The SBST/DOD Retirement Savings Experiment

One of the first experiments conducted by the 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.

Methods

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, 2016); (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 SU1 outlines the design of the experiment, and experimental stimuli are shown in Exhibit SU1. Specifically, random assignment proceeded as follows:

- 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*).

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.

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 SU2 presents the results of these regressions. Model 1 is the OLS regression, and Model 2 is the logistic regression.

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 SU2 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.

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 SU2). 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 SU2). 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.[1] 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.

      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

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[1] 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.

variables for different email messages with indicator variables for each of the different email

components summarized above (*action steps, compound interest clarification,* etc.). Table SU3

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.

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Tables and Exhibits

Table SU1. *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

Table SU2. *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

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

Table SU3. *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. The contributions regressions exclude the Marines, for whom we lack the necessary data.*

	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
R²/ Pseudo R²	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.

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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.

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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

SOM-U1

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.

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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.

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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.

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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.

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SOM-U1

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.

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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!

SOM-U1

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.

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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!

SUPPLEMENTAL ONLINE MATERIAL – UNREVIEWED

DETAILED DOCUMENTATION OF RELATIVE EFFECTIVENESS CALCULATIONS

Selecting focus areas and outcome variables

We formed our initial list of policy areas by combining the lists of focus areas from the most recent (as of 2015) summary reports of the U.S. and U.K. nudge units,[1,2] eliminating redundancies and excluding areas that are not major domestic policy foci for the U.S. government. Table A displays these SBST and BIT policy areas of focus, our categorization of these areas, and areas that were excluded.

Table A.

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	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 Skills & Youth
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

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[1] Social and Behavioral Sciences Team (2015). Social and Behavioral Sciences Team 2015 Annual Report. Washington, DC: Executive Office of the President of the United States National Science and Technology Council. Retrieved from
https://www.whitehouse.gov/sites/default/files/microsites/ostp/sbst_2015_annual_report_final_9_14_15.pdf
[2] Behavioural Insights Team (2015). The Behavioural Insights Team Update Report 2013-2015. London, UK: Behavioural Insights Ltd in partnership with the U.K. Cabinet Office. Retrieved from
http://www.behaviouralinsights.co.uk/publications/the-behavioural-insights-team-update-report-2013-2015

focus on energy consumption. (For Energy, we also considered adoption of energy-efficient technologies as an outcome variable, but we determined that energy consumption was more comparable across studies.) 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 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 to mid-2015 (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 to mid-2015 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 to mid-2015 did not reveal published research studying interventions for decreasing the types of crime mentioned (illegal migration, mobile phone theft, and online exploitation).

Table B contains our final list of focus areas and outcome variables.

Table B.

Focus areas and outcome variables included in our review.

Focus Area	Outcome Variable
Financial Security in Retirement	Retirement savings
Education	College enrollment among recent high school graduates
Energy	Energy consumption
Health	Adult outpatient influenza vaccinations

Selecting papers

We searched leading academic journals for original research published between 2000 and mid-2015 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,[3] we limited our set of academic journals to Google Scholar's three leading general interest journals, economics (excluding finance) journals, general psychology journals, and general medical journals. We excluded series containing papers that are not peer reviewed (in our case, *American Economic Review: Papers and Proceedings*) and journals that publish only review articles (in our case, *Trends in Cognitive Science*). The resulting journals appear in Table C.

Table C.

Primary academic journals included in our review, by topic.

Journal Topic	Academic Journals
General interest	*Science*
	Nature
	Proceedings of the National Academy of Sciences of the USA
Economics	*The American Economic Review*
	Quarterly Journal of Economics
	The Review of Economics and Statistics
Psychology	*Psychological Science*
	Journal of Personality and Social Psychology
	Journal of Applied Psychology
Medicine	*The New England Journal of Medicine*
	The Lancet
	JAMA: The Journal of the American Medical Association

The criteria for a paper to be included in our analyses were as follows: 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 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 beyond the restricted set of journals, using a process less systematic than our initial search.

To find candidate papers within each journal, we searched the titles and abstracts of all papers in that journal for the terms in Table D. Occasionally, when the number of search results was very high, we required certain keywords to be in the title. Three of our outcome variables did not produce any search results in any of our journals, as mentioned above.

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

Table D.

Search terms used for finding candidate papers, by focus area.

Focus Area	Outcome Variable	Search Terms
Financial Security in Retirement	Retirement savings	retirement OR ira OR 401 OR pension
Education	College enrollment among recent high school graduates	(college OR university) AND (enroll* OR tuition)
Energy	Energy consumption	(energy OR electricity) AND (consumption OR conservation OR efficiency)
Health	Adult outpatient influenza vaccinations	influenza AND vaccin*
Job Training	Job training program enrollment	training AND (employment OR job OR career)
Program Integrity & Compliance	Compliance with paying taxes or fees to government	(tax OR fee) AND (compliance OR evasion)
Home Affairs	Illegal migration, mobile phone theft, or online exploitation	crime OR theft OR illegal

Calculating relative effectiveness

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] 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

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[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.

[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.

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 we study within a domain operate over similar time horizons. We evaluate the retirement savings interventions over a horizon of one year. 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.

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. 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]

Papers included in our analysis

In this section, we list the papers for which relative effectiveness calculations appear in the Results section of our main paper. We briefly summarize each paper, explain how we found it, and describe how we performed the calculations.

Increasing Retirement Savings

Carroll, G. D., Choi, J. J., Laibson, D., Madrian, B. C., & Metrick, A. (2009). Optimal defaults and active decisions. *Quarterly Journal of Economics, 124*, 1639-1674.

