, darker-gray region shows the 95 percent confidence interval for rejecting a fixed null, whereas the outer, lighter-gray region shows the 95 percent confidence interval for rejecting the mean of the prediction survey, accounting for the fact that the mean is a noisy estimate of the population average.

where $Y_i$ is the outcome variable, $EndlineBeliefs_i$ is the respondent's endline beliefs about the likelihood that their child is removed, and $\mathbf{X}_i$ is a vector of controls and stratum fixed effects. To leverage the fact that our treatment had different effects on beliefs based on both the removal probability delivered and the respondent's baseline beliefs, we instrument for $EndlineBeliefs_i$ using $Information_i$ and the interactions between $Information_i$ and two prespecified predictors of how much the parent would update beliefs: the actual removal probability and the baseline perceived removal probability. We exclude the Information-Perverse group and include in the control vector the main effects of the two predictors, plus stratum fixed effects and the same controls used in the reduced-form specification. Our ex ante prediction was that $\beta > 0$.[30]

Column 4 of Table 2 shows the first-stage equation. The information treatment has heterogeneous effects with respect to both removal probabilities and baseline beliefs, and the heterogeneity is in the expected direction. Those with higher removal probabilities and/or lower baseline beliefs update their beliefs more positively. The first-stage $F$-statistic on the three excluded interactions (*Information*, *Information* interacted with baseline perceived removal probability, and *Information* interacted with predicted removal probability) is 94.[31]

We present the IV results in Table 5. Unsurprisingly given the absence of treatment effects, we find that the perceived likelihood of removal from SSI has no effect on any investment. All coefficients are numerically nearly zero. The 95 percent confidence interval on the pooled estimate in column 5 rules out that a 10 pp increase in the perceived likelihood of removal increases take-up of resources by more than 0.4 pp (1.4 percent). As a comparison, OLS estimation of equation (2) in the control group also yields point estimates that are near zero but slightly more positive in magnitude. (See columns 1 and 2 of online Appendix Table A.10 for estimates with and without the control variables.) To the extent that we expect OLS to be biased upward, the relatively small OLS estimate suggests that the null effect from our experiment is reasonable.

[30] IV estimation requires that the instrument(s) move the endogenous variable (removal beliefs) in the same direction for everyone (i.e., the monotonicity condition). In our case, it is likely that monotonicity would be violated if we only used a single instrument (Information) since 8.2 percent of parents overestimate the likelihood of removal at baseline and treatment could reduce the perceived probability of removal for them. However, our approach of interacting Information with baseline covariates that are predictive of the beliefs change (actual removal probability and baseline perceived removal probability) reduces the monotonicity concern since it allows the effect of treatment on beliefs to vary with the covariates (Słoczyński 2020). IV estimation also requires that the instrument(s) are not correlated with the outcome except through the endogenous variable (i.e., exclusion). This assumption could be violated if the treatment affects beliefs about the program other than the likelihood of receiving benefits in adulthood, such as the size of benefits. We think this is unlikely because the control and treatment groups receive the same basic program information, but we cannot rule it out entirely. Regardless, we think the IV results provide a useful scaling exercise.

[31] One complication with this approach is that, because measuring beliefs multiple times could prime the Control group to think about removal, we only measured *EndlineBeliefs* for a small subsample of the Control group. The IV approach is thus identified off of a subsample, which could reduce power. As a result, our pre-analysis plan specified that we would run the first stage and, if the first-stage $F$-stat was sufficient, proceed, and if not, run a different "predicted IV" specification. We believe a first-stage $F$-stat of 94 is likely sufficient, but we show the "predicted IV" results in online Appendix Section E.3 (online Appendix Table E.5). The results are consistent, and the estimates more precise.

Table 5—Changing Removal Beliefs Does Not Affect Human Capital Investment

| Dependent variable: | Job training (1) | Math skills (2) | Tutoring (3) | Career book (4) | Pooled (5) |
|---|---|---|---|---|---|
| *Endline Beliefs* | −0.0002 | −0.0028 | −0.0017 | −0.0012 | −0.0013 |
| | [0.0013] | [0.0013] | [0.0012] | [0.0013] | [0.0008] |
| Control mean | 0.28 | 0.32 | 0.27 | 0.30 | 0.29 |
| $N$ (Individuals) | 3,194 | 3,194 | 3,194 | 3,194 | 3,194 |
| $N$ (Control) | 436 | 436 | 436 | 436 | 436 |
| $N$ (Information) | 2,758 | 2,758 | 2,758 | 2,758 | 2,758 |
| $N$ (Observations) | 3,194 | 3,194 | 3,194 | 3,194 | 12,776 |

