Should Governments Invest More in Nudging?

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Abstract

Governments are increasingly adopting behavioral science techniques for changing individual behavior in pursuit of policy objectives. The types of “nudge” interventions that governments are now adopting alter people’s decisions without resorting to coercion or significant changes to economic incentives. We calculate ratios of impact to cost for nudge interventions and for traditional policy tools, such as tax incentives and other financial inducements, and we find that nudge interventions often compare favorably to traditional interventions. We conclude that nudging is a valuable approach that should be used in conjunction with traditional policies, but more relative effectiveness calculations are needed.

Keywords: nudge, nudge unit, choice architecture, behavioral science, behavioral economics, savings, pension plan, education, college enrollment, energy, electricity usage, preventive health, influenza vaccination, flu shot

Introduction

Recent evidence indicates that the burgeoning field of behavioral science can help solve a wide range of policy problems (Halpern, 2015; Johnson & Goldstein, 2003; Johnson et al., 2012; Larrick & Soll, 2008; Ly, Mazar, Zhao, & Soman, 2013; Sunstein, 2013; Thaler & Sunstein, 2008; World Bank, 2015). In response, governments are increasingly interested in using behavioral insights as a supplement to traditional economic levers, such as incentives, to shape the behaviors of citizens and government personnel in accordance with public priorities. A number of governments around the world have formed "nudge units": teams of behavioral science experts tasked with designing behavioral interventions with the potential to encourage desirable behaviors without restricting choice, testing those interventions rapidly and inexpensively, and then widely implementing the strategies that prove most effective. The United Kingdom established a nudge unit in 2010 and was soon followed by other countries, including Australia, Germany, the Netherlands, and Singapore as well as the United States, where an Executive Order issued in September 2015 directed federal agencies to incorporate behavioral science into their programs (Obama, 2015).

A key feature of the behavioral strategies deployed by nudge units is that they aim to change "people's behavior in a predictable way without forbidding any options or significantly changing their economic incentives. To count as a mere nudge, [an]…intervention must be easy and cheap to avoid. Nudges are not mandates" (Thaler & Sunstein, 2008). Nudges do not alter the economics underlying a decision but instead change the psychology of a decision, for example by changing the default option to take advantage of people's inertial tendency to accept defaults passively. Nudges stand in contrast to traditional policy tools, which change behavior by altering the outcome of a reasoned cost-benefit analysis, either by providing incentives or by providing education that changes beliefs regarding the attractiveness of available options.

The mechanisms through which nudges influence behavior are distinct from the mechanisms that traditional policy tools rely upon, and nudges are frequently deployed to *augment* the impact of traditional policy tools. For example, a nudge might streamline the process of applying for government financial aid for college attendance (Bettinger et al., 2012). As governments decide on the appropriate resources to invest in nudge policies, an important question is to determine how efficiently nudge initiatives achieve their objectives. If a traditional policy increases engagement in a desirable behavior (e.g., college attendance) by a certain amount per dollar spent, the opportunity to implement a nudge policy that increases engagement in the same behavior by a larger amount per dollar spent would be an attractive incremental investment of public resources.

This point may seem obvious. However, because extremely cost-effective nudges do not always create large shifts in behavior, scholars and policy makers may underappreciate the value of such nudges when cost effectiveness calculations are not conducted. As a motivating case study for assessing the cost effectiveness (rather than merely the effectiveness) of nudge policies, consider an experiment conducted by the White House Social and Behavioral Sciences Team (SBST)—the U.S. nudge unit—in collaboration with the U.S. Department of Defense (DOD). This experiment sought to increase savings among military personnel in the defined contribution retirement plan offered to federal government employees, a setting where the government already offers monetary incentives for saving (retirement plan contributions are tax-deductible). In the experiment, 806,861 military service members who were not contributing to the plan received emails nudging them to begin contributing (except for a control group, which received no email—the business-as-usual practice). The emails were experimentally varied to test different behaviorally-informed strategies for increasing sign-ups (see SOM-R and SOM-U for further information on the experiment and its results). The business-as-usual control group had a 1.1% savings plan enrollment rate over the month following the messaging campaign, while the groups who received emails had enrollment rates ranging from 1.6% to 2.1%. At first blush, this campaign's impact may seem quite modest. However, the incremental administrative costs of developing and deploying the email campaign were just $5,000, and the messages collectively increased savings plan enrollment by roughly 5,200 people and increased contributions by more than $1.3 million in the month post-experiment.[1] If we extrapolate and assume that the intervention's effect decays linearly to zero over one year (a conservative assumption given the stickiness of savings plan contribution rates), the program increased savings by approximately $8 million total. Thus, the intervention generated $1,600 in additional savings per dollar spent by the government, an impact that is more than one hundred times larger than the impact per dollar spent by the government on tax incentives, as we calculate later in this paper. This case study highlights that nudge policies do not need to produce a large impact in absolute terms to be useful. Indeed, it is important to calculate a nudge's impact per dollar spent precisely because valuable nudges sometimes have a small absolute impact but a large impact per dollar spent.

Past studies on nudges, including those disseminated by existing nudge units, have typically measured only the extent to which the intended behavior was changed (if at all). To be

[1] This estimate is relative to our estimate of what would have happened had everyone been in the control group. To estimate the overall effect of the email campaign on enrollment, we ran an ordinary least squares (OLS) regression with an indicator for enrollment as the outcome variable and with only a constant and an indicator variable for receiving an email as the explanatory variables. Multiplying the point estimate (and the endpoints of the 95% confidence interval) for the coefficient on the email indicator variable by the number of individuals who received emails, we estimate that the email campaign increased savings program enrollment by 5,265 people (95% CI: 4,563-5,968). Using the same methodology, we also estimate that the email campaign increased total contributions to retirement accounts in the month following the email campaign by $1,367,423. Note that this last calculation excludes Marines and is therefore an understatement of the effect.

maximally influential, future policy-oriented behavioral science research will need to measure the impact per dollar spent on behavioral interventions in comparison to more traditional interventions for influencing behavior. In the absence of such relative effectiveness calculations, policymakers lack the evidence needed to design optimal policies and to decide on the appropriate allocation of resources across behaviorally-informed and traditional interventions.

We identified four major domestic policy areas where the U.S. and U.K. nudge units have focused attention to date and where the U.S. government invests heavily—financial security in retirement, education, energy, and health. In this paper, we examine one, well-studied behavior that government agencies have sought to change within each of the four policy areas, and we provide relative effectiveness calculations using available evidence from published research in leading journals.

Method

Study Selection Criteria

We formed our initial list of policy areas by combining the lists of focus areas from the most recent summary reports of the U.S. and U.K. nudge units (Social and Behavioral Sciences Team, 2015; Behavioural Insights Team, 2015), eliminating redundancies and excluding areas that are not major domestic policy foci for the U.S. government. Table 1 displays these SBST and BIT policy areas of focus, our categorization of these areas, and areas that were excluded.

Table 1.

Categorization of all focus areas listed in the SBST 2015 Annual Report and the BIT 2013-2015 Update Report.

Our Categorization	Corresponding Focus Area(s) in SBST 2015 Annual Report	Corresponding Focus Area(s) in BIT 2013-2015 Update Report
Financial Security in Retirement	Promoting Retirement Security	Empowering Consumers[a]
Education	Improving College Access & Affordability	Skills & Youth Education
Energy	n/a	Energy & Sustainability
Health	Helping Families Get Health Coverage & Stay Healthy	Health & Wellbeing
Job Training	Advancing Economic Opportunity	Economic Growth & the Labour Market
Program Integrity & Compliance	Promoting Program Integrity & Compliance	Fraud, Error & Debt[b]
Home Affairs	n/a	Home Affairs

Note: Our list excludes the following SBST and BIT focus areas because they are not major areas of domestic policy for the U.S. government: Ensuring Cost-Effective Program Operations (SBST), Giving & Social Action (BIT), International Development (BIT), and Work with Other Governments (BIT).
[a]We group this focus area with SBST's Promoting Retirement Security area because its leading example has to do with pensions.
[b]We group this focus area with SBST's Promoting Program Integrity & Compliance area because both focus on improving tax and fee collection.

Within each category that we formed, we next identified one well-defined behavior to be our outcome variable of interest. When the policy area had an obvious behavior to focus on, the choice was simple—in the Financial Security in Retirement area, we focus on retirement savings, and in the Energy area, we focus on energy consumption. When the policy area did not have an obvious behavior to focus on, we looked to the outcome variable emphasized by the SBST. If the policy area was not studied by SBST, we looked to the outcome variable emphasized by the BIT. In the Education area, college enrollment among recent high school graduates was one of three targets for SBST in its first year of operations. The other two were increasing student loan repayments and increasing applications for income-driven repayment plans among student loan borrowers. Since neither of the latter two targets is a direct educational outcome, we focus on college enrollment among recent high school graduates. In the Health area, increasing adult outpatient influenza vaccinations, increasing health insurance marketplace enrollment, and improving global health initiatives were the three targets for SBST in its first year of operations. We focus on increasing adult outpatient influenza vaccinations because increasing health insurance marketplace enrollment is not a direct health behavior and because improving global health initiatives is not a domestic policy issue.

