SERVICE LEARNING AND YOUTH POLITICAL PARTICIPATION: A MIXED-METHOD THESIS

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Author's declaration

I, Patrick Taylor, confirm that the work presented in this thesis is my own. Where information has been derived from other sources, I confirm that this has been indicated in the thesis. No real names are used in this thesis. All names used in Study 2 are pseudonyms.

Signature: Date: 26/02/2021

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Abstract

Young people participate in politics less than any other age group, and they might suffer materially as a result. Service learning is one policy that has received a substantial amount of public investment to attempt to solve this problem. However, the evidence to date on what effect service learning has, how this effect is produced, and how we might maximise it, is lacking. This thesis addresses these three questions with three studies. Study 1 uses a large quasi-experiment (N=5,486) with the UK's National Citizen Service (NCS) to test the effects of service learning on young people's political participation. It finds that NCS has a substantial positive effect. This is a new finding, and directly contradicts what some have argued in the literature. The study also estimates effects on a range of potential mediating mechanisms. These tests suggest that the observed increases in political participation do not come via a process of self-efficacy spillover. Study 2 uses interviews with 27 graduates of NCS to build a theory that does explain the effect. It finds that: i. there is substantial heterogeneity in the effects of service learning on political participation; ii. there are twelve, sometimes interdependent mechanisms that mediate these effects; and iii. there are up to sixteen moderating factors. Study 3 investigates how best to encourage participation post-service. It uses a large randomised controlled trial (N=227,372) to test the effects of three different email messages on NCS graduates' participation in political letter writing. A 'plain' invitation is pitted against two alternative messages that draw on the theories of self-efficacy and identity. It finds that the theory-informed messages perform no better than the plain invitation in encouraging participation. These are important contributions to the literature that also have crucial significance to policy makers and practitioners who want to increase youth political participation.

Impact statement

This thesis aims to benefit academia, and to create useful knowledge for policy and practice. For academia, it offers the first robust estimates of the causal effects of service learning on youth political participation. It also provides the first detailed theory to explain how these effects are produced. In doing so, it settles a dispute in the literature between those who have found (or predicted) positive, negative, and null effects. Research in this field can now move forwards, exploring these effects, the mechanisms that drive them and the factors that moderate them in more detail. Some suggestions for future research projects based on the findings from this thesis are presented in Chapter 7.

Having said this, the thesis has always been more motivated by practical and policy concerns. The National Citizen Service (NCS) has been operating at a large scale, with substantial public investment, for many years but, until now, no research has focused on its effects on political participation. This is despite the clear need to increase youth participation (described in Chapter 1), and the fact that this is a stated aim of NCS. To address this, a partnership was formed with the NCS Trust – the body responsible for delivering the programme on behalf of government. This partnership has ensured that the Trust has been involved in all stages of the research. The research questions were developed in consultation with key staff, including the CEO, the Head of Curriculum, the Director of Innovation, and the Head of Research and Evaluation. After agreeing the aims of the research, NCS Trust also agreed to fund it in full. This helped to formalise the partnership. A small group of key stakeholders from outside of the Trust were also consulted in the design phase, including the Head of Civil Society Research at the Cabinet Office (who was responsible for the evidence-base for NCS as a government policy), the CEO of the Association of Citizenship Teaching, who chairs the expert panel that advises the Department for Education on citizenship education policy, and the Data and Quality Steering Group for the UK youth social action sector. As well as co-developing the aims of the thesis with these stakeholders, the design and execution of the research has also been carried out in partnership with NCS Trust staff.

By taking this cooperative approach to designing and delivering the research, it is hoped that these stakeholders feel more invested in the findings and are,

therefore, more likely to act on them. It is also hoped that the project has had a capacity building effect, by involving non-researchers in an in-depth process of evidence-based policy making. After viva, the findings will be disseminated to policymakers and practitioners in two ways. First, a separate report that summarises the findings will be written for this audience. Second, a series of workshops will be held to present the results, and to support NCS Trust staff to interrogate them. These workshops will be used to co-develop a set of recommendations for the Trust.

Acknowledgements

This research is about encouraging young people to play an active and positive role in society and is a direct result of what my parents taught me. My teacher dad taught me to care about politics and inequality. My mum gave 30-odd years of her life to the NHS. She showed me the power of a life dedicated to helping others. This is where my passion for service and politics started.

Thanks to my three amazing supervisors. Lucy Barnes read everything that I wrote in great detail, always made an effort to understand what I was trying to say, and her comments were incredibly helpful as a result. Michael Sanders became my personal stats tutor. Before my PhD, I had no real background in the social sciences, research methods, or statistics. Without Michael's support on the quant side, I would've been done for. Peter John was the senior prof on the team. In one way, he lived up to this status. He taught me what political science is, and I took a lot of inspiration from his writings, both in terms of his ideas and his clarity of expression. In another way, he bucked the senior-male-prof stereotype by being sensitive to my struggles and constantly encouraging.

Thanks to the National Citizen Service (NCS) Trust. They paid for this research and gave the substantial practical support that it needed to succeed. A lot of NCS Trust staff pitched in. I'm especially grateful to Victoria Harkness, Tom Wedell, John Britton, Jo Opoku, Harry Domzalski, Lawrence Newcombe, Elena Laverty, Iva Stancheva, Kerry Viner, Michael Price, Claire Lancaster, Paul Adnitt, Naim Moukarzel and Michael Lynas.

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Finally, thanks to my wife, Hannah, and my daughter, June. Hannah let me quit my job and pursue this PhD, just after we had committed to our first mortgage. June came along half-way through and gave me something far more important and joyful to think about than my thesis.

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1 Introduction

This thesis is about the relationship between young peoples' political participation and a type of civic education called 'service learning'. This introduction begins by explaining why this is an interesting and important topic, both academically and for public policy. It then presents the research questions, the approach taken to answering these questions, and summarises the key findings.

1.1 Why should we worry about youth participation in politics?

> *"Elected officials pay as much attention to those who are not registered to vote as butchers do to the food preferences of vegetarians." (Former US Congressman Barney Frank)[1]*

Young people participate in formal political activities – voting, contacting politicians, petitioning and protest – less than any other age group (Ipsos MORI 2019; Cabinet Office 2016; Sturgis & Jennings 2020). While there is some evidence that electoral turnout amongst young people increased over the 2010, 2015 and 2017 general elections (Sturgis & Jennings 2020, p.3), 18-24-year-olds remained the lowest participating age group throughout this period and youth turnout in the latest UK general election seems to have been at its lowest for over a decade (Ipsos Mori 2019; Sturgis & Jennings 2020, p.3). Figures 1.1 and 1.2 demonstrate clearly the inequality in political participation by age group in the UK.

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[1] Quote taken from Holbein & Hillygus (2020), originally from Frank (2017).

