



2017

## Should Governments Invest More in Nudging?

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### Recommended Citation

Benartzi, S., Beshears, J., Milkman, K. L., Thaler, R. H., Shankar, M., Tucker-Ray, W., Congdon, W. J., & Galing, S. (2017). Should Governments Invest More in Nudging?. *Psychological Science*, 28 (8), 1041-1055. <http://dx.doi.org/10.1177/0956797617702501>

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## Should Governments Invest More in Nudging?

### 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 coercion or significant changes to economic incentives. We calculated ratios of impact to cost for nudge interventions and for traditional policy tools, such as tax incentives and other financial inducements, and we found that nudge interventions often compare favorably with traditional interventions. We conclude that nudging is a valuable approach that should be used more often in conjunction with traditional policies, but more calculations are needed to determine the relative effectiveness of nudging.

### Disciplines

Business | Finance and Financial Management | Social and Behavioral Sciences

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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 more 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 or replacement for traditional economic levers, such as incentives, to shape the behavior of citizens and government personnel to promote 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 behavior 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). Of course, it is important to emphasize that behaviorally informed approaches can also be implemented by agencies without the use of designated nudge units.

A key feature of behavioral strategies 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 impose material costs but instead alter the underlying “choice architecture,” for example by changing the default option to take advantage of people’s tendency to accept defaults passively. Nudges stand in contrast to

traditional policy tools, which change behavior with mandates or bans or through economic incentives (including significant subsidies or fines).

For example, a behaviorally informed policy intervention might automatically enroll people in programs designed to reduce poverty (U.S. Department of Agriculture, 2013), eliminate or reduce paperwork requirements for obtaining licenses or permits, or streamline the process of applying for government financial aid for college attendance (Bettinger, Long, Oreopoulos, & Sanbonmatsu, 2012). Many nudges have this general form; they simplify processes to make benefits more readily available. As governments decide on the appropriate resources to invest in nudge policies, an important question is how efficiently nudge initiatives achieve their objectives. A nudge policy that increases engagement in a desired behavior (e.g., college attendance) by a larger amount per dollar spent than a traditional intervention would be an attractive investment of public resources.

This point may seem obvious, and some nudges do produce self-evidently large behavioral changes (Benartzi and Thaler, 2013). But because extremely cost-effective nudges do not always create large absolute shifts in behavior, scholars and policy makers may underappreciate their value in the absence of cost-effectiveness calculations. 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-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 seems 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 just the first month post-experiment.<sup>1</sup> If we extrapolate and assume that the intervention's effect decays linearly to zero over one year (a highly conservative assumption given the stickiness of savings plan contributions), 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 demonstrates that nudge policies do not need to produce a large impact in absolute terms to be effective.

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<sup>1</sup> 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.

Past studies on nudges, including those disseminated by existing nudge units, have typically measured only the extent to which an intended behavior was changed (if at all). To be maximally informative, future policy-oriented behavioral science research should measure the impact per dollar spent on behavioral interventions in comparison to more traditional interventions. In the absence of such 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.

## **Method**

### **Study Selection Criteria**

We formed a list of policy areas by combining the focus areas from the 2015 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. Within each category, we identified one well-defined behavior to be our outcome variable of interest. The SOM-R details our selection methodology. In short, when a policy area had an obvious behavior to focus on, the choice was simple (e.g., in “Energy,” we focus on energy consumption). When there was no obvious target, 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. Table 1 displays the SBST and BIT policy areas of focus, our categorization of these areas, areas that were excluded, and outcomes variables of interest.



**Table 1.**

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

<b>Our Categorization</b>	<b>Corresponding Focus Area(s) in SBST 2015 Annual Report</b>	<b>Corresponding Focus Area(s) in BIT 2013-2015 Update Report</b>	<b>Outcome Variable of Interest</b>
Financial Security in Retirement	Promoting Retirement Security	Empowering Consumers <sup>a</sup>	Retirement savings
Education	Improving College Access & Affordability	Education	College enrollment among recent high school graduates
Energy	n/a	Energy & Sustainability	Energy consumption
Health	Helping Families Get Health Coverage & Stay Healthy	Health & Wellbeing	Adult outpatient influenza vaccinations
Job Training	Advancing Economic Opportunity	Economic Growth & the Labour Market Skills & Youth	Enrollment in job training programs <sup>c</sup>
Program Integrity & Compliance	Promoting Program Integrity & Compliance	Fraud, Error & Debt <sup>b</sup>	Compliance with paying a required fee to the government <sup>c</sup>
Home Affairs	n/a	Home Affairs	Reducing crimes such as illegal migration, mobile phone theft, and online exploitation <sup>c</sup>

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).

