

Growth that compounds

What is a default option?

A default applies to whoever does not choose. At one US firm, new hires' 401(k) participation was 37 per cent without automatic enrolment and 86 with it.

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Management summary A default option is the option that takes effect for anyone who does not make an active choice, while every other option stays open. This concept guide defines it and the default effect (Jachimowicz et al.); reports Madrian and Shea's comparison of two hiring cohorts at one US company, in which 401(k) participation at equal tenure was 37.4 per cent before automatic enrolment and 85.9 per cent under it; sets that beside a meta-analysis of 58 opt-in versus opt-out comparisons whose effects varied widely; explains the channels both sources name; says when defaults moved fewer people and what the 401(k) default cost the employees who kept it; and gives a record for the terms a renewal applies when a customer does not reply. The sources are one study of one company's employee records and one meta-analysis.

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What is a default option?

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It is the option that applies to a person who makes no choice. Jachimowicz et al. (2019)¹ describe a default as “pre-selecting one choice option to increase the likelihood of its uptake”, and their abstract opens with the regularity behind it: people who decide “with a pre-selected choice option” are “more likely to select that option” (pp. 159 to 160). The studies they pool compare two conditions: in the opt-out condition the option is selected in advance and people must act to leave it, in the opt-in condition they must act to take it. The authors define the default effect as “the difference in choice between the opt-out condition versus that in the opt-in condition” (p. 162).

¹ Jachimowicz, J. M., Duncan, S., Weber, E. U., & Johnson, E. J. (2019). When and why defaults influence decisions: A meta-analysis of default effects. *Behavioural Public Policy*, 3(2), 159–186.

Madrian and Shea (2001)² studied one such change in a company retirement savings plan, a 401(k) plan in the United States. Before the change, “employees who enrolled in the 401(k) plan were required to affirmatively elect participation”. From 1 April 1998, “all newly hired employees were automatically enrolled in the 401(k) plan unless they affirmatively elected to opt out”, at a contribution rate of 3 per cent of pay, all of it in the plan’s money market fund, and they could change the rate or the fund at any time (pp. 1149 to 1152). The default set three terms at once: whether to save, how much and where. In the authors’ words, “none of the economic features of the plan changed”: the employer still matched 50 per cent of contributions up to 6 per cent of pay once an employee had a year of service, and the nine investment options stayed the same (p. 1150; Table I, p. 1152).

² Madrian, B. C., & Shea, D. F. (2001). The power of suggestion: Inertia in 401(k) participation and savings behavior. *The Quarterly Journal of Economics*, 116(4), 1149–1187.

In this guide, a default option is the option, or the set of terms, that takes effect for anyone who does not make an active choice, while every other option stays available and can be chosen instead. Setting one decides what happens to the people who do not respond. It differs from an active choice, in which there is no default and people “are required to make a choice”, and from removing an option, which no one can then choose; Jachimowicz et al.¹ call defaults “only a single tool in the choice architect’s toolbox” (pp. 160 to 161). The default effect is the difference a default makes to what people end up with, and this guide reports it as each source measures it: in percentage points of

participation in one company, and as a standardized difference across studies.

What happened to 401(k) participation under automatic enrolment?