Figure 1.1: Turnout at the 2019 UK general election by age
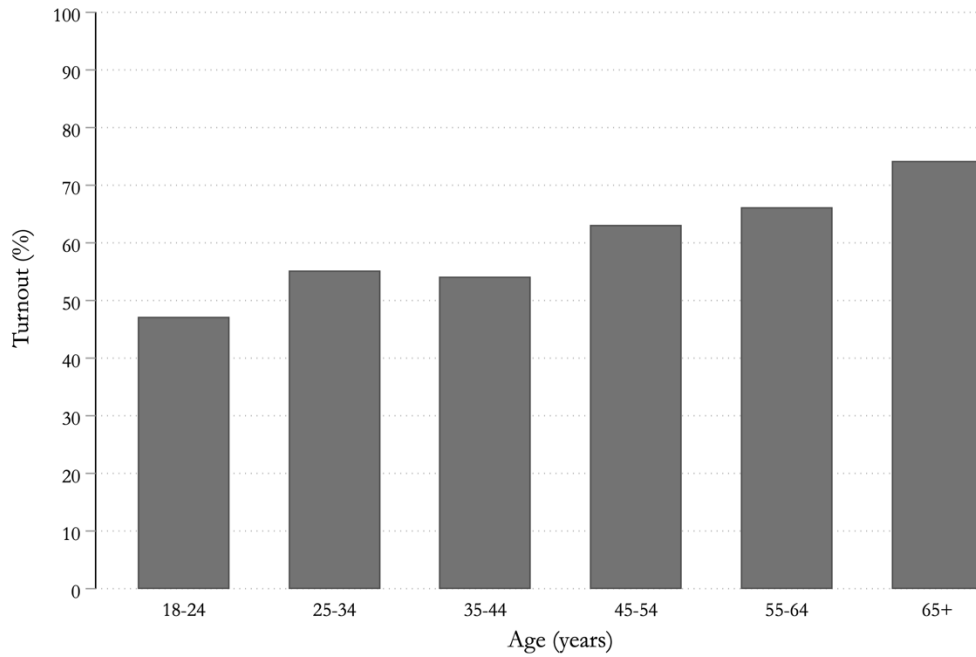
Figure 1.2: Non-electoral participation in the UK by age
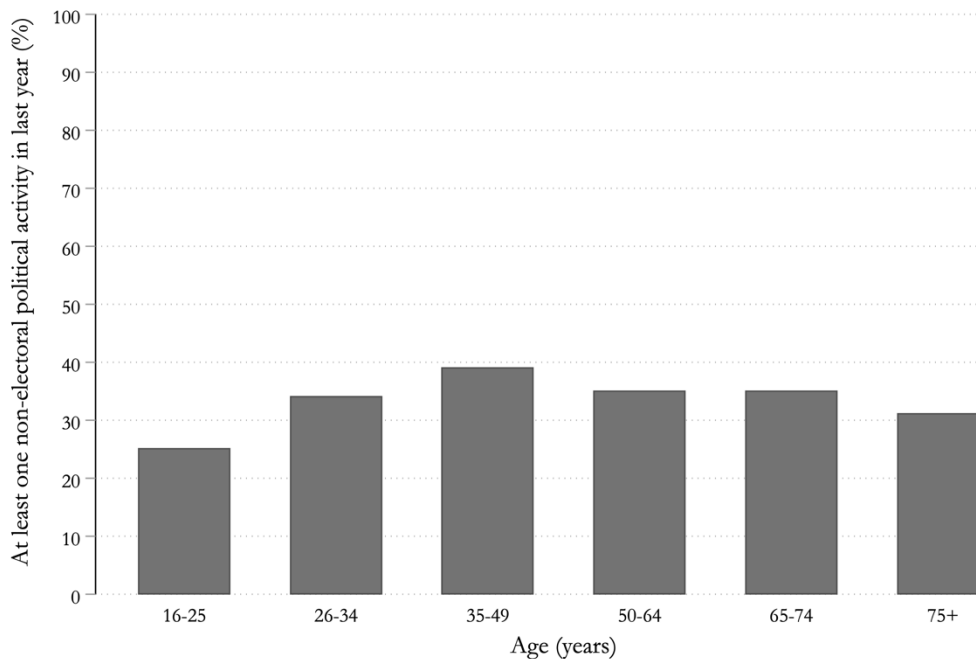
It has been suggested that this pattern of behaviour represents a generational shift in preferences away from formal political participation and towards modes

of participation such as non-political community service (Kahne et al. 2013, p.420; Dalton 2016, pp.5-6). This idea is supported by estimates in the UK that suggest that, when it comes to non-political civic participation, 16- to 25-year-olds are in fact the most active age group (Cabinet Office 2016). This shift in mode, and this inequality in political participation by age, matters for at least three reasons. First, who gets elected to government, and the policies that they put in place, seems to be strongly determined by who votes (Anzia 2013; Berry & Gersen 2011; Bertocchi et al. 2020; Birch et al. 2013; Fowler 2013; Lee et al. 2004; Madestam et al. 2013). For example, when substantial public spending cuts were introduced by the UK government in 2010, the average 16- to 24-year-old is estimated to have lost services and benefits valued at 28% of their household income. For all other age groups, the equivalent figure ranged between 10 and 16% (Birch et al. 2013, p.14). In another example, in the 2016 EU referendum, 75% of 18- to 24-year-old voters asked for Britain to remain in the EU, but their turnout was again eclipsed by older voters who voted to leave (Ipsos Mori, 2016). As a result of their lower rate of participation in politics, young people seem to suffer materially, and have fewer of their preferences expressed in government policy.

Second, political participation is a habit. People who participate early on in their lives are more likely to participate when they are older (Aldrich, Montgomery, & Wood, 2011; Collins, Kumar, & Bendor, 2009; Denny & Doyle, 2009; Gerber, Green, & Shachar, 2003; Green & Shachar, 2000; Plutzer, 2002). If we can find ways to encourage participation in childhood and youth, we might therefore be able to increase general levels of participation across the population. Figure 1.2 highlights a particular need here, with only a minority of people – across all age groups – currently participating politically in ways other than voting. Third, there are other important inequalities in participation – based on ethnicity and wealth, for example – that begin in youth. Understanding and addressing low participation in childhood might therefore be the best way of addressing these other inequalities (Holbein & Hillygus 2020, pp.6-7). If we can encourage a habit in youth, then maybe we can encourage a habit in these other groups. Habits are more malleable in youth, and marginalised groups are perhaps easier to access at this age (through the state education system, for example). In summary, addressing the low levels of youth participation in politics would increase the

fairness of political policies, improve participation across age groups, and potentially help us to solve other deep-seated inequalities.

1.2 What is service learning?

Service learning is a particular kind of citizenship education that supports young people to carry out voluntary service that assists 'individuals, families, and communities in need' (Hunter & Brisbin 2000, p.63). When delivered in formal education – schools, colleges, universities – this experiential core is supplemented by relevant classroom-based learning that covers topics such as how democracy works, and contemporary policy issues. Recent years, however, have seen a growing body of interventions (and an associated body of research) that fit some of this description but take place outside of formal education. These informal models of service learning are: usually funded by national governments; delivered outside of formal education, often by non-governmental organisations (NGOs); focussed on experiential learning (with limited or no knowledge-based curriculum); and are supported by non-civic personal development activities and guided reflection (Reinders & Youniss 2006, p.4; Pye & Michelmore 2017, p.24).

The specific programme of service learning that is studied in this research is the National Citizen Service (NCS); an example of an informal model of service learning. NCS is a voluntary programme of youth development and civic participation operating across England. Young people take part in the programme in the summer or autumn following their final year of secondary school, so the majority are 16 years old. Participants are placed into cohorts of approximately 60 peers, broken down into teams of roughly 12 young people, each supported by a non-professional youth worker.[2] The programme has three phases. Phase 1 is a one-week residential curriculum at an outdoor centre, aiming to build participants' confidence, skills and sense of team. In Phase 2, participants are based in a residential location near their home for a week, where they live 'independently' with their team (managing a food budget and cooking together), take part in skills-building workshops and visit local community organisations, such as day centres for senior citizens. During this week,

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[2] These roles are often filled by undergraduate students on their summer holiday.

participants are encouraged to think about issues in their local area that they could address though their own civic participation. In Phase 3, which is either one or two weeks long, participants develop and execute a civic participation project. Typical projects include volunteering to paint a local community centre, fundraising for a local charity or cleaning up a local park.

1.3 Why is service learning important in the context of youth participation in politics?

1.3.1 Policy importance

Service learning matters in this context for two reasons. First, a substantial amount of public resources are invested in it as a solution to the problem of low youth participation in politics. This is the case in England where, in 2015, the UK government committed over £1bn to NCS over 5 years, with an estimated cost per participant of £1,863 (National Audit Office, 2017, p.4). Approximately 600,000 young people have completed the programme to date,[3] and by 2019 the programme had a reached a scale such that 7% of 16-17-year-olds in England took part that year.[4] NCS graduates therefore represent a substantial proportion of the English population in their age group, and the aim is to continue to expand participation (NCS Trust 2019). Similar policies have been proliferating across Europe, through programmes led by the European Union and by individual states (Zimenkova 2013), and the idea is well-established in the US (under the banner of 'AmeriCorps') and elsewhere. This has been a controversial policy in England; particularly as it has coincided with a decline in government support for formal citizenship education in schools (Kerr 2014, pp.45-46). However, the critics of NCS have so far failed to answer (or even ask) the most important question in this regard; what effect does it actually have on political participation?

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[3] Figure calculated by adding published participation figures up to the beginning of 2019 (NCS Trust 2019, p.6) to NCS Trust's internal participation number for summer 2019 (98,331, unpublished at the time of writing).
[4] Figure calculated using NCS's internal participation number for 2019 and the Office for National Statistics (ONS) population estimate for 16- and 17-year-olds in the same year (ONS 2020).

