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ERC Research Paper No.4

June 2013

www.enterpriseresearch.ac.uk

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Acknowledgements: Funding for this project was provided by Nesta, the Economic and Social Research Council, the Arts and Humanities Research Council and the North West Development Agency. Haidee Bell (Nesta), Karen Brayne (previously Nesta), Matt Gostelow (Aston Business School), Adam Hewitt (Aston Business School) and Jon Kingsbury (Nesta) made valuable contributions to the policy experiment. We are also grateful to firms in the Manchester City Region who so generously gave their time to participate.

This paper is published by the independent Enterprise Research Centre. The Enterprise Research Centre is a partnership between Warwick Business School, Aston Business School, Imperial College Business School, Strathclyde Business School, Birmingham Business School and De Montfort University. ERC is funded by the Economic and Social Research Council (ESRC); the Department for Business, Innovation & Skills (BIS); the Technology Strategy Board (TSB); and, through the British Bankers Association (BBA), by the Royal Bank of Scotland PLC; Bank of Scotland; HSBC Bank PLC; Barclays Bank PLC and Lloyds TSB Bank PLC. The support of the funders is acknowledged. The views expressed are those of the authors and do not necessarily represent the views of the funders.

Contents

Abstract:

Experimental methods of policy evaluation are well-established in social policy and development economics but are rare in industrial and innovation policy. In this paper we consider the arguments for applying experimental methods to industrial policy measures, and propose an experimental policy evaluation approach (which we call RCT+). This combines the randomised assignment of firms to treatment and control groups with a longitudinal data collection strategy incorporating quantitative and qualitative data (so-called mixed methods). The RCT+ approach is designed to provide a causative rather than purely summative evaluation, i.e. to assess both 'whether' and 'how' programme outcomes are achieved. We test the RCT+ approach in an evaluation of Creative Credits – a UK business-to-business innovation voucher initiative intended to promote new innovation partnerships between SMEs and creative service providers. The results suggest the potential value of experimental approaches to industrial policy evaluation, and the benefits of mixed methods and longitudinal data collection in industrial policy evaluations.

Keywords: Evaluation, experimental, industrial policy, innovation, creative, qualitative research

1. Introduction

Although unusual in terms of industrial policy, experimental methods of policy evaluation are well established in social policy and development economics. Burtless (1995, p. 63), for example, cites Greenberg and Schroder (1991) who 'identified more than 90 separate field trials involving a wide range of distinctive research areas including health insurance, prisoner rehabilitation, labour supply, worker training and housing subsidies'. Banerjee and Duflo (2008, p. 32) also describe the 'recent surge in experimental work' in development economics. Typically such evaluations involve individual human subjects facing some common socio-economic problem, and random allocation of subjects to a treatment and control group. Differences in outcomes between the treatment and control groups are then attributed to the effect of the policy intervention. In terms of industrial policy, however, experimental policy evaluation approaches remain marginal, with non-experimental, ex post policy evaluations remaining the norm. In the context of small business policy evaluation, Potter and Storey (2007), for example, provide an extensive review of best practice in OECD countries without any mention of either the application or potential for experimental methods. Similarly, UK government guidance on industrial policy evaluation focuses entirely on non-experimental ex post evaluation approaches (BIS, 2009). Related observations might be made in relation to the evaluation of R&D and innovation policies: despite the increasing importance of evaluation as part of the process of development of technology policy, evaluation approaches remain almost universally ex post and non-experimental (Laredo, 1997).

The methodological and practical advantages of experimental and non-experimental evaluation methods have been widely debated in the context of social policy interventions (Burtless, 1995, Heckman and Smith, 1995, Bratberg et al., 2002, Banerjee and Duflo, 2011, Deaton, 2010). Experimental methods based on randomised allocation have – at least in theory – the advantage of transparency and may be more convincing to policy-makers than the results of more complex econometric evaluation

approaches (Burtless, 1995). In small samples, however, perhaps less than 300, randomisation may be ineffective at ensuring the homogeneity of control and treatment groups (Bratberg et al., 2002), though robust experiments can still be run with smaller samples if the intervention is sufficiently powerful (Bloom et al., 2011). Implementing experimental approaches may also lead to other specific biases (Heckman and Smith, 1995), while small-scale experimental studies may fail to replicate the likely macro-impacts of a scheme which is implemented at national level – that is, they lack external validity (Garfinkel et al., 1990). Perhaps the key advantage of experimental approaches, however, and the central issue with non-experimental evaluation approaches, relates to potential selection biases. In particular, if subjects which are allocated to a treatment group have a higher preponderance of some characteristic which is correlated with outcomes this will lead to bias in the estimation of treatment effects. Such biases may be significant where policy interventions are targeted at particular groups of subjects, where support is allocated on the basis of routinized decision rules, or where there is an element of self-selection into an experiment. One recent study, for example, illustrates how funding allocations in the Norwegian Research Council are based on ex ante project rankings generating a potential selection bias when evaluating the Research Council's funding decisions (Bremnes et al., 2011). In terms of policy for innovation or small and medium enterprises (SMEs), similar selection biases might arise where a policy initiative seeks to back winners or is focussed on firms which have an established track record of growth or innovation. In this sense the receipt of public support may itself need to be treated as endogenous (Garcia and Mohnen, 2010).

As many methodological and implementation issues arise in industrial policy evaluations, which focus on firms, as they do in social policy, where the subjects are likely to be individuals. The selection biases are likely to be as great. However, arguably some of the ethical issues which arise in implementing experimental approaches to social policy may be seen as less significant in industrial policy interventions. It may be more ethically acceptable, for example, to randomly allocate public support among firms

rather than adopting a similar allocation rule to the distribution of support among financially disadvantaged individuals. This makes it all the more surprising that experimental evaluation approaches in industrial policy are not more common.

In this paper we propose and test an experimental approach to the evaluation of new industrial policy interventions. Our approach (which we label RCT+ or Randomised Control Trial plus) takes advantage of randomisation, but combines this with a longitudinal and mixed methods data collection strategy to provide causative insights rather than only summative policy assessment. In other words, we seek to assess the validity of an underlying logic model rather than simply generating point estimates of policy impact (Donaldson and Gooler, 2003), and seek to explain why these results are observed (Denzin and Lincoln, 2005, Ludwig et al., 2011).