*Notes:* The table shows IV estimates where the dependent variables are 0/1 indicators for the take-up of human capital investments. "Job training" indicates completing an intake form for vocational rehabilitation services (in applicable states) or requesting information on how to sign up for those services. "Math skills" indicates requesting log-on information for the math/computer skills platform in the resource center. "Tutoring" indicates choosing the \$300 in tutoring versus \$50 in cash in the lottery or no response. "Career book" indicates choosing a \$35 survey payment plus career book (worth \$16) versus a \$40 survey payment or no response. Column 5 pools the outcomes from columns 1–4 into one regression. Robust standard errors are in brackets, except for regression (5), where standard errors are clustered at the individual level. In columns 1–4, each individual has one observation; in column 5, each individual has four observations—one observation for each resource. The sample limits to those who are in the Information group or in the random subset of the control group we solicited endline beliefs from. Endline Beliefs represent the endline perceived probablility of removal (on a scale of 0–100), as measured in our endline survey. Each regression includes stratum fixed effects and controls selected by double-LASSO, shown in online Appendix Table E.3. As described in online Appendix Section E.3, IV models in this table were fixed to account for a pathing issue in our results. IV results limited to the sample that completed the survey after the issue was fixed are similar, but precision is lower, as shown in online Appendix Table E.1.

## IV. Robustness: Probing the Validity of the Finding of Minimal Dynamic Discouragement

Our finding that information does not positively impact human capital investment is surprising relative to both expert predictions and the benchmark model of income and substitution effects. Why does information about SSI removal not lead parents to increase human capital investment in their children? There are two possible explanations for the null effect: that there is truly no (or minimal) dynamic discouragement of beliefs about the future safety net on human capital investment (i.e., that the effect of interest is null) or that the true dynamic discouragement effect of interest is nonzero but that our field experiment did not provide a valid or robust estimate of it (i.e., that our measurement of the effect broke down). In this section, we combine analyses from our main experiment and our mechanism experiment to rule out the latter explanation and show that our experimental findings are robust. In Section V, we investigate explanations for the absence of dynamic discouragement.

### A. *Framework for Establishing Robustness and Validity*

Our experiment measures the effect of delivering removal information to parents on their take-up of resources (Effect A):

$$\text{Delivering information} \rightarrow \text{Take-up of resources.}$$

The effect we are interested in estimating (the dynamic discouragement effect) is the effect of parents' beliefs about their child's likelihood of removal from SSI on the parents' investments in their child's human capital (Effect B):

$$\text{Beliefs about removal likelihood} \rightarrow \text{Investments in human capital.}$$

In order for our estimate of Effect A to teach us about Effect B, the following must be true. First, delivering information must impact beliefs about removal likelihood. Second, take-up of resources must be a good measure of investments in human capital. Finally, delivering information must not affect anything other than beliefs about the removal likelihood. In particular, there must not be a "perverse incentives" effect, as discussed in Section IIB, wherein information also affects beliefs about the responsiveness of removal to human capital. If all of those conditions are true, then the absence of a reduced-form effect of information on investments (Effect A) implies that there is minimal dynamic discouragement (Effect B). Here, we present evidence that all of these conditions hold.

### B. *Information Leads to a Large and Persistent Change in Parents' Beliefs about SSI Removal* (*i.e.*, *We Have a Strong First-Stage Effect*)

Figure 4 from the main experiment shows that Treatment group parents update their beliefs about their child's removal likelihood relative to the Control group. These updated beliefs persist to the end of the resource center in both the main experiment (panel B of Figure 5 and online Appendix Figure A.4) and the mechanism experiment (panel D of Figure 5). The fact that updated beliefs persisted in the mechanism experiment is particularly notable given that most respondents did not answer that question for several days after they completed the intervention.[32]

Moreover, to verify that parents fully internalized the information and its implications, we asked a question in the mechanism experiment about a hypothetical insurance product: "Suppose SSA gives you the option to receive \$100 less in SSI benefits each month over the next year .... In return, they would give you \$7,000 when your child turns 20 if your child is no longer receiving SSI benefits at that time, but nothing if your child is still receiving SSI. Would you take this offer?" As shown in panel A of Figure 9, take-up is a statistically significant 10 pp higher in the Treatment group than the Control group. This treatment effect indicates that parents are not simply regurgitating the removal information or responding to the endline beliefs question with the answer they think is expected of them. Instead, they appear to internalize that their child's removal risk is higher than expected and use that updated knowledge to make decisions about the future.

[32] Beliefs were collected for the third time at the end of the resource center. The question was asked to the treatment group and a randomly selected subset of the control group. In the main experiment, 50 percent of those who are in our analysis sample and were programmed to be asked the question ultimately answered the final beliefs question. In the mechanism experiment, the share answering is 11 percent (substantially lower because of the "cooldown" period discussed below). There was no selection across the Information and Control groups into who answered the resource center beliefs question since there was no selection into who visited the resource center (again, see Figure 11).