1.3.2 Theoretical importance

The second reason that service learning matters in this context is theoretical. While there is a literature on service learning and youth political participation, there is room for development in both the theory and the identification of causal effects. Combining the specific research on service learning with the wider evidence on voluntary associations, section 2.2 of the literature review suggests that three types of mechanism may link service learning to political participation: i. the development of key skills; ii. an increased motivation to participate via an increased sense of self-efficacy and/or social responsibility; and/or iii. an access to new networks. But this theory is largely untested, and the details on the activities that might trigger these mechanisms, and the factors that moderate them are very limited (Ayala 2000, p.100; Dartington Service Design Lab 2019). Beyond this theorising, researchers have also attempted to identify the causal effect of service learning on political participation but – as section 2.3 of the literature review demonstrates – they have so far come up short. There is no strong evidence to say whether service learning has a positive, negative or null effect on young people's political participation. Studies that have tried to identify causal effects have suffered from weak identification strategies and small samples (Burth 2016). There are, therefore, large gaps in our knowledge on this topic that have important practical implications.

1.4 Research questions and thesis outline

This thesis aims to improve our understanding of the relationship between service learning and political participation by answering three questions.

- **RQ1:** What is the effect of service learning on young people's political participation?
- **RQ2:** If there is an effect, how is it produced?
- **RQ3:** What is the most effective way to encourage political participation post-service?

These questions are addressed through three studies. Study 1 uses a quasi-experimental (matched difference-in-differences) design, with a large sample (N=5,486), to compare the political behaviour of young people who take part in NCS with a group of young people who do not. It finds that NCS has substantial

positive effect on participants overall political participation (3.1pp) and even larger effects on some forms of non-electoral participation (5.4pp on petition signing and 4.9pp on protest attendance). This is an important contribution to the literature. Until now, there has been no robust estimate of the effect of service learning on political participation, and there has even been speculation that interventions like NCS could, on average, have a negative effect. The results of this study suggest not only that service learning can have a positive effect on young people's democratic engagement, but also that this effect may be quite large.

The literature prior to this study suggested that, if a positive effect is realised by programmes like this, then self-efficacy might play a key mediating role. The idea being that an experience of service-based civic participation can lead to an increased sense of service-based or general civic self-efficacy, which can lead to gains in political self-efficacy, which in turn can lead to more political participation (a self-efficacy 'spillover effect'). To test this theory, Study 1 also estimates the effects of NCS on service-based participation, general civic self-efficacy, and political self-efficacy. Positive effects are identified for the first two of these outcomes (3.7pp and 8pp respectively), but the programme has almost no effect on political self-efficacy. This suggests that the self-efficacy spillover story is not quite right. As a study of mechanisms, however, this first study is limited by the small number of outcomes measured, the reductive quantitative indicators used to measure these outcomes, and an inability to say whether coinciding outcomes have a causal connection.

Study 2 builds on the findings of Study 1 by attempting to identify in detail the causal mechanisms that link a service learning experience to an individual's future political participation, as well as the factors that moderate this effect. It does so through in-depth interviews with 27 graduates of NCS. These interviews draw on life and oral history approaches; tracing the interviewees' political motivations and behaviours over the course of their lives, seeking to identify the main influencing factors, and the role that NCS did or did not play within this wider context. The findings from Study 2 make two important general contributions: i. they reveal substantial heterogeneity in the effects of service learning on political participation; and ii. they add important detail to our understanding of *how* these effects are produced. Two substantive results within

these general findings may be particularly surprising to some. First, an increase in *social* self-efficacy seems to be the most powerful mediating mechanism, acting directly on political participation. Second, gains in service-based self-efficacy can lead to losses in political self-efficacy and, therefore, a reduction in political participation for some individuals. Preventing such losses – and encouraging a positive spillover – is difficult and requires new activities and conditions to be present in service learning. These contributions to the literature also have crucial significance to policy makers and practitioners who want to increase youth political participation through programmes of voluntary service.

Together, Study 1 and Study 2 aim to develop a more complete picture of the relationship between service learning and political participation; with the former identifying the causal effect (RQ1) and the latter helping us to understand how this effect is produced (RQ2). Study 3 attempts to go one step further and to provide insights into how to maximise the positive effects observed in Study 1 (RQ3). There is a large amount of evidence to suggest that 'Get Out the Vote' (GOTV) campaigns can increase voter turnout, and to tell us what form and content a message should take to be most effective in this context (Gerber & Green 2000; Gerber et al. 2008; Middleton & Green 2008; Nickerson 2006; Nickerson 2008). There is less evidence, however, on the effects of such campaigns on non-electoral political participation, and none at all when the target population is young people who have participated in service learning. Programmes such as NCS are uniquely placed to encourage democratic participation post-service, but there is no direct evidence to suggest how this could be done most effectively. This study addresses this final gap, using a large (N=227,372) three-arm randomised controlled trial to test the effects of three different email messages on young people's participation in a political letter writing competition. The first of these messages relies on the idea of a self-efficacy spillover from the domain of service to politics, the second aims to draw on participants' sense of identity, and the third is a plain encouragement message that acts as a control. The results of this experiment show that the two theory-informed messages perform no better than the plain invitation in encouraging participants to write a political letter. There is also some evidence to suggest that the plain invitation is slightly more effective in sparking initial interest in participation. These findings are contrary to the hypotheses. They provide further support for the idea – suggested by Study 1 and Study 2 – that

encouraging a spillover from a sense of service-based or general civic self-efficacy to a sense of political self-efficacy is very hard to do (among this population at least). They also suggest that young people do not incorporate NCS significantly into their identities, and that email does not appear to be an effective tool for mobilising political participation among this group of young people.

Overall, the combined findings from the three studies suggest that service learning can have a substantial positive effect on young people's political participation. How it achieves this effect is perhaps surprising, and enhancing the effect through post-programme communications is not easy.

2 Literature review

2.1 Introduction

This review aims to answer three questions about the existing literature: i. 'How might service learning might influence youth political participation (the theory)?'; ii. 'What do we know about the effect of service learning on youth political participation?'; and iii. 'What do we know about the most effective way to encourage political participation post-service?'. For the purposes of this research, political participation is defined as, 'activity that is intended to or has the consequence of affecting, either directly or indirectly, government action' (Verba et al. 1995, p.9). Examples of political participation under this definition include voting, signing a petition, contacting a public official, protesting, joining a political party, or taking part in a public consultation. Of course, political participation is not always defined in these terms. Political actions are often considered effective if they achieve other aims, such as influencing non-political decision-makers, building a movement ('intragroup concerns') and providing citizens with an opportunity to express their values ('individual concerns') (Hornsey et al. 2006, p.1708). However, the introduction to this thesis has argued that it is the more formal attempts to influence political decision-makers that are particularly underused by young people, and that there are important negative consequences that result from this. Verba et al.'s definition is therefore the one adopted here. Non-political or 'service-based' participation in this context is voluntary activity that benefits the wider citizenry directly, rather than through the influencing of public officials. Examples of this form of participation include volunteering at a care home, taking part in a litter pick, or fundraising for a charitable cause.

On the question of how service learning might have an effect on political participation, no comprehensive theory is identified in the literature. However, the little that has been said on the topic, combined with the wider research on voluntary associations and political participation suggests that, if service learning does have a positive effect on political participation, it might do so via one or more of three mechanisms: i. by developing key skills; ii. by increasing the motivation to participate via an increased sense of self-efficacy and/or social responsibility; and/or iii. by giving access to new networks. The detail on the

activities that trigger these mechanisms, and the factors that moderate them are even more limited. Study 2 in this thesis aims to address these gaps in the theory.

On the causal effects of service learning on political participation, the review presents a three-way disagreement in the literature, with arguments for positive, negative and null effects. A number of large, nationally representative surveys – covering the US and many European countries – have found positive associations between community service in youth, and political participation in young adulthood. This is promising evidence in support of positive effects, but is not focussed specifically on service learning, the identification strategies do not allow for causal interpretations, and one large survey study – very similar in design to those reporting positive correlations – finds no correlation. Five intervention studies that focus on specific programmes of service learning, and attempt to identify causal effects, are also reviewed. Three of these report positive effects and two report negative effects (though the latter are not significant at conventional levels). None of these five studies are convincing, however, because they suffer from a range of design flaws that are discussed in some detail below. This part of the review, therefore, suggests that we cannot say with much confidence whether service learning has a positive or null effect on political participation. The idea that it has a negative effect has the least support. This disagreement between researchers, and the lack of high-quality intervention studies in this field, provides the motivation for Study 1 in this thesis.