Our paper makes three main contributions. First, we provide an assessment of the value of experimental evaluation approaches to industrial policy initiatives, suggesting an alternative enhanced approach to the development of industrial policy. Second, we extend standard (quantitative) experimental evaluation approaches beyond the summative to provide causal explanations for policy outcomes – i.e. to identify the 'why, how, and at what cost' an intervention may have worked (White, 2008, p. 98). Adopting this type of rigorous qualitative approach provides potentially frame-breaking insights (Eisenhardt, 1989), and may enable the conceptualisation of the context in which an intervention was implemented, facilitating the generalisation of results (White, 2008). Third, we show how a longitudinal approach can provide a time profile of policy outcomes, without which policy-makers can make incomplete, and potentially misleading, inference.

The focus of our analysis is the Creative Credits scheme, a UK-based business-to-business innovation voucher programme designed to foster new innovative partnerships between SMEs and creative service providers.

The remainder of the paper is organised as follows. In Section 2 we discuss the rationale for industrial policy evaluation and review previous debates about the relative merits of experimental and non-experimental evaluation approaches. Subsequent sections of the paper report the application of the RCT+ evaluation approach to the Creative Credits experiment. Section 3 provides a brief overview of the rationale for the Creative Credits scheme and outlines the logic model for the intervention. Section 4 describes the implementation of our policy experiment involving both scheme implementation and the RCT+ evaluation. Section 5 provides an overview of the evaluation results, Section 6 deals with implications and Section 7 concludes.

2. Evaluating industrial policy initiatives

Potter and Storey (2007) identify five reasons why industrial policy evaluation might be undertaken: to establish the impact of industrial policy; to inform the allocation of funding to alternative policy measures; to demonstrate value for money; to stimulate debate about forms of public intervention; and, to contribute to improvements in the design and administration of policy interventions. In each case the problem of causal inference is the same, i.e. that the treated and non-treated outcomes for any single firm are never observed (Holland, 1986). The analytical problem this raises is how to estimate the difference between the actual realised outcomes and the potential outcomes if no treatment had been administered. Ideally, the substitute for the unobserved (un-treated) outcome needs to meet two criteria: (i) it should be observable to the researcher; and, (ii) it should be an 'internally' valid substitute for the set of un-treated outcomes. Internal validity in this sense requires that ‘the only difference between the member of the control group and the member of the treated group corresponds to the fact that the latter is treated and the first one is not’ (Reiner, 2011, p. 18).

More comprehensively, Imbens and Wooldridge (2009) outline three situations which describe the allocation of subjects to a control and

treatment group. The first, and simplest, is the classical experimental situation of randomised allocation in which allocation is unrelated to outcomes. The second allocation mechanism – 'un-confounded allocation' - occurs where assignment is independent of outcomes but may be related to subject characteristics. Here, where the assignment mechanism is either observable or discoverable, sampling and/or statistical approaches can be used to minimise any systematic differences between the characteristics of the treatment and control groups and provide a valid estimate of treatment effects (Burtless, 1995). In practice, evaluations of SME policy vary in the sophistication of their approach to un-confoundedness. Potter and Storey (2007), for example, cite evaluation studies which compare the performance of treated firms with control groups of 'typical' firms (Lambrecht and Pirnay, 2005), and studies which use 'matched' control groups based on treatment and control groups with similar baseline characteristics (Lerner, 1999). However, despite careful matching or selection of control groups, the potential remains for bias in terms of the background characteristics of the two groups (Bratberg et al., 2002).

This has led to the development and widespread application of a third group of econometric approaches which can 'control' ex post for potential selection biases by either implicitly or explicitly modelling the probability that a firm will be in the treatment rather than the control group, and then estimating the impact of the treatment 'controlling' for any selection biases (Bratberg et al., 2002, Imbens and Wooldridge, 2009). Implementing this type of non-experimental approach, however, often involves making assumptions about the underlying causal process which determines the allocation of firms to the treatment and non-treated groups (Burtless, 1995). Specific approaches may also pose challenges in terms of identification, requiring, for example, the use of a variable or group of variables which influence allocation but which have no influence on subsequent outcomes. Early comparisons of non-experimental econometric estimators with experimental estimators tended to favour experimental estimators (Heckman and Hotz, 1989), and highlighted the sensitivity of the results of non-experimental studies to the choice of econometric approach (Bratberg

et al., 2002). This led Burtless (1995) to conclude that: 'the classical experimental estimator still has a major advantage over non-experimental estimators for users who care about the statistical precision of the estimates they use. But the more important advantage is that the validity of the experimental estimator depends upon assumptions that are ordinarily much easier to evaluate – and to believe' (Burtless, 1995, p. 73). More recently, however, developments in propensity scoring, matching estimators and instrumental variables provide alternative statistical approaches to dealing with un-confoundedness in different contexts (Imbens and Wooldridge, 2009). This type of evaluation, allowing for potential selection biases, has been emphasised by the OECD as best practice in ex post evaluation and has been widely applied in recent years in the context of SME support measures (Potter and Storey, 2007).

Thus, in non-experimental evaluations either un-confounded or confounded allocation – where allocation has some dependence on potential outcomes – can have profound implications for a researcher's ability to obtain reliable estimates of treatment effects. In experimental approaches, by contrast, random assignment to the treatment and control group should avoid such biases and allow more accurate estimation of treatment effects (Burtless, 1995). A number of potential implementation issues arise, however, even with experimental designs involving randomised allocation (Reiner, 2011). First, in small samples randomised allocation may fail to eliminate differences in the characteristics of firms in the treatment and control groups, influencing the internal validity of the experiment. As a result Bruhn and McKenzie (2009) suggest that in small samples (30-100 observations in their simulations) the similarity of treatment and control groups is better where pair-wise matching, stratification or re-randomisation approaches are used rather than simple random allocation. For larger sample sizes (>300), however, their simulation analysis suggests that the choice of randomisation approach is a much less significant factor. A second threat to internal validity in experimental studies highlighted by Heckman and Smith (1995) is the potential for substitution bias where members of a control group are able to substitute alternative forms of support for the focal

treatment effect. If significant, this contamination may undermine the estimate of the treatment derived from the difference in outcomes between the treatment and control groups. In the context of an industrial policy initiative where the subjects are firms rather than individuals it is also possible to envisage a related 'signalling bias' where firms in the treatment group are able to attract additional investment because they are in the treated group. Meuleman and Maeseneire (2012), for example, find that Belgian SMEs which obtained an R&D subsidy were better able to access long-term debt than other firms. This type of effect may again influence the scale of any measured treatment effect.