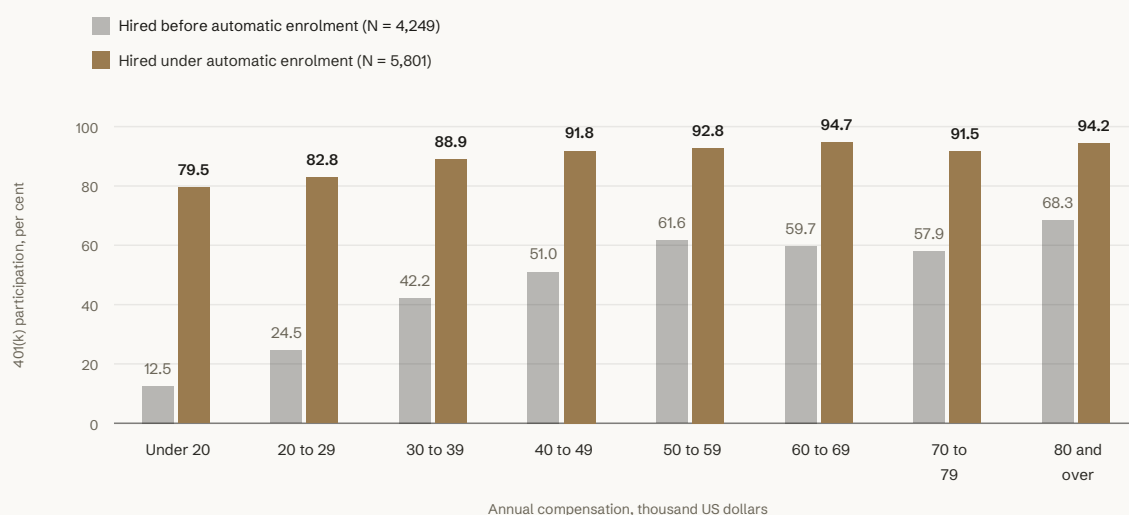
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It was more than twice as high in the cohort hired under it. Madrian and Shea² compare cohorts of employees at “a large, publicly traded Fortune 500 company in the health care and insurance industry” (p. 1151). Employees hired from April 1997 to March 1998 were not automatically enrolled; those hired from April 1998 to March 1999 were. Measured at the same tenure of 3 to 15 months, one year apart, 37.4 per cent of the first cohort took part in the plan and 85.9 per cent of the second; the first rate was “less than half the 86 percent participation rate of the NEW cohort with a similar amount of tenure” (pp. 1159 to 1160). The difference was 48.5 percentage points, and 50.4 after the authors adjusted for gender, race, age, pay and months of tenure, both marked significant (Table VI, p. 1167). This is a comparison of two hiring cohorts around a plan change, not a randomized trial.

A second change took effect on the same day: all employees “were made immediately eligible to participate in the 401(k) plan regardless of service” (p. 1151). To separate the two changes, the authors compare the two cohorts hired before automatic enrolment at a tenure of 15 to 27 months, only one of which had become eligible early: 48.7 and 49.4 per cent took part, a raw difference of 0.6 points and an adjusted one of 4.1, the latter also marked significant (Table VI, p. 1167). They call this “small, however, relative to the overall combined impact of automatic enrollment and immediate eligibility”, while warning that “This comparison does not provide a completely untainted assessment of the impact of immediate eligibility”, because the later of those cohorts became eligible on the date of the change rather than at hire (pp. 1161 to 1162).

Figure 1 401(k) participation by pay band, for employees hired before automatic enrolment and under it

Participation was higher in every pay band in the cohort hired under automatic enrolment. The difference was widest in the lowest band: 12.5 against 79.5 per cent.



Two hiring cohorts at one company, measured at the same tenure one year apart; not a randomized trial. Table IV prints rates without standard errors; the overall difference of 48.5 points, or 50.4 after adjustment, is marked significant in Table VI.

Source Madrian and Shea (2001), p. 1160, Table IV, columns 1 and 2: 401(k) participation rate in per cent among eligible active employees under 65, by annual compensation band, for the cohort hired April 1997 to March 1998, measured on 30 June 1998 and not automatically enrolled (N = 4,249), and the cohort hired April 1998 to March 1999, measured on 30 June 1999 and automatically enrolled (N = 5,801); both cohorts had 3 to 15 months of tenure. Overall rates 37.4 and 85.9 per cent.