In the Job Training policy area, the leading example in the SBST 2015 Annual Report is a study that aimed to increase enrollment in a job training program. However, when we searched in our set of journals from the year 2000 onward (see below for an explanation of our journal selection criteria), we could not find published research studying this outcome variable, so we exclude this variable from our list. In the Program Integrity & Compliance policy area, the leading example in the SBST 2015 Annual Report is a study that aimed to increase compliance with paying a required fee to the government. As in the Job Training policy area, searching our set of journals from 2000 onward did not reveal published research studying interventions deployed to increase compliance with paying taxes or required fees to the government, and we therefore exclude this variable from our list. In the Home Affairs policy area, which SBST did not study, the BIT 2013-2015 Update Report tends to emphasize the goal of reducing crime, but we exclude this outcome from our list because searching our set of journals from 2000 onward did not reveal published research studying interventions for decreasing the types of crime mentioned (illegal migration, mobile phone theft, and online exploitation).

We next searched leading academic journals for original research published since the year 2000 studying interventions aimed at directly influencing the outcome variables of interest. We sought research designed to influence the outcome in question using nudges, tax incentives, targeted rewards, or educational programs. Using Google Scholar to determine academic journal rankings,[2] we limited our set of academic journals to Google Scholar's three leading general interest journals (*Science*, *Nature*, and *Proceedings of the National Academy of Sciences of the United States of America*); three leading economics journals, excluding finance journals (*The American Economic Review* excluding *American Economic Review: Papers and Proceedings* papers because these manuscripts are not subject to peer review, *The Quarterly Journal of Economics*, and *The Review of Economics and Statistics*); three leading general psychology journals, excluding journals that publish only review articles (*Psychological Science*, the *Journal of Personality and Social Psychology*, and the *Journal of Applied Psychology*); and, in the case of interventions aimed at increasing adult outpatient influenza vaccinations, three leading general medical journals (*The New England Journal of Medicine*, *The Lancet*, and *JAMA: The Journal of the American Medical Association*).

Criteria for inclusion in our analyses were: the entire research paper was available online; the paper analyzed a (i) nudge, (ii) tax incentive, (iii) reward, or (iv) educational program targeting one of the dependent variables of interest; and the paper presented the necessary information to construct relative effectiveness calculations, or we could obtain this information by contacting the author(s). If our search identified papers meeting our inclusion criteria for a particular outcome but not for all four of the intervention types above, we attempted to fill the gaps by widening our search beyond the restricted set of journals.

Our method for choosing dependent variables for inclusion in our relative effectiveness analysis ensured the selection of outcomes for which the ex ante belief of policy makers was that nudges had a chance to impact behavior. This selection process may have given an advantage to nudges over incentives and educational interventions in our relative effectiveness calculations.

[2] *Top Publications*. (September 29, 2015). Retrieved from https://scholar.google.com/citations?view_op=top_venues

However, if we were to study the full range of outcome variables pursued by major government departments, our analysis would be far less useful because in areas where governments would not typically expect ex ante that nudges will work, there is little hope of finding relevant published nudge research and limited interest in nudge efficacy. Although our methodology likely oversamples settings where nudges have high potential for impact, we are careful to focus only on settings of major domestic policy interest,[3] making our findings highly policy-relevant regardless of any selection concerns.

Relative Effectiveness Calculations

We offer a comparison between the effectiveness of behaviorally-motivated policies and the effectiveness of standard policies by using a single measure that takes both the cost of a program and its impact into account. Specifically, we examine the ratio between an intervention's causal effect on a given outcome variable and its (inflation-adjusted) implementation cost.[4] When standard errors for a treatment effect are available, we scale them by the cost of the intervention and report the scaled standard errors, ignoring any uncertainty regarding the cost of the intervention.

Our definition of the impact of an intervention follows from the main findings of the paper reporting on it. When a paper studies the effect of an intervention on multiple outcome variables or target populations, we select the outcome and target population that are most comparable to the outcomes and target populations studied in other papers on the same topic.[5]

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[3] See, for example, *Budget of the United States Government, Fiscal Year 2017*. (September 13, 2016). Retrieved from https://www.whitehouse.gov/omb/budget/Overview

[4] We adjust all costs to June 2015 levels using the annual CPI from the year of intervention. If interventions took place over multiple years, we adjust using the midpoint year.

[5] For example, Bettinger, Long, Oreopoulos, and Sanbonmatsu (2012) study the effect of Free Application for Federal Student Aid (FAFSA) assistance on FAFSA completion rates, college attendance rates, Pell Grant receipt rates, and years of postsecondary education for both traditional and non-traditional students. We focus on the effect on college attendance rates among traditional students for comparability with other studies.

We often need to make additional assumptions to produce estimates for the cost of an intervention. Some interventions affect an outcome by increasing take-up of another program that affects the outcome.[6] One may argue that in these situations, interventions have additional, indirect costs because they increase the usage of other programs. However, in most of the cases that we study, the intervention simply encourages individuals to use existing, under-capacity institutions in a way that better fulfills those institutions' missions. Some interventions may create perverse outcomes that are costly, and in those situations, we explicitly account for those costs.[7] That said, we do not include any indirect costs that result from increases in the intended use of other, existing institutions.

In most cases, the different interventions that we study within a domain operate over approximately the same time horizon. We evaluate the retirement savings interventions over a horizon of one year, a natural length of time given that tax incentives for savings apply annually. Similarly, the college education interventions are measured in terms of their impact on annual enrollment, and the influenza vaccination interventions operate over the course of a single year's vaccination cycle (approximately September through December). In contrast, results from the energy conservation interventions are reported for time horizons ranging from a few months to several years, and we note these differences when discussing the energy conservation calculations. However, even in the case of the energy conservation interventions, our relative effectiveness calculations provide useful guidance to policy makers who apply a low intertemporal discount rate to future financial costs and energy savings.

[6] For example, Bettinger et al. (2012) provided assistance in completing the FAFSA to increase college enrollment through improved access to financial aid. Milkman, Beshears, Choi, Laibson, and Madrian (2011) and Chapman, Li, Colby, and Yoon (2010) used nudges to encourage take-up of flu shots during free vaccination campaigns.
[7] An instance of a costly side effect occurs with the Chapman et al. (2010) implementation of an opt-out vaccination appointment system, which increased no-shows at the vaccination clinic.

Some experimental studies have multiple treatment arms, and experimenters incur research costs (e.g., data collection costs, participant payments) for all treatment groups, including the control group. Given that any treatment effect is estimated as the marginal increase in the outcome variable over the control group, we take a similar stance on costs and consider the cost of an intervention to be its marginal cost over the cost of the control treatment. We further focus our attention on capturing the primary costs for each intervention, and we omit the costs of any minor unreported aspects of the program.[8]

Importantly, relative effectiveness calculations do not speak to the question of whether it is socially beneficial to devote resources to changing a given behavior. Conditional on a government deciding that it is desirable to change behavior, however, they do provide guidance about how resources may be best allocated across interventions targeting that behavior.

Results

We now describe the results of our relative effectiveness calculations in the four policy domains. The results are summarized in Table 2 and Figure 1.

Increasing Retirement Savings

Carroll et al. (2009) studied the effect of an active decision nudge on retirement savings. A company's new employees were required to indicate their preferred contribution rate in a workplace retirement savings plan within their first month of employment. Compared to an enrollment system that asked employees to choose a contribution rate on their own and that implemented a default contribution rate of zero for employees who had not chosen another rate,

[8] This may lead us to account for a category of cost in one setting but not in another. For example, administrative/marketing costs for a purely informational intervention may be the most significant costs of the intervention, and we would therefore include them in our cost accounting. However, for grant programs or tax credits, administrative/marketing costs are small compared to the total amount of money transferred, so accounting for them would not significantly affect our estimates. Thus, we do not explicitly incorporate such costs.

the active decision nudge increased the average contribution rate in the first year of employment by more than one percent of pay. The nudge is effective because it does not allow new employees to procrastinate on signing up for the savings plan (O'Donoghue & Rabin, 2001). Median annual income among these employees was approximately $30,000 in 1998 dollars, but because the active decision nudge was more impactful for low-income employees, we conservatively apply the one percentage point average contribution rate increase to an annual salary of $20,000 in 2015 dollars, for a contribution increase of $200 per employee. We estimate that the cost of including the savings plan enrollment form in the information packet for newly hired employees and following up with the 5% of employees who failed to return the form was no more than $2 in 2015 dollars per employee, so the active decision nudge generated an impressive $100 of additional savings per dollar spent.