<sup>a</sup>We group this focus area with SBST's Promoting Retirement Security area because its leading example has to do with pensions.

<sup>b</sup>We group this focus area with SBST's Promoting Program Integrity & Compliance area because both focus on improving tax and fee collection.

<sup>c</sup>In "Job Training," "Program Integrity & Compliance," and "Home Affairs," the targeted behaviors were not studied in published research papers in leading academic journals from 2000 to mid-2015 (see below for an explanation of our journal selection criteria), so we exclude these areas from our analysis.

We next searched leading academic journals for original research, published from 2000 to mid-2015, studying interventions aimed at directly influencing outcome variables of interest. Using Google Scholar to determine academic journal rankings,<sup>2</sup> we limited our set of academic journals to Google Scholar's three leading general interest journals (*Science*, *Nature*, and

<sup>2</sup> *Top Publications*. (September 29, 2015). Retrieved from [https://scholar.google.com/citations?view\\_op=top\\_venues](https://scholar.google.com/citations?view_op=top_venues)

*Proceedings of the National Academy of Sciences*); three leading economics journals, excluding finance journals (*American Economic Review*, *Quarterly Journal of Economics*, and *Review of Economics and Statistics*); three leading psychology journals, excluding journals that publish only review articles (*Psychological Science*, *Journal of Personality and Social Psychology*, and *Journal of Applied Psychology*); and, in the case of Health, three leading general medical journals (*New England Journal of Medicine*, *Lancet*, and *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). (Note that reminders and streamlined or salient disclosure policies can qualify as nudges, but for present purposes, we do not count traditional educational programs as such.) If our search for papers studying a given outcome variable did not identify a paper that met our inclusion criteria, we dropped that outcome variable from our analysis. If our search for papers studying a given outcome variable identified papers that met our inclusion criteria and that covered some but not all of the four intervention types above, we attempted to fill the gaps by widening our search.

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 method likely gave an advantage to nudges over incentives and educational interventions in our relative effectiveness calculations. However, it may be appropriate to confer this advantage if policy makers are indeed selective in applying

nudges where they have a high potential for impact. Furthermore, we are careful to focus only on settings of major domestic policy interest,<sup>3</sup> making our findings highly policy-relevant regardless of any selection concerns.<sup>4</sup>

### **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.<sup>5</sup>

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.<sup>6</sup> We often need to make additional assumptions to produce intervention cost estimates. Some interventions affect an outcome by increasing take-up of another program that affects the outcome.<sup>7</sup> One may argue that in these situations, interventions have additional, indirect costs

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

<sup>4</sup> Another potential concern is that our process for selecting research papers might be likely to identify false-positive results. We have conducted *p*-curve analyses for the key results identified by our process. The collection of results concerning nudge interventions has evidential value, as does the collection of results concerning traditional interventions. See the SOM-U.

<sup>5</sup> We adjust all costs to June 2015 levels using the annual CPI from the year of intervention. For multi-year interventions, we adjust using the midpoint year.

<sup>6</sup> For example, Bettinger et al. (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.

<sup>7</sup> 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.

because they increase the use of other programs. However, in most of cases we study, the intervention simply encourages use of 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.<sup>8</sup> 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 we study within a domain operate over similar time horizons. We evaluate retirement savings interventions over a horizon of one year. Similarly, college education interventions are measured in terms of their impact on annual enrollment, and influenza vaccination interventions operate over the course of a single year's vaccination cycle (approximately September through December). In contrast, results from energy conservation interventions are reported for time horizons ranging from a few months to several years, and we note these differences when discussing energy conservation calculations. However, even in the case of 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.

Some experimental studies have multiple treatment arms, and experimenters incur research costs (e.g., data collection costs, participant payments) for all study arms, including the control group. Treatment effects are estimated based on the marginal increase in the outcome variable in the treatment group over the control group, and we calculate intervention costs in the same way: as the marginal cost of the treatment over the cost of the control. We further focus our

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<sup>8</sup> 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.

attention on capturing the primary costs for each intervention, and we omit the costs of any minor unreported aspects of the program.<sup>9</sup>

Of course, relative effectiveness calculations do not address the question of whether increasing the behavior in question is socially beneficial. Our approach is to take stated government goals as given and then to address how best those goals can be achieved.