The difference was widest where participation had been lowest. Across pay bands, the highest and the lowest participation rates differed by about 55 points in the earlier cohort and by 15 in the later one, and the authors find the largest differences “among the groups with the lowest participation rate under the previous regime of affirmative elections”: Black and Hispanic employees, the young and the lower paid. In their words, automatic enrolment “equalizes participation rates across various demographic groups” (p. 1161); in their conclusion, it “serves to partially equalize participation differences” (p. 1185).

Is the 401(k) result typical of default effects?

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No single result is typical of a literature whose results vary widely; the 401(k) case is one of the two with which Jachimowicz et al.¹ open their meta-analysis. The other is organ donation, from a 2003 report by Johnson and Goldstein that they cite: when defaults are taught, “a figure that depicts the effect of defaults on organ donation often features prominently”. The default can be a one-word difference, “If you want to be an organ donor, please check here” against “If you don’t want to be an organ donor, please check here”, and sign-up ran “with percentages in the high nineties for opt-out countries and in the tens for opt-in countries” (p. 160). Only the supporting online material of that report is held for this guide, not the report itself, so its result is given as Jachimowicz et al. summarise it.

The meta-analysis pooled 58 opt-in versus opt-out comparisons from 55 studies in 35 articles, with a pooled sample of 73,675, and the reference list marks both the organ donation report and Madrian and Shea’s² study as included (pp. 159, 163, 179, 182). On average, a pre-selected option was chosen more often by $d = 0.68$ standard deviations, which the authors call “a medium-sized effect given conventional criteria”, and in the 38 datasets with a yes-or-no outcome by “an absolute average of 27.24%” (pp. 164 to 165).

The average hides a wide spread. In 46 of the 58 observations the effect was significant and positive, in ten it was not significant and in two it was significant and negative; the heterogeneity statistic I^2 was 98.21 per cent, and 98.01 in the three-level model, which counts as very high (pp. 166, 168). The authors note that “in some cases, the implementation of an opt-out default may even reduce the take-up of the pre-selected option” (p. 161), and they warn that for a given application the effect “may be substantially lower or higher than the meta-analytic average” (p. 175). On this guide’s reading, neither the organ donation gap nor the 401(k) gap is a forecast for another default.

Why do people keep the option that was set for them?

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Jachimowicz et al.¹ test three channels from earlier work by Dinner and colleagues. People keep a pre-selected option because “they believe that the intentions of the choice architect, as suggested through the choice design, are beneficial to them” (endorsement), because “they can exert less effort when staying with the pre-selected option” (ease), and because “they will evaluate other options in reference to the pre-selected option with which they are already endowed” (endowment, p. 172). Two trained coders outside the author team rated how strongly each study’s design was likely to trigger each channel. Endorsement ($b = 0.32$, $p = 0.038$) and endowment ($b = 0.31$, $p = 0.044$) went with larger effects, while “Contrary to our prediction, ease is not a significant moderator”; with a more robust estimator, endowment was significant only at the 10 per cent level (pp. 173 to 174). The authors call the ratings “only noisy approximations” and note that the pooled studies rarely varied how hard it was to leave a default, though real applications do: Chile, for example, moved from a box ticked when renewing a national identity card to “requiring a notarized statement” to opt out of organ donation (p. 176).

Madrian and Shea² reach their explanations from the records alone. The economic terms had not changed, and changes “could be made on a daily basis throughout this period with a simple phone call” (p. 1176). In their account the cost of leaving the default lay in the decision, not the act: employees had to pick a rate from 1 to 15 per cent and split it across nine funds, and “if the cost of gathering and evaluating the information needed to make a 401(k) savings decision exceeds the short-run benefit from doing so, individuals will procrastinate” (p. 1177). Their first explanation is “a status quo bias resulting from employee procrastination in making or implementing an optimal savings decision” (p. 1177). They add an endowment effect, which “could also explain a preference for the default contribution rate and default investment option”, and anchoring, since under automatic enrolment “the primary reference point is clearly the default” (pp. 1180 to 1181). One finding fits none of these: employees hired earlier who joined the plan only after the change were also substantially more likely to choose the default fund, and “The most likely explanation for this type of behavior is that employees view the default investment allocation under automatic enrollment as implicit advice from the company” (p. 1182).