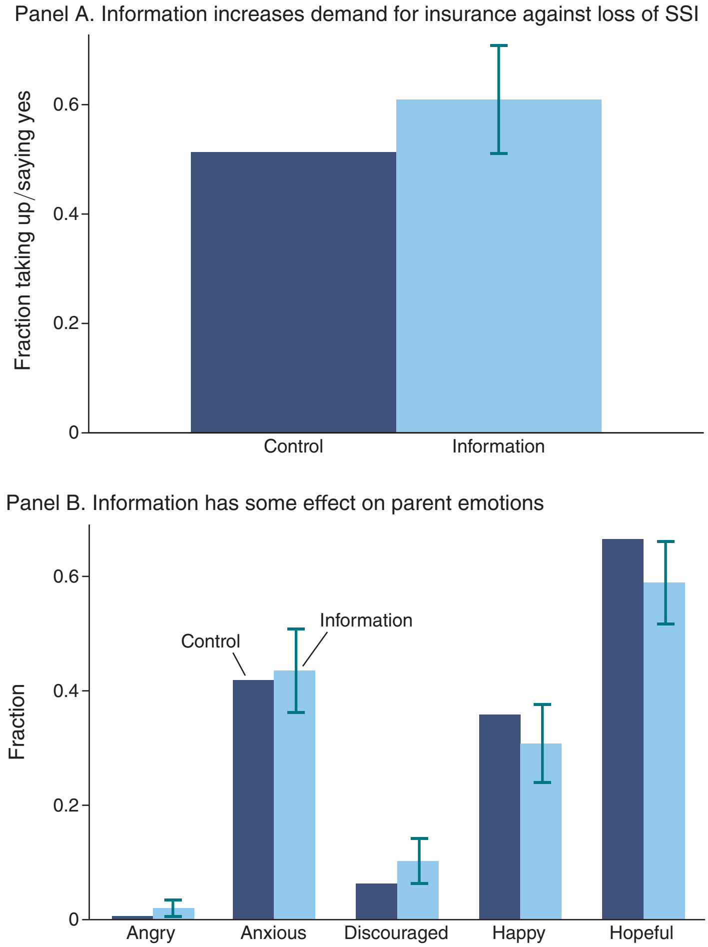


FIGURE 9. INFORMATION GROUP INTERNALIZES INFORMATION

*Notes:* Panel A shows the fraction of respondents saying "Yes" to the following question (and the 95 percent confidence interval) in the endline survey of the mechanism experiment: "This is a hypothetical question about your budget for the next few years. Suppose SSA gives you the option to receive \$100 less in SSI benefits each month over the next year (a total of \$1,200 less over the year). In return, they would give you \$7,000 when your child turns 20 if your child is no longer receiving SSI benefits at that time, but nothing if your child is still receiving SSI. Would you take this offer?" See online Appendix Table A.17 for estimates. Sample size: Control group observations = 148; Information group observations = 328. Panel B shows the fraction of respondents in the mechanism experiment choosing a given emotion (and 95 percent confidence interval) in response to the endline survey question "In this moment, how are you feeling about your child's future? Select all that apply." See online Appendix Table A.14 for estimates. For both graphs, the sample limits to those who (i) underestimate the removal probability by at least 30 pp at baseline and (ii) are in the Information group (i.e., Treatment group excluding Information-Perverse) or a randomly selected subset of the Control group. Sample size: Control group observations = 348; Information group observations = 335.

Further evidence that the Treatment group understands the information comes from differences in the emotional state of the Treatment group and Control group at endline. As shown in panel B of Figure 9, the Treatment group has somewhat more negative emotions after receiving the removal information: they are more likely than the Control group to report feeling "discouraged" and less likely to report feeling "hopeful." (Below, we provide evidence that this emotional reaction does not drive the null effect.)

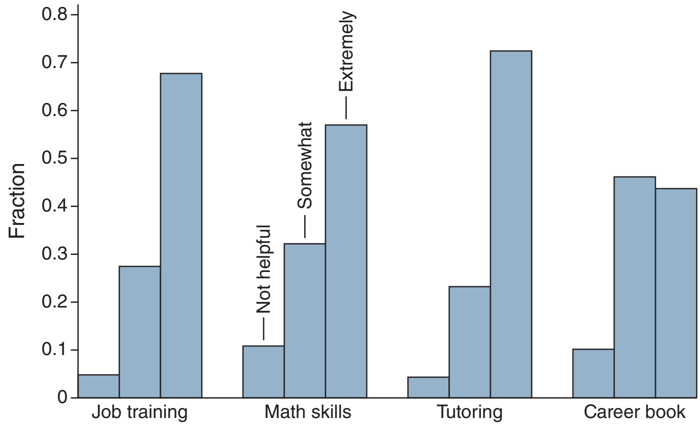


Figure 10. Vast Majority of Parents Think Resources Are Useful for Their Child's Future

*Notes:* The figure shows the fraction of parents responding in the baseline survey of the main experiment with "Not helpful," "Somewhat helpful," or "Extremely helpful" when asked "How much would [RESOURCE] help your child excel in school and/or their career?" The sample includes all parents in the Control and Treatment groups in the main experiment. Sample size: observations = 5,923.