### **Results**

We now describe the results of our relative effectiveness calculations, summarized in Table 2 and Figure 1. Except where noted, monetary amounts are reported in 2015 dollars. Readers interested in additional details should consult the SOM-U.

#### **Increasing Retirement Savings**

Carroll, Choi, Laibson, Madrian, and Metrick (2009) studied an active decision nudge for retirement savings. A company's new employees were required to indicate their preferred contribution rate in a workplace 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, 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 ensures that procrastination will not prevent new employees from signing up for the plan (O'Donoghue & Rabin, 2001).

We conservatively apply the one percentage point average contribution rate increase to an annual salary of \$20,000 (well below these employees' median income), for a contribution

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<sup>9</sup> 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.

increase of \$200 per employee. We estimate that the cost of including the savings plan enrollment form in the information packet for new hires and following up with the 5% of employees who failed to return the form was approximately \$2 per employee, so the active decision nudge generated \$100 of additional savings per dollar spent.

Perhaps the best-known nudge for promoting savings in workplace retirement accounts is to enroll employees automatically and/or to use automatic escalation to 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 already exists.<sup>10</sup> By contrast, the same paper studies a reduction in the tax deduction available for contributions to a particular type of retirement account, showing that this traditional policy change reduced contributions by 2,449 DKr, or \$540 in U.S. dollars, and increased government revenues by 883 DKr, or \$195 in U.S. dollars, for each person affected by the change, implying the tax deduction generated only \$2.77 of additional savings in this type of account per dollar of government expenditure.<sup>11</sup>

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<sup>10</sup> Madrian and Shea (2001) and Card and Ransom (2011) study automatic enrollment and related nudges and find similar results.

<sup>11</sup> We convert Danish kroner to U.S. dollars using the 6.5-to-1 exchange rate preferred by Chetty et al. (2014), and we then adjust from 1999 to 2015 price levels. Chetty et al. (2014) also study the extent to which savings increases in a retirement account caused by changes to automatic contributions or caused by changes to tax incentives are offset by savings *decreases* in an individual's other financial accounts. The offset is minor in the case of changes to

Duflo and Saez (2003) tested a (traditional) educational intervention, offering a university's employees \$20 to attend a benefits fair to receive information about their retirement savings plan. This intervention increased plan contributions over the next year by \$58.95 at a cost of \$4.04 per employee, generating \$14.58 in additional contributions in the year per dollar spent.<sup>12</sup>

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 \$76.9 [\$162.1] more to the account relative to the control group and received average matching contributions of \$16.7 [\$82.4], for total incremental contributions of \$93.6 [\$244.5] per treated client, and a mere \$5.59 [\$2.97] 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 specifically to studying these tax credits. The authors estimate that an increase in the tax credit from 20% to 50% of contributions generates an additional \$11.6 of deposits to a retirement account, from an average of \$12.0 to \$23.5. This increase translates to just  $\$11.6 / (0.5 * 23.5 - 0.2 * 12.0) = \$1.24$  of retirement savings per dollar of tax credits.

### **Increasing College Enrollment among Recent High School Graduates**

When H&R Block tax professionals facilitated the process of filing the Free Application for Federal Student Aid (FAFSA) for their clients, high school seniors whose families received

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automatic contributions. However, when savings in a retirement account respond to changes to tax incentives for the account, this response is almost completely offset by adjustments in other accounts. The other papers that we analyze do not report results regarding the extent of such offsetting because the data are not available.

<sup>12</sup> 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.

the assistance were 8.1 percentage points more likely to attend college the following year. The incremental cost of this nudge intervention over the control group was \$53.02 per participant. Thus, it produced 1.53 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, alleviating anxiety about making errors, reducing the stigma for low socioeconomic status individuals associated with filling out the FAFSA, and increasing the salience and perceived value of completing it. When this nudge 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 percentage points.<sup>13</sup> The average annual subsidy for each student in 1980 was \$9,252, and 56% of the eligible group attended college for a cost per eligible individual of \$5,181. The program therefore generated  $0.182/5,181 \times 1,000 = 0.0351$  additional college enrollees per thousand dollars spent.<sup>14</sup> This impact per thousand dollars spent is approximately 40 times smaller than the corresponding impact of the Bettinger et al. (2012) nudge.

Long (2004a) studied 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

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<sup>13</sup> This study evaluated the elimination of an incentive rather than the addition of an incentive, which may not have symmetric effects given past research showing that losses loom larger than gains (Kahneman and Tversky, 1979).