Perhaps the best-known behavioral strategy for promoting savings in workplace retirement accounts is to automatically enroll employees or automatically increase their contribution rates. Automatic enrollment is effective because people exhibit inertia, which favors sticking to defaults, because people infer that policy makers are recommending the default option, and because defaults become reference points, making deviations from the default feel like losses, which loom larger than gains (Johnson & Goldstein, 2003). The most definitive study of savings plan automatic enrollment uses data from Denmark (Chetty, Friedman, Leth-Petersen, Nielsen, & Olsen, 2014). Changing the fraction of an individual's salary that is automatically directed to a retirement account can generate savings changes of several percentage points of annual salary at essentially zero cost if the infrastructure for payroll deduction into a retirement account has already been set up.[9] By contrast, the same paper studies how people responded to a reduction in the tax deduction available for contributions to a particular type of retirement account, showing that the policy change reduced contributions by 2,449 DKr (121), or $540 (27) in 2015 U.S. dollars, and increased government revenues by 883 DKr, or $195 in 2015 U.S. dollars, for each person affected by the change, implying the tax deduction generates a paltry $2.77 (0.14) of additional savings in this type of retirement account per dollar of government expenditure.[10]

Duflo and Saez (2003) offered a large university's employees $20 to attend a benefits fair to receive information about their retirement savings plan. This increased retirement plan contributions among 4,000 employees over the next year by $175,000 at a cost of $12,000 in 2001 dollars, or by $58.95 at a cost of $4.04 per employee in 2015 dollars, generating a respectable $14.58 in additional retirement contributions in the year per dollar spent.[11]

Duflo, Gale, Liebman, Orszag, and Saez (2006) provided clients of a tax-preparation company matching contributions for deposits to a retirement savings account. Clients who were offered a 20% [50%] match contributed $62.9 [$132.7] in 2005 dollars, or $76.9 [$162.1] in 2015 dollars, more to the retirement account relative to the control group and received average matching contributions of $13.7 [$67.4] in 2005 dollars, or $16.7 [$82.4] in 2015 dollars, for total incremental contributions of $76.6 (7.4) [$200.1 (10.5)] per treated client in 2005 dollars, or

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[9] Madrian and Shea (2001) and Card and Ransom (2011) study automatic enrollment and related nudges and find similar results.

[10] We convert Danish kroner to U.S. dollars using the exchange rate of 6.5 to 1 preferred by Chetty et al. (2014), and we then adjust from 1999 to 2015 price levels using U.S. CPI growth. Chetty et al. (2014) also study the extent to which savings increases in a retirement account caused by changes to automatic retirement account contributions or caused by changes to tax incentives for the retirement account are offset by savings *decreases* in an individual's other financial accounts. The offset is minor in the case of changes to automatic retirement account contributions. However, when savings in a retirement account respond to changes to tax incentives for the retirement account, this response is almost completely offset by adjustments in other financial accounts. The other papers that we analyze do not report results regarding the extent of offsetting that occurs in an individual's other accounts.

[11] Choi, Laibson, and Madrian (2011) analyze a similar intervention but do not find a statistically significant impact, so the Duflo and Saez (2003) results are potentially overly optimistic.

$93.6 (9.0) [$244.5 (12.8)] per treated client in 2015 dollars, and a mere $5.59 (0.54) [$2.97 (0.16)] in total contributions per dollar of matching expenditures.

Duflo et al. (2006) also calculated the effect of tax credits on retirement account contributions, but we focus on the results from a companion paper (Duflo, Gale, Liebman, Orszag, & Saez, 2007) devoted to studying these tax credits. The authors estimate that an increase in the tax credit from 20% to 50% of contributions generates an additional $9.5 (0.82) in 2005 dollars, or $11.6 (1.00) in 2015 dollars, of deposits to a retirement savings account, from an average of $9.8 to $19.2 in 2005 dollars, or $12.0 to $23.5 in 2015 dollars. This increase translates to just $9.5/(0.5*19.2–0.2*9.8)=$1.24 (0.11) of retirement account savings per dollar of tax credits.

Increasing College Enrollment among Recent High School Graduates

When H&R Block tax professionals streamlined and facilitated the process of filing the FAFSA for their clients, high school seniors whose families received the assistance were 8.1 (3.5) percentage points more likely to attend college the following year. The incremental cost of the intervention over the control group was $48 per participant in 2008 dollars ($3 for training and compensation for tax professionals, $15 for material and software costs, and $30 for call center support), or $53.02 in 2015 dollars. Thus, the intervention produced a remarkable 1.53 (0.66) additional college enrollees per thousand dollars spent (Bettinger et al., 2012). This streamlined personalized assistance nudge likely reduced procrastination by making the FAFSA easier to complete, alleviated anxiety about making an error, reduced the stigma for low socioeconomic status individuals associated with filling out the FAFSA, and increased the salience and perceived value of completing it. In contrast, when this behaviorally-informed intervention was replaced with a more traditional educational intervention providing families with details about their aid eligibility, there was a statistically insignificant decrease in college enrollment relative to the untreated control group (Bettinger et al., 2012).

Turning to monetary incentives, Dynarski (2003) estimated the effect of the Social Security Student Benefit Program, a federal subsidy for post-secondary education, on college enrollment. The elimination of benefit eligibility reduced attendance rates for affected students by 18.2 (9.6) percentage points. The average annual benefit for each student in 1980 was $6,700 in 2000 dollars, and 56% of the eligible group attended college for a cost per eligible individual of $3,752 in 2000 dollars, or $5,181 in 2015 dollars. The program therefore generated only 0.182/5,181*1,000=0.0351 (0.0185) additional college enrollees per thousand dollars spent.[12] This impact per thousand dollars spent is approximately 40 times smaller than the impact per thousand dollars spent of the Bettinger et al. (2012) nudge.

Long (2004a) studied the effect of state higher education subsidies for enrollment in public universities. Long's estimates indicate that in the absence of any state support, 5,535 students in the sample would enroll in college. If the state provided vouchers proportional to the expected years of study, 5,664 students would enroll, with 3,766 in four-year colleges and 1,898 in two-year colleges. According to the working paper version of the article, the vouchers provide $3,167 per student at a four-year college and $1,583 per student at a two-year college. The total voucher expenditure would therefore be (3,766*$3,167+1,898*$1,583)=$14,931,456 in 1992 dollars ($25.3 million in 2015 dollars). The educational vouchers therefore increased college enrollment by just (5,664–5,535)/25,304,980*1,000 = 0.0051 students per thousand dollars spent.

[12] Linsenmeier, Rosen, and Rouse (2006) and Conley and Taber (2011) do not find statistically significant estimates of the effect of grants on college enrollment. We focus on the Dynarski (2003) results as a potentially overly optimistic view of the effect of educational subsidies.

Tax incentives for college enrollment such as the Hope Tax Credit, the Lifetime Learning Tax Credit, and the American Opportunity Tax Credit appear to produce no measurable increases in college attendance (Long, 2004b; Bulman & Hoxby, 2015).

Increasing Energy Conservation

Schultz, Nolan, Cialdini, Goldstein, and Griskevicius (2007) and Allcott and Rogers (2014) considered the effects of nudging households to reduce home electricity consumption by sending them letters comparing their energy use to that of their neighbors. This social norms information harnesses both competitiveness and the tendency to follow the herd. Allcott and Rogers (2014) directed readers to Allcott (2011) for simpler cost effectiveness calculations for the program. We focus on the Allcott (2011) calculations for this reason and because they are based on much larger sample sizes than the Schultz et al. (2007) analysis. Allcott (2011) found that the program averaged 3.31 cents of expenditure for each kWh of electricity saved over the course of approximately two years. Adjusting for inflation from 2009 levels, the program spent 3.67 cents per kWh saved, or saved 27.3 kWh per dollar spent.[13] This impact per dollar spent is substantially larger than the impact per dollar spent on traditional policies.

However, as one should expect, not all energy nudges have been cost-effective. (That is why it is crucial to use randomized controlled trials or other evaluation methods to measure cost effectiveness.) Asensio and Delmas (2015) studied a nudge that gave households detailed information from meters recording appliance-level electricity usage. Giving households access to a webpage with this information along with messages linking pollution from electricity usage to health and environmental issues, perhaps sparking moral concerns (Haidt, 2001), reduced electricity consumption by 8.192 (4.306) percent, or (0.0819*8.66*100)=70.9 (37.3) kWh over

[13] Allcott and Mullainathan (2010) report similar results.

the 100 day treatment period relative to the control group, which had baseline average electricity usage of 8.66 kWh per day. We assume energy savings decayed linearly over one year, translating to a total of 149.8 kWh saved per household. The authors report (via private correspondence) that the cost of the treatment was $3,019 per household in 2015 dollars. The intervention thus saved an unremarkable 0.050 kWh per dollar spent. The authors also tested a nudge providing detailed information on electricity usage and messages linking usage to increased utility bills, seeking to increase the salience of the pain of paying (Prelec & Loewenstein, 1998), and they did not find a statistically significant effect on electricity consumption.[14]

In contrast, when California utilities offered residential customers a 20% rebate off of their summer electricity bills in 2005 if they reduced usage by at least 20% relative to the previous year's summer total, energy consumption during those summer months decreased by 60.5 million kWh. Ito (2015) calculates that the program spent 24.1 cents for each kWh saved (29.3 cents in 2015 levels), and it saved a respectable 3.41 kWh per dollar spent.