On the final question of how best to encourage political participation post-service, again, very little is known. The GOTV literature is well-developed, but focuses only on voting, has not been tested on the population of interest for this thesis, and has produced findings that do not seem transferable to it (as they focus on modes of contact that are often not viable for youth civic engagement). However, combining insights from the review on mechanisms, and the wider literature on social mobilisation suggests that two avenues that might be fruitful, when aiming to encourage political participation post-service. The first is to appeal to the sense of enhanced civic self-efficacy that young people may feel after their service learning experience. The second is to use identity as a lever, focusing in particular on participants' sense of youth and of being active citizens.

These two different approaches to encouraging political participation post-service are tested in Study 3.

2.2 How might service learning influence youth political participation?

No detailed or comprehensive theory of how service learning might influence political participation has been identified in the literature. However, Verba et al.'s theory of 'Civic Voluntarism' is the most complete and influential explanation of how participation in non-political civic institutions might influence political participation. This theory argues that three elements need to be in place for political participation to occur: 'capacity', 'motivation', and 'networks of recruitment' (Verba et al. 1995, p.3). The first two conditions are said to be necessary for participation. The third – being asked to participate – is, according to the theory, often an important catalyst, but not always necessary (1995, p.270). According to Civic Voluntarism, non-political institutions – such as the family, school, workplace and church – play a key role in the formation of political skills and motivations, and often act as sites of recruitment for political activity (1995, pp.271-273). So, participation in non-political voluntary activity can, according to this theory support all three conditions for political participation and there is some evidence of correlations that support this claim (Verba et al. 1995; Wilson 2000). This theory was not developed as an explanation of service learning. It aims to explain the effects of a wider group of activities – non-political civic participation – of which service learning is a member. But the theory does highlight participation in voluntary youth activities in particular as an important predictor of future political participation (1995, p.425) and it provides a helpful framework for thinking about potential mechanisms in more detail. According to this broad framework then, service learning will influence political participation to the extent that it develops participants' political capabilities, motivations and networks of recruitment.

The **capabilities** required for political participation differ depending on the type of participation. Some political activities – such as voting, signing a petition, and attending a protest – are relatively easy to do and, arguably, do not require any special capabilities. Other activities – such as attempting to influence a politician as an individual (for example by writing her/him a letter) or playing an active

role in organising petitions or protests – do require particular skills however, and it is plausible that some of these skills could be developed through a service learning experience. The skills that are likely to be important for these latter activities according to Civic Voluntarism are: participating in decision-making meetings, planning decision-making meetings, letter writing and verbal communication (particularly making speeches and presentations) (Verba et al. 1995, pp.304-313). If an increase in these skills does increase an individual's level of political participation, it is not clear from the literature whether this would be via an increase in motivation, or as a direct result of the increase in capability.

The relationship between service learning and an individual's **motivation** to participate politically is probably more complex. The sense of self-efficacy developed by participants during service learning seems to be an important motivating factor. There is an extensive literature demonstrating a correlation between political self-efficacy and political participation (r = .20 to .50) (Almond & Verba 1989; Converse 1972; Craig 1979; Craig et al. 1990; Karp & Banducci 2008; Niemi et al. 1991; Pateman 1970; Pollock III 1983; Vecchione & Caprara 2009; Verba et al. 1995). Some also argue that: i. service learning can increase non-political civic self-efficacy; and ii. this can lead to an increase in political self-efficacy (Condon & Holleque 2013, p.168; Reinders & Youniss 2006). If this argument holds, service learning should increase a participant's likelihood of political participation via a three-step process. In step 1, participation in service learning increases non-political civic self-efficacy. In step 2, this leads to an increase in political self-efficacy. And in step 3, this leads to an increase in political participation.[5] However, there are some who doubt this 'simple political spillover thesis' (Ayala 2000, p.101), that self-efficacy developed in non-political domains will lead to increased self-efficacy in the political domain. Service learning experiences often promote non-political civic participation as particularly effective – by its very nature – because activities such as volunteering with the elderly or cleaning up a park bring the individual into direct contact with an issue, and have immediate and observable positive outcomes (Walker

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[5] There is also some evidence to suggest that the relationship between political self-efficacy and political participation is reciprocal (Finkel 1985; Quintelier & Hooghe 2012; Valentino et al. 2009) and this idea is supported by Bandura's general theory of self-efficacy, which suggests that giving an individual a practical experience – an 'enactive mastery experience' – is the most powerful way of intervening to increase her/his sense of efficacy (Bandura 1997, p.79).

2000; Reinders & Youniss 2006; Youniss & Yates 1997). Political participation compares unfavourably in this light (Hornsey et al. 2006, p.1702), as it involves influencing the behaviour of others (politicians) to achieve a change. Service learning may not therefore enhance political self-efficacy unless this issue is overcome.

The extent to which an increase in service-based self-efficacy, leads to an increase in political self-efficacy may therefore depend on how similar an individual considers the two domains to be. Such a consideration is part of what Bandura calls the individual's wider 'cognitive appraisal' of her experience (1997). Bandura's theory suggests that, for an experience to be effective in this context, this cognitive appraisal should also have three more important characteristics. The experience should: i. be perceived to be successful; ii. be congruent with basic existing self-beliefs (or accompanied by compelling feedback to contradict these beliefs); and iii. be perceived as difficult enough for it to be a 'new' achievement (Bandura 1997, pp.82-83). Some studies on service learning have also suggested that participants' feeling of involvement in 'selecting and defining' their service could also influence their resultant self-efficacy (Galston 2001, p.230). A final potential mechanism in this category that is often posited, is that service learning increases participants' general sense of prosocial responsibility which, according to the argument, has a direct effect on her motivation to participate politically (Metz et al. 2003; Penner 2002; Piliavin et al. 2002; Reinders & Youniss; Wilson 2000).

Perhaps due to these complexities in the potential mechanisms, high quality guided reflection is considered by some to be a pre-requisite for service learning to have a positive effect on political participation (Billig 2000; Melchior et al 1999; Morgan & Streb 2002, p.166; Walker 2000). As well as encouraging the cognitive appraisal described above, some suggest that this process of reflection should encourage participants to reflect on the systemic causes of the issues being addressed through voluntary service, and the role of government in addressing these issues (Walker 2000, pp.646-647). The third component of the theory of Civic Voluntarism is more self-evident in how it might apply in this context. If service learning increases an individual's **networks of recruitment** in the political domain, it will increase the access that she has to participation opportunities. This may in turn increase the likelihood that she participates.

Very little is said in the literature about the factors that might moderate these effects. Only one of the intervention studies cited above tests for differential effects by subgroups (Metz & Youniss 2005). The results of this test suggest that only participants with a strong inclination to participate civically prior to service learning experience positive effects on their political participation in later life. However, this seems to be an isolated finding in the literature, and the methodological issues discussed in section 2.3, below, suggest that we should be cautious about taking too much from this particular study.

Finally, the literature lacks detailed descriptions of the specific activities in service learning that might trigger the hypothesised mechanisms described, and often conflates experiences that are quite different and should be expected to have different effects (Ayala 200, p.100). Implicitly however, there seem to be three types of activity within a service learning experience that could lead to the mechanisms in question: i. non-civic personal development activities that focus on skills relevant to civic participation; ii. non-political civic participation activities; and iii. reflection activities that aim to support a positive cognitive appraisal of (i) and (ii), and that encourage participants to reflect on the systemic causes of the issues being addressed through voluntary service as well as the role of government in addressing these issues.

In summary, no detailed and comprehensive theory on the causal mechanisms that might connect service learning to political participation has been identified in the literature. However, the little that has been said on the topic, combined with the wider research on voluntary associations and political participation suggests that, if service learning has a positive effect on political participation, it might do so via one or more of three types of mechanism: i. by developing key skills; ii. by increasing the motivation to participate via an increased sense of self-efficacy and/or social responsibility; and/or iii. by giving access to new networks. The details on the activities that trigger theses mechanisms, and the factors that moderate them are even more limited. Study 2 in this thesis aims to address these gaps in the theory.