- Summary: Employees at a firm saved more for retirement when required to actively choose a contribution rate in their first month instead of being allowed to enroll (or not enroll) at their leisure.
- How it was found: We found this paper in our initial search of the *Quarterly Journal of Economics*.
- Relative effectiveness calculations: The active decision nudge increased the average contribution rate in the first year of employment by over one percent of pay, which we find from looking at the 12-month mark of Figure 3. Although the median income of employees in the active decision and standard enrollment cohorts was about $30,000 in 1998 dollars (Table II), we conservatively apply the one percentage point increase to an annual salary of $20,000 in 2015 dollars, as the nudge affected low-income employees more (see negative coefficient for *Active decision cohort ×*

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[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.

[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.

Log(*Base pay*) in Table III). The resulting contribution increase is therefore $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 (Section 1) was no more than $2 in 2015 dollars per employee, so the active decision nudge generated $100 of additional savings per dollar spent.

Chetty, R., Friedman, J. N., Leth-Petersen, S., Nielsen, T. H., & Olsen, T. (2014). Active vs. passive decisions and crowd-out in retirement savings accounts: Evidence from Denmark. *Quarterly Journal of Economics, 129*, 1141-1219.

- Summary: In Denmark, increasing government subsidies of tax-advantaged retirement accounts was relatively ineffective at inducing contributions, compared to interventions not requiring individual action.
- How it was found: We found this paper in our initial search of the *Quarterly Journal of Economics.*
- Relative effectiveness calculations: We focus on the analysis of Denmark's 1999 reduction of subsidies for capital pensions. From column 1 of Table V, we find that capital pension contributions by treated persons dropped by DKr 2,449 (121), or $540 (27) in 2015 U.S. dollars,[9] following the subsidy reduction. (The authors find that individuals partially made up this loss through contributions to other kinds of accounts, which we conservatively ignore.) Furthermore, the authors estimate a per-person government revenue increase of DKr 883 ($195 in 2015 U.S. dollars) as a result of the subsidy reduction (p. 1209). Since the ratio of $540 to $195 is 2.77, we infer that while the subsidy was in place, it generated $2.77 (0.14) in additional retirement savings per dollar of government expenditure.

Duflo, E., & Saez, E. (2003). The role of information and social interactions in retirement plan decisions: Evidence from a randomized experiment. *Quarterly Journal of Economics, 118*, 815-842.

- Summary: Employees who were offered an incentive to attend a benefits fair (or who worked with those employees) were more likely to participate in a retirement plan.
- How we found it: We found this paper in our initial search of the *Quarterly Journal of Economics*.
- Relative effectiveness calculations: The authors calculate cost effectiveness in their conclusion. The average yearly contribution of an employee enrolled in a Tax Deferred Account (TDA) was approximately $3,500 in the sample, and 50 additional employees out of 4,000 enrolled as a result of the experiment, for a total additional savings of $175,000 per year assuming new enrollees contributed at the same rate. A total of $12,000 in incentives were paid. In 2015 dollars, this is equivalent to $58.95 yearly per employee at a cost of $4.04, or $14.58 per dollar spent.

Duflo, E., Gale, W., Liebman, J., Orszag, P., & Saez, E. (2006). Saving incentives for low- and middle-income families: Evidence from a field experiment with H&R Block. *Quarterly Journal of Economics, 121*, 1311-1346.

- Summary: Individuals who were offered matching incentives by a tax-preparation company to contribute to an IRA were more likely to contribute.

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[9] 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.

- How we found it: We found this paper in our initial search of the *Quarterly Journal of Economics*.
- Relative effectiveness calculations: From Table II, individuals offered a 20% match contributed $62.9 (row 2) more than the control group, receiving $13.7 (difference of row 4 and row 2) more in matching funds for a total additional contribution of $76.6 (7.4), or, in 2015 dollars, $93.6 (9.0) including a $16.7 match. Individuals offered a 50% match contributed $132.7 and received $67.4 more in matching funds for a total additional contribution of $200.1 (10.5), or, in 2015 dollars, $244.5 (12.8) including an $82.4 match. The contribution per dollar of cost in 2005 dollars was therefore $76.6/$13.7 = $5.59 (0.54) for the 20% match treatment and $200.1/$67.4 = $2.97 (0.16) for the 50% match treatment.

Duflo, E., Gale, W., Liebman, J., Orszag, P., & Saez, E. (2007). Savings incentives for low- and moderate-income families in the United States: Why is the Saver's Credit not more effective? *Journal of the European Economic Association, 5*, 647-661.

- Summary: Compared to the experiment in Duflo et al. (2006), a program called the Saver's Credit that allows a tax credit for retirement account contributions is largely ineffective at increasing contributions.
- How we found it: This is a companion paper to Duflo et al. (2006).
- Relative effectiveness calculations: The authors estimate that an increase in the tax credit from 20% to 50% of contributions generated an additional $9.5 (0.82) (Table 2, column 6, row 2), or $11.6 (1.00) in 2015 dollars, of deposits to a retirement savings account. The average contribution for individuals at the top of the 50% bracket was $19.2, or $23.5 in 2015 dollars, and the average for individuals at the bottom of the 20% bracket was $9.8, or $12.0 in 2015 dollars. Therefore, the average cost in tax credits in 2005 dollars was 0.50*19.2-0.20*9.8 = $7.6, and the contribution per dollar in cost was 9.5/7.6 = $1.24 (0.11).