Arimura, Li, Newell, and Palmer (2012) estimated the effect of demand-side management and energy efficiency policies using data from 307 U.S. utilities from 1992-2006. They found that the programs, which operate over the course of several years, spent on average 5.0 cents per kWh saved (7.1 cents in 2015 dollars), and they saved an impressive 14.0 kWh per dollar spent.[15]

Increasing Adult Outpatient Influenza Vaccinations

[14] Sexton (2015) demonstrated that withdrawing consumers from automatic electricity bill payment programs significantly reduced energy usage. This intervention does not fit into any of the traditional policy categories we evaluate; it comes closest to being a nudge. We exclude it from our analysis because it imposes significant transaction costs on consumers and therefore is not truly a nudge.

[15] Friedrich, Eldridge, York, Witte, and Kushler (2009) also studied the effectiveness of efficiency programs. However, their estimates of energy savings are not comparable to other studies in our analysis, and previous work suggests that their approach may overstate cost effectiveness (Allcott & Greenstone, 2012).

Milkman et al. (2011) studied a nudge prompting people to plan the date and time when they would obtain an influenza vaccination. Such prompts embed intentions more firmly in memory and associate cues like the intended time of action with the intended behavior, thereby reducing forgetfulness. They also help people think through logistical hurdles and strategies for overcoming those hurdles. Finally, they create a commitment that is uncomfortable to break (Rogers, Milkman, John, & Norton, 2015). The authors found that planning prompts increased flu shot take-up by 4.2 (1.9) percentage points. Adding the prompts to reminder letters that were already being mailed required 5 hours of labor at a cost of $75 per hour, totaling $415.58 in 2015 dollars. With 1,270 employees receiving the prompts, the intervention generated (0.042*1,270)/415.58*100=12.8 (5.8) additional vaccinations per $100 spent. This impact is considerably larger than the impact per $100 spent on traditional interventions promoting flu shots.

Chapman et al. (2010) studied the effect of opt-out appointments on vaccination rates. As explained in the discussion of automatic savings plan enrollment, defaults capitalize on inertia, inferences about what is recommended, and loss aversion. In the treatment group, the authors automatically scheduled individuals for vaccination appointments, whereas individuals in the control group were only given a web link to schedule appointments on their own. In both conditions, participants were not penalized for missing scheduled appointments, and they could walk into the clinic without an appointment. The opt-out treatment increased the vaccination rate by 11.7 (4.5) percentage points over the opt-in control. In follow-up correspondence, one of the authors estimated that a typical clinic would face a cost of $1.25 for each request it received to change (cancel/add/reschedule) an appointment, a cost of $5 to add staff for each extra appointment, and a cost of $30 for stocking each extra unused vaccine. In the opt-out group, 39 people changed or cancelled appointments, and in the opt-in group, 50 people scheduled appointments. We assume that a clinic must provide enough staff to cover the number of people who have appointments or the number of people who keep their appointment plus the number of walk-ins, whichever is greater, for a total of 221 appointments for the opt-out group and 80 appointments and walk-ins for the opt-in group. We also assume that clinics accurately anticipate the proportion of people who keep their automatic appointments, making the number of vaccines that expire negligible. The opt-out condition then has a total cost of ($1.25*39+$5*221)=$1,153.75, while the opt-in condition has a total cost of ($1.25*50+$5*80)=$462.50 in 2009, so the inflation-adjusted marginal cost of the opt-out condition is $766.06. Given that 239 people were in the treatment group, the opt-out nudge generated a respectable (0.117*239)/766.06*100=3.65 (1.40) additional vaccinations per hundred dollars spent.

As for more traditional policies, Bronchetti, Huffman, and Magnenheim (2015) found that offering a $30 incentive ($31.07 in 2015 dollars) increased vaccination rates at campus clinics by 10.7 (0.9) percentage points. The baseline vaccination rate in the control group was 8.7%, so the treatment generated just 0.107/(31.07*(0.107+0.087))*100=1.78 (0.15) additional vaccinations per hundred dollars spent.

Kimura, Nguyen, Higa, Hurwitz, and Vugia (2007) examined the effect of education and free workplace vaccination clinics. Applying a difference-in-differences approach to their findings, we calculate that the educational campaign increased vaccination rates by 8.19 (2.9) percentage points, while free vaccinations increased vaccination rates by 15.3 (2.9) percentage points. The authors estimated that an educational campaign for 100 employees costs $70 in 2002 dollars ($92.54 in 2015 dollars), while free vaccinations cost $1,080 in 2002 dollars ($1,427.77

in 2015 dollars). The educational and free vaccination treatments therefore generated an impressive (8.19/92.54)*100=8.85 (3.1) and a less remarkable (15.3/1,427.77)*100=1.07 (0.20) additional vaccinations per hundred dollars spent, respectively.

Table 2.

Panel A. Relative effectiveness of interventions targeting retirement savings.

Authors	Treatment	Impact	Cost	Relative effectiveness
Carroll et al. (2009)	**New employees at a company were required to indicate their preferred contribution rate in a workplace retirement savings plan within their first month of employment**	**$200 increase in savings plan contributions per employee[a]**	**$2 per employee for distributing form and for following up with employees who did not respond**	**$100 increase in savings plan contributions per $1 spent[a]**
Chetty et al. (2014)	The Danish government changed the tax deduction for contributions to one type of pension account for the roughly 20% of earners who were in the top tax bracket	$540 (27) change in contributions to the affected pension account per person affected	$195 change in government revenue per person affected	$2.77 (0.14) change in contributions to the affected pension account per $1 spent
Duflo and Saez (2003)	Monetary inducements were offered to receive information about the retirement savings plan available to employees of a large university	$58.95 increase in savings plan contributions per employee[a]	$4.04 per employee for monetary inducements	$14.58 increase in savings plan contributions per $1 spent[a]
Duflo et al. (2006)	Clients preparing a tax return at offices in low- and middle-income neighborhoods in St. Louis were offered 20%, 50%, or no matching contributions for the first $1000 of additional contributions to a retirement savings account	20% match: $93.6 (9.0) in incremental contributions per person; 50% match: $244.5 (12.8) in incremental contributions per person	20% match: $16.7 in matching dollars per person; 50% match: $82.4 in matching dollars per person	20% match: $5.59 (0.54) increase in contributions per $1 spent; 50% match: $2.97 (0.16) increase in contributions per $1 spent
Duflo et al. (2007)	The U.S. federal government increased the tax credit on the first $2000 of retirement savings from 20% to 50% when adjusted gross income dropped below a threshold	$11.6 (1.00) increase in retirement account contributions per person	$9.35 increase in tax credits per person	$1.24 (0.11) increase in retirement account contributions per $1 spent

Note: **Interventions in bold are nudges.** Interventions in normal typeface are traditional interventions. Standard errors are reported in parentheses. Standard errors for the relative effectiveness measure are calculated by scaling the standard errors for the overall impact by the cost of the intervention, ignoring any uncertainty regarding the cost of the intervention.

[a]For this estimate, standard errors could not be calculated using the information reported.

Table 2 continued.

Panel B. Relative effectiveness of interventions targeting college enrollment.

Authors	Treatment	Impact	Cost	Relative effectiveness
Bettinger et al. (2012)	**Tax professionals offered to help low-income families fill out financial aid forms and calculate potential aid amounts at the time of tax preparation**	**8.1 (3.5) percentage point increase in likelihood of attending college the next year**	**$53.02 per participant for training and pay of tax professionals, materials, software, and call center support**	**1.53 (0.66) additional students enrolled in college within the next year per $1,000 spent**
Dynarski (2003)	The Social Security Student Benefit Program gave out monthly stipends to young adults enrolled in college with a parent who was eligible for benefits as a federal post-secondary educational subsidy until the 1980s	18.2 (9.6) percentage point change in likelihood of attending college	$5,181 per eligible person for stipends	0.0351 (0.0185) additional students enrolled in college per $1,000 spent
Long (2004a)	Some states offered state education subsidies for students attending their in-state public universities	2.3 percent increase in number of students attending college (5,535 to 5,664 students)[a,b]	$4,468 per college student ($25.3 million total) for subsidies[b]	0.0051 additional students enrolled in college per $1,000 spent[a]
Long (2004b); Bulman and Hoxby (2015)	The federal government offered the Hope, Lifetime Learning, and American Opportunity Tax Credits to subsidize spending on higher education	Negligible effect		Negligible effect

Note: **Interventions in bold are nudges.** Interventions in normal typeface are traditional interventions. Standard errors are reported in parentheses. Standard errors for the relative effectiveness measure are calculated by scaling the standard errors for the overall impact by the cost of the intervention, ignoring any uncertainty regarding the cost of the intervention.

[a]For this estimate, standard errors could not be calculated using the information reported.

[b]It was not possible to calculate a figure that is strictly comparable to the other figures in the same column.

Table 2 continued.

Panel C. Relative effectiveness of interventions targeting energy conservation.