2.3 What do we know about the effect of service learning on youth political participation?

There is disagreement in the literature on the effect of service learning on political participation. Some argue for a positive effect. In a review of the wider evidence on volunteering, Wilson highlights a number of studies that show that non-political civic participation is a strong predictor of political participation (2000, p.231). Verba et al. highlight participation in voluntary youth activities in particular as a significant predictor of future political participation (1995, p.425). Others have subsequently found similar results. In their analysis of nationally representative, longitudinal data in the US, Callahan et al. (2008) find that young people who participate in voluntary community service during high school are 48% more likely to vote as young adults, even when controlling for a wide range of individual and school-level characteristics (p.22). Van Der Meer and Van Ingen provide evidence from a very large dataset (the European Social Survey) that this basic result also holds across a wide range of European countries. Hart et al. (2007) also confirm that the finding is consistent whether community service in high-school is purely voluntary or is prescribed by the school. While these results are promising, they relate to a very broadly defined set of activities, and leave open the question of whether voluntary service experiences *cause* young people to become more politically engaged, or whether they are simply done by the type of person who also participates in politics. In other words, are programmes of voluntary service 'pools or schools of democracy' (Van Ingen & Van Der Meer 2016)? At the very least, it seems likely that the size of the effects reported by these studies will be overstating the contribution of voluntary service to political participation. Even the most comprehensive models in survey studies such as these will likely be masking important differences in personal characteristics and other experiences of political socialisation that will be correlated with both participation in service in youth and participation in politics in later life (MacFarland & Thomas 2006, p.411).

A different type of research design – that focusses on a specific intervention and has a stronger identification strategy – is needed to get closer to the truth. Five quantitative intervention studies that attempt to isolate the causal effects of service learning on political participation have been identified in the literature. The designs and results of these studies are summarised in Table 2.1. Of these

intervention studies, three report positive effects of service learning on political participation that are statistically significant at the 95% confidence level (Morgan & Streb 2002; Reinders & Youniss 2006; Metz & Youniss 2005). The types of political participation covered by these studies are intention to contact public or elected officials, intention to vote, intention to work on a political campaign, and intention to boycott. Effect sizes range from 0.18 to 0.31 points on 5-point Likert scales, where respondents were asked to report the likelihood of taking part in the activity in the future. However, while they focus on more specific interventions than the broader studies cited above, these studies still have a range of issues in their design. First, all three studies rely on self-reported intentions to participate in the future; a measure that is known to over-estimate actual behaviour (Achen & Blais 2015). Second, they all use small convenience samples (N=390 to 620), which limits their external validity. And third, the studies conducted by Morgan and Streb (2002) and Reinders and Youniss (2006) do not include a comparison group of individuals who do not take part in service learning. Morgan and Streb use a pre/post survey design with a group of service learners, so have no way of estimating confounding factors. Reinders and Youniss compare two different types of service learning; one that involves direct contact with beneficiaries in 'obvious states of need' (p.5), and one that does not. They are, therefore, answering a slightly different question to the one at hand.

Metz and Youniss (2005) do have a comparison group of non-participants, and they also have a panel survey that starts prior to service learning. They can, therefore, control for pre-intervention scores on their outcome variable (intention to vote), and a range of background characteristics. However, the distribution of these covariates is substantially different across the intervention and comparison groups, and they use simple regression modelling to estimate effects. The results are therefore likely to be sensitive to minor changes in the specification (Imbens 2015, p.373). Using matching to define the sample would have helped to overcome this. Also, the results from Metz and Youniss are not as clear cut as they first seem. In this study, the authors do not find a positive effect on average. Instead, they find that the intervention being evaluated – an extra-curricular programme of voluntary service in a group of US high schools – has positive effects for those who were less inclined to participate civically prior to service learning, but no effect on those were already interested in this

type of activity. From this body of research that argues for positive effects, we can conclude that service learning is positively correlated with intended political participation in some settings for some individuals, but there is no strong evidence that the observed relationships are causal, and it is not clear that these effects would generalise beyond the small populations studied.

Contrary to the proponents, some researchers argue that service learning can have a negative effect on political participation (a 'depoliticising' effect), and there is evidence from two quasi-experimental studies to support this view. First, in a matched comparison group study in US high-schools, Newmann and Rutter (1983) report small negative effects on students' intention to participate politically in the future and on their sense of political self-efficacy. However, the comparison group in this study was not robustly selected[6] and the results were not statistically significant at conventional levels (p = 0.241 and 0.459 respectively). Second, a study of service learning in a US community college used a difference-in-differences design and identified marginally significant (p < 0.1) negative effects on attendance at political rallies and volunteering for a political candidate (Smith 2006). This study had the advantage of using self-reported past behaviour (as opposed to intentions) but had a very small sample (N=43) and the result was again not significant at the 95% confidence level. On a different measure of political participation – that combined registration and voting behaviour at the last election – the same study found no effect. So, while on the face of it the designs used in these studies that present negative effects might seem more sophisticated than those that present positive effects, they also have issues with their identification strategies, and none of their results are significant at conventional levels.

A final group of researchers argue that service learning has no effect on political participation either way. A two-wave panel survey carried out by Kahne et al. (2013) aimed to compare the effects of service learning to those of an alternative form of civic education (open classroom discussion of 'societal issues'). When controlling for baseline scores on the outcomes of interest, ethnicity, gender,

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[6] Comparison classes were selected using the qualitative judgements of teachers in participating schools and different procedures were followed in each school. This resulted in a sample that was substantially imbalanced on a range of important characteristics such as gender and participation in similar activities.

baseline academic attainment, socio-economic status, parental political engagement and political ideology, it was found that service learning had no significant effect (at the 95% confidence level) on participants' intention to vote (p.427). The sample for this study was relatively large (N=1,700) and the analysis is more sophisticated than other studies in this field, using structural equation modelling in the primary analysis and propensity score matching as a robustness check (whose results are consistent with those from the primary analysis). However, this study does not focus on a particular intervention, and participation in service learning is self-reported based a on broad, single question. Like Reinders and Youniss' study (2006) the research question is also slightly different to the main question at hand, as the comparison group all take part in a specific alternative type of civic education (rather than 'business as usual' for young people in the general population).

This review of the evidence suggests that service learning could have a positive or null effect on political participation, and it is hard to say which is more likely. The idea that it has a negative effect has the least support in the literature. Despite this fact, some researchers in the UK, who are focussed on youth political participation and government policy, have criticised NCS on exactly these grounds, arguing that it is a vehicle for a service-based notion of citizenship (encapsulated by the Conservative government's 'Big Society' agenda), and will therefore discourage political participation (Mycock & Tonge 2011, pp.63-64; Bacon et al. 2013; Bulley & Sokhi-Bulley 2014). However, these researchers provide no evidence of a negative causal effect from their own work, and this review has shown that the wider literature does not support this idea.

There are two probable causes of this uncertainty and disagreement in literature. Firstly, there is a lack of strongly designed and executed intervention studies in this field. Secondly, there is substantial variation in – or just an absence of detailed description of – what constitutes a service learning experience in each study (as well as a lack of precise theory), so trying to make general claims about the effectiveness of service learning from this evidence is difficult (Ayala 2000, p.100). Whatever the cause of the uncertainty and disagreement, it is clear that the literature does not provide a satisfactory answer to the question, 'What is the effect of service learning on young people's political participation?'. Study 1 attempts to address this gap in the literature.

Table 2.1: Summary of studies on the effect of service learning on political participation

Study and intervention	Study design and analysis	+ / - / 0 effect(s)?	Results
Morgan & Streb 2002. School-based service learning, embedded in a range of classes: government, history, English, science and home economics. Service included tutoring and volunteering at a shelter for homeless people.	Pre/post survey, no comparison group. N=390 middle and high school students, from 27 different classrooms, 11 schools, 5 states (before 43% attrition). Outcome measures: Self-reported behavioural intentions.	+	Point changes on 5-point Likert scale: - Intention to contact public or elected official in next 5 years: 0.22, $p < 0.01$ - Intention to always vote in elections when older: 0.31, $p < 0.001$
Reinders & Youniss 2006. School-based service learning, embedded in religious education. Service included tutoring, coaching, raising money, doing office work, working at soup kitchens, doing environmental clean-ups, and volunteering in care homes for the elderly.	Three-wave panel survey, comparing the effects of two different types of service. Propensity score matching as robustness check. N=620 high school students from 2 schools (before 3% attrition). Outcome measures: Self-reported behavioural intentions.	+	Point estimates not provided, but 'positive' effects reported on: - Intention to vote - Intention to work on political campaign - Intention to boycott a product or service
Smith 2006. University-based service learning, embedded in a government class. Service included language tutoring, and computing tutoring.	Quasi-experiment (difference-in-differences). N=43 university students from one US community college. Outcome measures: Self-reported behavioural intentions.	0	Normalised scores from 0 to 1: Combined registration to vote and voting in last election: -0.02, $p > 0.1$

