Do Firms Report More University Knowledge Sourcing When Informed About Engagement Channels? Evidence from a Field Experiment

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Abstract

Does providing information about a wide range of university engagement channels influence the likelihood that companies report that they source knowledge from universities in their innovative efforts? To address this question, we embedded a field experiment in a multi-wave survey on university engagement that randomly exposed firms to information about a variety of engagement channels before asking about knowledge sources for innovation. We find that exposure to these channels significantly increases the likelihood of firms reporting universities as a knowledge source. The magnitude of this effect is substantial, with the propensity to report sourcing knowledge from universities increasing by 34 percent in our preferred estimate. This increase is concentrated among firms rating universities as of 'low' or 'medium' importance and among those firms with some university engagement. We discuss the implications for understanding university-industry engagement and the design of innovation surveys.

Keywords: university-industry collaboration, sources of innovation, knowledge sourcing, innovation sources, question order

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1. Introduction

Two related survey approaches to studying firm engagement with universities have been developed in the literature, one involving innovation surveys and another based on firm-level surveys of university engagement. Innovation surveys have been widely employed to map knowledge sourcing and collaboration patterns across sectors, showing that large, R&D-intensive, innovative, and open firms are more likely to source from and collaborate with universities (Fontana et al., 2006; Laursen and Salter, 2004; Tether and Tajar, 2008; Veugelers and Cassiman, 2005). Engagement patterns often show the importance of geographical proximity, with firms preferring local university partners (Alpaydın and Fitjar, 2021; Audretsch and Belitski, 2024; Laursen et al., 2011; Mansfield and Lee, 1996). Additionally, these studies have documented differences between knowledge-intensive service firms and manufacturers (Mina et al., 2014), economic impacts of university engagement (Cassiman et al., 2008), industry characteristics (Segarra-Blasco and Arauzo-Carod, 2008), learning from prior collaborations (Gómez et al., 2020; Hewitt-Dundas et al., 2019) and an association between university collaboration and innovation novelty (Añón Higón, 2016). However, innovation surveys offer limited insights into the role of universities in innovation because they typically include only two items related to universities—as one among a list of knowledge sources and collaboration partners. In addition, firms may not systematically record their engagements with universities, and some indirect engagement channels, such as student placements or hiring of skilled staff, may not be recognized as an input from universities. As such, it is not clear if responding managers are aware of the broad range of channels in which universities could be considered knowledge sources for their innovative efforts.

In contrast, firm-level surveys of university engagement have sought to describe the array of channels through which firms engage with universities. These studies have created extensive lists of potential engagement channels (Bekkers and Bodas Freitas, 2008; Bercovitz and Feldman, 2007; Bruneel et al., 2010; Cohen et al., 2002). Hughes and Kitson (2012) offer one of the most comprehensive lists, categorizing university-industry interaction into people-based, problem-solving, community-based, and commercialization activities. These categories closely align with the approach suggested by Cosh et al. (2006) and Cosh and Hughes (2010), who emphasize the importance of capturing not only directly technology-focused interactions, such as licensing or patenting, but also broader, indirect knowledge exchange channels. However, these surveys also have notable limitations. Some are restricted to R&D labs (Klevorick et al., 1995) or collaborating firms (Bishop et al., 2011; Bruneel et al., 2010), raising concerns about representativeness. In addition, by focusing on university engagement, these surveys often exclude broader information about the innovation approach of the firm, which might discourage participation from firms with limited university engagement, a substantial portion of firms overall.

While both approaches have made important contributions, there have been few attempts to understand whether patterns observed in one survey type might shape the outcomes in the other. Despite their overlapping research goals, these approaches have largely operated independently. It remains unclear whether industry-university engagement surveys attract respondents with greater awareness of the role of universities in innovation and therefore lead to an overreporting of university knowledge sourcing. In contrast, while being broad and representative, innovation surveys tend to capture information from firms with limited understanding of universities' role in the wider innovation system, potentially resulting in underreporting. Considering this gap, we explore the question: *Does exposing firms to greater information about university engagement channels lead them to report higher levels of knowledge sourcing from universities in their innovation activities?*

To address this question, we embedded a question-order field experiment in a multi-wave survey in which we adjusted the sequence in which managers viewed questions about university engagement across survey waves. In survey research, question order changes have been used to help respondents recall additional experience and knowledge when answering subsequent questions and to prime them to greater attention to specific behaviors or attitudes (Haaland et al., 2023; Stantcheva, 2023). Our expectation is that firms who are exposed to a comprehensive list of university engagement channels before being asked about knowledge sources for innovation will report higher levels of university knowledge sourcing than those receiving this information later in the survey. Overall, we find that firms exposed to this information upfront are 34% more likely to state that universities were used as a knowledge source for innovation. This effect is robust across a variety of analytical approaches and is concentrated among firms with prior university engagement. Treatment increases the likelihood of firms assigning 'low' or 'medium' importance to universities but does not affect the likelihood of assigning 'high.' We discuss the implications of these findings for understanding and measuring university-industry engagement.