Authors	Treatment	Impact	Cost	Relative effectiveness
Allcott (2011)	**An independent company sent reports to residential consumers that contained both comparisons to neighbors' electricity usage and tips for conservation**	**2.0 percent reduction in energy usage on average[a]**	**Approximately $1 per report, with reports sent monthly, bi-monthly, or quarterly**	**27.3 kWh saved per $1 spent[a]**
Asensio and Delmas (2015)	**Researchers granted residential consumers access to a website sharing their detailed appliance-level electricity usage information, with messages either linking this usage to health and environmental issues or to increased utility bills**	**Health/environmental messages: 8.192 (4.306) percent reduction in energy usage; Billing-oriented messages: negligible effect**	**$3,019 per household**	**Health/environmental messages: 0.050 (0.026) kWh saved per $1 spent; Billing-oriented messages: negligible effect**
Ito (2015)	Residents in California received discounts on their electricity bills if they reduced their summer energy usage by at least 20% relative to the previous summer	4.2 (1.3) percent reduction in energy usage in inland areas and negligible effect in coastal areas	$3.70 per customer for rebates plus $1.39 per customer for administrative and marketing costs	3.41 kWh saved per $1 spent[a]
Arimura et al. (2012)	Utilities provided incentives and education to reduce energy usage during peak times and promote efficiency investments	0.9 (0.5) percent reduction in energy usage during intervention period and 1.8 (1.1) percent reduction when including effects in future periods	$10.83 per customer on average	14.0 kWh saved per $1 spent[a]

Note: **Interventions in bold are nudges.** Interventions in normal typeface are traditional interventions. Standard errors are reported in parentheses. Standard errors for the relative effectiveness measure are calculated by scaling the standard errors for the overall impact by the cost of the intervention, ignoring any uncertainty regarding the cost of the intervention.

[a]For this estimate, standard errors could not be calculated using the information reported.

Table 2 continued.

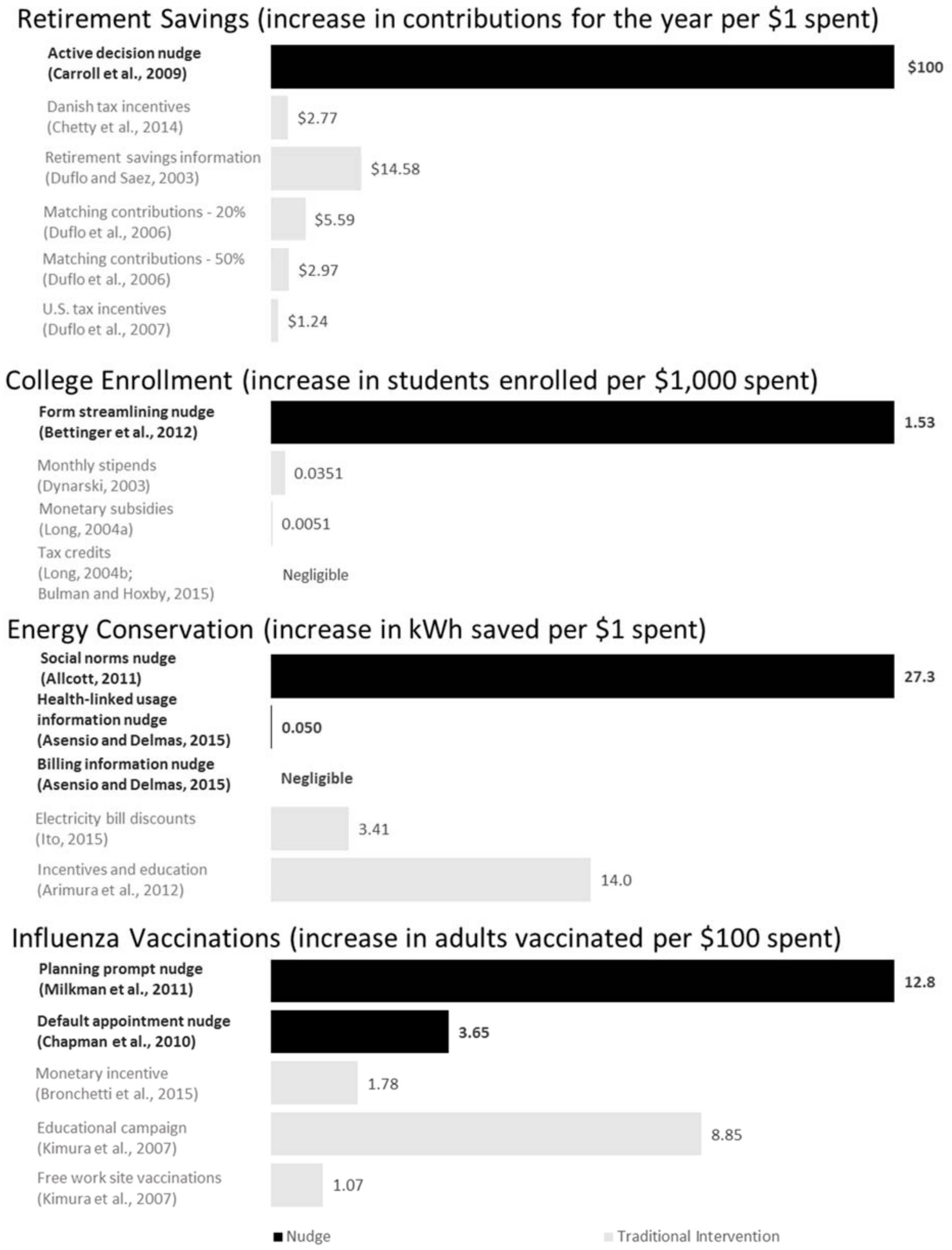
Panel D. Relative effectiveness of interventions targeting influenza vaccination.

Authors	Treatment	Impact	Cost	Relative effectiveness
Milkman et al. (2011)	**An employer modified the normal informational mailings regarding free flu shot clinics to prompt employees to write down details about when they planned to obtain vaccinations**	**4.2 (1.9) percentage point increase in flu shot take-up**	**$0.33 per employee for adding planning prompts to reminder letters**	**12.8 (5.8) additional people vaccinated per $100 spent**
Chapman et al. (2010)	**A university automatically assigned its faculty and staff to (non-mandatory) flu shot appointment times**	**11.7 (4.5) percentage point increase in flu shot take-up**	**$3.21 per person for excess (unutilized) clinic capacity**	**3.65 (1.40) additional people vaccinated per $100 spent**
Bronchetti et al. (2015)	Experimenters paid college students a $30 incentive to get a flu shot at the campus clinic	10.7 (0.9) percentage point increase in flu shot take-up	$6.03 per eligible student for incentive	1.78 (0.15) additional people vaccinated per $100 spent
Kimura et al. (2007)	Conducted an educational campaign on the benefits of influenza vaccination;	Education: 8.19 (2.9) percentage point increase in flu shot take-up	Education: $0.93 per employee	Education: 8.85 (3.1) additional people vaccinated per $100 spent
	Provided free onsite influenza vaccines	Free vaccines: 15.3 (2.9) percentage point increase in flu shot take-up	Free vaccines: $14.28 per employee	Free vaccines: 1.07 (0.20) additional people vaccinated per $100 spent

Note: **Interventions in bold are nudges.** Interventions in normal typeface are traditional interventions. Standard errors are reported in parentheses. Standard errors for the relative effectiveness measure are calculated by scaling the standard errors for the overall impact by the cost of the intervention, ignoring any uncertainty regarding the cost of the intervention.

Figure 1.

Relative effectiveness of interventions in four domains.



Discussion

The contribution of this paper is to extract critical new information from past work by calculating comparable relative effectiveness numbers and examining them side by side to illustrate how different interventions measure up on this important dimension. The results do not provide an exhaustive review of the relative effectiveness of nudges—tools designed to change "people's behavior…without forbidding any options or significantly changing…economic incentives" (Thaler & Sunstein, 2008)—compared to traditional policy tools such as incentives or education. However, our selective but systematic calculations indicate that the impact of nudges on several key outcome variables is often greater, on a cost-adjusted basis, than the impact of traditional tools.

In which situations are nudges more impactful per dollar spent than traditional policy tools, and in which situations are traditional policy tools more impactful? Monetary incentives are likely to be particularly impactful when the policy maker's objective is to correct a clear misalignment between the public interest and the private interests of citizens making carefully reasoned decisions. However, the policy maker's objective in many situations is to change the day-to-day behavior of individuals who are making rushed or otherwise imperfect decisions. As seen in Table 2, monetary incentives in these settings *can* generate large increases in desirable behavior but are often too expensive to generate a favorable ratio of impact to cost. Educational interventions are likely to be impactful when individuals are already considering a decision carefully and are motivated to gather information relevant to the decision. In many settings, however, individuals devote limited attention to policy-relevant decisions, making it difficult for educational interventions to yield large benefits per unit cost in spite of their lower price tags than monetary incentives. Because interventions based on incentives and education seek to change behavior by altering the cost-benefit calculation that individuals undertake when focusing on a particular decision, these interventions face the challenge that individuals' ability (and desire) to engage high-level cognitive capacities is often limited (Shah et al., 2012). Nudges, on the other hand, operate by harnessing individuals' intuitions, emotions, and automatic decision-making processes. These processes can be triggered with simple cues and subtle changes to the choice environment, so nudges can be effective yet cheap, generating high impact per dollar spent.