			• Combined past attendance at a political rally and volunteering for a political candidate/party: -0.17, p < 0.1
Newmann & Rutter 1983. School-based service learning, embedded in Community Service classes. Service included tutoring, volunteering in care homes for the elderly, and volunteering at a shelter for homeless people.	Quasi-experiment (difference-in-differences). N=approx. 360 (actual number not reported) high-school students from 8 schools. Outcome measures: Self-reported behavioural intentions.	0	Mean point change on 5-point Likert scale: • Combined intention to vote, contribute money to a political campaign, volunteer for a political campaign, sign petitions, contact public officials, and participation in a march or demonstration: -0.08, p = 0.241
Metz & Youniss 2005. School-based service learning outside of formal curriculum. 40 hours of service. Service included tutoring, coaching, assisting at shelters or nursing homes, organising food or clothing drives, and assisting 'value-centred service organisations' or churches.	Three-wave panel survey. N(total)=243 US high-school students. Analysis conducted on subgroups (those who were more inclined to serve and those who were less inclined to serve). • N(more-inclined-to-serve)=114 • N(less-inclined-to-serve)=129 Outcome measures: Self-reported behavioural intention.	0/+	Point changes on 5-point Likert scale: • Intention to vote (amongst more-inclined-to-serve): null (coefficient and p-value not reported) • Intention to vote (amongst less-inclined-to-serve): 0.18, p < 0.05

Notes: Result classified as null ('0') in the third column if p > 0.05, but effect sizes are still reported in the final column in these cases.

2.4 What do we know about the most effective way to encourage political participation post-service?

The first two sections of this literature review have summarised what we know, outside of this thesis, about why we might expect service learning to have a positive effect on political participation (the theory), and what evidence there is of a causal effect. This present section aims to lay out what we know about how to encourage more political participation post-service. This is a difficult question to answer, because no research has been identified that attempts to answer it directly. There is a large amount of evidence to suggest that 'Get Out the Vote' (GOTV) campaigns can increase voter turnout, and to tell us what form and content a message should take to be most effective in this context. There is less evidence, however, on the effects of such campaigns on non-electoral political participation, and none at all when the target population is young people who have participated in service learning. This section of the review therefore proceeds as follows. First, the main results from the research on GOTV are briefly summarised, because this has been the focus of political scientists in the field of encouraging participation. When we look at the details of these findings, they are less helpful than we might expect when trying to understand how best to encourage youth participation. So, the review then returns to the literature on service learning and the wider literature on social mobilisation, to look for features of the intervention and its participants that could be leveraged to encourage political participation post-service. This part of the review suggests that theories of self-efficacy and identity could both be used effectively to encourage political participation among the population of interest, but the findings are speculative and suggest the need for further research.

Twenty years of GOTV field experiments have resulted in three headline findings. First, personalised forms of contact and personalised messages are more effective than impersonal ones. When mobilising voters, door-to-door canvassing is more effective than telephone calls (Gerber & Green 2000), and both of these methods work better when they are tailored to the individual (as opposed to delivering a standardised 'script') (Nickerson 2006). Second, social networks can have a positive effect on turnout; people are persuaded to vote by people that they live with and know (Nickerson 2008; Middleton & Green 2008).

Third, social pressure and perceived social norms can also support turnout. People are more likely to vote if they think that others in their local area will know that they did (Gerber et al. 2008). These findings are clear and robust. However, they relate to only one form of political behaviour – voting – and come from tests on general adult populations, not young people. They also focus on two methods of communication (door-to-door canvassing and telephone calls) that are not generally used by service learning organisations, or in youth engagement in general. NCS, for example, communicates with approximately 300,000 graduates of its programme, spread across all regions of England. Door-knocking is obviously not feasible for regular communications with such a large population. And even if it were feasible (when communicating with small subgroups, for example), it would likely be considered inappropriate. Telephone calls are sometimes used for small subgroups who have consented to more regular and personal contact but are again not viable for mass communications. So, while the GOTV literature is robust and answers a very similar question to the one at hand, it is not obvious that its lessons can be readily applied in the context that this thesis is focussed on. To identify potentially effective ways of encouraging post-service political behaviour amongst young people, it may be more fruitful, therefore, to return to the literature on service learning and combine that with insights from the wider literature on social mobilisation.

The review of mechanisms above suggests that the sense of self-efficacy developed by participants during service learning might be an important determinant of political participation post-service. The potential causal chain in this case is complex and contested, but both sides of the argument agree that the perceived effect of a civic action is an important determinant of an individual's civic self-efficacy. And the extent to which an increase in non-political civic self-efficacy will lead to an increase in political self-efficacy will likely depend on how similar or related an individual considers the two domains to be. So, it may be that an individual's sense of post-service self-efficacy could be used to encourage political participation if the individual can be induced to: i. consider the increased sense of self-efficacy that she may have developed in the non-political domain; ii. link these gains in the non-political domain to the political one (encouraging a spillover effect); and iii. believe that her political actions could make a difference (that the system will respond).

Having said this, we also know that, in certain circumstances, individuals engage in political action even when they have low (or no) expectations of success (Klandermans & Oegema 1987; Schofield & Pavelchak 1989). Some have hypothesised that this is because identity plays an important motivational role in civic action (Simon et al. 1998; Hornsey et al. 2006, p.1704), and there is experimental evidence to support this idea (Rodgers et al. 2018). The experimental literature suggests that there are three key factors that determine whether identity can be used effectively to encourage political participation (Rodgers et al. 2018, p.370): i. the extent to which the individual relates part of her identity to political participation; ii. how strongly an individual identifies with those parts of her identity; and iii. the salience of that part of her identity at the point of deciding whether or not to participate. There are two social identities that members of the population of interest may hold that are relevant to political participation: 'youth' and 'active citizens'. These two identities are discussed below in relation to the three factors that determine identity's efficacy as a mobiliser.

This thesis is focused on young people who, as a group, are marginalised in political decision-making; many cannot vote (those under the age of 18), and those who can often choose not to (Ipsos MORI 2015 & 2017; Sloam 2012, pp.4-5). In the case of political mobilisation, the effect of a social identity can be particularly powerful when the social group in question thinks of itself as marginalised or subject to 'unjust treatment' (Rodgers et al. 2018, p.370; van Zomeren et al. 2008). Reminding individuals that they are young and highlighting the marginalisation of youth voice in politics may therefore encourage political participation, though there is no direct evidence to support this claim.

Programmes of service learning also encourage their participants to think of themselves as people who can 'use [their] voice]' and make 'change in [their] community' (NCS Trust 2020b), as active citizens. When an identity is socially desirable, it can be used to encourage a behaviour by presenting the behaviour's performance as an opportunity to affirm that identity (Rodgers et al. 2018, p.371). Bryan et al. have employed this method effectively in a GOTV experiment that increased electoral turnout by encouraging participants to think of themselves as 'voters' (2011). This type of intervention can be particularly

effective if the behaviour that is being encouraged is aligned with an identity that is already held by the participant and is associated with a behaviour that she has performed before (Kessler & Milkman 2017).

Participants of service learning have performed similar behaviours to political participation during their service experience, and there is some theory, and evidence to suggest that they may incorporate their experience into their identities. Adolescence is a crucial period for identity-building (Erikson 1994; Youniss et al. 1997, p.624) and some researchers argue that service learning can influence participants' identities because of this (Kahne et al. 2013, p.421; Penner 2002; Piliavian et al. 2002). Youniss et al. postulate a role for voluntary youth groups in giving young people the opportunity to experiment with new (civic) values and incorporate the ones that they favour into their identities (1997, p.625). This may lead to a greater commitment to the common good (Youniss et al. 1997, p.625) and a sense of oneself as a 'civic actor' (Flanagan et al. 1999, p.3). There is some quasi-experimental evidence to suggest that NCS can have these types of positive effects, but also that it can encourage new social bonds that, in turn, could lead to a sense of attachment to NCS and, by extension, active citizenship as a social identity (Panayiotou et al. 2017). However, any sense of identity that is developed in relation to service learning is not based on an innate characteristic in the way that 'youth' is. As such, if an 'active citizen' identity is to be effectively used to induce political participation post-service, participants may need special encouragement to consider themselves a part of this group in a particular moment.

In summary, there is no direct evidence to say how best to encourage political participation post-service. The GOTV literature is well-developed, but focuses only on voting, has not been tested on the population of interest for this thesis, and has produced findings that do not seem transferable as they focus on modes of contact that are not viable for youth civic engagement. However, combining insights from the review on mechanisms and the wider literature on social mobilisation suggests two avenues that might be fruitful when aiming to encourage political participation post-service. The first is to appeal to the sense of enhanced civic self-efficacy that people may feel after their service learning experience. The second is to use identity as a lever; focusing in particular on participants' sense of youth and of being active citizens. These two different

approaches to encouraging political participation post-service are tested in Study 3.