Increasing College Enrollment among Recent High School Graduates

Bettinger, E. P., Long, B. T., Oreopoulos, P., & Sanbonmatsu, L. (2012). The role of application assistance and information in college decisions: Results from the H&R Block FAFSA experiment. *Quarterly Journal of Economics, 127*, 1205-1242.

- Summary: In a field experiment, individuals who were offered help completing the Free Application for Federal Student Aid (FAFSA) were more likely to submit the FAFSA and attend college.
- How we found it: We found this paper in our initial search of the *Quarterly Journal of Economics*.
- Relative effectiveness calculations: Treated individuals were 8.1 (3.5) percentage points more likely to attend college in the year following the experiment (Table III, column 2, row 2). The cost of the treatment was $48 per participant in 2008 dollars (or $53.02 in 2015 dollars), comprising $3 in training and compensation for tax professionals, $15 for material and software costs, and $30 for call center support (p. 1238). We exclude participation incentives of up to $40 per individual. Therefore, the program generated 0.081/53.02*1000 = 1.53 (0.66) additional college enrollees per thousand dollars spent.

Dynarski, S. M. (2003). Does aid matter? Measuring the effect of student aid on college attendance and completion. *American Economic Review, 93*, 279-288.

- Summary: The 1982 elimination of the Social Security Student Benefit Program modestly decreased the likelihood of college attendance among previously benefit-eligible participants.
- How we found it: We found this paper in our initial search of the *American Economic Review*.
- Relative effectiveness calculations: From the author's differences-in-differences analysis in Table 1, the existence of the benefit program increases the probability of college attendance by 18.2 (9.6) percentage points among eligible individuals (column 5, row "Attend college by 23"). (The same estimate can be derived from the OLS regression results appearing in Table 2.) Out of the eligible group, 56.0% attended college (Table 1, column 2, row "Attend college by 23"), and in 1980 they received an average annual benefit of $6,700 (p. 279) in 2000 dollars. The cost per eligible individuals is therefore 0.560*$6,700 = $3,752 in 2000 dollars, or $5,181 in 2015 dollars. The additional enrollment per thousand dollars spent is 0.182/5,181*1,000 = 0.0351 (0.0185).

Long, B. T. (2004a). Does the format of a financial aid program matter? The effect of state in-kind tuition subsidies. *Review of Economics and Statistics, 86*, 767-782.

- Summary: State tuition subsidies encourage college enrollment, and the format and conditions of these subsidies influence decisions about which college to attend.
- How we found it: We found this paper in our initial search of the *Review of Economics and Statistics*.
- Relative effectiveness calculations: We derive enrollment estimates from Table 5, panel A, which predicts that with vouchers offered for any in-state college, enrollment would increase from 5,535 (column 4, row 1) to 5,664 (column 3, row 1), with 3,766 attending four-year colleges (column 3, row 2) and 1,898 at two-year colleges (column 3, row 7). Costs are not present in the *Review of Economics and Statistics* version of this paper, but do appear in the working paper.[10] In Table 6, column 5, rows 1 and 2, we find that the mean voucher is $3,167 for four-year colleges and $1,583 for two-year colleges. The total voucher expenditure is therefore (3,766*$3,167+1,898*$1,583) = $14,931,456 in 1992 dollars, or $25,304,980 in 2015 dollars, and the increased enrollment per thousand dollars spent is (5,664-5,535)/25,304,980*1,000 = 0.0051 students.

Long, B. T. (2004b). The impact of federal tax credits for higher education expenses. In C. M. Hoxby (Ed.), *College choices: The economics of where to go, when to go, and how to pay for it* (pp. 101-168). Chicago, IL: University of Chicago Press.

- Summary: The Hope and Lifetime Learning Tax Credits, instituted in 1997, have resulted in benefits for middle-class more than low-income families, and have not produced a measurable increase in college enrollment.
- How we found it: We looked broadly for papers studying the effect of tax credits on college enrollment, since the search of our restricted set of journals did not produce any.
- Relative effectiveness calculations: Since the author does not find a statistically significant effect on college enrollment, we mark relative effectiveness as "negligible" instead of calculating it.

Bulman, G. B., & Hoxby, C. M. (2015). The returns to the federal tax credits for higher education. *Tax Policy and the Economy, 29*, 13-88.

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[10] Long, B. (2003). *Does the format of a financial aid program matter? The effect of state in-kind tuition subsidies* (NBER Working Paper 9720). Cambridge, MA: National Bureau of Economic Research. Retrieved from http://www.nber.org.ezp-prod1.hul.harvard.edu/papers/w9720.pdf.

- Summary: The 2009 expansion of tax credits for college did not have a significant effect on various outcome variables, including college attendance.
- How we found it: We looked broadly for papers studying the effect of tax credits on college enrollment, since the search of our restricted set of journals did not produce any.
- Relative effectiveness calculations: Since the authors do not find a statistically significant effect on college enrollment, we mark relative effectiveness as "negligible" instead of calculating it.

Increasing Energy Conservation

Allcott, H. (2011). Social norms and energy conservation. *Journal of Public Economics, 95,* 1082-1095.

- Summary: Consumers who received letters comparing their electricity consumption to that of their peers reduced their consumption.
- How we found it: A different paper found in our initial search of the *American Economic Review*, Allcott and Rogers (2014), directed readers to this paper for simpler cost-effectiveness calculations.
- Relative effectiveness calculations: Across multiple experiments over the course of two years, the author calculates an average cost effectiveness of 3.31 cents per kilowatt-hour saved (p. 1088), or 3.67 cents per kWh in 2015 dollars. Therefore, the treatment saved $100/3.67 = 27.3$ kWh per dollar spent.