<sup>14</sup> 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.



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 \$5,367 per student at a four-year college and \$2,683 per student at a two-year college. The total voucher expenditure would therefore be  $(3,766 * \$5,367 + 1,898 * \$2,683) = \$25.3$  million. The educational vouchers therefore increased college enrollment by just  $(5,664 - 5,535) / 25,300,000 * 1,000 = 0.0051$  students per thousand dollars spent.

Two studies of tax incentives for college enrollment examining the Hope, Lifetime Learning, and American Opportunity Tax Credits estimate that these 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 electricity consumption by sending them letters comparing their energy use to that of their neighbors. This intervention harnesses both competitiveness and the power of social norms. 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 \$0.0367 (\$0.0331 in 2009 dollars) of expenditure for each kWh of electricity saved over the course of approximately two years, or saved 27.3 kWh per dollar spent.<sup>15</sup>

Asensio and Delmas (2015) studied a nudge that strategically framed information provided to households from meters recording appliance-level electricity usage. Giving

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<sup>15</sup> Allcott and Mullainathan (2010) report similar results.

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 percent, or  $(0.0819 \times 8.66 \times 100) = 70.9$  kWh over 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 149.8 kWh saved in total per household. The authors report (via private correspondence) that the cost of the treatment was \$3,019 per household. The intervention thus saved an unremarkable 0.050 kWh per dollar spent. The authors also tested an alternative nudge providing 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.<sup>16</sup>

In the category of economic incentives, 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 the summer decreased by 60.5 million kWh. Ito (2015) calculates that the program spent 29.3 cents (24.1 cents in 2005 levels) for each kWh saved, and it therefore saved 3.41 kWh per dollar spent.

Arimura, Li, Newell, and Palmer (2012) estimated the effect of demand-side management and energy efficiency policies, which combined education and incentives, 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 \$0.071 (\$0.050 in 1999 dollars) per kWh saved, and they saved an impressive 14.0 kWh per dollar spent.

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<sup>16</sup> 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.

### **Increasing Adult Outpatient Influenza Vaccinations**

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 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 in 2011 dollars, totaling \$415.58 in 2015 dollars. With 1,270 employees receiving the prompts, the intervention generated  $(0.042 * 1,270) / 415.58 * 100 = 12.8$  additional vaccinations per \$100 spent.

Chapman et al. (2010) studied the effect of opt-out appointments (a nudge) on vaccination rates. As explained in the discussion of automatic savings plan enrollment, defaults capitalize on inertia, inferences about recommendations, and loss aversion. In the treatment group, individuals were automatically scheduled for vaccination appointments, while individuals in the control group were only given a web link to schedule their own appointments. In both conditions, participants were not penalized for missing appointments, and they could walk into the clinic without an appointment. The opt-out treatment increased the vaccination rate by 11.7 percentage points over the opt-in control. In follow-up correspondence, one of the authors estimated that a clinic faces a cost of \$1.25 for each request 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. In the opt-in group, 50 people scheduled appointments (none were changed or cancelled).

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  $(\$1.25*39+\$5*221)=\$1,153.75$  in 2009 dollars, while the opt-in condition has a total cost of  $(\$1.25*50+\$5*80)=\$462.50$  in 2009 dollars, 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  $(0.117*239)/766.06*100=3.65$  additional vaccinations per hundred dollars spent.

As for price-based 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 percentage points. The baseline vaccination rate in the control group was 8.7%, so the treatment generated just  $0.107/(31.07*(0.107+0.087))*100=1.78$  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 percentage points, while free vaccinations increased vaccination rates by 15.3 percentage points. The authors estimated that an educational campaign for 100 employees costs \$92.54, while free vaccinations cost \$1,427.77. The educational and free vaccination treatments therefore generated an impressive  $(8.19/92.54)*100=8.85$  and a less remarkable  $(15.3/1,427.77)*100=1.07$  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)	<b>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</b>	<b>\$200 increase in savings plan contributions per employee<sup>a</sup></b>	<b>\$2 per employee for distributing form and for following up with employees who did not respond</b>	<b>\$100 increase in savings plan contributions per \$1 spent<sup>a</sup></b>
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 employees of a large university for attending a benefits fair where they would receive information about the retirement savings plan	\$58.95 increase in savings plan contributions per employee <sup>a</sup>	\$4.04 per employee for monetary inducements	\$14.58 increase in savings plan contributions per \$1 spent <sup>a</sup>
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 (financial incentives, educational programs or some combination of the two). 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.