2.5 Conclusions

This thesis is interested in the relationship between service learning and political participation. The review of the literature has presented what we knew prior to the thesis about this relationship and, in doing so, has identified three important gaps in our knowledge. First, we do not know what the effect of service learning is on political participation. Second, if service learning does have an effect in this domain, we do not have a clear sense of how the effect is produced. Third, while there is extensive, robust evidence on adult voter mobilisation, there is no direct evidence to say how best to encourage broader political participation amongst young people, post-service. The methodology section that follows presents an empirical strategy for addressing these three gaps.

3 Methodology

3.1 Introduction

This thesis aims to improve our understanding of the relationship between service learning and political participation by answering three questions: i. 'What is the effect of service learning on young people's political participation?' (RQ1); ii. 'If there is an effect, how is it produced?' (RQ2); and iii. 'What is the most effective way to encourage political participation post-service?' (RQ3). These three questions are best answered by a combination of research methods, so a mixed-methods approach is taken. RQ1 is a question about causal effects, and random assignment is not possible in the study setting, so a quasi-experiment is used with the aim of estimating an unbiased average effect. RQ2 is a question about causal mechanisms and moderating factors, and there is little detail in the existing literature on this question, so a qualitative design is used to address this. RQ3 asks about the relative effects of different interventions, in a setting where random assignment is possible, and is therefore answered with a randomised controlled trial (RCT). All three studies are focussed on NCS as an example of service learning. Focussing on a single, well-defined intervention, adds clarity to the causal inferences made, as well as the qualitative identification of mechanisms and moderators.

The research designs for each study are described and justified in detail at the beginning of each relevant chapter. The aim of the current chapter is to explain the rationale for the overall design of the thesis. For this purpose, the design of each study is briefly summarised below, along with a description of how these designs complement each other. The three studies are more than the sum of their parts. They each use a different approach to generate different types of results. These results are interpreted together, and studies 1 and 2 were conducted sequentially, so that the findings from the former could be used to inform the design of the latter (Teddlie & Tashakkori 2009, p.120). Though not planned for at the outset, the qualitative findings from Study 2 also prompt a return to Study 1's dataset for some further exploratory analysis. Together, these two studies provide a detailed description of the effects of service learning on political participation and how these effects are produced. Study 3 further investigates elements of the theory, using a large-sample RCT with a behavioural

outcome measure. It attempts to show how progress could be made in policy and practice by testing newly-designed interventions that aim to encourage more political participation post-service. This chapter begins with a presentation of the designs of each study, and ends with a discussion of the strengths of and challenges in mixing these methods.

3.2 Study 1: A quasi-experiment

Study 1 addresses both RQ1 and RQ2. The primary aim of Study 1 is to provide an unbiased estimate of the average effect of service learning on participants' political participation (RQ1). The secondary aim is to generate unbiased estimates of the average effects on service-based civic participation, general civic self-efficacy, and political self-efficacy; providing the beginnings of an answer to the question of how the effect on political participation is produced (RQ2). The study tests six hypotheses, as summarised in Table 3.1.

Table 3.1: Study 1 hypotheses

Hypotheses
H1.1: Service learning will increase general civic self-efficacy.
H1.2: Service learning will increase service-based civic participation.
H2.1: Service learning will increase political self-efficacy.
H2.2: Service learning will increase political participation.
H3.1: The effect on general civic self-efficacy will be greater than the effect on political self-efficacy.
H3.2: The effect on service-based civic participation will be greater than the effect on political participation.

As with all studies in this thesis, the example of service learning that is used to test these hypotheses is NCS. Take up of NCS is not random. The programme is promoted through a national campaign, and young people choose whether or not to participate. Hypotheses must therefore be made about the mechanisms behind selection and, for a plausible causal inference to be made, these mechanisms should be controlled for in the analysis or by the way in which units are selected for analysis. The data available allows for a combination of difference-in-differences (DiD) analysis and matching to be used to test the study hypotheses. A detailed discussion of this matched DiD design, the assumptions that underpin it, and the plausibility of these assumptions in the case of this study is given in Chapter 4. The matching part of the design selects

a subsample of data such that the intervention and comparison groups are as close as possible to each other on their observed characteristics (Stuart & Rubin 2008, p.155). For each outcome tested, the DiD analysis compares the average change over time in the intervention group to the average change over time in the comparison group. In doing so, it removes all confounding caused by factors (both observed and unobserved) that are constant over time. If the parallel trends assumption holds, it also means that confounding factors that vary over time, and tend to influence both groups similarly, do not bias the estimated effects of the intervention (Angrist & Pischke 2009, p.230).

So, Study 1 focusses on a single, clearly defined intervention, and combines matching with a DiD analysis to reduce bias in the estimated effects that may be caused by omitted variables and the fact that selection into the intervention is non-random. In doing so, it provides the most robust answer to RQ1 to date, identifying positive effects of service learning on political participation. It also identifies some proximal outcomes of service learning that may partially explain this effect on political participation. However, the analysis in relation to RQ2 is limited in three key ways. The first limitation is down to the measurement tool; because a quantitative survey is used, the analysis is restricted to the outcomes measured in that survey, and to the necessarily reductive quantitative indicators used to measure these outcomes (Brady & Collier 2004, p.9). Second, the analysis is unable to tell us whether the proximal outcomes observed are causally linked to political participation. Indeed, no quantitative research design is capable of definitively testing causal relationships between the outcomes of an intervention. This is because any post-intervention outcomes, such as increased political self-efficacy, could be systematically related to other unobserved variables that influence political participation (Montgomery et al. 2016). Third, the analysis says nothing about the factors that moderate the effects of service learning on political participation. Which characteristics of participants make them more or less likely to experience a positive effect? And which features of the context surrounding their service experience have an influence on its efficacy? In theory, subgroup analysis could help answer these questions but in practice the quasi-experimental dataset does not contain the range of variables necessary for a

comprehensive answer, and the sample is too small to detect effects amongst subgroups anyway.[7]

For these reasons, the results of Study 1, while giving us a good estimate of some causal effects, leaves us with a limited picture of how these effects are produced, and how they relate to each other. These limitations reflect a general critique of the experimental approach, that while experiments are well suited to causal inference, they leave a 'black box' over the parts that sit between the activity and the outcome(s) (Bonell et al. 2012; Cartwright 2010; Deaton & Cartwright 2018; Hawe et al. 2004; Jamal et al. 2015). This critique is most commonly levelled at RCTs, but applies equally to quasi-experimental approaches. In a sense, the 'black box' critique is as much about research questions as it about methods: it is saying that it is not enough to ask, 'What is the causal effect of X?', but that we should also ask, 'How is this effect produced, and in what context?'. This is particularly important when one of the aims of a research project is to produce results that can be applied in policy and practice.

3.3 Study 2: Ideational process tracing

Study 2 aims to build on Study 1 by improving our understanding of how the observed effect on political participation is produced (RQ2). It breaks this question down into three parts: i. 'What are the mechanisms in a service learning experience that lead to a change in participants' future political participation?' ii. 'Which activities trigger these mechanisms?'; and iii. 'What are the factors that moderate the effect of service learning on participants' future political participation?'. To answer these questions, the study employs 'ideational process tracing' (Jacobs 2015, p.43), using in-depth interviews with 27 graduates of NCS. These interviews cover the graduates' participation in democratic activities, their motivations for such participation and the role, if any, that NCS has played in this regard. The analysis looks for explanations for each individual's behaviour

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[7] These are not strictly issues of design, but of data scarcity. However, the data for this study was not collected by the researcher, and the number of survey items that could be collected for the purposes of this study were restricted by the study partners. It was not possible, therefore, to build a larger sample or a wider range of variables into the design of Study 1. Section 7.4 suggests new research projects that could follow on from this thesis, one of which is conduct a new (quasi-)experimental study that does support subgroup analysis, using the qualitative findings from Study 2 to generate the hypotheses and to identify the variables that need to be collected.

and makes comparisons across cases to develop a theory. This differs from Study 1 in terms of the approach to data collection and analysis, and these differences add value in three main ways.