Asensio, O. I., & Delmas, M. A. (2015). Nonprice incentives and energy conservation. *Proceedings of the National Academy of Sciences of the United States of America, 112*, E510-E515.

- Summary: Households who received messages about the health and environmental effects of energy use reduced their consumption.
- How we found it: We found this paper in our initial search of the *Proceedings of the National Academy of Sciences of the United States of America.*
- Relative effectiveness calculations: Our numbers come from the paper's Supporting Information supplement. The control group had a baseline average electricity usage of 8.66 kWh per day (Table S10, column 1, row 1). From Table S11, column 5, row 2, households in the health-information treatment group lowered their consumption relative to the control group by 8.192% (4.306%) or, over the 100-day treatment period, $0.08192*8.66*100 = 70.9$ (37.3) kWh. We assume energy savings decayed linearly over one year (implying savings of 0.822 kWh on the first day of the year declining to zero savings on the last day of the year, with $(265/365)*0.822 = 0.597$ kWh of savings on the 100th day of the 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 therefore saved $149.8/3,019 = 0.050$ (0.026) kWh per dollar spent. A separate treatment group that received information about cost savings did not exhibit a statistically significant consumption decrease.

Ito, K. (2015). Asymmetric incentives in subsidies: Evidence from a large-scale electricity rebate program. *American Economic Journal: Economic Policy, 7,* 209-237.

- Summary: A California rebate program encouraging reduced electricity use decreased consumption in inland areas but not coastal areas.
- How we found it: We looked broadly for papers studying the effect of monetary incentives on energy consumption, since the search of our restricted set of journals did not produce any.

- Relative effectiveness calculations: The author estimates the total cost of the program at 24.1 cents per kWh saved (p. 232), or $0.293 in 2015 dollars, including indirect costs as well as rebates. This is equivalent to 1/0.293 = 3.41 kWh per dollar spent.

Arimura, T. H., Li, S., Newell, R. G., & Palmer, K. (2012). Cost-effectiveness of electricity energy efficiency programs. *Energy Journal, 33*, 63-99.

- Summary: Spending on various demand-side management programs, including consumer education and financial incentives, resulted in decreased energy consumption across many utilities.
- How we found it: We looked broadly for papers studying the effect of educational programs or monetary incentives on energy consumption, since the search of our restricted set of journals did not produce any.
- Relative effectiveness calculations: Preferring a nonlinear least squares model (p. 86) with an assumed 5% discount rate (p. 76), the authors calculate a cost effectiveness of 5.0 cents per kWh saved (Table 3, column 1, row 4), or $0.071 in 2015 dollars. This is equivalent to 1/.071 = 14.0 kWh per dollar spent.

Increasing Adult Outpatient Influenza Vaccinations

Milkman, K. L., Beshears, J., Choi, J. J., Laibson, D., & Madrian, B. C. (2011). Using implementation intentions prompts to enhance influenza vaccination rates. *Proceedings of the National Academy of Sciences of the United States of America, 108*, 10415-10420.

- Summary: Employees who received letters prompting them to plan a date and time to receive a vaccine had a higher vaccination rate than those who received letters without a prompt.
- How we found it: We found this paper in our initial search of the *Proceedings of the National Academy of Sciences of the United States of America*.
- Relative effectiveness calculations: The vaccination rate for employees in the date-and-time condition was higher by 4.2 percentage points (Table 2, column 3, row "Difference relative to the control condition: Full sample, regression-adjusted difference, %"). Since the width of the provided 95% confidence interval is 7.4 percentage points, the standard error of this estimate is 7.4/3.92 = 1.9. The authors report privately that adding the prompts to reminder letters that were already being mailed required 5 hours of labor at a cost of $75 per hour, for a total of $375, or $415.58 in 2015 dollars. With 1,270 employees receiving the prompts (Table 1 notes, sample size for "time plan"), the intervention generated 0.042*1,270/415.58*100 = 12.8 (5.8) additional vaccinations per $100 spent.

Chapman, G. B., Li, M., Colby, H., & Yoon, H. (2010). Opting in vs opting out of influenza vaccination. *Journal of the American Medical Association, 304*, 43-44.

- Summary: Employees who were automatically scheduled for vaccination appointments instead of being prompted to schedule appointments themselves had a higher vaccination rate.
- How we found it: We found this paper in our initial search of the *Journal of the American Medical Association*.
- Relative effectiveness calculations: Data can be found in the paper's Results section. In the control group, 80 of 239 participants received a vaccine, while in the treatment group, 108 of 239 were vaccinated. Therefore, the intervention increased the vaccination rate by (108-80)/239 = .117, or 11.7 percentage points. The 95% confidence interval has a width of 12 percentage points

for the control group and 13 percentage points for the treatment group. Therefore, the control group standard error is 12/3.96 = 3.0 percentage points, the treatment group standard error is 13/3.96 = 3.3 percentage points, and the standard error for the difference of means is $\sqrt{(3.0^2+3.3^2)}$ = 4.5 percentage points. 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, $5 to add staff for each extra appointment, and $30 for stocking each extra unused vaccine. In the treatment group, 39 people changed appointments, and in the control 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 treatment group and 80 appointments and walk-ins for the control 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 treatment then has a total cost of $1.25*39+$5*221 = $1,153.75, while the control group has a total cost of $1.25*50+$5*80 = $462.50. Therefore the marginal intervention cost is $691.25 in 2009 dollars, or $766.06 in 2015 dollars. Given that 239 people were in the treatment group, the nudge generated 0.117*239/766.06*100 = 3.65 (1.40) additional vaccinations per hundred dollars spent.