### C. *The Resources We Offer Appear to Accurately Reflect Parents' Short-Run Investments in Human Capital (i.e., We Have Good Measurement of Outcomes)*

We turn next to evaluating whether the take-up of our resources is a good measure of parents' broader intentions to invest in human capital. We present several pieces of evidence that parents believe the resources we offered are valuable. The first is revealed preference: take-up of our primary outcomes among the Control group is around 30 percent (see Figure 6) despite nontrivial financial and/or time costs to obtain them. In addition, for three of the four primary outcomes, the majority of parents say that the resource would be "extremely" useful in helping their child excel in school and/or their career (versus "somewhat" or "not" useful; see Figure 10)—responses that are not simply noise since they vary across resources and also predict take-up. Even when we limit the sample to the parents saying that the resource would be "extremely" useful, we do not find a treatment effect (see "Resource extremely useful" subgroup in Figure 8). Moreover, as discussed in Section IIC, estimates from the literature suggest that parents are correct and that the resources would be very helpful for their child's future earnings.

Further evidence that our resources are capturing true investment decisions comes from take-up levels that are consistent with our priors about which groups value resources the most—e.g., higher take-up among parents who believe college is a worthwhile investment and parents who think the resources are "extremely" useful (see online Appendix Table A.9).[33]

[33] Another concern might be that our zero overall treatment effect is masking significant heterogeneity, with some people responding to information by increasing their take-up of the resources and others responding by decreasing their take-up. The fact that we do not see significant negative or positive treatment effects in any subsample suggests that this is not the case (see Figure 8). In addition, a mix of heterogeneous effects would likely affect the *variance* of number of resources taken up even if it does not affect the mean. However, the distribution of

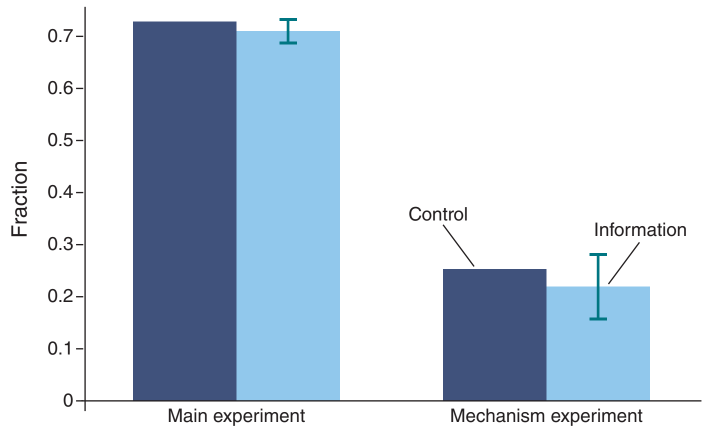


Figure 11. No Treatment Effect Even with "Cooldown" Period before Resources

*Notes:* The figure shows the fraction of parents who visit the resource center after completing the survey (and the 95 percent confidence interval), in both the main experiment and the mechanism experiment. In the main experiment, respondents received the resource center link immediately after completing the survey, as well as several email and text reminders. In the mechanism experiment, respondents received the resource center link by email several days after completing the survey, as well as several email and text reminders. For the main experiment, the sample limits to those who (i) underestimate the removal probability by at least 30 pp at baseline and (ii) are not in the Information-Perverse group. The mechanism experiment did not have an Information-Perverse group, so the sample limits to those who underestimate the removal probability by at least 30 pp at baseline. Sample sizes: Main experiment: Control group observations = 2,282, Information group observations = 2,307; Mechanism experiment: Control group observations = 348, Information group observations = 335.

A final set of concerns surrounds the timing of measurement of the outcomes. Recall from panel B of Figure 9 that the Treatment group has slightly more negative emotions at endline. This raises the concern that our offer of the resources and measurement of their take-up comes too soon, before parents have a chance to process the information. To probe this issue, in our mechanism experiment we implemented a "cooldown" period of several days before sending the link to the resource center. Even with the cooldown period, which on average put 11 days between information and resource center visits (versus 30 hours in the main experiment), the treatment effect remains 0 (with lower overall take-up; see Figure 11 and online Appendix Table A.16). The lack of any difference in the treatment effect between immediate measurement and measurement after the "cooldown" period suggests that insufficient time does not explain our null effect.