Finally, it is not clear how readily the results of any single policy experiment involving a small proportion of a target population can be generalised to the entire target population, i.e. whether the results of small scale policy experiments are 'externally' valid. Garfinkel et al (1990), for example, focus on the 'macro' or society-wide effects which would occur if an intervention was implemented at macro-level but may not be replicated in small scale studies. These include: policy effects on the economic equilibrium; widespread diffusion of information about the programme; and, social interaction and norm formation which might influence programme participation or other related decisions. External validity may also be reduced if the characteristics of those firms participating in a programme differ significantly from the wider target group (Burtless, 1995). Applicants for innovation support measures, for example, may be more strongly innovation-oriented than firms in general, a capability which has been linked to above average business performance (Rosenbusch et al., 2011). External validity may also be impacted by 'randomisation bias', the idea that the adoption of random allocation itself might either induce or discourage some types of firms from applying for a scheme (Heckman and Smith, 1995).

While internal and external validity have been widely discussed in the summative, ex post evaluation literature, similar themes also emerge in respect of evaluations which adopt a constructivist or qualitative approach.

Guba and Lincoln (1989), for example, emphasise trustworthiness, the nature of the evaluation process and authenticity as key attributes of robust qualitative evaluation approaches. They describe trustworthiness as a group of robustness criteria (i.e. credibility, transferability, dependability and confirmability) that are 'parallel' to the experimental criteria of internal/external validity, reliability and objectivity; parallel due to ontological and epistemological differences (p. 233). Guba and Lincoln (1989) describe a second criterion, which they call process, as evaluating how the data was accumulated and the rigour used to ensure it accurately reflects the context (p. 244). However, because the trustworthiness criterion does not ensure that stakeholders' constructions have been collected and faithfully represented within the messiness of research (p. 245), and the process criterion does not provide explicit evidence, there is a third criterion – authenticity. Authenticity provides balancing aspects of: the fairness of integration of all viewpoints; the extent to which (through the process of the evaluation) stakeholders expand their own constructions and their constructions of others; the action facilitated by the evaluation; and, the empowerment of stakeholders to act. Attending to these three criteria helps to develop constructivist evaluations that are rigorous and from which translatable results can emerge.

The statistical and interpretative arguments favouring experimental evaluation approaches may in some circumstances be reinforced by economic arguments related to the potential for adverse selection in more traditional evaluation approaches, and the relative costs of basing allocation on randomisation and, say, peer group evaluation. For example, in SME and innovation programmes, public support is often allocated on the basis of some ex ante screening or peer evaluation process (which GPrix (2012) calls 'cream-skimming'). As in equity investments, this situation involves asymmetric information with the firm having an incentive to over-estimate the strength of its project proposal. This creates the potential for adverse selection and the backing of weaker projects (Reiner, 2011). Evaluation or screening of programme applications is also resource intensive, and may be uneconomic particularly where the public support

being offered to each firm is relatively small (Takalo and Tanayama, 2010). In the case of innovation voucher schemes, for example, where the support available to each firm is typically small, undertaking detailed due diligence on each applicant company and project is not cost-effective, providing an additional economic rationale for randomised allocation.

3. Creative Credits logic model and causative mechanisms

A growing body of evidence suggests that collaborating with external partners on innovation (or 'openness') may influence the innovation performance of firms through stimulating creativity, enhancing product quality and providing reputational benefits which signal the quality of firms' innovation activities (Powell, 1998). External innovation linkages may also provide access to networks which create commercial opportunities, and allow firms to search their technological environment in a more systematic fashion, resulting in improved access to technology developed elsewhere (Niosi, 2003). Open innovation poses particular challenges for resource-constrained SMEs, however, associated with their need to: (i) develop mechanisms for identifying useful external knowledge; (ii) build organizational structures to support collaborations; and, (iii) absorb externally developed ideas and technologies and make them fit for the purpose of their own businesses (van de Vrande et al., 2009). Arguably, these cognitive demands are perpetuated by behavioural failures on the part of managers running SMEs – such as inertia (the tendency to accept the status quo, no matter how strong the case for change might be), excessive risk aversion (cognitive biases push owners of SMEs to make choices that anticipate more certain outcomes, particularly at the boundaries of their knowledge or experience), and myopia (the tendency to opt for short-term gain at the expense of longer term, strategic decisions) (Potts and Morrison, 2009). These behavioural failures may contribute to reluctance among SMEs to undertake open innovation which requires novelty, risk tolerance and a willingness to make strategic investments.

Policy-makers have responded with instruments such as innovation vouchers to overcome some of these behavioural failures and encourage SMEs to engage in new open innovation partnerships (DG ENTR-Unit D2, 2009).

Typically, innovation voucher programmes have aimed to overcome or lower the barriers which SMEs face in developing innovative partnerships with universities. Research conducted in the UK Manchester City Region, however, emphasised the weakness of innovation linkages between SMEs and local creative services firms (MIER, 2009). This suggested the potential value of an initiative designed to stimulate new innovative business-to-business partnerships between SMEs and local creative service providers (Woolthuis et al., 2005)[1]. The logic model for the resulting Creative Credits scheme is shown in Figure 1 and links the justification for public intervention in the relationship between SMEs and creative service providers, the scheme's objectives, the process by which the scheme operates, its immediate outputs and intended longer term outcomes (Donaldson and Gooler, 2003). More specifically, the logic model describes how the award of a Creative Credit should help an SME to overcome the financial barriers to innovation and/or behavioural failures. As a result, SMEs develop new collaborations with a business partner in the creative industries, which leads to innovations and improvements in firms' longer term competitive positions. In the short term, scheme outputs are measured primarily in terms of increased levels of project additionality, i.e. – the frequency of interactions between SMEs and creative businesses. In the longer term, we anticipate that this might generate three types of scheme outcome: output additionality, as the innovations developed impact on sales and growth (Hewitt-Dundas and Roper, 2009); behavioural additionality, as organisational learning takes place and SMEs learn to work with and value more highly creative inputs to innovation (OECD, 2006, Buiseret et al., 1995, Georghiou, 2004); and, network additionality,

[1] Supporting evidence is found in Bakhshi and McVittie (2009)(2009), Stam, de Jong and Marlet (2008) and Muller, Rammer and Truby (2009) which all report that businesses which make proportionately greater use of creative services introduce more innovations.

as SMEs extend their future collaborative networks (Consult, 2006). The logic model also highlights that these medium to long-term outcomes are contingent on other factors, most notably the business environment.