<sup>a</sup>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)	<b>Tax professionals offered to help low-income families fill out financial aid forms and calculate potential aid amounts at the time of tax preparation</b>	<b>8.1 (3.5) percentage point increase in likelihood of attending college the next year</b>	<b>\$53.02 per participant for training and pay of tax professionals, materials, software, and call center support</b>	<b>1.53 (0.66) additional students enrolled in college within the next year per \$1,000 spent</b>
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) <sup>a,b</sup>	\$4,468 per college student (\$25.3 million total) for subsidies <sup>b</sup>	0.0051 additional students enrolled in college per \$1,000 spent <sup>a</sup>
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 (financial incentives, educational programs or some combination of the two). 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.

<sup>a</sup>For this estimate, standard errors could not be calculated using the information reported.

<sup>b</sup>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)	<b>An independent company sent reports to residential consumers that contained both comparisons to neighbors' electricity usage and tips for conservation</b>	<b>2.0 percent reduction in energy usage on average<sup>a</sup></b>	<b>Approximately \$1 per report, with reports sent monthly, bi-monthly, or quarterly</b>	<b>27.3 kWh saved per \$1 spent<sup>a</sup></b>
Asensio and Delmas (2015)	<b>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</b>	<b>Health/environmental messages: 8.192 (4.306) percent reduction in energy usage; Billing-oriented messages: negligible effect</b>	<b>\$3,019 per household</b>	<b>Health/environmental messages: 0.050 (0.026) kWh saved per \$1 spent; Billing-oriented messages: negligible effect</b>
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 <sup>a</sup>
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 <sup>a</sup>

Note: **Interventions in bold are nudges.** Interventions in normal typeface are traditional interventions (financial incentives, educational programs or some combination of the two). 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.

<sup>a</sup>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)	<b>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</b>	<b>4.2 (1.9) percentage point increase in flu shot take-up</b>	<b>\$0.33 per employee for adding planning prompts to reminder letters</b>	<b>12.8 (5.8) additional people vaccinated per \$100 spent</b>
Chapman et al. (2010)	<b>A university automatically assigned its faculty and staff to (non-mandatory) flu shot appointment times</b>	<b>11.7 (4.5) percentage point increase in flu shot take-up</b>	<b>\$3.21 per person for excess (unutilized) clinic capacity</b>	<b>3.65 (1.40) additional people vaccinated per \$100 spent</b>
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;  Provided free onsite influenza vaccines	Education: 8.19 percentage point increase in flu shot take-up <sup>a</sup>  Free vaccines: 15.3 percentage point increase in flu shot take-up <sup>a</sup>	Education: \$0.93 per employee  Free vaccines: \$14.28 per employee	Education: 8.85 additional people vaccinated per \$100 spent <sup>a</sup>  Free vaccines: 1.07 additional people vaccinated per \$100 spent <sup>a</sup>

Note: **Interventions in bold are nudges.** Interventions in normal typeface are traditional interventions (financial incentives, educational programs or some combination of the two). 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.

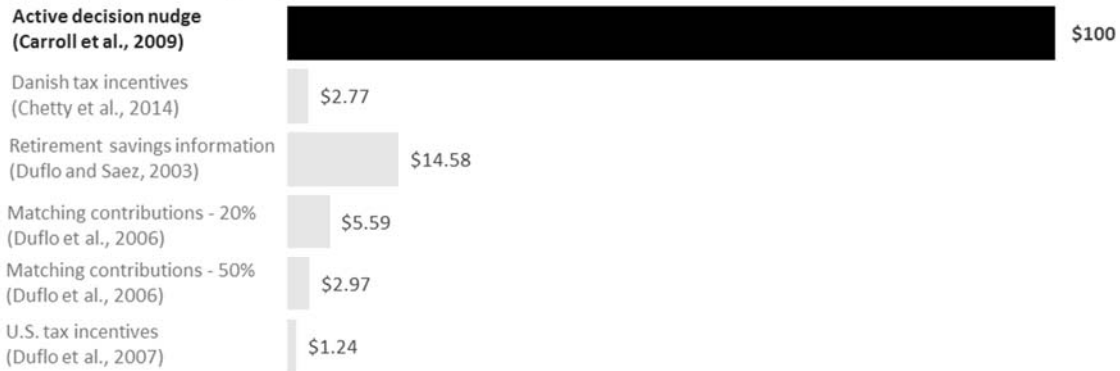
<sup>a</sup>For this estimate, standard errors could not be calculated using the information reported.