2. Question order effects

Survey research has employed question-order experiments to examine whether the placement of information within a survey affects the visibility and salience of topics for respondents. One way in which this influence may occur is by providing respondents with context or background knowledge before asking them about their views on the same topic. This provision of information can enhance memory access or recall among respondents (Hopkins and King, 2010: 207) and increase their awareness of different aspects and the interconnections among different elements of a decision or activity. In the case of university knowledge sourcing, a list of university engagement channels might invoke a richer sense of the breadth of managerial activities associated with university engagement, encouraging respondents to consider distinct engagement activities as part of a coherent whole. For example, seeing both "attending conferences with academics" and "hosting student placements" in a list of engagement channels may encourage respondents to see these as related activities, inducing them to report greater knowledge sourcing from universities.

Another way question order effects may shape responses is by priming respondents for later parts of the survey. "[I]tems presented early may establish a cognitive framework or standard of comparison that guides interpretation of later items", and hence, "early items may be accorded special significance in subsequent judgments" (Krosnick and Alwin, 1987: 203). Early questions can thus subtly increase respondents' awareness of an issue, with this awareness spilling over to subsequent questions. Hopkins and King (2010: 208) compare this effect to "taring a scale before weighing something." Such question order effects are likely strongest when respondents have limited prior knowledge or difficulty recalling details, making them more receptive to provided information (Hopkins and King, 2010: 207). In our context, positioning questions with a comprehensive list of university engagement channels early in the survey may prime respondents to focus more on universities when later reporting knowledge-sourcing activities for innovation. This mechanism helps to raise attention to an issue rather than expanding knowledge of it, as suggested above. Of course, these two mechanisms may be related, as greater recall may be associated with heightened attention to the topic.

To assess the impact of question order on knowledge sourcing, we embedded a field experiment in a multi-wave survey on university engagement. Our study contributes to a growing body of research on questionnaire design in innovation surveys. Hoskens and Debackere (2024) use a randomized experiment in the Flemish Community Innovation Survey to compare the standard long CIS questionnaire with a shortened version and show that the short form yields systematically higher innovation rates. Cirera and Muzi (2020) contrast standard CIS-style yes/no innovation questions with more detailed open-ended descriptions of firms' innovations and find that many self-reported innovations do not meet standard Oslo criteria, so that innovation rates are substantially lower once these descriptions are scrutinized and recoded. Together, these studies demonstrate that questionnaire length, framing and cognitive demands can lead to substantial mismeasurement in innovation surveys. In contrast, Han et al. (2024) use a regression discontinuity design to show that combining R&D and innovation questions in the same survey of US micro-enterprises (with fewer than ten employees) does not significantly affect firms' innovation reporting. Building on this tradition, our experiment provides respondents with comprehensive information about university engagement channels before asking them about universities as knowledge sources, raising attention to and awareness of universities' contributions to the wider innovation system. We draw on a comprehensive list of university engagement channels developed by Hughes and Kitson (2012) that covers a broad range of different engagement types, grouped into people-based, problem-solving, community-based, and commercialization activities (see Table 1). Our goal is to determine whether exposure to this list increases the reporting of knowledge sourcing from universities.

[Table 1 about here]

3. Data and Methodology

3.1 Data

We embedded our experiment in an online survey of UK companies conducted between late 2020 and May 2021. The survey examines the extent and nature of firms' knowledge-exchange interactions with universities over the three years preceding the COVID-19 pandemic and is described in detail in Hughes et al. (2022). Firms from all sectors, regions and size classes were sampled from Bureau van Dijk's FAME database, which provides company-level financial and contact information based on official filings. We oversampled medium and large firms to ensure sufficient statistical power.

The survey was administered in nine separate waves, each comprising approximately 10,000 firms. Each firm received a single personalized email invitation containing a unique survey link, followed by up to four reminder emails to non-respondents. Fieldwork overlapped with the UK's second and third national lockdowns and the intervening local "tier system"; the final response was recorded on 25 May 2021 (Figure 1). Further details on the sampling frame, email validation procedures, and benchmarking of our sampling frame and respondents against the ONS 2020 Business Population Estimates are provided in Online Appendix B.

[Figure 1 about here.]

3.2 Experimental approach

Our experimental approach is a between-subjects survey experiment that alters the order in which certain survey questions are presented to respondents. Firms are randomly assigned to one of two questionnaire versions that differ only in the ordering of two sets of questions: (i) those related to knowledge sources for innovation and (ii) those addressing channels of engagement with universities. The knowledge-sourcing question is part of the innovation section of the survey and follows the wording used in the Community Innovation Survey (CIS).

The engagement section presents firms with a comprehensive list of university–industry channels based on Hughes and Kitson (2012), grouped as depicted in Table 1. Following Cosh et al. (2006) and Cosh and Hughes (2010), this list goes beyond formal "problem-solving" and commercialization links to include "community-based" and "people-based" forms of interaction, such as public events, informal networking and personnel exchanges, which align closely with the "public space" role of universities. These often informal and weakly documented links can be important for tacit knowledge exchange yet are easily overlooked in standard surveys. By explicitly naming them, our treatment may prompt firms to recognise such interactions as part of their knowledge sourcing.