4. The policy experiment

Our evaluation experiment focuses on the 'causative' elements of the logic model outlined in Figure 1. In other words, our evaluation objective is not just to assess scheme outcomes but also to consider whether these outcomes are being achieved through the mechanisms envisaged in the logic model (Chen, 1990). This requires a theoretically grounded analysis of process and causal mechanisms alongside the evaluation of outcomes (Ludwig et al., 2011). In empirical terms, this defines our methodological approach: a mixed-methods evaluation combining a qualitatively structured examination of underlying processes and decisions with a quantitative assessment of causal process and outcomes (Jackson, 2001, White, 2008).

The policy experiment was conducted in the Manchester City Region of North-West England between September 2009 and October 2010. In total one hundred and fifty Creative Credits were distributed in two equal groups roughly six months apart. On receipt, applications from SMEs were checked for eligibility[2]. A total of 672 SMEs made eligible applications: 312 in the first wave and 501 in the second, 141 firms applied in both waves. Once eligible applications had been received, a lottery was held in each of the two waves with 75 firms in each group being notified that they had been 'awarded' a Creative Credit. Each Creative Credit had a face value of £4,000 with recipient firms also required to contribute a minimum of £1,000 to the cost of the joint project with their creative partner. Subsequent to the award, SMEs were encouraged to identify a creative partner and develop a

[2] Eligibility details are outlined in 'A Guide to *Creative Credits*', Nesta, March 2011. Available at: http://www.nesta.org.uk/assets/features/guide_to_creative_credits.

collaborative project proposal[3]. Once a partnership was formed, all projects were required to be completed within five months.

Data collection for the quantitative element of the evaluation comprised four sequential surveys of the treatment and control groups. Survey 1 was a baseline survey undertaken at the time firms were awarded a Creative Credit and allocated to the treatment and control groups. Survey 2 was undertaken around six months after Survey 1, at a point just after the firms in the treatment group had completed their projects. Questions in Survey 2 related primarily to project additionality. Surveys 3 and 4 undertaken six month and twelve months later focussed on output, behavioural and network additionality. Over the course of the four surveys, and despite the payment of small cash incentives to firms to encourage continued participation in the data collection, significant attrition in response was experienced. By Survey 4, response numbers in the control group had fallen to 157, 52.2 per cent of those firms responding to Survey 1. In the treatment group attrition was less severe, with 78.0 per cent of Survey 1 respondents also responding to Survey 4. A key issue in terms of the internal validity of the experiment was whether the characteristics of the respondents to all of the four surveys and those firms which dropped out were similar, or whether attrition was systematically related to some respondent characteristic. Comparing the starting characteristics (from Survey 1) of stayers and those firms which did drop out, however, suggested no systematic differences to the initial control and treatment groups. This suggests that despite significant attrition this aspect of the internal validity of the experiment was maintained.

Data collection for the qualitative element of the evaluation comprised a series of longitudinal qualitative case studies with SMEs and their creative partners, the timing of which aligned with the quantitative surveys. The

[3] To help with this process a web-based marketplace – a Creatives Gallery – of eligible creative firms was designed and made available to all eligible SMEs in the treatment and control groups. The aim of creating the online Gallery was to explore the potential for a minimal brokerage model and reduce the burden of administrative costs of the pilot project. SMEs were not allowed to work with creative companies which they had previously worked with, however.

case studies involved semi-structured interviews, multi-organisational workshops and direct observations of SME practices. Alongside Survey 1, 43 semi-structured interviews were undertaken with SMEs (24) and their creative partners (19). Firms were re-interviewed alongside Survey 2 (22 SMEs and 14 creative partners) and Survey 4 (19 SMEs and 8 creative partners). Attrition in the qualitative sample, due to firms ceasing trading or refusing to continue to participate in interviews, resulted in longitudinal quantitative data being available from 11 pairings plus the 'surviving' member of five other partnerships. Survey 3 was accompanied by two group workshops facilitated by members of the research team which included fourteen firms in the treatment group (Eden and Ackermann, 1998). These workshops focussed on themes arising from the qualitative data gathered alongside Surveys 1 and 2, and contributed to the development of topics and questions for the Stage 4 quantitative survey and qualitative interviews[4].

Qualitative data collection and analysis was guided by a grounded theory methodology (Glaser and Strauss, 1967, Glaser, 1998, Katz, 1983) which allowed substantive concepts and trends to emerge from a comparison of the data collected over the four stages. Themes identified from each stage informed subsequent stages of both the qualitative and quantitative data collection, providing theoretical sampling (Glaser and Strauss, 1967). The longitudinal aspect of the interviews provided opportunities to confront the emerging theory with further data, and move towards theoretical saturation. Rigour was ensured through three types of triangulation (Yin, 2003). Data triangulation corroborated findings in different types and sources of qualitative data. Investigator triangulation ensured that analyses were conducted on data by more than one researcher to avoid investigator bias (Bryman, 2004). Methodological triangulation involved different data

[4] Where results from the qualitative data are reported in the form of quotations or conclusions from interviews, each business has been allocated a letter and number to signify SMEs in the treatment group (S), their creative partners (C) and their participation in wave 1 or wave 2 of the experiment. So, for example, W1S1 relates to wave 1, SME 1. Any unattributed quotes come from the 'free format comments' boxes in the quantitative surveys and therefore cannot be attributed under the non-disclosure terms of those surveys.