Bronchetti, E. T., Huffman, D. B., & Magenheim, E. (2015). Attention, intentions, and follow-through in preventive health behavior: Field experimental evidence on flu vaccination. *Journal of Economic Behavior and Organization, 116*, 270-291.

- Summary: A financial incentive was effective in encouraging college students to be vaccinated on campus, while several other email interventions were not effective.
- How we found it: We looked broadly for papers studying the effect of monetary incentives on vaccination rates, since the search of our restricted set of journals did not produce any.
- Relative effectiveness calculations: The incentive offered was $30, or $31.07 in 2015 dollars. Vaccination rates increased by 10.7 (0.9) percentage points (Table 5, column 5, row 1). Since the baseline vaccination rate in the control group was 8.7% (Table 5, column 5, row "Constant"), the treatment cost $31.07*(0.087+0.107) = $6.03 per person (unconditional on vaccination) and generated 0.107/6.03*100 = 1.78 (0.15) additional vaccinations per hundred dollars spent.

Kimura, A. C., Nguyen, C. N., Higa, J. I., Hurwitz, E. L., & Vugia, D. J. (2007). The effectiveness of vaccine day and educational interventions on influenza vaccine coverage among health care workers at long-term care facilities. *American Journal of Public Health, 97*, 684-690.

- Summary: A combination of an educational program and free vaccines was effective in increasing the vaccination rate of health care workers; the educational program alone did not have a statistically significant effect, but the free vaccines alone did.
- How we found it: We looked broadly for papers studying the effect of educational programs on vaccination rates, since the search of our restricted set of journals did not produce any.
- Relative effectiveness calculations: We apply a differences-in-differences approach to the authors' findings, focusing on the single-technique treatments instead of the treatment combining education with incentives. From Table 3, the vaccination rate for the control group is 467/1517 = 30.78% at baseline and 450/1517 = 29.66% post-intervention; for the educational campaign group, 240/821 = 29.23% at baseline and 298/821 = 36.30% post-intervention; and for the free vaccines group, 292/832 = 35.10% at baseline and 410/832 = 49.28% post-intervention. Therefore, we calculate that the educational campaign increased vaccination rates by (36.30-

29.23)-(29.66-30.78) = 8.19 percentage points, while free vaccinations increased vaccination rates by (49.28-35.10)-(29.66-30.78) = 15.30 percentage points. The authors estimate that an educational campaign for 100 employees cost $70 in 2002 dollars, or $92.54 in 2015 dollars, while free vaccinations cost $1,080 for 100 employees in 2002 dollars, or $1,427.77 in 2015 dollars (p. 687). Since a one percentage point increase in vaccination rate is equivalent to one additional vaccination per 100 people, the educational and free vaccination treatments generated (8.19/92.54)*100 = 8.85 and (15.3/1,427.77)*100 = 1.07 additional vaccinations per hundred dollars spent, respectively.

Papers excluded from our analysis

Here we list papers that we came across during our search but that we decided to exclude from our Results section despite their apparent relevance. We give a brief summary of each paper along with its reason for exclusion.

Increasing Retirement Savings

Madrian, B. C., & Shea, D. F. (2001). The power of suggestion: Inertia in 401(k) participation and savings behavior. *Quarterly Journal of Economics, 116,* 1149-1187.

- Summary: Automatically enrolling employees in a 401(k) led to a higher participation rate and adoption of the default contribution rate.
- How we found it: We found this paper in our initial search of the *Quarterly Journal of Economics*.
- Reason for exclusion: The cost of this nudge is difficult to quantify. Conditional on having set up the infrastructure necessary for automatically enrolling employees in a 401(k), the cost of increasing the default contribution rate is very low, so the incremental savings per dollar spent on this nudge is likely higher than the incremental savings per dollar spent on the Carroll et al. (2009) nudge reported in the Results section.

Card, D. & Ransom, M. (2011). Pension plan characteristics and framing effects in employee savings behavior. *Review of Economics and Statistics, 93,* 228-243.

- Summary: Employees were more likely to reduce contributions to a savings account following an increase in mandatory employee pension contributions than an equivalent increase in employer pension contributions.
- How we found it: We found this paper in our initial search of the *Review of Economics and Statistics*.
- Reason for exclusion: The cost of this nudge is difficult to quantify. Conditional on having both mandatory employee pension contributions and guaranteed employer pension contributions in place, it is not costly to replace a plan to increase mandatory employee contributions with a plan to increase guaranteed employer contributions (holding overall compensation costs the same by reducing the size of salary raises). Such a move generates high incremental savings at low cost.

Choi, J. J., Laibson, D., & Madrian, B. C. (2011). $100 bills on the sidewalk: Suboptimal investment in 401(k) plans. *Review of Economics and Statistics, 93,* 748-763.

- Summary: Educating employees who contribute less to their 401(k) than the maximum employer match did not have a statistically significant effect on contribution rates.

- How we found it: We found this paper in our initial search of the *Review of Economics and Statistics*.
- Reason for exclusion: The intervention in this paper is similar to that of Duflo and Saez (2003), but this paper does not report a statistically significant impact, so we include the Duflo and Saez (2003) results instead as an optimistic estimate of effect size.

Increasing College Enrollment among Recent High School Graduates

Linsenmeier, D. M., Rosen, H. S., & Rouse, C. E. (2006). Financial aid packages and college enrollment decisions: An econometric case study. *Review of Economics and Statistics, 88*, 126-145.

- Summary: Replacing loans with grants in the financial aid packages offered to low-income students did not have a statistically significant effect on college matriculation.
- How we found it: We found this paper in our initial search of the *Review of Economics and Statistics.*
- Reason for exclusion: The intervention in this paper is similar to that of Dynarski (2003), but this paper does not report a statistically significant impact, so we include the Dynarski (2003) results instead as an optimistic estimate of effect size.