In the "control version" of the survey, the innovation section appears first, followed by the engagement section. In the "treatment version", the engagement section is presented first, followed by a section on motivations for and impacts of university engagement, with the innovation section moved to third place (Figure 2). We define a *treatment indicator* equal to one for firms receiving the version that begins with the engagement section and zero otherwise.

[Figure 2 about here.]

Firms were randomly allocated to nine invitation waves. All waves except wave 6 (i.e., waves 1–5 and 7–9) received the treatment version of the questionnaire, while wave 6 received the control version. For our primary analysis, we compare wave 5 (treatment) and wave 6 (control), which were fielded closest in time and under the same national COVID-19 restrictions.

Online Appendix D shows that our results are robust to alternative sample definitions that use additional waves.

This type of question-order experiment, in which information blocks are moved earlier or later in the questionnaire for randomly assigned respondents, follows standard practice in survey methodology and recent work on survey and information-provision experiments in social science research (see the references cited in Section 2).

3.3 Variables used

Dependent variable

Our primary dependent variable is a binary indicator of whether a firm reports using universities as a source of knowledge or information for its innovation activities ("university information sourcing"). The underlying survey item follows the Community Innovation Survey and asks firms to classify universities as "not used" or as a source of "low", "medium", or "high" importance. We code the indicator as one if a firm reports universities as of low, medium, or high importance, and zero if it indicates that universities are not used as an information source for innovation (Laursen and Salter, 2004). The percentage of firms indicating university knowledge as highly important on the survey is six percent, slightly above the four percent reported in the 2021 and 2023 UK Innovation Surveys (BEIS, 2021; DBT, 2024); the distribution of high-importance ratings across all sources is very similar in the two surveys (correlation 0.96). Although this single-item measure has been widely used, it cannot capture the full range of ways in which university engagement may shape firms' innovative efforts and relies on respondents' subjective interpretation of 'importance' and 'use'.

Treatment variable

Our key independent variable is the treatment indicator described in Section 3.2, a dummy equal to one for firms receiving the questionnaire version in which the engagement section precedes the innovation section, and zero for firms receiving the version where the innovation section comes first.

Control variables

Following prior work on university knowledge sourcing (Hewitt-Dundas, 2013; Laursen and Salter, 2004; Tether and Tajar, 2008), we control for firm size (log number of employees in 2019 from FAME, supplemented by self-reports when missing), two-year asset growth, a start-up dummy equal to one for firms established at most seven years before the survey, and an indicator for being R&D active, equal to one if the firm reports any R&D expenditure in the three years before March 2020. We further include five regional dummies and five broad industry dummies based on technological intensity, but results are robust to finer regional and industry partitions. This set of controls follows prior work on university knowledge sourcing using CIS-type data. Further details on all control variables are included in Online Appendix B.

In addition, we consider two variables whose location in the questionnaire differs between the control and treatment groups. *Innovator* is a binary variable equal to one if the firm reports at least one of a product innovation, process innovation, or new logistics or distribution processes in the reference period. *External search breadth* counts the number of external information sources other than universities, this is another important predictor of a firm's use of information from universities (Laursen and Salter, 2004). Because this variable is derived from the same question as our dependent variable and thereby was moved further to the end in the treatment group survey, we exclude it from our main specification and consider it separately below.

In selected specifications, we additionally include *response-time fixed effects* (half-month dummies) to capture changes in the COVID-19 context during fieldwork. Including these dummies increases the estimated treatment effect, and using a single dummy for each month has a similar effect (see Table D.1 in the Online Appendix).

Finally, we define a binary variable *Interactor* equal to one if the firm reports using at least one of the university engagement channels listed in Table 1.

3.4 Empirical approach

We use regression analysis to account for compositional differences between treatment and control firms that may have arisen despite the random assignment of firms to waves. Invitations to each wave were sent about one week apart during the COVID-19 pandemic, and variations in firm characteristics across waves may reflect changes in firms' willingness or ability to respond over time.

Our main results are based on the following logistic regression at the level of the respondent firm i:

$$\text{logit}[\text{P}(uniuse_i|X_i)] = \beta_0 + \beta_1 treat_i + \beta_2{}' X_i + \lambda_r + \mu_s,$$

where $uniuse_i$ is the binary indicator for university innovation sourcing, $treat\_i$ is the treatment indicator, X_i is the vector of controls described in Section 3.3, and λ_r and μ_s represent regional and industry sector fixed effects. The coefficient of interest, β_1, captures the treatment effect conditional on observables. In the regression tables, the β_1 is reported in log-odds form. To facilitate interpretation, we include marginal effects on the probability scale at the bottom of each column. As a robustness check, we estimated an equivalent linear probability model by OLS and found very similar results. Further, in Section 4.6, we examine shifts in the relative importance of universities using a multinomial logit model.

We restrict the sample to firms for which all relevant variables are observed and that completed all survey questions. Including partial responses would increase the sample size, but differences in section ordering create uneven thresholds for partial completion. This yields a baseline sample of 548 firms (2,432 when including responses from all available waves in the treatment group in a robustness analysis).