Should nudges therefore *replace* traditional policy tools? We warn against jumping to this conclusion. Nudges cannot be the only tool for pursuing policy objectives, as the purpose of many nudges is to make it easier for individuals to take advantage of traditional policies already in place to promote certain behaviors. For example, the retirement savings active decision nudge directed greater attention to an already-available tax-advantaged savings plan; the FAFSA intervention increased college attendance by simplifying the process of applying for pre-existing student aid programs; and the vaccination planning prompts caused individuals to focus on how they could follow through on the intention to attend an existing free workplace clinic. Savings plan automatic enrollment and default flu shot appointments required no up-front effort on the part of individuals but nonetheless started them down the path of engaging with existing savings plans and free vaccination clinics, respectively. Even the peer energy use letters, which were not deployed to increase take-up of a pre-existing program, were similar in spirit to the other nudges because they promoted energy-saving techniques that were already viewed as common-sense behaviors.

Thus, in many cases, nudges are valuable when used *in conjunction with* traditional policy tools. Nudges rarely alter individuals' fundamental attitudes towards a behavior, but by tackling motivational barriers, nudges can increase engagement in desirable behaviors. To be effective, nudges do not necessarily require the existence of a traditional policy tool that promotes the target behavior. However, given that many traditional policies are already established, the evidence indicates that governments would generate larger changes in behavior by investing the marginal dollar in nudges to increase utilization of existing programs than by investing the marginal dollar in simple expansions of tax incentives or other financial inducements.

One important caveat to our calculations is that they are not apples-to-apples exercises in that they compare the effectiveness of different interventions without holding fixed the population studied. In the absence of more studies comparing multiple policy interventions simultaneously, this is a limitation of the data available for calculating interventions' costs and benefits. Furthermore, it would be desirable to examine additional consequences of interventions beyond their effects on the narrow behavior targeted (e.g., costs incurred by individuals as they react to the interventions; see Allcott and Kessler, 2015). Finally, diminishing marginal returns from investing in nudges may set in eventually as the "low-hanging fruit" is taken, but research in this domain is so new that there is little reason to believe that the set of highly cost-effective nudges has been exhausted. Furthermore, the downside risk to increased investment in nudges is minimal. The operational philosophy of nudge units is to test competing behavioral interventions and then to cull ineffective ones from the portfolio of nudges. This rapid testing cycle—along with the low cost of deploying most nudges in the first place—ensures that failures are inexpensive.

Conclusion

We offer three recommendations. First, in light of the demonstrated effectiveness of recent behavioral interventions, there should be increased investment in behaviorally-informed policies to supplement traditional policies both inside and outside of governments. Second, existing nudge units should share data and knowledge (e.g., through a central repository) and coordinate efforts to maximize their learning from one another. Tracking failures is just as important for knowledge creation as tracking successes. SBST strictly follows the policy of disclosing results from *all* of its studies. Finally, behavioral scientists should measure relative effectiveness explicitly in their studies in order to quantify the impact of nudge interventions compared to other available policy tools (and to learn which nudge interventions work best). Nudging has entered government in the U.K., in the U.S., and beyond, but in light of growing evidence of its relative effectiveness, we believe that policymakers should nudge more.

Author Contributions

All authors developed the concept for this article. S. Benartzi, J. Beshears, and K. L. Milkman developed the criteria for selecting policy domains and prior studies for inclusion in the relative effectiveness analysis. J. Beshears conducted this analysis. M. Shankar, W. Tucker, W. J. Congdon, S. Galing, and K. L. Milkman designed and implemented the SBST/DOD experiment. J. Beshears and K. L. Milkman conducted the data analysis for the SBST/DOD experiment. S. Benartzi, J. Beshears, and K. L. Milkman drafted the manuscript, and C. Sunstein, R. H. Thaler, M. Shankar, and W. Tucker provided critical revisions. All authors approved the final version of the manuscript for submission.

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SUPPLEMENTAL ONLINE MATERIAL – FOR REVIEW

The SBST/DOD Retirement Savings Experiment

Methods

One of the first experiments conducted by the new White House Social and Behavioral Sciences Team (SBST) was in collaboration with the U.S. Department of Defense (DOD) and sought to help the DOD promote an internal priority—increased retirement savings among its personnel. The experiment included all 806,861 military service members (12.7% Marines, 45.6% Army, 17.8% Navy, 23.9% Air Force) who, as of April 27, 2015, were not contributing to the Thrift Savings Plan (TSP), the defined contribution retirement plan offered to federal government employees. SBST and DOD sent new, experimental emails to these service members on April 29, 2015, with the exception of the control group, which received no email (the business-as-usual practice). Participants were assigned to experimental conditions based on the last two digits of their Social Security numbers. The experimentally-varied components of the mailing were: (1) a list of action steps participants could take to enroll (to simplify their understanding of the process; Sunstein, 2013); (2) a statement that it was the start of spring and end of tax season, making it a perfect time to enroll in the TSP (to take advantage of the "fresh start effect" whereby motivation to pursue goals is greater at moments that are framed as the start of new periods; Dai, Milkman, & Riis, 2014, 2015); (3) framing the opportunity to enroll now as an active choice between saying "yes" and saying "no" to saving (to capitalize on negative feelings associated with saying "no," including regret and guilt; Keller, Harlam, Loewenstein, & Volpp, 2011; Putnam-Farr & Riis, forthcoming); (4) active choice framing that additionally described the "no" option as work-intensive (so inertia would not favor failing to enroll; Johnson & Goldstein, 2003); (5) an example of how small contributions today produce large account balances in the future (to demystify how compound interest works; Stango & Zinman, 2009); and (6) an emphasis on the short-term tax benefits of saving (to counter present bias; Ainslie & Haslam, 1992). Table S1 outlines the design of the experiment, and experimental stimuli are shown in SOM-U.

Table S1.

The design of the SBST/DOD retirement savings experiment.

Sample Size	SSNs	Group	Received Email	Action Steps	Fresh Start Framing	Active Choice Framing	"No" Requires Effort	Compound Interest Clarification	Short-Term Tax Benefits
80,460	00-09	A	No	No	No	No	No	No	No
80,520	10-19	B	Yes	No	No	No	No	No	No
80,615	20-29	C	Yes	Yes	Yes	Yes	No	No	No
81,023	30-39	D	Yes	Yes	Yes	Yes	Yes	No	No
80,797	40-49	E	Yes	Yes	Yes	No	No	No	No
80,184	50-59	F	Yes	Yes	No	Yes	No	No	No
80,921	60-69	G	Yes	Yes	No	Yes	Yes	No	No
80,796	70-79	H	Yes	Yes	No	No	No	No	No
80,925	80-89	I	Yes	Yes	No	No	No	Yes	No
80,620	90-99	J	Yes	Yes	No	No	No	No	Yes

Results

In the SBST/DOD experiment, the business-as-usual control group received no email message and had a 1.1% enrollment rate over the following month. A simple informational email making the TSP program salient produced a 1.6% enrollment rate in the month following the message. All versions of the message that incorporated behaviorally-informed elements beyond the simple informational message generated enrollment rates higher than 1.6% (see regression estimates in Table S2). The most effective behaviorally-informed message provided clear action steps for enrolling and gave a concrete example of how small contributions today can produce large account balances in the future; it led 2.1% of recipients to enroll in the TSP (see Table S2).

This impact may not seem particularly large, but that is precisely why it is important to assess the initiative's effectiveness relative to its cost. Overall, the messages increased TSP enrollment by roughly 5,200 people and increased contributions by more than $1.3 million in the month post-experiment, relative to our estimate of what would have happened had everyone been in the control group.[xvi] The incremental administrative costs of developing and deploying the email campaign were just $5,000. Thus, each dollar spent on the program delivered an estimated $273 increase in savings in the first month, and each additional enrollee cost just $0.96. If we extrapolate and assume that the intervention's effect decays linearly to zero over one year (a conservative assumption given the stickiness of savings plan contribution rates), the program increases savings by approximately $8 million, a $1,600 increase in savings during the year per dollar spent. When measured in terms of incremental savings in a year per dollar spent, the SBST/DOD intervention compares very favorably to alternative strategies that have been used to encourage retirement savings.

[xvi] To estimate the overall effect of the email campaign on enrollment, we ran an OLS regression with only a constant and an indicator variable for being in Groups B-J. Multiplying the point estimate (and the endpoints of the 95% confidence interval) for the coefficient on that indicator variable by the number of individuals in Groups B-J, we estimate that the email campaign increased TSP enrollment by 5,265 people (95% CI: 4,563-5,968). Using the same methodology, we also estimate that the email campaign increased total contributions to the TSP in before-tax and Roth accounts combined in the month following the email campaign by $1,367,423. Note that this last calculation excludes the Marines (for whom we lack the necessary data) and is therefore an understatement of the effect.

Table S2.

Ordinary least squares (OLS) and logistic regressions to predict the impact of different email messages on the TSP enrollment rates and contributions of military service members. See Table S1 for group definitions. Group A (the control group) is omitted. The contributions regressions exclude the Marines, for whom we lack the necessary data.