**Figure 1.**

*Relative effectiveness of interventions in four domains.*

**Retirement Savings (increase in contributions for the year per \$1 spent)**



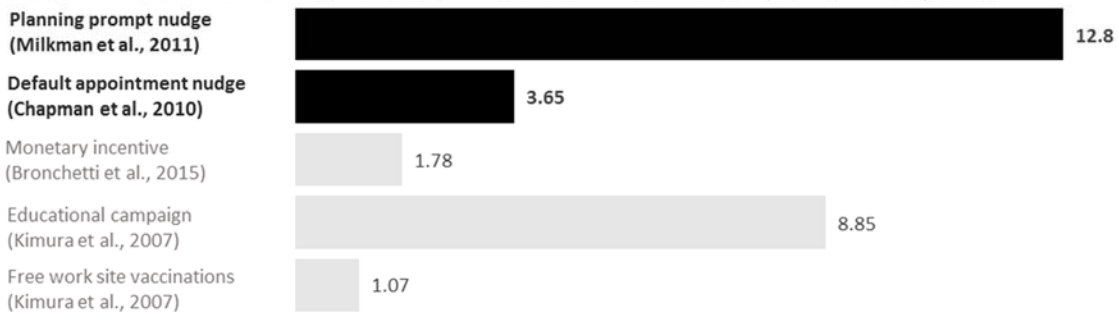
**College Enrollment (increase in students enrolled per \$1,000 spent)**



**Energy Conservation (increase in kWh saved per \$1 spent)**



**Influenza Vaccinations (increase in adults vaccinated per \$100 spent)**



■ Nudge  
 ■ Traditional Intervention (financial incentives, educational programs or some combination the two)

### 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 hardly provide an exhaustive review of the relative effectiveness of nudges compared to traditional policy tools, such as bans and incentives. Nonetheless, our selective but systematic calculations indicate that the impact of nudges is often greater, on a cost-adjusted basis, than that of traditional tools.

In which situations are nudges more impactful per dollar spent than traditional policy tools and vice versa (Goldin and Lawson, 2016)? Far more work needs to be done on this question (*ibid.*), but monetary incentives may well do better, along that dimension, when the policy maker's objective is to correct a misalignment between the public interest and the private interests of citizens making carefully reasoned decisions (as in cases where private decisions impose externalities). To be sure, nudges can help even there, and sometimes they may be preferable (Sunstein and Reisch, 2014). But their comparative advantages will typically be greater when the policy maker's objective is to change the day-to-day behavior of individuals who are making biased, rushed, or otherwise imperfect decisions. As seen in Table 2, monetary incentives in these settings *can* generate large increases in desirable behavior, but are sometimes too expensive to generate a favorable ratio of impact to cost. Because traditional interventions 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, Mullainathan, & Shafir, 2012). Nudges, by contrast, can succeed by taking account of 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? Sometimes, but we warn against jumping to this conclusion. Nudges cannot be the only tool for pursuing policy objectives. In many cases, nudges make it easier for individuals to take advantage of policies that are already in place. For example, the retirement savings active decision nudge directed greater attention to an existing savings plan; the FAFSA intervention increased college attendance by simplifying the process of applying for student aid programs; and the vaccination planning prompts helped 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.

An important caveat to our calculations is that they are not apples-to-apples exercises: they compare the effectiveness of different interventions without holding fixed the population studied. We lack sufficient studies comparing multiple policy interventions simultaneously across similar populations. It would also 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). Importantly, the operational philosophy of nudging 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—increases the likelihood that failures will be inexpensive.

### **Conclusion**

We offer three recommendations. First, there should be increased investment in behaviorally-informed policies to supplement traditional policies both inside and outside of governments. Second, nudge units and others enlisting nudges should share data and knowledge (e.g., through a central repository) and coordinate efforts to maximize their learning from one another. Tracking failures is as important for knowledge creation as tracking successes. Third, 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 far 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.

### Acknowledgements

The authors thank Andrew Joung, David Mao, Byron Perpetua, and Sean (Yixiang) Wang for outstanding research assistance, as well as Leslie Bales, Hyunsoo Chang, Daniel J. Hopkins, Bruce Keith, and the staff of the Defense Finance and Accounting Service for integral support on the SBST-DOD experiment.

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