4. Results

4.1 Sample characteristics, response behavior, and balance

We begin the results section by describing the sample, documenting the simple treatment–control difference in university information sourcing, and examining response behavior and covariate balance.

Table 2 reports descriptive statistics for the main sample (waves 5 and 6). We find that the reported mean use of universities is higher in the treatment group (0.47) compared to the control group (0.37), a difference of 10 percentage points (p-value 0.03) corresponding to a 26% relative increase. We interpret this difference more formally in the regression analyses below.

Notably, the treatment group is larger than the control group because some firms initially assigned to the control wave were re-invited later with updated email addresses. For administrative simplicity, all re-invitations coincided with invitations for wave 7 and received only the treatment version of the questionnaire. Importantly, no firm had access to both questionnaire versions or could respond more than once. Given that updated email addresses were likely of higher quality than initial ones from FAME, the response rate from these re-invited firms was higher, further contributing to the larger treatment group size. Since firms in waves 5 and 6 were randomly drawn from the same sampling frame and are therefore virtually indistinguishable in the sampling frame, we retain all of them in our baseline sample. In Table D.1 (Online Appendix), we show that excluding re-invited firms leaves the main results unchanged.

We invited 90,113 firms across the nine waves and obtained 2,415 fully completed questionnaires (overall response rate 2.7%), with wave-specific response rates ranging from 2.3% to 3.1%.[1] Response rates by wave are reported in Table C.1 in the Online Appendix. For our baseline analysis, waves 5 and 6 have very similar completion rates (2.7% and 2.9%).

Nonetheless, in order to examine potential response bias, we compare early and late responders, treating late responders as proxies for non-responders (Armstrong and Overton, 1977), and contrast responders with non-responders on firm size, asset growth, region, and sector. These analyses, including inverse-probability-weighted regressions, suggest that any response bias is limited and does not materially alter the estimated treatment effect (Online Appendix C).

Non-rejection of the Null of no difference in means between treatment and control group alone cannot be interpreted as evidence of balance between groups (Altman and Bland, 1995).[2] We therefore assess balance using standardized mean differences and two one-sided equivalence tests (TOST); the full diagnostics are reported in Table C.7 in the Online Appendix. These checks indicate mixed balance: most covariates pass as balanced, but firm size, asset growth and some regional and sector dummies show significant residual differences. We address these imbalances using regression adjustment in Section 4.2 and re-weighting procedures (matching) in Section 4.3.

4.2 Main results

Given the observed differences between the treatment and control groups in terms of size, region, and industry, we estimate logistic regressions that control for these covariates. Table 3 reports the results. Across all specifications, firms exposed to the catalogue of university engagement channels before answering the knowledge-sourcing question (treatment group) are significantly more likely to report universities as an information source. Including additional control variables increases the estimated average marginal treatment effect from 9.6 percentage points in column (1) to 12.1 percentage points in column (3), which controls for size, asset growth, start-up status, R&D activity, and region and industry fixed effects and which we consider our preferred specification. There, the logit model implies an average baseline probability of reporting universities as a knowledge source of 0.354 when the information catalogue is not shown. The treatment raises

[1] When restricting responses to the first survey questions, we obtain an overall response rate of 4.3%. However, as described in Section 3.4, we restrict the sample to complete responses to improve comparability between the two questionnaire versions.
[2] We are grateful to an anonymous reviewer for emphasizing this point.

this probability by 0.121 to 0.475, which corresponds to an increase of about 34% in the likelihood of reporting university information sourcing. This adjusted estimate is slightly larger than, but broadly consistent with, the unadjusted difference in means discussed in the previous section.

[Table 3 about here.]

The regressions in Table 3 use only responses from survey waves 5 and 6. When we expand the treatment sample to include earlier waves (1-4) or later waves (7-9), the estimated treatment effects remain positive and of similar magnitude, despite the differences in sampling and response timing across waves (see Online Appendix D for details).

Columns (4) and (5) of Table 3 add further controls derived from survey questions whose position differs between treatment and control versions of the questionnaire. Column (4) includes an indicator for whether the firm reports an innovation, and column (5) additionally controls for external search breadth (the number of other external information sources reported, as in Laursen and Salter (2004)). These variables may themselves be affected by the treatment and are therefore not included in our baseline specification; they are used here only to explore conditional effects. Including the innovation dummy slightly but insignificantly increases the estimated treatment effect, suggesting that firms in the treatment group do not rate universities as more important merely because they declare higher innovation activity. Adding external search breadth, instead, reduces the estimate towards the unconditional mean difference, reflecting the strong correlation between overall external search and university sourcing. We return to this issue when considering whether the treatment shapes overall respondent reports of innovation or knowledge sourcing in section 4.5.

4.3 Using matching to improve balance between control and treatment group

Table 2 indicates residual imbalances between treatment and control firms on several pre-treatment characteristics—most notably firm size (log-employees), the *innovator* dummy, and several region- and sector-specific dummies. Because the *innovator* variable may itself be affected by exposure to the information-sourcing catalogue, we do not consider it as a matching variable. Instead, we focus on balancing the exogenous regression covariates: log employment, two-year asset-growth, the start-up and R&D dummies, and the complete sets of region and industry dummies.