	Model 1 Enrolled (Percentage Points)	Model 2 Enrolled (Odds Ratios)	Model 3 Monthly Before-tax Contributions ($)	Model 4 Monthly Roth Contributions ($)
Constant	1.143***	0.012***	1.018***	1.385***
	(0.047)	(0.000)	(0.160)	(0.159)
Group B	0.415***	1.369***	0.473*	0.750**
	(0.066)	(0.060)	(0.226)	(0.224)
Group C	0.753***	1.671***	1.016***	1.221***
	(0.066)	(0.070)	(0.226)	(0.224)
Group D	0.670***	1.596***	1.021***	1.338***
	(0.066)	(0.068)	(0.226)	(0.224)
Group E	0.649***	1.578***	0.973***	1.081***
	(0.066)	(0.067)	(0.226)	(0.224)
Group F	0.813***	1.726***	1.062***	0.881***
	(0.066)	(0.072)	(0.226)	(0.224)
Group G	0.797***	1.711***	1.363***	1.203***
	(0.066)	(0.072)	(0.226)	(0.224)
Group H	0.716***	1.638***	0.932***	0.941***
	(0.066)	(0.069)	(0.226)	(0.224)
Group I	0.960***	1.857***	1.234***	1.659***
	(0.066)	(0.077)	(0.226)	(0.224)
Group J	0.751***	1.669***	1.003***	1.247***
	(0.066)	(0.070)	(0.226)	(0.224)
Observations	806,861	806,861	704,294	704,294
Regression Modeling Approach	OLS	Logistic	OLS	OLS
R^2/Pseudo R^2	0.0004	0.0022	0.0001	0.0001
Log likelihood	N/A	-72,447.31	N/A	N/A

* $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$

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SUPPLEMENTAL ONLINE MATERIAL – UNREVIEWED

Design Details of the SBST/DOD Retirement Savings Experiment

On April 29, 2015, the White House Social and Behavioral Sciences Team (SBST) and the Department of Defense (DOD) conducted an experiment including 806,861 military service members (12.7% Marines, 45.6% Army, 17.8% Navy, 23.9% Air Force) who, as of April 27, 2015, were not contributing to the Thrift Savings Plan (TSP), the defined contribution retirement plan offered to federal government employees. The Coast Guard was not part of this study. The experiment consisted of a text-based email campaign designed to encourage savings in the TSP, with a control holdout group that received no message (the business-as-usual practice). Participants were assigned to experimental conditions that differed only in the text of the email message based on the last two digits of their Social Security numbers (SSNs). Specifically, random assignment proceeded as follows (see Exhibit SU1 for the text of the emails):

- 80,460 participants with SSNs ending in 00-09 were assigned to treatment Group A (the control group) and did not receive an email.
- 80,520 participants with SSNs ending in 10-19 were assigned to treatment Group B and received a baseline text-based email message that used language taken from the TSP website.
- 80,615 participants with SSNs ending in 20-29 were assigned to treatment Group C and received a text-based email message that (1) contained a simple list of action steps they could take to enroll (*action steps*), (2) noted that it was currently the start of spring and the end of tax season, making it a perfect time to enroll in the TSP (*fresh start framing*), and (3) framed enrolling now as an active choice between saying “yes” versus “no” to saving (*active choice framing*).

- 81,023 participants with SSNs ending in 30-39 were assigned to treatment Group D and received a text-based email message with *action steps*, *fresh start framing*, and *active choice framing* that additionally described the "no" option under active choice framing as work-intensive (*no requires effort*).
- 80,797 participants with SSNs ending in 40-49 were assigned to treatment Group E and received a text-based email message with *action steps* and *fresh start framing*, but no *active choice framing* (it only invited participants to say "yes" to savings or refrain from responding at all).
- 80,184 participants with SSNs ending in 50-59 were assigned to treatment Group F and received a text-based email message with *action steps*, no *fresh start framing* (it made no mention of the start of spring and end of tax season), and *active choice framing*.
- 80,921 participants with SSNs ending in 60-69 were assigned to treatment Group G and received a text-based email message with *action steps,* no *fresh start framing*, and *active choice framing* in which *no requires effort*.
- 80,796 participants with SSNs ending in 70-79 were assigned to treatment Group H and received a text-based email message with *action steps*, no *fresh start framing*, and no *active choice framing*.
- 80,925 participants with SSNs ending in 80-89 were assigned to treatment Group I and received a text-based email message with *action steps,* no *fresh start framing*, and no *active choice framing* that gave a concrete example of how small contributions today produce large account balances in the future (*compound interest clarification*).
- 80,620 participants with SSNs ending in 90-99 were assigned to treatment Group J and received a text-based email message with *action steps,* no *fresh start framing*, and no

active choice framing that emphasized the short-term tax benefits of saving (*short-term tax benefits*).

Table S1 illustrates the design of the experiment. As of June 1, 2015, the DOD reported to us the number of individuals in the experiment in each treatment group (i.e., Group A, Group B, etc.) and in each division of the military (i.e., Marines, Army, Navy, and Air Force) who had enrolled in the TSP since April 29, 2015. Except in the case of the Marines, the DOD also reported the percentage contribution rate to the TSP in both before-tax and Roth accounts for each participant who enrolled, as well as that participant's pay grade. An individual's base pay is determined by pay grade and years of service. Since we do not observe years of service, we conservatively estimate an individual's base pay using pay grade and the minimum number of years required to attain that pay grade.

Detailed Results of the SBST/DOD Retirement Savings Experiment

Enrollment

To evaluate the impact of the email campaign and the different experimental messages on TSP enrollment, we ran both ordinary least squares (OLS) and logistic regressions to predict whether a given individual enrolled in the TSP between April 29, 2015, and June 1, 2015, as a function of the email message he or she received. Group A is the omitted category. Table S2 presents the results of these regressions. Model 1 is the OLS regression, and Model 2 is the logistic regression.

Dollar Contributions

Data on contribution rates are not available for the Marines, so our analysis of dollar contributions drops the Marines from the sample. To calculate before-tax and Roth contributions to the TSP for the month following the email campaign, we multiply the before-tax and Roth contribution rates by estimated monthly salary. Table S2 reports the results of OLS regressions predicting dollar contributions to the TSP in both before-tax accounts (Model 3) and Roth accounts (Model 4) using indicator variables for each experimental treatment. Group A is the omitted category. The dollar contribution results largely mirror the enrollment results.

Components of the Email Messages

The design of this experiment allows us to assess which elements of the emails increased TSP enrollment and contributions. We estimate additional regressions that replace the indicator variables for different email messages with indicator variables for each of the different email components summarized above (*action steps*, *compound interest clarification*, etc.). Table SU1 displays the results. This analysis reveals that simply sending an email, regardless of its exact contents, significantly increases military service members' enrollment rate in the TSP ($p<0.001$) and contributions to the TSP ($p<0.05$). Presenting clear *action steps* further significantly increases enrollment ($p<0.001$) and before-tax contributions ($p<0.05$). In addition, including *compound interest clarification* significantly increases enrollment ($p<0.001$) and Roth contributions ($p<0.01$). The *fresh start framing*, *active choice framing*, and *no requires effort* email components do not have a significant impact on enrollment or contributions. Similarly, emphasizing the *short-term tax benefits* of savings does not have a significant effect on enrollment or contributions.

Table SU1.

Ordinary least squares (OLS) and logistic regressions to predict the impact of different email components on the TSP enrollment rates and contributions of military service members. Group A (the control group) is omitted.

	Model 5 Enrolled (Percentage Points)	Model 6 Enrolled (Odds Ratios)	Model 7 Monthly Before-tax Contributions ($)	Model 8 Monthly Roth Contributions ($)
Constant	1.143***	0.012***	1.018***	1.385***
	(0.047)	(0.000)	(0.160)	(0.159)
Received Email	0.415***	1.369***	0.473*	0.750**
	(0.066)	(0.060)	(0.226)	(0.224)
Action Steps	0.300***	1.196***	0.459*	0.190
	(0.066)	(0.046)	(0.226)	(0.224)
Fresh Start Framing	-0.067	0.963	0.041	0.140
	(0.066)	(0.036)	(0.226)	(0.224)
Active Choice Framing	0.098	1.054	0.130	-0.060
	(0.066)	(0.038)	(0.226)	(0.224)
Active Choice Framing × Fresh Start Framing	0.007	1.006	-0.087	0.200
	(0.094)	(0.052)	(0.320)	(0.317)
No Requires Effort	-0.017	0.991	0.301	0.323
	(0.066)	(0.036)	(0.226)	(0.224)
No Requires Effort × Fresh Start Framing	-0.068	0.963	-0.296	-0.206
	(0.093)	(0.050)	(0.319)	(0.316)
Compound Interest Clarification	0.244***	1.134***	0.302	0.718**
	(0.066)	(0.041)	(0.226)	(0.224)
Short-term Tax Benefits	0.035	1.019	0.071	0.306
	(0.066)	(0.037)	(0.226)	(0.224)
Observations	806,861	806,861	704,294	704,294
Regression Modeling Approach	OLS	Logistic	OLS	OLS
R^2/ Pseudo R^2	0.0004	0.0022	0.0001	0.0001
Log likelihood	N/A	-72,447.31	N/A	N/A

* $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$

Exhibit SU1.

SBST/DOD retirement savings experiment stimuli.