The authors cannot say which explanation carries the most weight: “there is no way to disentangle the magnitude of rational, transaction

costs” motivated procrastination from procrastination that comes from problems of self-control, and “further research is necessary to disentangle the importance of these various factors” (pp. 1180, 1184). On this guide’s reading, their explanations line up with the meta-analysis’s channels: implicit advice with endorsement, procrastination over a complex choice with ease, and the endowment effect with endowment. The two sources reach them differently, one by inference from one company’s records and the other from coders’ ratings of study designs, and neither measured the channels in the people who chose.

When does a default move fewer people?

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When people have reasons of their own to choose, as the authors read their data. Among the employees hired under automatic enrolment, 61.1 per cent kept every term of the default. The share fell with pay, from 70.9 per cent below US\$20,000 a year to 28.7 per cent between US\$70,000 and US\$79,000 (34.2 at US\$80,000 and over), and it was lower for men than for women and for older than for younger employees (Table VIII, pp. 1172 to 1173). Across these groups, the share taking part on terms other than the default was highly correlated with the share of the earlier cohort that had chosen to join (0.94), though it tended to be lower. One interpretation, the authors write, is that “individuals who were inclined to save prior to automatic enrollment are also more likely to change their savings behavior from the default under automatic enrollment” (p. 1173). Employees with longer tenure had left the default more often: in data from March 2000, about 10 per cent of those with one month of tenure took part with a rate or a fund other than the default, about 35 per cent of those with one year and 50 per cent of those with two. The authors conclude that “many individuals do appear to recognize that the automatic enrollment default is not” their best savings strategy, while the limited data “appear to show the fraction of automatic enrollees moving away from the default slowing over time” (pp. 1173 to 1174).

On this guide’s reading, a simple exit shows in how quickly people left, not in how many the default moved. Under automatic enrolment, participation stayed at about 85 per cent after the first month, and “individuals who opt out do so immediately”. The authors explain the lack of delay: “the decision to opt out of the 401(k) plan is not a complicated one”, and for anyone who would rather spend than save, delay was costly, because taking money out of the plan later meant income tax plus a further 10 per cent tax penalty for early withdrawal (p. 1179). Participation, the part of the default that was simple to leave, was still 48.5 points higher than in the cohort hired before automatic

enrolment, at the same tenure (Table VI, p. 1167). Jachimowicz et al.¹ expected that “the harder it is for decision-makers to switch away from the pre-selected option, the more effective the default is likely to be” (p. 173), but ease was not a significant moderator in their data, whose studies rarely varied it (pp. 174, 176). Neither source shows that a simple exit makes a default move fewer people.

In some domains, and when people distrust whoever set the default. In the meta-analysis, effects were larger for purchases, which the authors code as consumer choices, “defined as decisions related to buying a product or service” ($b = 0.73$, $p = 0.003$), and smaller for environmental choices, with a coefficient significant at the 10 per cent level, and at the 5 per cent level only in the model with the channel ratings and without robust variance estimation; the authors “can only speculate” why (pp. 168 to 171, 174, 176). They cite a study in which defaults were less effective when people did not trust the choice architect, “because the individuals believe that the choice design was based on intentions differing from their own” (p. 172).

Three further moderators are proposals for future research, not findings, because the pooled studies offered no data on them. The authors suggest that “defaults may be less likely to influence those who have strong preferences”; that when a decision matters a great deal to people, “defaults that operate primarily through the ease channel may be less likely to have an effect”; and that defaults “may be less effective in settings where preferences vary more widely”, or where preferences agree but “are misaligned with the default” (pp. 178 to 179).

What did the 401(k) default cost the employees who kept it?