To demonstrate that our findings are not driven by any particular re-weighting strategy, we implement three established designs: propensity-score matching (PSM), covariate-balancing propensity-scores (CBPS) (Imai and Ratkovic, 2014), which estimate propensity scores so that covariate means are directly balanced, and entropy balancing (EB) (Hainmueller, 2012), which matches the moments of the covariate distributions. For each design, we compute the balance diagnostics described in Section 4.1. With CBPS and EB, all covariates satisfy our SMD and TOST criteria, whereas caliper PSM leaves a few region and sector dummies marginally outside the equivalent bounds. We treat CBPS as our preferred approach and report the corresponding summary statistics in Table D.2 in the Online Appendix.

Table 4 reports logistic regressions of our benchmark logit model (column 3, Table 3) using each set of weights. The estimated average marginal treatment effects range from 12.2 to 12.5 percentage points, essentially identical to the 12.1 percentage points obtained in the unweighted baseline. Because these weighting schemes remove the residual covariate imbalances in different ways yet yield very similar treatment effects, we conclude that our main results are not driven by imbalance between treatment and control groups. Further sensitivity checks with matching on narrower covariate sets (not reported) yield the same qualitative conclusion.

[Table 4 about here.]

4.4 Placebo tests

Our interpretation of the treatment is that it provides additional information and salience about university engagement channels before respondents answer the knowledge-sourcing question. If the observed effects were instead driven by the mere position of the innovation section in the questionnaire, we would expect similar changes in other outcomes from the same section. As placebo tests, we therefore examine (i) whether the firm reports having introduced an innovation in the previous three years and (ii) the number of other external information sources used for innovation ("external search breadth"; (Laursen and Salter, 2004).

Table 5 shows that the treatment has no systematic effect on external search breadth and that any differences in reported innovation activity between treatment and control firms disappear once the standard covariates are included. Moreover, including the innovation dummy in our baseline regressions in Table 3 leaves the estimated treatment effect virtually unchanged (12.8 instead of 12.1 percentage points). We also test whether moving the engagement section affects the reporting of university engagement itself. Table D.4 in the Online Appendix indicates no significant differences between survey versions in the probability of reporting any engagement with universities or in the number of engagement channels used. These placebo results suggest that the treatment effect is specific to how firms report universities as an information source, rather than reflecting a general response-style shift induced by question order changes.

[Table 5 about here.]

Finally, the information treatment should matter primarily for respondents who have engaged with universities, since only these firms can meaningfully evaluate their value as information sources. We identify these firms as those that have selected at least one of the engagement channels listed in Table 1, captured by the dummy variable *Interactor.* Consistent with this interpretation, we find a sizeable and significant treatment effect exclusively among respondents that report at least one engagement channel. Within this subgroup, the probability of reporting universities as an information source increased by 16.9 percentage points (SE = 0.056, significant at the 1% level), a relative increase of about 32%. Among firms without reported engagement, the estimated effect is small and insignificant (0.012, SE = 0.049). These results are robust to the inclusion of controls and to matching within the subset of firms with prior university engagement. In addition, our analysis of early and late responders (Online Appendix Table C.4) indicates that late responders show greater response to the treatment. This suggests that the treatment effect in the population may be higher than in our sample since non-responders may be similar to late responders (Armstrong and Overton, 1977) and are liable to have modest levels of engagement with universities.

4.5 Relative importance of universities

Beyond whether an information source is used at all, the survey records its perceived importance. Table 6 reports results from multinomial regressions comparing the three available levels of importance—low, medium, and high—to the base category of "not used". We restrict the sample to firms with *Interactor* = 1, that is, firms that report at least one form of university interaction from the list of Table 1, as this is where the treatment effect on university information sourcing is concentrated (as shown at the end of Section 4.4).

The results show that treated firms are significantly more likely to assign 'low' or 'medium' importance to universities rather than selecting 'not used,' with both categories affected to a similar extent. In contrast, there is no significant impact on reporting high importance. A natural interpretation is that exposure to the catalogue of engagement channels primarily prompts firms to recognize minor or indirect university interactions that they had previously overlooked, leading them to acknowledge universities as somewhat useful or moderately important sources of knowledge. By contrast, firms for which universities are genuinely central innovation partners—those assigning 'high' importance—are unlikely to have failed to notice these ties in the first place, so their responses are less sensitive to the additional information exposure. Further model checks and alternative specifications are reported in Online Appendix D.

[Table 6 about here.]

5. Conclusion

We find that randomly exposing firms to a comprehensive list of university engagement channels significantly increases their likelihood of reporting universities as a knowledge source for innovation. Specifically, firms exposed to this information before answering the knowledge sourcing question reported university knowledge sourcing at a rate 34% higher than those not receiving prior exposure. This effect remains consistent across various econometric approaches. Notably, the effect does not extend to other innovation sources or outcomes and is restricted to firms with some prior university interactions. Moreover, exposure increased firms' reporting of 'low' and 'medium' importance of university knowledge for innovation rather than 'high' importance. These findings suggest that information provision through question order significantly affects the level of recognition that firms give to universities' role in innovation. In turn, this implies that traditional innovation surveys may underreport university knowledge sourcing, especially among firms with limited or moderate reliance on university knowledge in their innovative efforts.