*Potential Long-Run Effects.*—A related timing concern is that updating removal beliefs could have long-term effects on children's long-run outcomes despite having no short-term effect on investment behavior. While it is common to see positive effects fade out over time (see, e.g., Bailey et al. 2020 for a review of fade-out in educational interventions), it is much less common to see null effects turn positive over time. Still, this is possible if parents need more time to think about their investments

the number of resources taken up is exactly the same for the Control and Information groups (see online Appendix Figure A.6).

than our experiment allowed for. One test of this hypothesis is whether parents' expectations about their child's long-term plans for school and work change since this would indicate that parents were planning to adjust other human capital investments in the future. We find a null effect on parents' expectations about whether their child will work in adulthood (Table 4), suggesting that parents are unlikely to change their long-term investments.

Another possibility is that parents share the information with their child and the child responds more to the information than the parent did. In that case, our results would identify whether there is dynamic discouragement in *parents'* investments in their children, not children's investments in themselves.

### D. *The "Perverse Incentives" Effect Discussed in Section IIB Appears to Be Negligible*

Recall from Section IIB the possibility that parents could respond to information about SSI removal by decreasing human capital investments if they think that higher human capital increases the likelihood of removal. This "perverse incentives" effect could potentially offset the dynamic discouragement effect, thereby leading to a null effect that masks two opposing effects. However, we have two pieces of evidence that the perverse incentives effect is negligible. First, perverse incentives are possible only if parents think that having higher human capital leads to a higher likelihood of removal, but parents in our baseline survey for the most part do not believe this.[34] Second, online Appendix Table A.12 shows that there is no effect of our subtreatments intended to pick up the perverse incentives effect.

## V. Investigating Hypotheses for the Lack of Dynamic Discouragement

Having provided evidence that our experiment yields a valid estimate of dynamic discouragement, we now investigate various hypotheses for why there is minimal dynamic discouragement in our context—that is, why updating parents' beliefs about SSI removal does not appear to affect human capital investment in children. We first show evidence that updating parents' beliefs about SSI removal does lead them to update their beliefs about the need for future income and the return to work. In other words, the ingredients for the income and substitution effects at the core of the benchmark model are present in our context. Then why is there no dynamic discouragement? We discuss several possible explanations. We find evidence for the explanations that parents have alternative plans (other than children working more) to recover the lost income, that nonfinancial objectives outside of the benchmark model influence parents' decisions, that parents are at the limit of the investments they can make, and that the reduction in permanent income due to the SSI loss might

[34] At baseline, we asked parents in our Information-Perverse group, "If your child were to graduate from high school and excel academically, do you think that would make them more or less likely to remain eligible for SSI?" From online Appendix Table A.13, the most common response was to say "About as likely" (33 percent of parents), and, if anything, more parents responded that their child was *less* likely to be removed (with 21 percent responding "somewhat less likely and 23 percent responding "much less likely") than more likely (11 percent saying much more likely and 12 percent saying somewhat more likely). Although it was a baseline question, we only asked it to the Information-Perverse group because we felt the question would be too priming regarding the potential for perverse incentives for any other groups.

cause an offsetting wealth effect that counters dynamic discouragement. We find less evidence for the hypothesis that parents highly discount the future. We use unincentivized survey questions as outcome variables in several of our analyses; online Appendix F discusses why we do not think that demand effects drive our results.

### A. *The Necessary Conditions for Meaningful Income and Substitution Effects on Human Capital Investment Are Present in Our Context*

For there to be a meaningful income effect on human capital investment, parents must believe that the income shock of losing benefits is meaningful in size. The mechanism experiment provides evidence that this is the case: 81 percent of treated parents said the loss would be catastrophic or that they would be "much" or "somewhat" (versus "not much") worse off (see online Appendix Table A.14). Parents also expect to support their child in early adulthood (ages 18–25), with 61 percent saying their child will continue living with them in adulthood and another 30 percent saying they will support their child even if the child lives separately.[35] For the substitution effect to be present, parents must understand that SSI receipt in adulthood decreases the financial return to work. This appears to be the case as well, as roughly two-thirds of parents say that their child's SSI benefit in adulthood would fall by either $1 or 50 cents for every dollar their child earns from working in adulthood (the correct answer is 50 cents).

In order for the income effect and substitution effect to change human capital investment in particular, parents must also believe that human capital would have an earnings return for their child. In the baseline survey of the mechanism experiment, nearly 80 percent of parents say high school would increase their own child's earnings from work "a little" or "a lot," and a plurality of parents say that four-year college would increase their child's earnings enough to cover the cost (see online Appendix Table A.14).[36] Moreover, parents express a high degree of confidence in their child's abilities in school and work: 64 percent think their child could attend college, and 84 percent expect their child to have a part-time or full-time job in adulthood (see baseline questions section of online Appendix Table A.13), although we acknowledge that these responses could be influenced by social desirability bias.[37]

### B. *We Find Suggestive Evidence for Several Explanations for the Absence of Dynamic Discouragement*

Given that parents update their beliefs about income needs and the return to human capital, it is surprising that they do not follow through with higher human capital

[35] These figures are consistent with the National Survey of SSI Children and Families, which shows that 65 percent of young adults who received SSI as children live with their parents.