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On the authors' reading, inertia: those who kept it stayed at a rate and a fund that very few employees choosing for themselves had picked, and some who would have joined anyway saved at a rate they would not otherwise have chosen. Six in ten employees hired under automatic enrolment kept the full default, 3 per cent in the money market fund, while only 1 per cent of employees hired earlier had chosen that combination, so "almost all of the 61 percent default rate for the NEW cohort represents participant inertia rather than a duplication of savings choices that many would have made regardless of the default" (pp. 1171 to 1172). Among employees hired before the change, the most common contribution rate was 6 per cent, the point at which the employer match is maximized, chosen by slightly more than a third of participants; among those hired under it, 76 per cent of participants contributed 3 per cent (pp. 1162 to 1163, 1181).

The default also appears to have moved people who would have saved anyway. Assuming that every employee who joined only because of automatic enrolment contributed 3 per cent, and that the others chose as the earlier cohort had, the authors predict that 52.9 per cent of the new cohort would be at 3 per cent; 65.1 per cent were, so "12.2 percent of employees in the NEW cohort are at a 3 percent contribution rate over what we would predict". They take this to suggest that automatic enrolment moved a substantial fraction of the employees who would have joined in any case to 3 per cent, "although they would have chosen a different contribution rate otherwise" (pp. 1164 to 1165). Among participants at the same tenure, the average contribution rate was 7.3 per cent in the earlier cohort and 4.4 in the later one, a decline of 2.9 points, or 2.2 after adjustment. On 30 June 1999, with each cohort at a different tenure, the authors report: "For the NEW cohort, 80 percent of 401(k) contributions are allocated to the money market fund, while only 16 percent of contributions go into stock funds", against roughly 70 per cent in stock funds for the cohorts hired earlier (pp. 1165, 1167, 1169 to 1170).

In the authors' words, automatic enrolment "appears to be a win-lose approach to changing 401(k) savings behavior": the win is higher participation, largest among the groups that took part least before, and the loss is participant inertia. Simulations in an earlier version of their paper, which the published paper reports but does not show, suggest that the default "may actually lead to lower total 401(k) savings after only a few years" compared with a plan that requires an active decision

(p. 1185). The approaches they suggest are to improve the default, at once or over time, for instance by enrolling at a low rate and raising it automatically “in small increments in the future”, and to educate participants. They report a concern of plan administrators about a higher starting rate, that it may lead employees to opt out, and add that “just as there is participant inertia, there is nonparticipant inertia as well” (p. 1185).

Jachimowicz et al.¹ name the assumption on which defaults rest: that they “allow those whose preferences are different from the default option to easily select an alternative” (p. 179). They also warn that defaults “are often easy to implement, and this creates a temptation” to prefer them to other tools, and that “setting a default may not always be the most suitable intervention” (p. 177). To learn what people want before a default is set, they suggest a forced-choice condition, which shows “what occurs in the absence of defaults”, and they mention “tailor-made” defaults, in which the option selected in advance differs with people’s likely preferences (p. 179). On this guide’s reading, a default is a decision made for everyone who will not choose, and the 401(k) case shows that the same default can raise one outcome and lower another for the people who keep it.

Which terms apply when a customer does not reply?

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Whatever the contract or the renewal says, within the law that governs it. A supplier sets a default whenever its terms say what happens if the customer does nothing: whether an agreement renews or lapses, at which service level, on which payment terms. None of the sources studied such decisions, so what follows is this guide’s application. A record of a contract default has six fields: the decision and its options, the terms that apply without a reply, how a customer chooses otherwise, what the default may signal, which customers would choose otherwise, and when the default is checked against active choices. The fields follow Jachimowicz et al.¹’s definition, channels and proposals, and Madrian and Shea’s² findings on who kept the default and what it cost them.

Figure 2 A contract default record

Write down the decision, the default, the exit, the signal, the fit and the review before a default goes into a contract.

Decision	Default	Exit	Signal	Fit	Review
Whose choice, which options?	What applies with no reply?	How is another option chosen?	What may the default suggest?	Who would choose otherwise?	When checked against choices?