(interviews, workshops, observations) being analysed using different techniques (manual and software-supported approaches) to search for generic findings (Bryman, 2001). Furthermore, as respondents agreed to be taped and all interviews were transcribed, data was analysed in its totality several times (Gillham, 2000). Qualitative data analysis involved a process of open coding to identify concepts and then axial coding to establish higher-level categories and identify relationships between the open codes (Glaser, 1998). Then selective coding refined codes until clear relationships between them were identified, leading to the development of a theory about the data.

5. Evaluation results

The first question suggested by the logic model in Figure 1 is the extent of project additionality, i.e. the extent to which the award of a Creative Credit increased the probability that an SME entered into a new relationship with a creative business. Modelling suggests that the Creative Credits treatment increased the probability that firms went ahead with their project within the five months since allocation of the Creative Credits by around 84 per cent (Table 1) [5], a level of project additionality very similar to that reported for the pilot Dutch innovation vouchers scheme (Cornet et al., 2006)[6]. Evaluations of the Swiss and Austrian innovation voucher schemes suggest similarly high levels of project additionality (Good and Tiefenthaler, 2011).

The quantitative Dutch, Swiss and Austrian innovation voucher evaluations explained their results summatively but not causally. With RCT+, our qualitative data identified two factors that underpinned the robust

[5] Of the 301 firms in the control group which responded to our baseline survey, 36 firms (12 per cent) went ahead anyway with their projects. Among the group of 150 firms which were assigned Creative Credits 144 (96.0 per cent) actually commissioned projects.

[6] We modelled project additionality using both simple OLS and as a robustness check a treatment model allowing for potential sample selection. As expected, selection effects proved insignificant. As part of the same exercise we also investigated whether the small cash incentives provided to firms to help encourage survey response had biased these results. No evidence of any significant bias could be identified Details of these models and those referred to later in this section can be found in Bakhshi et al, (2012). Available at: http://www.nesta.org.uk/library/documents/Creating_Innovation_in_SMEs_v13.pdf.

quantitative estimates of project additionality. First, SMEs described how their project fed operational plans and how they anticipated it would enable them to market their company offering more widely (in line with the stated objectives of the scheme). Second, a number of companies indicated how the Creative Credit had helped them to 'accelerate things' (W1S8): 'I'd have waited until we'd accumulated more money. And then probably programmed it in for sort of the back end of this year' (W1S10). In only a minority of cases included in the interviews did Creative Credits instigate projects that would not otherwise have taken place: 'couldn't have afforded [the project, so] wouldn't have done it but it has made a real difference' (W1S9).

Output additionality should be reflected in identifiable differences in the sales and innovation profiles of the treatment and control groups in the period shortly after the completion of the treatment. In the evaluation, survey-based comparisons were made after 6 months and 12 months. Six months after the end of the treatment, firms in the treatment group were significantly more likely to be undertaking product and process innovation, and had (at the 10 per cent level) a significantly more positive distribution of sales growth rates than firms in the control group (Table 2). These output additionality effects were short-lived, however, with no significant differences between the treatment and control groups in product or process innovation or sales distribution evident 12 months after the end of the treatment (Table 2). Robustness checks using multivariate models allowing for potential selection effects confirmed these results[7].

Inevitably there were exceptions but, in the short-term, our qualitative analysis suggested a largely positive feeling about the Creative Credits scheme and its results. The majority of innovations involved website enhancements and/or the development of marketing collateral and SMEs

[7] These sales effects may under-estimate the longer sales benefits of Creative Credits. Twelve months after the end of the project – in Survey 4 – firms were asked how long they expected the benefits to persist. On average firms indicated that the benefits of their projects were likely to persist for a further 2.5 years, or 3.5 years after the end of the project.

had generally positive expectations of the impact of these on their business. However longer-term, the qualitative data supported the absence of any boost to sales growth. Many SMEs, like their wider markets, experienced a drop in sales over the period which tended to push them towards focusing on winning contracts or obtaining support from banks, taking attention away from their innovation, so undermining the value of the project. As one SME reported: 'like sticking a Band Aid on a much bigger problem' (W1S3). For a few SMEs the impact was low because they were dissatisfied with their innovation outcome: 'If I was to pay that out of my own account I would just not be happy at all'.

Behavioural additionality is measured here primarily in terms of firms' future innovation intentions. Here, we found no significant differences in innovation intentions between the treatment and control groups after either 6 or 12 months, a finding that was again confirmed in a multivariate analysis (Table 3). Despite this, significant proportions of firms in the treatment group reported enhancements to their innovative capacity as a result of their Creative Credits project, though this result was not testable in comparison with the control group. This result was echoed in the qualitative analysis which revealed significant creativity transfer. In previous innovations many SMEs had 'muddled along' (W2S11) alone, either delivering creativity in-house without the required skills, outsourcing to cheap (unprofessional) alternatives, or outsourcing to a professional supplier without a proper budget (e.g. W2S11, W2S2). Creative Credits provided a learning opportunity for SMEs, and some SMEs received knowledge on the creative process: 'to see how it's all put together … how they do it' (W1S3). In some cases this was used to educate senior managers who preferred (low quality) in-house creative supply that 'sometimes you outsource' and bring 'fantastic agencies on board' and help them realise the added value by 'open[ing] ... people's eyes' (W2S11) to the need for changing behaviour. Creativity transfer was designed into some contracts (e.g. media training (W1S1) and website management (W2S2)), while other SMEs received reusable knowledge/skills as a by-product, for example on: writing marketing collateral (W1S3); web site

design (W1S4); non-traditional advertising (W1S5). However, this learning was sometimes painful (W1S11) and complex 'a complete can of worms' (W1S10) which hindered creativity transfer, limiting behavioural additionality. For example, the inaccessible/technical language which suppliers and customers use (W1S2) delayed benefits and disrupted/jeopardised the innovation.

Some creative companies aimed to encourage behavioural additionality by transferring knowledge and tools (e.g. W2C14, W2C17), however, this was burdensome (W2C12): 'small client syndrome, education time is high... a lot of hand-holding, it's the worst part of the job' (W2C18).