[36] Even among parents who say four-year college is worth it, we do not estimate a treatment effect (see "Parent says college worth it" group in Figure 8).

[37] Note that these figures are much higher than actual educational achievement or employment rates for this population (Social Security Administration 2012; Davies, Rupp, and Wittenburg 2009). However, they are consistent with findings from PROMISE of very optimistic beliefs by both parents and youth (with some variation across sites): nearly 100 percent of youth expect to complete high school, around 60 percent (45 percent) of youth (parents) expect the youth to go to college, and around 95 percent (95 percent) of youth (parents) expect the youth to be employed at age 25 (Mamun et al. 2019).

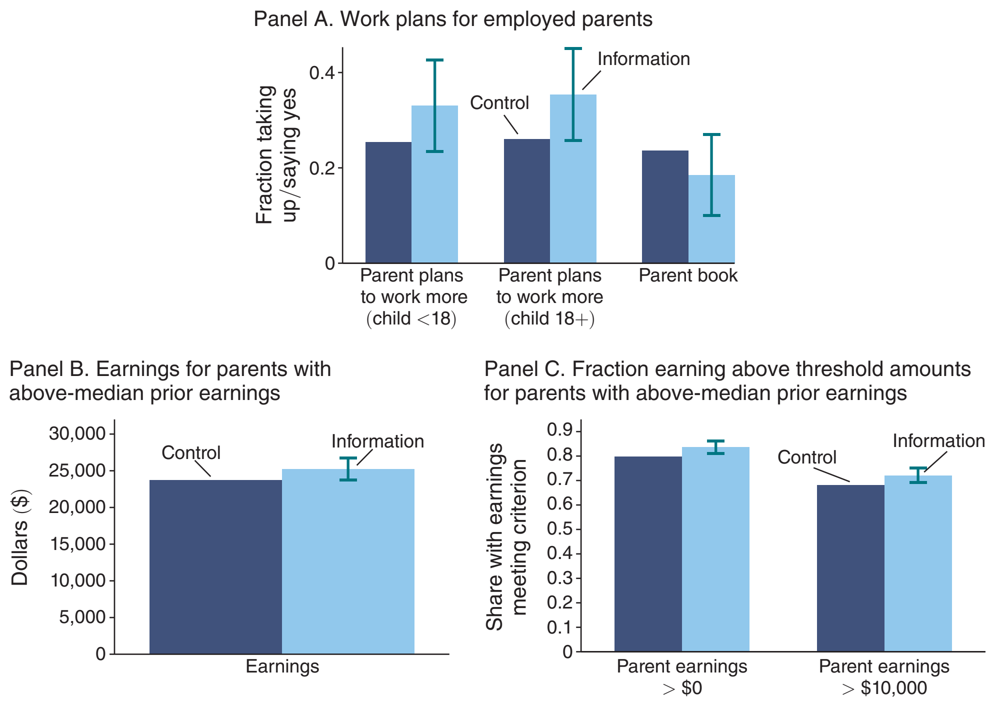


Figure 12. Labor Force: Attached Parents Increase Planned and Actual Work in Response to Removal Information

*Notes:* The left and middle sets of bars in panel A show, among currently employed parents in the mechanism experiment, the fraction saying "Work more" (and 95 percent confidence interval) to the endline survey questions "In the next few years, while your child is under 18, do you plan to work more or less than you are currently?" and "Once your child becomes a young adult, do you plan to work more or less than you are currently?" respectively. The right set of bars in panel A shows the fraction choosing the \$35 survey payment plus parent career book (versus \$40 survey payment or no response) and the 95 percent confidence interval. The sample for panel A is all parents (Treatment and Control groups) in the mechanism experiment who underestimate the removal probability by at least 30 pp at baseline. See online Appendix Table A.18 for estimates. See online Appendix Figure A.7 for an analogous figure for unemployed parents. Panel B shows 2022 earnings for parents with above-median prior earnings (measured as average earnings over the years when the child is 6 to 12 years old). Panel C shows the likelihood of parents earning more than \$0 (left) or \$10,000 (right) in 2022 for parents with above-median prior earnings. The sample for panels B and C is all parents (Treatment and Control groups) in the combined main and mechanism studies who underestimate the removal probability by at least 30 pp at baseline and have above-median prior earnings. We cut on median prior earnings as a proxy for parental employment. Many parents have positive but very low prior earnings, which we interpret as weak labor force attachment. Since 50 percent of parents in our sample report being employed in the survey, we divide the sample for the administrative data analysis at median earnings. See online Appendix Table A.11 for estimates. Sample sizes: Panel A: Control group observations = 161, Information group observations = 166. Panels B and C: Control group observations = 1,312, Information group observations = 1,315.

investment. We evaluate several different explanations for the lack of response, and the evidence gives us varying levels of confidence in each.