A default assumes that anyone who prefers another option can easily choose it; record whether that holds.

One row per default. Record the exit in the same terms as the default: the channel, the deadline and the effort. Check the fit against customers who had to choose.

Source Author's decision worksheet. The decision and default fields follow Jachimowicz et al. (2019), pp. 159 to 162, and Madrian and Shea (2001), pp. 1151 to 1152; the exit field follows Madrian and Shea (2001), pp. 1176 to 1179, and Jachimowicz et al. (2019), pp. 173, 176, 179; the signal field follows Madrian and Shea (2001), p. 1182, and Jachimowicz et al. (2019), p. 172; the fit field follows Madrian and Shea (2001), pp. 1164 to 1165, 1171 to 1173, and Jachimowicz et al. (2019), pp. 178 to 179; the review field follows Madrian and Shea (2001), pp. 1173 to 1174, 1185, and Jachimowicz et al. (2019), pp. 177, 179. The fields are reader inputs; no company data is supplied.

An illustration, with no company data: a distributor of laboratory consumables moves 250 accounts to a new annual framework agreement at renewal and must decide what applies to an account that returns nothing. The decision belongs to the customer's purchasing lead, who can renew on the current delivery schedule, renew on a weekly schedule or not renew. The default is renewal on the current schedule, a default that reflects the status quo, which is the endowment case. The exit is the reply form or a call to the account manager at any time before the renewal date: each other option takes one reply, and the default takes none. The signal is what a customer may read into the default, here that the supplier thinks the current schedule suits the account. Fit and review come from the supplier's own records: which accounts picked a different schedule when they

last had to choose, and, a year on, whether the accounts that kept the default differ from those that replied. The 401(k) case is the reason for the last two fields: there the default held most firmly among the lowest paid, and on the authors' estimate it moved some people who would otherwise have chosen a different rate to the default rate. These are the author's examples and have not been tested, and the example records a default without recommending one.

A renewal recorded under such a default does not show, by itself, that the customer weighed the options and chose to stay. The [renewal essay](#) separates a recorded renewal from proof of what caused it.

How far do these studies reach for a business customer?

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Not far on their own. Madrian and Shea² studied one company, in which “almost 78 percent of the employees in this company are female, while only 47 percent of the U. S. workforce is female” (p. 1157). Automatic enrolment had been in effect a little over a year, so “it is difficult to say what the long-run impact of automatic enrollment on 401(k) participation will be” (p. 1159). Lacking data on the employees' other assets, the authors “cannot definitively rule out the possibility” that higher participation partly moved savings from elsewhere, although participation in the company's employee stock purchase plan “is completely unaffected by the switch to 401(k) automatic enrollment” (p. 1183). Their comparison is of cohorts hired a year apart, adjusted for observed characteristics, not of employees assigned at random.

Jachimowicz et al.¹ pooled laboratory and field studies of choices in health, consumer and environmental domains, organ donation and retirement saving among them, and their study characteristics “reflect the decisions of researchers about what studies to conduct and how to implement the default intervention” (pp. 168 to 169, 176, 182); the channel ratings are coders' judgements of study designs. Business buyers, buying committees and contract renewals are outside both sources. The [external validity guide](#) asks whether a result transfers across a named population, treatment, outcome and context, and the [buying committee guide](#) maps the roles a business purchase passes through.

Three points travel, on this guide's reading. A default decides the outcome for everyone who does not reply, and in both sources it moved many people. The size of its effect cannot be assumed: in two of 58 comparisons it was significantly negative, and in ten it was not

significant. And who keeps a default matters as much as how many: in the one company studied, the lower paid kept it most, and on the authors' estimate some employees kept a rate they would not otherwise have chosen. Whether a business customer, deciding through several people with a contract in front of them, keeps a renewal default as employees kept a savings default, the sources do not say.

 End of essay

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