Perhaps unsurprisingly given these results on behavioural additionality, we also found no evidence of any significant network additionality, i.e. no significant differences between the treatment and control groups in the propensity to co-operate on future innovation (Table 4). Again this result was confirmed using multivariate models allowing for potential selection bias. In this respect our results are again similar to those in the Dutch innovation voucher scheme, where follow-up after 18 months suggested no evidence of persistent additionality effects either in the formation of new partnerships or the development of new products or processes (Cornet et al., 2007). One possible reason for the lack of longer-term effects suggested in relation to the Austrian and Swiss innovation voucher evaluations, is the small size of many voucher recipients and the difficulty which they have in establishing follow-up partnerships (Good and Tiefenthaler, 2011).

In interviews, findings on network additionality pointed to three considerations. First, for some SMEs their Creative Credits project had been based on a 'transactional' relationship with their creative service providers: 'a one-off hit where they got something for free' (W1C7). Indeed, fifteen of the nineteen SMEs interviewed at the time of Survey 4 referred to having already worked with other creative businesses prior to the Creative Credits scheme. For these firms, Creative Credits encouraged them to

work with a new creative partner, but generated little new organisational learning about effective partnering with creative businesses in general. Consequently, most SMEs ended their working relationship with their Creative Credits partner when their project had completed, consistent with their previous working relationships with creative businesses (W1S1, W1S10, W2S8, W2S6, W2S7, W2S12, W1S12). A second reason for the lack of sustained network additionality was dissatisfaction with their creative partner (W2S2, W1S7, W1S8, W1S11, W1S3). Sometimes this related to not building lasting personal relationships with the supplier. At other times, the dissatisfaction related to the attitude of their creative partner: 'it did feel that they were in a sense had bigger fish to fry than our project' (W2S4) but SMEs 'trusted in them to do it all really' but more SME involvement could have ensured it went 'in the direction I wanted it to go in' (W1S7). Creative suppliers verified these difficulties, also emphasising communication issues, different interpretations of project objectives, not receiving input data, client dithering, and differing priorities: 'We had to hold their hand all the way through it' (W2C12). The third reason for the lack of sustained networking additionality was the lack of brokerage or assistance with identifying/managing the creative partner. SMEs advised that additional networking could help by 'actually go[ing] and meet[ing] a few of these other companies … I would get heaps more out of the project'.

6. Discussion

The aim of the evaluation reported here is to illustrate the potential value of experimental approaches to industrial policy evaluation and inter alia to test the logic model of Creative Credits, permitting a causative as well as summative evaluation of the scheme and its impacts (Chen, 1990). The RCT+ experimental evaluation approach we develop has three main elements; randomised assignment of firms to the treatment and control groups, a longitudinal approach to data collection, and the use of a mixed-methods data collection strategy. In this section we discuss each of these elements and comment on their contribution to the evaluation. We also

make some comments on contingent factors which may have influenced evaluation outcomes, particularly the effects of the post-2008 recession.

The statistical advantages of randomised allocation of subjects to the treatment and control groups are potentially significant in ensuring internal validity (Burtless, 1995). In large samples randomisation should result in no significant differences between the characteristics of the treatment and control groups. We examined the effectiveness of randomisation by comparing the baseline characteristics – at the time of Survey 1 - of control and treatment firms in the longitudinal sample of SMEs. As anticipated, no significant differences were evident in the prior ownership status, exporting behaviour or prior innovation activity of the treatment and control firms. Firms in the treatment group were, however, significantly less likely to have engaged in prior R&D than firms in the control group and, as R&D is generally linked positively to innovation, we might anticipate ceteris paribus that future innovation might also be lower among the treatment group (biasing downwards estimates of output additionality). These systematic and significant differences between the characteristics of the treatment and control group illustrate the potential limitations of randomised allocation even where samples are above the critical threshold (c. 300 observations) highlighted by Bruhn and McKenzie (2009).

Another aspect of internal validity is the appropriateness of the timescale over which policy effects are measured. In our RCT+ approach, where we aim to capture both short-term impacts and longer-term outcomes, we adopt a longitudinal data collection strategy. Here, this proved important in revealing significant output additionality after six months, an effect which had lost its significance after twelve months. Measuring output additionality at either six or twelve months only would have provided an incomplete assessment of output additionality. Adopting a longitudinal data collection approach in industrial policy experiments therefore helps to avoid potentially misleading inference and assess any additionality-sustainability trade-offs in policy outcomes (Hewitt-Dundas and Roper, 2011).

Understanding fully why such trade-offs occurred, however, relies on qualitative research, the third element of the RCT+ methodology. In particular, although many SMEs in the treatment group highlighted positive aspects of working with their creative partner, our longitudinal case-studies highlighted issues which may have contributed to the lack of sustained output additionality e.g.: the transactional nature of the relationship with their creative partners; communication and co-ordination issues; and, in some cases, unsatisfying collaborations. Such data provide insights into the causal processes underlying additionality outcomes and potentially useful suggestions for subsequent implementation of any scheme beyond the initial policy experiment.

Beyond the internal validity and insights generated by the Creative Credits experiment, the other primary issue is external validity, i.e. the generalisability of the evaluation results to the wider population of firms (Garfinkel et al., 1990). To assess external validity in our experimental evaluation we conducted a baseline survey of the characteristics of non-applicants in the Manchester City Region (MCR). This suggested that members of the treatment and control groups were more focused on innovation than non-applicants to the Creative Credits scheme, and also were more likely to have previously worked with external partners and had higher internal skill levels than non-applicants (Bakhshi et al., 2011, Annex 2). As each of these factors is likely to influence scheme outcomes this clearly limits the external generalisability of our evaluation results to the general population of SMEs in the MCR. More generally, where external validity is important in industrial policy experiments, this suggests the importance of ensuring that the treatment and control groups are representative of the wider target population (Duflo et al., 2006). Even where this is achieved, however, short-term (and small-scale) experimental studies are unlikely to capture the macro or social effects highlighted by Garfinkel et al. (1990). One additional 'macro' effect not considered by Garfinkel et al. (1990) is the potential for industrial policy initiatives to

generate potential synergies between schemes[8]. In the case of the Creative Credits scheme, for example, a number of scheme applicants had already received support from another innovation voucher scheme operating in the region which focussed on developing SME-university linkages. Such synergies are likely to be positive in the medium-term but are unlikely to be evident during any short-term experimental evaluation.