Conley, T. G., & Taber, C. R. (2011). Inference with "difference in differences" with a small number of policy changes. *Review of Economics and Statistics, 93*, 113-125.

- Summary: A Georgia tuition grant program was found not to have a significant effect on college enrollment.
- How we found it: We found this paper in our initial search of the *Review of Economics and Statistics.*
- Reason for exclusion: The intervention in this paper is similar to that of Dynarski (2003), but this paper does not report a statistically significant impact, so we include the Dynarski (2003) results instead as an optimistic estimate of effect size.

Hoxby, C., & Turner, S. (2013). Expanding college opportunities for high-achieving, low income students. *Stanford Institute for Economic Policy Research Discussion Paper*, 12-014.

- Summary: Providing students with information on the college application process improved a variety of college outcomes.
- How we found it: This paper was referenced in Hoxby and Turner (2015),[11] which we found in our initial search of the *American Economic Review*.
- Reason for exclusion: Matriculation rate is not the primary outcome variable in this paper, and this is a working paper and therefore not peer reviewed.

Angrist, J. & Lavy, V. (2009). The effects of high stakes high school achievement awards: Evidence from a randomized trial. *American Economic Review,* 99, 1384-1414.

- Summary: Offering cash incentives increased the rate of Israeli matriculation certification (required for college enrollment) and (to a lesser degree) college attendance, primarily among higher-achieving girls.
- How we found it: We found this paper in our initial search of the *American Economic Review*.

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[11] Hoxby, C., & Turner, S. (2015). What high-achieving low-income students know about college. *American Economic Review, 105,* 514-517.

- Reason for exclusion: The incentive payments are linked to passing exams, not college enrollment. If, however, we were to categorize this intervention as a reward for college enrollment, it would not change the conclusions of our analysis. To estimate the change in probability of college enrollment, we examine the values in the first four columns of Table 8, Panel B, row "Estimation method: OLS," which give the OLS estimates for the treatment effect on enrollment in an Israeli university or college (excluding academic, teachers, and practical engineering colleges, which do not always require certification) for boys in the top half of the sample by lagged test score, bottom-half boys, top-half girls, and bottom-half girls. None of these values are significantly different from zero, so we conclude that the treatment had a negligible effect on college enrollment.

Increasing Energy Conservation

Schultz, P. W., Nolan, J. M., Cialdini, R. B., Goldstein, N. J., & Griskevicius, V. (2007). The constructive, destructive, and reconstructive power of social norms. *Psychological Science, 18*, 429-434.

- Summary: Informing high-consumption households of typical neighborhood energy use lowered their consumption.
- How we found it: We found this paper in our initial search of *Psychological Science*.
- Reason for exclusion: Allcott (2011) studies a similar nudge with much larger sample sizes.

Allcott, H., & Mullainathan S. (2010). Behavior and energy policy. *Science, 327*, 1204-1205.

- Summary: Consumers who received letters comparing their electricity consumption to that of their peers reduced their consumption.
- How we found it: We found this paper in our initial search of *Science*.
- Reason for exclusion: Allcott (2011) studies a similar nudge with much larger sample sizes.

Allcott, H. (2015). Site selection bias in program evaluation. *Quarterly Journal of Economics, 130*, 1117-1165.

- Summary: Consumers who received letters comparing their electricity consumption to that of their peers reduced their consumption.
- How we found it: We found this paper in our initial search of *Quarterly Journal of Economics*.
- Reason for exclusion: Allcott (2011) studies the same nudge.

Sexton, S. (2015). Automatic bill payment and salience effects: Evidence from electricity consumption. *Review of Economics and Statistics, 97*, 229-241.

- Summary: Withdrawing consumers from automatic electricity bill payment programs significantly reduced energy usage.
- How we found it: We found this paper in our initial search of the *Review of Economics and Statistics*.
- Reason for exclusion: While similar to a nudge, the intervention imposes significant transaction costs on consumers. It is not truly a nudge, yet does not fit into any of our other policy categories.

Dietz, T., Gardner, G., Gilligan, J., Stern, P., & Vandenbergh, M. (2009). Household actions can provide a behavioral wedge to rapidly reduce US carbon emissions. *Proceedings of the National Academy of Sciences of the United States of America*, *106*, 18452-18456.

- Summary: This paper reviews the effects of various interventions on specific energy efficiency behaviors.
- How we found it: We found this paper in our initial search of the *Proceedings of the National Academy of Sciences of the United States of America*.
- Reason for exclusion: The interventions in neither this paper nor the previous work it referenced were comparable to the interventions in our other studies.

Jessoe, K., & Rapson, D. (2014). Knowledge is (less) power: Experimental evidence from residential energy use. *American Economic Review*, *104*, 1417-1438.

- Summary: Households who were informed of temporary energy price increases responded by changing their consumption.
- How we found it: We found this paper in our initial search of the *American Economic Review*.
- Reason for exclusion: The intervention type is hard to classify, having elements of both a nudge and an incentive, and the outcome variable is price sensitivity instead of overall consumption.

Schwartz, D., Fischhoff, B., Krishnamurti, T., & Sowell, F. (2013). The Hawthorne effect and energy awareness. *Proceedings of the National Academy of Sciences of the United States of America, 110,* 15242-15246.