(i) *Parents respond to the expected SSI loss by working more themselves* (*Confidence: High*). Figure 12 shows that information leads to an increase in parent *plans* to work more in the future among those currently employed (from our endline survey) and also an increase in *actual* earnings among labor force–attached parents (from 2022 SSA administrative earnings data). The increase in intended and actual parent work could help explain the lack

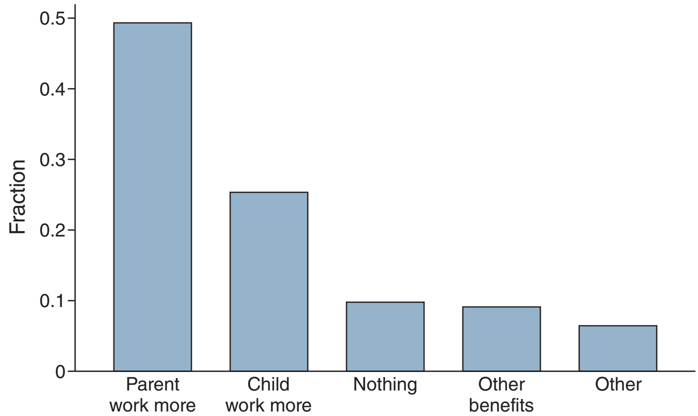


Figure 13. Parents Would Make Up for Lost Income from Benefits in Many Ways

*Notes:* The figure shows responses among parents in the mechanism experiment to the endline survey question "If your child were to lose SSI benefits at the age of 18, would you try to make up for that lost income? If so, what is the primary way you would do that?" The response options were the following: "No, I wouldn't try to make up for the lost income." / "Yes, by trying to have my child work more in adulthood." / "Yes, by trying to work more myself." / "Yes, by trying to get benefits from another program." / "Other, specify:". The sample is all parents in the Treatment group in the mechanism experiment. See online Appendix Table A.14 for estimates. Sample size: observations = 450.

of a human capital investment response. In particular, when parents receive the removal information, they could decide to spend their time in one of two ways: investing in their child's human capital, which is more risky and may only pay off after several years, or working more themselves, which is less risky and has more immediate returns. Especially for parents with little savings or informal insurance, they might decide to work more themselves to recover the expected SSI loss. This treatment effect also aligns with treated parents' responses to survey questions regarding how they would recover the lost income if their child were to lose SSI benefits in adulthood. Nearly half of parents (49 percent) say the main way they would make up for lost SSI income is by working more themselves. Only 25 percent say they would mainly respond by having their child work more in adulthood (see Figure 13).[38]

(ii) *Parents make decisions about their child's education based not only on financial objectives but also on nonfinancial ones* (*Confidence: Medium*). In the mechanism experiment, when we ask parents their "primary goal when making decisions about [their] child's education," 53 percent say it is to help their child "realize their potential" versus 30 percent saying it is to help them "achieve a stable financial future" (with another 14 percent saying it is to help them "engage in activities they enjoy"), as shown in Figure 14. Of course,

[38] Online Appendix Figure A.7 shows results for unemployed or less labor force–attached parents. A natural question is why these parents do not also respond by working more. One hypothesis is that these parents face higher barriers to work: they have higher rates of disability, and even beyond disability, evidence suggests that the longer one has been out of the labor force, the harder it is to reenter (e.g., Kroft, Lange, and Notowidigdo 2013).

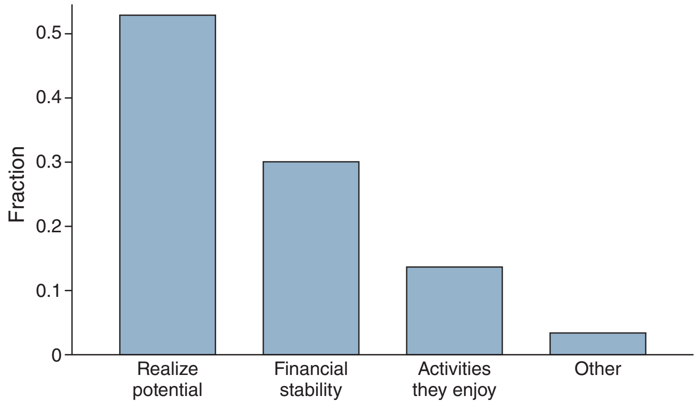


FIGURE 14. EVIDENCE THAT PARENTS MAKE INVESTMENT DECISIONS BASED ON NONFINANCIAL GOALS

*Notes:* The figure shows responses among parents in the mechanism experiment to the baseline survey question "My primary goal when making decisions about my child's education is to help them:". The response options were the following: "Realize their potential" / "Engage in activities they enjoy" / "Achieve a stable financial future" / "Other, specify:". Sample size: observations = 915. See online Appendix Table A.14 for estimates.

these responses could be affected by social desirability bias. Other evidence also suggests that parents make decisions about work based on factors other than financial well-being. Among parents who think there is no chance their child will lose benefits and who think SSI's marginal tax rate is 100 percent (meaning there is no financial return to work if receiving SSI), more than 75 percent still expect their child to work in adulthood, and the take-up of our resources is just as high as in other groups. Taken together, this evidence suggests that parents may believe that work and human capital are good for their child for reasons beyond income generation. The evidence is also consistent with findings from other contexts that parents care about more than just the financial returns to education (Berry, Dizon-Ross, and Jagnani 2020). Of course, valuing the nonfinancial components of education and work does not mean that parents should not *also* care about and respond to the financial returns. However, it could dampen responsiveness to financial returns.