GROUP A: SSNs ending in 00-09
No Email Sent
GROUP B: SSNs ending in 10-19
Subject: Contribute to TSP to Invest in Your Future
You are eligible to invest in the Thrift Savings Plan (TSP). The TSP is similar to the 401K plan or a deductible Individual Retirement Account (IRA) offered by many private corporations – we encourage you to consider the benefits of TSP. You may want to choose to enroll today by logging onto MyPay and selecting a contribution percentage.
You may start, change or stop your contributions at any time. If you are enrolling for the first time, select a contribution percentage of at least 1% equivalent of your basic pay.
Your elections may be submitted quickly and securely using MyPay. You may also use a TSP-U-1 form available at www.tsp.gov; this website also has information about Traditional vs. Roth TSP. Forms must be submitted to your servicing finance office.
For more information about the TSP visit the tsp website (above), http://www.dfas.mil/militarymembers/tspformilitary/tspac.html/, or speak to your installation personal financial manager.
GROUP C: SSNs ending in 20-29
Subject: TSP: Our Records Indicate You Aren't Enrolled
With tax season over and spring beginning, now is the perfect time to take action and make a choice to ensure you don't lose out on a secure future by investing with a Thrift Savings Plan (TSP). TSP is like a 401k or a deductible Individual Retirement Account.

DO YOU WANT TO SIGN UP TO SAVE?
CHOICE 1: YES, I WANT TO SAVE THROUGH TSP! Follow these simple steps (<5 mins):
(1) Log in at mypay.dfas.mil*
(2) Click on the "Traditional TSP and Roth TSP" link.
(3) Enter the percentage of your basic, special, incentive, and bonus pay that you want to contribute, press submit and you're done!
CHOICE 2: NO, I DON'T WANT TO SAVE THROUGH TSP.

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* If you prefer a paper form, complete the TSP-U-1 form at www.tsp.gov; this website also has information on Traditional versus Roth TSP and investment options; or you can visit with your installation personal financial manager.

PS. With tax day behind you and spring beginning, it is the perfect time to start fresh: Go to mypay.dfas.mil and make your choice to start saving today!
GROUP D: SSNs ending in 30-39
Subject: TSP: Our Records Indicate You Aren't Enrolled
With tax season over and spring beginning, now is the perfect time to take action and make a choice to ensure you don't lose out on a secure future by investing with a Thrift Savings Plan (TSP). TSP is like a 401k or a deductible Individual Retirement Account.

DO YOU WANT TO SIGN UP TO SAVE?
CHOICE 1: YES, I WANT TO SAVE THROUGH TSP! Follow these simple steps (<5 mins):
(1) Log in at mypay.dfas.mil*

(2) Click on the “Traditional TSP and Roth TSP” link.
(3) Enter the percentage of your basic, special, incentive, and bonus pay that you want to contribute, press submit and you’re done!
CHOICE 2: NO, I DON’T WANT TO SAVE THROUGH TSP. Go to mypay.dfas.mil and follow steps (2) and (3) if you want to invest in your future or make changes down the line.

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* If you prefer a paper form, complete the TSP-U-1 form at www.tsp.gov; this website also has information on Traditional versus Roth TSP and investment options; or you can visit with your installation personal financial manager.

PS. With tax day behind you and spring beginning, it is the perfect time to start fresh: Go to mypay.dfas.mil and make your choice to start saving today!

GROUP E: SSNs ending in 40-49

Subject: TSP: Our Records Indicate You Aren’t Enrolled

With tax season over and spring beginning, now is the perfect time to take action to ensure you don’t lose out on a secure future by investing with a Thrift Savings Plan (TSP). TSP is like a 401k or a deductible Individual Retirement Account.

DO YOU WANT TO SIGN UP TO SAVE?
YES, I WANT TO SAVE THROUGH TSP! Follow these simple steps (<5 mins):
(1) Log in at mypay.dfas.mil*
(2) Click on the “Traditional TSP and Roth TSP” link.
(3) Enter the percentage of your basic, special, incentive, and bonus pay that you want to contribute, press submit and you’re done!

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* If you prefer a paper form, complete the TSP-U-1 form at www.tsp.gov; this website also has information on Traditional versus Roth TSP and investment options; or you can visit with your installation personal financial manager.

PS. With tax day behind you and spring beginning, it is the perfect time to start fresh: Go to mypay.dfas.mil and start saving today!

GROUP F: SSNs ending in 50-59

Subject: TSP: Our Records Indicate You Aren’t Enrolled

Now is the perfect time to take action and make a choice to ensure you don’t lose out on a secure future by investing with a Thrift Savings Plan (TSP). TSP is like a 401k or a deductible Individual Retirement Account.

DO YOU WANT TO SIGN UP TO SAVE?
CHOICE 1: YES, I WANT TO SAVE THROUGH TSP! Follow these simple steps (<5 mins):
(1) Log in at mypay.dfas.mil*
(2) Click on the “Traditional TSP and Roth TSP” link.
(3) Enter the percentage of your basic, special, incentive, and bonus pay that you want to contribute, press submit and you’re done!
CHOICE 2: NO, I DON’T WANT TO SAVE THROUGH TSP.

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* If you prefer a paper form, complete the TSP-U-1 form at www.tsp.gov; this website also has information on Traditional versus Roth TSP and investment options; or you can visit with your installation personal financial manager.

PS. Go to mypay.dfas.mil and make your choice to start saving today!

GROUP G: SSNs ending in 60-69

Subject: TSP: Our Records Indicate You Aren’t Enrolled

Now is the perfect time to take action and make a choice to ensure you don’t lose out on a secure future by investing with a Thrift Savings Plan (TSP). TSP is like a 401k or a deductible Individual Retirement Account.

DO YOU WANT TO SIGN UP TO SAVE?
CHOICE 1: YES, I WANT TO SAVE THROUGH TSP! Follow these simple steps (<5 mins):
(1) Log in at mypay.dfas.mil*
(2) Click on the “Traditional TSP and Roth TSP” link.
(3) Enter the percentage of your basic, special, incentive, and bonus pay that you want to contribute, press submit and you’re done!
CHOICE 2: NO, I DON’T WANT TO SAVE THROUGH TSP. Go to mypay.dfas.mil and follow steps (2) and (3) if you want to invest in your future or make changes down the line.

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* If you prefer a paper form, complete the TSP-U-1 form at www.tsp.gov; this website also has information on Traditional versus Roth TSP and investment options; or you can visit with your installation personal financial manager.

PS. Go to mypay.dfas.mil and make your choice to start saving today!
GROUP H: SSNs ending in 70-79
Subject: TSP: Our Records Indicate You Aren’t Enrolled
Now is the perfect time to take action to ensure you don’t lose out on a secure future by investing with a Thrift Savings Plan (TSP). TSP is like a 401k or a deductible Individual Retirement Account.

DO YOU WANT TO SIGN UP TO SAVE?
YES, I WANT TO SAVE THROUGH TSP! Follow these simple steps (<5 mins):
(1) Log in at mypay.dfas.mil*
(2) Click on the “Traditional TSP and Roth TSP” link.
(3) Enter the percentage of your basic, special, incentive, and bonus pay that you want to contribute, press submit and you’re done!

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* If you prefer a paper form, complete the TSP-U-1 form at www.tsp.gov; this website also has information on Traditional versus Roth TSP and investment options; or you can visit with your installation personal financial manager.

PS. Go to mypay.dfas.mil and start saving today!
GROUP I: SSNs ending in 80-89
Subject: TSP: Our Records Indicate You Aren’t Enrolled
Now is the perfect time to take action to ensure you don’t lose out on a secure future by investing with a Thrift Savings Plan (TSP). TSP is like a 401k or a deductible Individual Retirement Account - you can invest in your future – if you’d put away just $25 a month starting in 1980, it’d be worth over $66,700 today.

DO YOU WANT TO SIGN UP TO SAVE?
YES, I WANT TO SAVE THROUGH TSP! Follow these simple steps (<5 mins):
(1) Log in at mypay.dfas.mil*
(2) Click on the “Traditional TSP and Roth TSP” link.
(3) Enter the percentage of your basic, special, incentive, and bonus pay that you want to contribute, press submit and you’re done!

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* If you prefer a paper form, complete the TSP-U-1 form at www.tsp.gov; this website also has information on Traditional versus Roth TSP and investment options; or you can visit with your installation personal financial manager.

PS. Go to mypay.dfas.mil and start saving today!

GROUP J: SSNs ending in 90-99

Subject: TSP: Our Records Indicate You Aren’t Enrolled

Now is the perfect time to take action to ensure you don’t lose out on a secure future by investing with a Thrift Savings Plan (TSP). TSP is like a 401k or a deductible Individual Retirement Account: save on taxes today while investing for the future.

DO YOU WANT TO SIGN UP TO SAVE?

YES, I WANT TO SAVE THROUGH TSP! Follow these simple steps (<5 mins):

(1) Log in at mypay.dfas.mil*

(2) Click on the “Traditional TSP and Roth TSP” link.

(3) Enter the percentage of your basic, special, incentive, and bonus pay that you want to contribute, press submit and you’re done!

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* If you prefer a paper form, complete the TSP-U-1 form at www.tsp.gov; this website also has information on Traditional versus Roth TSP and investment options; or you can visit with your installation personal financial manager.

PS. Go to mypay.dfas.mil and start saving today!