One other element of external validity which is perhaps rather specific to the current policy experiment relates to the impact of recession on output, behavioural and network additionality. Our policy experiment took place between September 2009 and October 2010, a period when the UK economy was either contracting or experiencing only marginal growth. For example, one firm in the treatment group commented on their Creative Credits project that 'it was useful at the time, but because of the recession and all the rest of it, I don't believe we've really seen the benefit yet, but I suspect we will do'. Another SME could not fully implement the outcome of their project because it had not been able to secure bank funding, saying that 'We are working our guts out, I have to try and raise finance'. Such comments were relatively typical, emphasising the contingent nature of evaluation outcomes as suggested by the logic model (Figure 1), and the potential for the current experiment to under-estimate the potential additionality of Creative Credit type interventions in more tranquil economic conditions.

7. Conclusions

The top level objective of the policy experiment described here was to test the causative processes underlying the Creative Credits logic model in Figure 1. The RCT+ evaluation approach adopted combined the random allocation of scheme applicants to control and treatment group with a mixed methods and longitudinal data collection strategy. It proved possible to

[8] Heckman and Smith (1995) consider a related point in their discussion of substitution bias. This relates to a situation where schemes are direct substitutes, however, rather than complementary to each other.

maintain the internal validity of the evaluation experiment even given the longitudinal nature of the evaluation and significant attrition in responses. The combination of the mixed methods and longitudinal aspects of the RCT+ approach also provided valuable insights into the validity of the logic model. The longitudinal element of the evaluation method proved important in highlighting the time profile of additionality, while the mixed methods approach suggested some of the underlying causal factors. Overall, this suggests the feasibility of experimental methods for industrial policy evaluation, especially where quantitative analyses are complemented by structured qualitatitive data collection.

Two other aspects of our evaluation experiment suggest, however, that generalisations from such small-scale experiments need to be made with care. First, it is important to consider the external validity of the evaluation in terms of both the representativeness of scheme applicants and the contingent nature of scheme outcomes. Both potentially limit the generalisability of results from the experimental evaluation beyond the immediate spatial and temporal context. Second, our evaluation is subject to the micro to macro biases discussed in Garfinkel et al. (1990) and Heckman and Smith (1995). Importantly, however, neither of these caveats are specific to experimental evaluation studies; non-experimental evaluation approaches implemented on a similar geographic and temporal time scale are subject to the same critiques.

Experimental approaches to industrial policy evaluation remain rare despite the long tradition of experimental evaluation in social policy and development studies. Critically combining experimental and qualitative approaches is novel. Hopefully, the example of Creative Credits described here encourages others to combine experimental and qualitative approaches to industrial policy evaluation such as RCT+.

Table 1: Project Additionality - The likelihood of firms undertaking their innovation project

Dependent variable: Whether or not firms undertook their project
Number of observations 451
Adjusted R-squared 0.653

Variable	Coefficient	Std. Err.	t-statistic	Signif
Creative Credit	0.840	0.028	29.11	0.000***
Constant term	0.119	0.017	7.18	0.000***

Notes: Analysis is based on respondents to the initial baseline survey undertaken immediately after the allocation of Creative Credits. *** denotes significance at the 1 per cent level.

Table 2: Output additionality in terms of the probability of innovation: 6 months and 12 months after the completion of the Creative Credits projects

	N	Control % firms	Treatment % firms	t-statistic	Signif.
A. After 6 months					
Product or service innovation	145/105	55.9	72.4	2.740	0.007***
New to the market innovation	126/92	23.0	35.9	2.089	0.038**
Process innovation	142/105	47.2	63.8	2.618	0.009***
B. After 12 months					
Product or service innovation	154/113	63.0	70.8	1.345	0.180
New to the market innovation	135/97	32.6	40.2	1.192	0.235
Process innovation	153/111	51.0	47.7	0.517	0.606
	N	Control %	Treatment %	$\chi2(6)$	Signif.
C. Average Sales Growth					
After 6 months	146/107	6.4	7.5	11.5	0.075*
After 12 months	155/114	4.7	7.8	7.7	0.261

Notes: Table is based on the longitudinal sample. See Annex 1 for details. Differences in response numbers between questions and between 6 and 12 months reflect those respondents not answering particular questions and those selecting the ('Don't know") response. * denotes significance at the 10 per cent level; ** at 5 per cent and *** at the 1 per cent level. The $\chi2(6)$ test statistic is based on the difference in distribution of sales growth rates, not the average sales growth rates.

Table 3: Behavioural additionality - future innovation intentions after completion of Creative Credits projects

	N	Control			Treatment			$\chi^2(2)$	Signif.
		Not Likely %	Quite Likely %	Very Likely %	Not Likely %	Quite Likely %	Very Likely %		
A. After 6 months									
Goods or services	248	4.3	35.5	60.3	5.6	27.1	67.3	2.036	0.361
Processes	245	7.9	34.5	57.6	8.5	33.0	58.5	.074	0.964
Strategy	235	15.7	41.8	42.5	12.9	38.6	48.5	.912	0.634
New technologies	204	38.5	36.8	24.8	41.4	42.5	16.1	2.321	0.313
Organisation	216	39.5	28.7	31.8	44.8	32.2	23.0	1.984	0.371
Marketing	231	10.4	40.7	48.9	11.5	45.8	42.7	.864	0.649
B. After 12 months									
Goods or services	257	6.7	40.9	52.3	7.4	35.2	57.4	.876	0.645
Processes	260	9.3	42.4	48.3	11.9	36.7	51.4	1.059	0.589
Strategy	249	14.6	45.8	39.6	18.1	38.1	43.8	1.583	0.453
New technologies	206	47.6	29.8	22.6	45.1	32.9	22.0	.224	0.894
Organisation	221	49.3	36.6	14.2	47.1	32.2	20.7	1.676	0.433
Marketing	235	18.4	38.2	43.4	15.2	48.5	36.4	2.464	0.292