(iii) *The wealth effect—the reduction in permanent income due to the SSI loss—chokes off some types of human capital investment* (*Confidence: Medium*). In the main experiment, we find a statistically significant *negative* effect of the information on the secondary outcome of college-going plans (see Table 4). This suggests that credit-constrained parents may believe they can no longer afford the tuition or the opportunity cost of their child being in college.[39] This could lead to reductions in investments that are complementary to college going. However, two pieces of evidence suggest that the

[39] Assuming college is primarily seen as a financial investment, the credit constraints are relevant because, in the absence of credit constraints, the level of investment should not depend on wealth—only on returns. Credit constraints are, however, not necessary for there to be a wealth effect if college going also has consumption value or other nonfinancial value.

wealth effect cannot fully explain our null effect for our primary outcomes. First, there is no treatment effect even for resources like job training that are not complementary to college going. Second, we do not find a treatment effect among parents who are less credit constrained or among the majority of parents who believe the resources are useful even if their child does not attend college (see "Less credit constrained" and "Resource useful if no college" in Figure 8).[40]

(iv) *Parents of children receiving SSI are already at the limit of investment subject to time and resource constraints* (*Confidence: Medium*). As shown in online Appendix Table A.14, 89 percent of parents in the mechanism experiment report that they are already doing "all they can do" to help their child succeed. While this response could reflect some social desirability bias, if we take the response at face value, it could mean that parents have reached either their own limits (in terms of money, time, or bandwidth) or their child's limits for investment opportunities and there is no room for additional investment even in light of SSI removal information. Because the resources we offer are free of charge and relatively low-cost to take up, this is unlikely to explain our full null effect. Still, it could be that parents cannot afford the time costs associated with the resources (e.g., driving their child to job training), especially given that they work more as a result of the removal information.

(v) *High discount rates dampen investment* (*Confidence: Low*). We also investigate the possibility of high discount rates dampening plans for human capital investment but do not find much evidence for this explanation. In the mechanism experiment, roughly 45 percent of parents "strongly disagree" (and another 30 percent "disagree") with the statement "It's too early to start thinking about my child's life as an adult," although this could be partly due to social desirability bias. Even among parents who "strongly disagree," we do not estimate a treatment effect (see "Parent says not too early to plan" in Figure 8).

## VI. Conclusion

Our results indicate that, at least in the context of the SSI program, expectations of future benefit receipt from the social safety net have limited, if any, impacts on educational investments in childhood. We thus provide some of the first evidence on the dynamic discouragement, or lack thereof, of the social safety net on human capital investment.

From the perspective of how to improve the life outcomes of children who receive SSI, our findings are disappointing news. Removing children at age 18 from SSI has adverse effects on children and society, including large increases in criminal activity (Deshpande and Mueller-Smith 2022). Our results suggest that dynamic

[40] Note that the latter is defined at the parent × resource level and so focuses only on the specific resources that parents find useful in the absence of college.

discouragement stemming from lack of information about SSI removal alone is not the cause of the adverse effects and hence that providing information about removal is unlikely to counter the adverse effects. Improving the adult outcomes of children who receive SSI benefits will thus require other policies, potentially in conjunction with information.

From a broader policy perspective, however, our finding that there is limited dynamic discouragement is encouraging news for the social safety net. Both economic theory and experts predict that the expectation of future benefits reduces human capital investments in children, which could be detrimental to their well-being. If this effect is minimal, it would mean that social safety net programs are not having unintended harmful effects on children. It would also mean that redistribution is more efficient, and thus the optimal amount of redistribution potentially higher, than previously thought.

Our findings suggest several questions for future research. First, SSI is a particular context with children who have disabilities and grow up in low-income households, which likely face more constraints on investment than the average household. While many social safety net programs serve families facing serious resource constraints, it would be useful to study dynamic discouragement in other contexts to determine whether there is more dynamic discouragement among families that are less constrained. Second, in most cases, our study evaluates the effect of *reducing* expectations about the availability of government benefits, which may or may not have symmetric effects to increasing expectations about the availability of government benefits. Finally, future research can continue to study the specific reasons that changing expectations about future benefits does not affect parents' investment in their children's human capital but does change parents' own work effort.

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