- Summary: Individuals reduced energy consumption while (but not after) participating in a study, even though they received no information or incentives to do so.
- How we found it: We found this paper in our initial search of the *Proceedings of the National Academy of Sciences of the United States of America*.
- Reason for exclusion: The intervention type is hard to classify, as "Hawthorne effects" may be part of the overall effect of a nudge, an incentive, or an educational intervention.

Increasing Adult Outpatient Influenza Vaccinations

Stockwell, M., Kharbanda, E., Martinez, R., Vargas, C., Vawdrey, D., & Camargo, S. (2012). Effect of a text messaging intervention on influenza vaccination in an urban, low-income pediatric and adolescent population. *JAMA: The Journal of the American Medical Association, 307*, 1702-1708.

- Summary: Text message reminders increased the influenza vaccination rate among low-income youth.
- How we found it: We found this paper in our initial search of *JAMA: The Journal of the American Medical Association*.
- Reason for exclusion: The population studied is children and teenagers, while our other studies focus on vaccination in adults.

Evidential value of the underlying studies

We conducted *p*-curve analyses to assess the evidential value of the studies underlying our relative effectiveness calculations. To find a test statistic for each study, we generally sought the regression coefficient that corresponded to the effect size in the Impact column of Table 2 of the main manuscript. We then divided the coefficient by its standard error, giving a *z*-statistic under the assumption of asymptotic normality. For some papers, no regression coefficient corresponded exactly to our Impact, but we were able to calculate a test statistic by transforming the relevant coefficient or using a related coefficient, and in those cases we explain our decisions. If a paper did not find a statistically significant effect or did not provide standard errors or any form of significance testing, we did not calculate a test

statistic. Effects that were not significant at the 5% level did not enter the analysis. Finally, some papers have two measures of impact and therefore two test statistics, and for those papers both test statistics were used in our analysis. In Table E below, we report the test statistics used in our analysis.

With these test statistics, we performed two *p*-curve analyses, one for the papers that studied nudges and one for the papers that studied other intervention types, using an online tool.[12] The full results of these analyses appear below in both graph and table form. In each analysis, the right-skew test rejects its null, and the flatness test does not reject its null. Both of these tests suggest that the studies have evidential value and that their results would likely survive replication.

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[12] Simonsohn, U., Simmons, J., & Nelson, L. (2016). *p*-curve app (version 4.05) [software]. Available at http://www.p-curve.com/app4/.
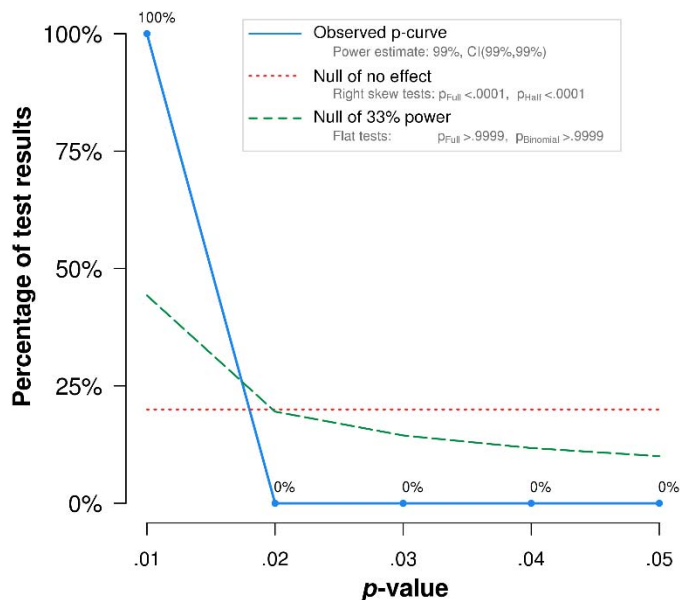
Nudges *p*-curve analysis
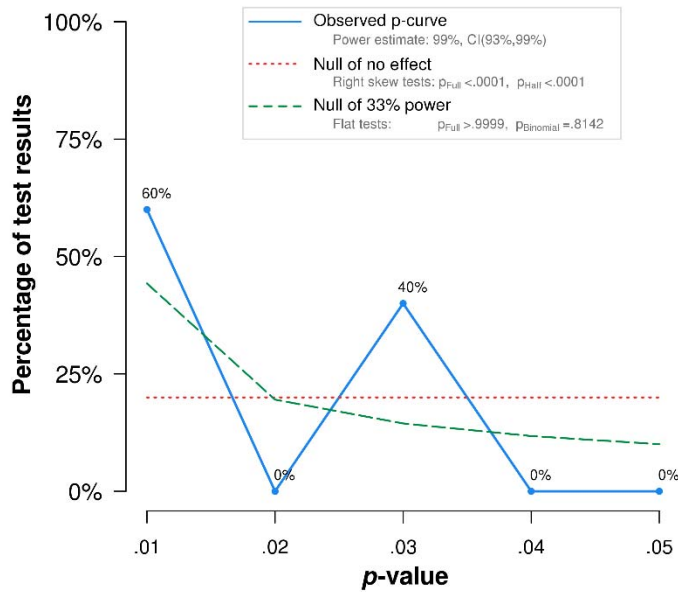
Note: The observed p-curve includes 5 statistically significant (p<.05) results, of which 4 are p<.025.
There were no non-significant results entered.