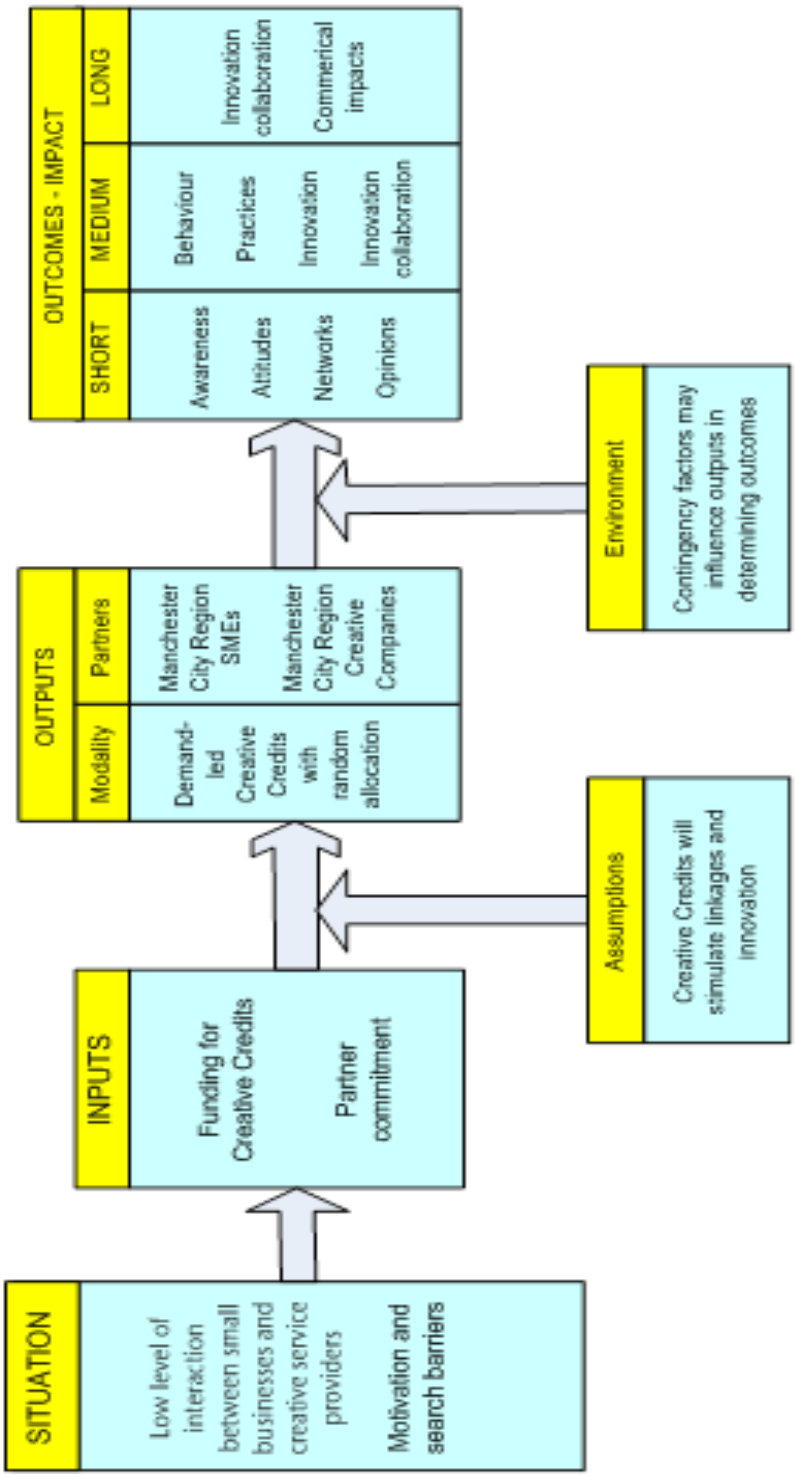
Notes: Table is based on the longitudinal sample. See Annex 1 for details. Differences in response numbers between different questions and between 6 and 12 months reflect those respondents not answering particular questions and those selecting the ("Don't know") response. * denotes significance at the 10 per cent level; ** at 5 per cent and *** at the 1 per cent level.

Table 4: Network Additionality – probability of innovation cooperation after completion of Creative Credits projects

	N	Control % firms	Treatment % firms	t-statistic	Signif.
A. After 6 months					
Other group companies	79/73	27.8	31.5	-0.490	0.625
Suppliers	81/74	58.0	52.7	0.662	0.509
Creative service suppliers	80/75	53.8	58.7	-0.613	0.540
Customers	81/74	58.0	55.4	0.327	0.744
Competitors	79/73	24.1	9.6	2.429	0.016
Higher Education Institutes	81/75	22.2	20.0	0.338	0.736
Public Laboratories	81/74	12.3	10.8	0.297	0.767
B. After 12 months					
Other group companies	93/75	33.3	37.3	-0.536	0.593
Suppliers	95/77	52.6	63.6	-1.458	0.147
Creative service suppliers	95/77	52.6	64.9	-1.637	0.104
Customers	95/77	65.3	71.4	-0.864	0.389
Competitors	96/77	24.0	32.5	-1.228	0.221
Higher Education Institutes	93/78	29.0	26.9	0.305	0.761
Public Laboratories	94/76	11.7	18.4	-1.204	0.231

Notes: Table is based on the longitudinal sample. See Annex 1 for details. Respondent numbers are given as "control group/treatment group". Differences in response numbers different questions and between 6 and 12 months reflect those respondents not answering particular questions and those selecting the ("Don't know") response. * denotes significance at the 10 per cent level; ** at 5 per cent and *** at the 1 per cent level.

Figure 1: Logic model for Creative Credits



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The Enterprise Research Centre is an independent research centre funded by the Economic and Social Research Council (ESRC); the Department for Business, Innovation & Skills (BIS); the Technology Strategy Board (TSB); and, through the British Bankers Association (BBA), by the Royal Bank of Scotland PLC; Bank of Scotland PLC; HSBC Bank PLC; Barclays Bank PLC and Lloyds TSB Bank PLC.