Our findings could have some important implications for policy and research on university engagement by firms. First, accurately assessing the contribution of universities to firm-level innovation is challenging and may require informing firms about the diversity of engagement channels. Our results suggest that innovations surveys may systematically underreport the extent and depth of university influence on private firms' innovative activities. Policymakers and researchers should therefore be cautious using this information for an accurate assessment the levels of university-industry interactions. Instead, they should consider investments in developing more robust, harmonized, and reliable measures and data collection procedures on firm-university interactions, akin to the efforts behind the Oslo Manual for innovation surveys (Online Appendix A elaborates on this suggestion). Second, researchers should not assume that managerial respondents are familiar with the multitude of university engagement pathways. Information provision about university channels can increase the incidence of knowledge sourcing, especially among firms with limited university interactions. This suggests that although survey research is valuable to help assess the level of university-industry interaction, it is not able to eliminate measurement error that arises from other sources, such as patent citations (Roach and Cohen, 2013), as it too is subject to significant measurement error. Third, additional field experiments could explore further how measurement error in innovation surveys can be reduced. Following approaches such as those by Hoskens and Debackere (2024), Cirera and Muzi (2020) and Han et al. (2024), future research could experimentally vary information provision across survey waves to observe shifts in reporting patterns. Such designs could reveal if richer contextual information similarly influences the reporting of other external knowledge sources, such as customers or suppliers.

Several limitations of our study suggest directions for further research. First, our experiment uses a between-subject design, randomizing question order across different respondents. Future studies could use within-subject designs that track how individual respondents might answer differently across time or different survey formats. Second, future research could try to build better measures of university knowledge sourcing rather than relying on single-item measures, as the innovation survey currently does. Second, since both survey waves include all questions, it is possible that respondents returned to their original answers about the sources of innovation after their subsequent exposure to the broad range of engagement channels. Moreover, the list of engagement channels we used is comprehensive but includes some channels that firms do not widely use. Such a long list might have encouraged firms to reflect on engagement types that are somewhat distant from their core innovation activities, such as working with schools. Identifying a 'minimum effective dose' of information required to alter knowledge sourcing reports would be a valuable direction for future studies. Fourth, our survey was conducted in the UK during the COVID-19 pandemic. It would be valuable to know whether the information exposure treatment has similar effects in other regions or under different—less challenging and uncertain—circumstances. Fifth, because our treatment moved a section of the survey rather than individual questions, it is difficult to isolate specific drivers of the observed effects. Although supplementary analyses suggest question order was the primary factor in our survey, future research should experimentally isolate the influence of individual questions versus section order. Finally, we lack detailed information about respondents' backgrounds, such as prior university experience or organizational roles. Examining how these respondent characteristics influence the effectiveness of information exposure represents another valuable avenue for future research.

Despite these limitations, our study demonstrates that exposing respondents to information about the broad range of university engagement channels significantly increases their reported reliance on university knowledge in their innovative activities. We hope our results encourage further research on how questionnaire design in surveys can contribute to a better understanding of the role of universities in firm-level innovation.

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Figures

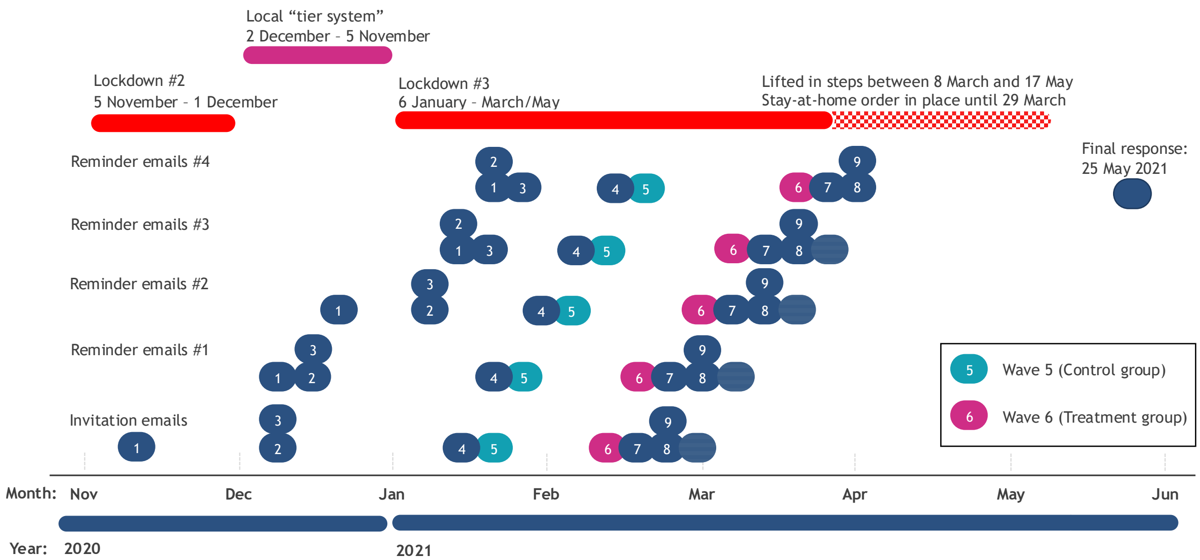

Figure 1. Timeline of survey administration and response waves. Timing is indicative; horizontal positions illustrate relative ordering only.