	Binomial Test (Share of results *p*<.025)	Continuous Test (Aggregate with Stouffer Method)	
		Full *p*-curve (*p*'s<.05)	Half *p*-curve (*p*'s<.025)
1) Studies contain evidential value. *(Right skew)*	*p*=.1875	*Z*=-5.25, *p*<.0001	*Z*=-4.91, *p*<.0001
2) Studies' evidential value, if any, is inadequate. *(Flatter than 33% power)*	*p*=.8142	*Z*=4.15, *p*>.9999	Test not needed.
Power of tests included in *p*-curve *(correcting for selective reporting)*	Statistical Power		
	Estimate: 99% 90% Confidence interval: (93%, 99%)		

Other interventions *p*-curve analysis

Note: The observed p-curve includes 7 statistically significant (p<.05) results, of which 7 are p<.025.
There were no non-significant results entered.

	Binomial Test (Share of results *p*<.025)	Continuous Test (Aggregate with Stouffer Method)	
		Full *p*-curve (*p*'s<.05)	Half *p*-curve (*p*'s<.025)
1) Studies contain evidential value. *(Right skew)*	*p*=.0078	Z=-15.99, *p*<.0001	Z=-15.56, *p*<.0001
2) Studies' evidential value, if any, is inadequate. *(Flatter than 33% power)*	*p*>.9999	Z=15.92, *p*>.9999	Test not needed.
Power of tests included in *p*-curve *(correcting for selective reporting)*	Statistical Power		
	Estimate: 99% 90% Confidence interval: (99%, 99%)		

Table E.

Test statistics used in p-*curve analyses.*

Authors	Treatment	Impact	Test statistic
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**	**$200 increase in savings plan contributions per employee[a]**	**$z \approx 4$** **We find a standard error of 0.247 from Table III, row "Active decision cohort." However, we do not use the coefficient estimate of 0.086. This estimate understates significance, as the dependent variable, contribution rate, was measured thirty months after hire for the standard enrollment cohort but three months after hire for the active decision cohort, and average contribution rate rose for the active decision cohort between three and thirty months from hire. Instead, we observe from Figure III that contribution rates at the twelve-month mark are higher by approximately one percentage point for the active decision cohort. We divide 1 by the original standard error of 0.247 to estimate z.**
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	$z = -20.2$ Derived from Table V, column 1, coefficient for "Above cutoff × post" divided by corresponding standard error.

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.

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

Table E continued.

Authors	Treatment	Impact	Test statistic
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[a]	z = 1.92 (not statistically significant) Derived from Table II, Panel A, column 3, coefficient for "Treated Department" divided by corresponding standard error. The authors focus on TDA enrollment as a dependent variable and do not report standard errors or significance testing for contribution rate as a dependent variable. We choose TDA participation after 11 months instead of 4.5 months as our dependent variable.
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: $244.5 (12.8) in incremental contributions per person	z = 9.4, z = 19.8 Derived from Table II, Panel A, columns 5 (20% match) and 7 (50% match), coefficients for "Amount contributed ($) (unconditional)" divided by corresponding standard errors.
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	z = 11.6 Derived from Table 2, Panel A, column 6, coefficient for "X-IRA average contributions (unconditional)" divided by corresponding standard error.

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.

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

Table E continued.

Authors	Treatment	Impact	Test statistic
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**	**$z = 2.31$** **Derived from Table III, column 2, panel "Dependent participants," coefficient for "FAFSA treatment effect" divided by corresponding standard error.**
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	$z = 1.90$ (not statistically significant) Derived from Table 1, column 5, value in row "Attend college by 23" divided by corresponding standard error.
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]	Omitted since the author does not provide standard errors or significance testing for the effects of vouchers on enrollment.
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	Omitted since the authors do not find a statistically significant effect on college enrollment.

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.

[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 E continued.

Authors	Treatment	Impact	Test statistic
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]	**z = -10.8 Derived from Table 3, column 3, coefficient for "T × Quarterly × Post" divided by corresponding standard error. We conservatively use the coefficient for quarterly treatments instead of monthly treatments, since the effect size is smaller. This coefficient is based on a regression including data from multiple experiments, while the author's estimate of overall cost effectiveness is based on an unweighted average of the cost effectiveness of the individual experiments.**
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**	**z = 1.90 (not statistically significant) Derived from Table S11 (Supporting Information), column 5, coefficient for "Post-Treat*Health Group" divided by corresponding standard error. The test statistic for billing-oriented messages is omitted because the effect is negligible.**
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	z = 3.2 Derived from Table 2, column 3, coefficient for "Treatment effect" divided by corresponding standard error. This estimate is for inland areas; the test statistic for coastal areas is omitted because the effect is negligible.

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.

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

Table E continued.

Authors	Treatment	Impact	Test statistic
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	$z = 1.8$, $z = 1.6$ (neither statistically significant) Derived from Table 3, Model 1, coefficients for "Demand effect of DSM spending (data period)" and "Demand effect of DSM spending (total effect)" divided by corresponding standard errors.

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.

Table E continued.

Authors	Treatment	Impact	Test statistic
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**	*z* **= 2.2** **Derived from Table 2, column 3, row "Difference relative to the control condition: Full sample, regression-adjusted difference, %" and "95% CI" underneath. The standard error is inferred from the 95% confidence interval.**
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**	*z* **= 2.6** **Derived from data in the first paragraph of the Results section.**
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	*z* = 11.9 Derived from Table 5, column 5, coefficient for "Incentive" and corresponding standard error.
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[a] Free vaccines: 15.3 percentage point increase in flu shot take-up[a]	*z* = 1.2 (not statistically significant), *z* = 3.0 Derived from Table 3, column 5, estimate and confidence interval for "Educational campaign group" and "Vaccine Day group" respectively. Standard errors are inferred from the confidence intervals. Since the authors estimate the *ratio* of vaccine prevalence between the treatment and control groups, we subtract one from each estimated ratio to obtain the percentage (not percentage point) increase in vaccination, then divide by the standard error.

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.

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