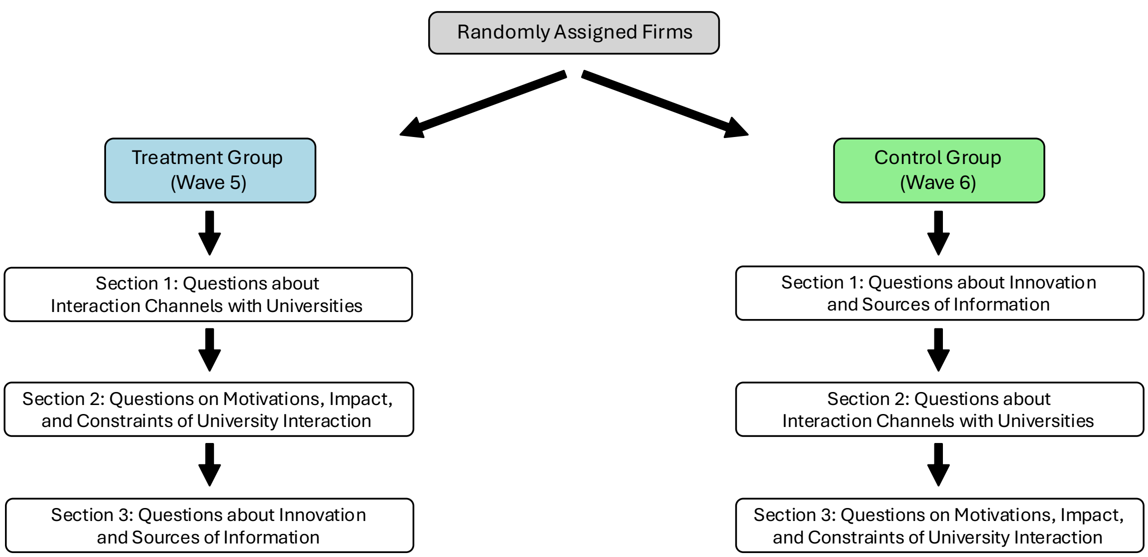

Figure 2. Experimental design of the survey and baseline estimation sample

Notes: Wave 6 received the control questionnaire version. All other waves (1–5 and 7–9) received the treatment questionnaire version. The baseline analysis compares respondents in wave 5 (treated) and wave 6 (control). Additional treated waves are used in robustness checks (see Online Appendix D).

Tables

Table 1. List of university engagement channels covered in the survey

No.	Engagement channel	% of firms selecting
"People-based" activities		
1	Training employees	16.8
2	Student projects/placements	17.5
3	Curriculum development	2.9
4	Conference attendance	26.1
5	Standard setting forums	4.0
6	Network participation	21.0
7	Advisory boards	3.6
8	Invited lectures/brainstorming	15.1
9	Entrepreneurship education	6.0
10	Studentship sponsoring	9.9
"Problem-solving" activities		
1	Setting up physical facilities	2.7
2	Joint publications	7.3
3	Hosting of university personnel	6.6
4	Secondment of staff to universities	1.5
5	Joint research	8.9
6	Contract research	4.4
7	Consultancy purchased	4.4
8	Consultancy sold	4.7
9	Research consortia	8.8
10	Providing informal advice	12.6
11	Receiving informal advice	8.6
12	Prototyping or testing	6.6
"Community-based" activities		
1	Public lectures	8.4
2	Performing arts & related	5.5
3	Museums and art galleries	5.7
4	Heritage and tourism	5.3
5	Social enterprises	7.8
6	Community-based sports	1.3
7	Public exhibitions	3.6
8	School projects	13.9
"Commercialization" activities		
1	Licensing	1.7
2	Collaborating with spin out	9.6
3	Using academic publications	21.0

Table 2. Summary statistics for the main sample (survey waves 5 & 6)

Variable	Control group N=200 mean	SD	med.	Treatment group N=348 mean	SD	med.	Mean |diff|	t-test p-val.
Univ. information sourcing	0.37			0.47			0.10	0.03
Unis low importance	0.20			0.24			0.05	0.21
Unis medium importance	0.12			0.18			0.06	0.09
Unis high importance	0.06			0.05			0.01	0.75
Employees (log.)	3.76	1.42	3.40	3.32	1.17	3.18	0.44	0.00
Start-up dummy	0.04			0.04			0.00	0.99
R&D active dummy	0.49			0.45			0.04	0.35
Asset growth (2-year)	0.59	2.91	0.16	0.30	1.30	0.10	0.29	0.16
Innovator dummy	0.65			0.55			0.09	0.03
Interactor dummy	0.61			0.58			0.02	0.62
External search breadth	6.88	2.25	7	6.81	2.55	7	0.07	0.75
Regional dummies:								
London	0.22			0.16			0.06	0.08
North of England	0.17			0.15			0.02	0.53
Midlands	0.10			0.19			0.09	0.02
South of England	0.41			0.38			0.03	0.51
Scotland, Wales, N. Ireland	0.12			0.13			0.02	0.56
Industry dummies:								
ICT, Professional & Scientific	0.31			0.22			0.08	0.04
Other KI Services	0.18			0.17			0.00	0.94
Other Service and Non-Mfg. Sectors	0.39			0.40			0.02	0.69
High-Tech Manuf.	0.04			0.07			0.03	0.12
Low-Tech Manuf.	0.10			0.14			0.04	0.23

All variables except employees (log), asset growth and external search breadth are binary. Mean |diff| denotes the absolute difference in means between treatment and control groups. “Other KI Services” refers to other knowledge-intensive services. The column “t-test p-val.” reports the results of a two-tailed test for equality of means between both groups, or a chi-2 test for binary variables this is equivalent to a chi-2 test. For those continuous variables that did not pass a (unreported) test for equality of variances, we conducted t-test without the assumption of equal variances.

Table 3. Baseline regression results

	(1)	(2)	(3)	(4)	(5)
Treatment	0.394**	0.514***	0.543***	0.586***	0.571***
	(0.182)	(0.191)	(0.196)	(0.197)	(0.215)
Employees (log.)		0.123*	0.122*	0.077	-0.061
		(0.073)	(0.074)	(0.076)	(0.080)
Start-up		-0.305	-0.325	-0.345	-0.278
		(0.404)	(0.426)	(0.418)	(0.526)
R&D active		0.860***	0.798***	0.529**	0.462**
		(0.181)	(0.191)	(0.211)	(0.233)
Asset growth		0.024	0.013	0.018	0.020
		(0.043)	(0.048)	(0.049)	(0.039)
Innovator				0.701***	0.428*
				(0.214)	(0.232)
External search breadth					0.522***
					(0.065)
Constant	-0.532***	-1.447***	-1.521***	-1.677***	-4.546***
	(0.147)	(0.320)	(0.436)	(0.445)	(0.655)
Observations	548	548	548	548	548
Log lik.	-372.178	-357.392	-351.337	-345.908	-287.696
Pseudo-R2	0.006	0.046	0.062	0.076	0.232
Region and industry fixed effects			yes	yes	yes
Average Marginal Effect	0.096**	0.117***	0.121***	0.128***	0.101***
of Treatment	(0.043)	(0.042)	(0.043)	(0.042)	(0.038)

Parentheses report robust standard errors. The bottom row of the table presents the marginal effect calculated using the estimated regression coefficient for the treatment dummy. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4. Regressions using matched samples

	(1)	(2)	(3)
Matching method:	Propensity score	Covariate-balancing Propensity Score	Entropy balancing
Treatment	0.569***	0.552***	0.576***
	(0.201)	(0.196)	(0.202)
Observations	545	548	548
Log lik.	-436.235	-347.921	-431.521
Pseudo-R2	0.071	0.069	0.080
Marginal effect	0.125***	0.122***	0.125***
	(0.043)	(0.042)	(0.043)

Each logistic regression contains the same set of covariates and fixed effects as column 3 in

Table 3. Observations in the control group are re-weighted using the matching algorithm mentioned in the top row. These same covariates are also used for matching observations. Column (1) is based on radius matching with a 0.1 caliper (many-to-one, with replacement) using the `psmatch2` Stata module (Leuven and Sianesi, 2003). Column (2) implements just-identified covariate-balancing propensity score weights using the `psweights` Stata module (Kranker et al., 2021). Column (3) uses the `ebalance` module (Hainmueller and Xu, 2013) to match the first moments of the controls to the treated. Parentheses report robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5. Regression of search breadth and innovation variables

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	External search breadth			Innovator dummy		
Treatment	-0.010	0.018	0.025	-0.389**	-0.278	-0.241
	(0.031)	(0.030)	(0.030)	(0.183)	(0.208)	(0.214)
Control variables		yes	yes		yes	yes
Region and industry dummies			yes			yes
Observations	548	548	548	548	548	548
Log lik.	-1302.414	-1284.547	-1281.435	-369.447	-299.583	-293.571
Pseudo-R2	0.000	0.014	0.016	0.006	0.194	0.210

Columns (1)-(3) are the result of Poisson regression; columns (4)-(6) of logistic regression. The high values for log-likelihood and pseudo-R^2 in Columns (5) and (6) occur because the R&D dummy is a strong predictor of having introduced an innovation. Parentheses report robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6. Multinomial regression of relative university importance

	(1)	(2)	(3)
Low importance (variable = 1)			
Treatment	0.655**	0.683**	0.663**
	(0.283)	(0.287)	(0.298)
Medium importance (variable = 2)			
Treatment	0.932***	1.022***	1.013***
	(0.314)	(0.329)	(0.341)
High importance (variable = 3)			
Treatment	0.291	0.420	0.554
	(0.433)	(0.455)	(0.476)
Control variables		yes	yes
Region and industry fixed effects			yes
Observations	324	324	324
Log lik.	-409.738	-402.935	-386.035

Parentheses report robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$