First, the different approach to data collection generates broader and more nuanced data. Where the surveying of Study 1 necessarily restricts the variables studied, and reduces them to quantitative indicators, the semi-structured interviewing of Study 2 allows for the investigation of factors that go beyond the variables in the quasi-experiment's dataset, and also adds depth of meaning to those that are contained within it. Second, the different approach to analysis allows for the tracing of causal pathways. Where Study 1 estimates average differences between the intervention and comparison groups, Study 2 traces causal processes for individual cases. This latter approach helps to make explicit the connections between the intervention's activities, their proximal outcomes, and the primary outcome (political participation). Third, the focus on individual cases also exposes a heterogeneity that is masked by the analysis of average effects, helping us to understand the factors that moderate the effects of the intervention; who does the intervention work for, and under what circumstances? Of course, this qualitative approach also has its limitations. The interviewer has an unavoidable effect on the data that is generated (Holstein & Gubrium 2011), the quality of the data is dependent on the self-knowledge and recall of the interviewee, and the processes of analysis, drawing inferences and making generalisations are complex. All of these issues are discussed in detail in Chapter 5. Here, the focus of the discussion is on how the designs of each study relate to and reinforce each other.

The qualitative analysis in Study 2 reveals some surprises. It suggests that social self-efficacy – an individual's belief in her social interaction and communication skills, and her belief that applying these skills can achieve positive outcomes – is a more powerful mediating mechanism than political self-efficacy and general civic self-efficacy in this context. It also suggests that social anxiety, and social and communication skills are important mechanisms. This generates four new quantitative hypotheses, summarised in Table 3.2.

Table 3.2: New hypotheses generated by Study 2

Hypotheses
H4.1: NCS increases social self-efficacy more than general civic self-efficacy.
H4.2: NCS increases social self-efficacy more than political self-efficacy.
H4.3: NCS decreases social anxiety.
H4.4: NCS increases social and communication skills.

Fortunately, the quasi-experimental dataset contains good indicators for all of the newly identified outcomes. These hypotheses are therefore tested by returning to this data and estimating average treatment effects, following the same matching and estimation procedures as in Study 1. The results of this quantitative analysis provide support for H4.2 and H4.3, but not the other two hypotheses. Taken alongside the qualitative findings, this provides strong evidence that the two mediating factors that are perhaps most surprising – social self-efficacy and anxiety – also seem to be the most powerful. This part of the thesis in particular demonstrates the power of mixing methods. On its own, the qualitative analysis could not draw this conclusion, but without the qualitative analysis, these quantitative hypotheses would not have been generated in the first place (Teddlie & Tashakkori 2009, p.120).

In summary, the design of Study 2 comes with four features that make it complementary to Study 1: i. it generates broader and more nuanced data to support theory development; ii. it allows for the explicit tracing of causal pathways; iii. it exposes heterogeneity that is masked by the analysis of average effects; and iv. it generates hypotheses about new mechanisms that are testable in the quantitative dataset. Mixing qualitative enquiry with quasi-experimentation in this way places this research in the 'realist' tradition of intervention studies (Pawson 2004). In this tradition however, when qualitative methods are used it is usually just for the purposes of helping to interpret the quantitative findings (Drabble & O'Cathain 2015). This thesis goes further. The qualitative results do help with the interpretation of Study 1's findings by developing a theory, but they are also used to develop and test new quantitative hypotheses that further refine this theory. So, while Study 2 is led by qualitative enquiry, it is, in itself, also a mixed-methods project.

3.4 Study 3: A randomised controlled trial

Studies 1 and 2 are descriptive, in the sense that they investigate the effects of an existing policy. Study 3 is more generative in the sense that it develops and tests new interventions, with the aim of producing the first direct evidence of how best to encourage more political participation post-service (RQ3). This involves the design and testing of three messaging interventions; one based on the theory of self-efficacy, one on theories of identity, and one plain message that acts as the control. These messages invite graduates of NCS to participate in a political letter writing competition, which was designed by the researcher in partnership with NCS Trust and the UK government Department for Culture Media and Sport (DCMS), and run for the purposes of this study. Individuals are randomly assigned to receive one of the three messages, and the competition entry rates from these three trial arms are compared. This study has two strengths. First, by using random assignment with a large sample (over 200,000 individuals), it is able to produce a reliable and unbiased estimate of the causal effects of the interventions tested. Second, it tests the effect of these interventions on a genuine act of political behaviour (as opposed to self-reported intentions or past behaviours).

Again, the findings from this study are interpreted in relation to those from the other two. In particular, this study provides further support for the conclusion – that emerges at the end of Study 2 – that a self-efficacy spillover effect from the domain of service to the domain politics is very hard to create.

3.5 Discussion

This thesis asks three questions about the relationship between service learning and political participation. To answer the questions well, a range of methods is needed. The estimation of unbiased causal effects (the goals of RQ1 and RQ3) is best achieved through an experimental approach; ideally an RCT. An RCT is not viable for answering RQ1 (because participants cannot be randomly assigned to NCS), so a quasi-experiment is used instead. The question of how a causal effect is produced (RQ2) is harder to address. The inherent challenge built into questions of this type is that both the mechanism and the outcome that it causes are realised after the intervention. Mechanisms cannot therefore be randomly assigned, so it is impossible to directly causally identify them using an

experimental design.[8] For this reason, RQ2 is addressed qualitatively (by interviewing participants of NCS to trace possible causal pathways from their service learning activities through to their political participation) *and* quantitatively (by estimating the average effects of NCS on a set of proximal outcomes identified as potential mechanisms). In taking this approach, this thesis identifies new and surprising mechanisms through qualitative enquiry, and tests which of these mechanisms are realised in average treatment effects through quantitative analysis. In this way, the research capitalises on the relative strengths of each method used.

Some may believe that it is impossible to meaningfully combine the results of qualitative and quantitative enquiry. These beliefs might be informed by purist positions in one of two research paradigms: positivism or constructivism. The most extreme positivist position is to believe that social science is like the physical sciences, in the sense that there are objective facts and laws in the social world, and that the researcher is entirely separate from the objects being observed (Johnson & Onwuegbuzie 2004, p.14). On the other hand, the most extreme constructivist position holds not only that the researcher will influence the attitudes and beliefs of the subjects of the research (and therefore the findings), but that there is no such thing as attitudes and beliefs that are independent of a particular social interaction. Objectivity, under this view, is therefore impossible in the social sciences.

Both extremes of this debate suggest that the mixing of quantitative and qualitative methods is impossible due to this incompatibility in ontological and epistemological positions (Johnson & Onwuegbuzie 2004, p.14). A full discussion of these incompatibilities is beyond the scope of this thesis, but is also not necessary to argue for the validity of the findings. This thesis takes a

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[8] It is possible to do 'implicit mediation analysis' (Gerber & Green 2012, p.333) using an experimental design, where variants of an intervention are tested against each other. However, it is only possible to experimentally manipulate the activities within an intervention, not the mechanisms (which are proximal outcomes of these activities). So, while it is possible to causally identify a difference in effect between two variants, the question will always remain as to what causes that difference. Hence why this approach is prefixed with the word 'implicit'. This approach to theory development is also better suited to either very simple theories, or ones that are very well-developed, because it takes a relatively large amount of resource to test differences on only a single dimension using an RCT. In the case of this study, the theory is potentially complex and is under-developed in the literature, so implicit mediation analysis is not well-suited to the challenge.

pragmatic approach, which says that, when it comes to a specific research project, these differences in ontology and epistemology only matter in so far as they have practical implications for how the research is conducted, how the data is analysed and how the findings are interpreted (Johnson & Onwuegbuzie 2004, p.17). In this thesis, an argument is presented to justify the use of mixed methods to answer a set of research questions that lend themselves to different methods. In doing so, it accepts that there are differences in the type of data that is generated by the different methods used, and that the alternative study designs support distinctive types of inferences.

The point in this thesis where the debate about conflicting research paradigms becomes a live one, is where quantitative and qualitative findings that supposedly relate to the same construct are compared. This is the case in Study 2, when new mechanisms are identified in the interviews and then looked for in the quasi-experimental dataset. This presents the potential issue that the ideas presented by interviewees are incorrectly matched to variables in the quantitative dataset. There is no way of knowing if this is truly the case, and there will inevitably be some error introduced in this process. However, care is taken when looking for appropriate matches and good results are achieved in this respect. In the case of social skills and social self-efficacy, new composite variables are created that closely reflect the range of ideas described by interviews in these categories. For example, the social self-efficacy variable that is created includes survey items that reflect participants' confidence in meeting new people, working with other people in a team, being a leader of a team, speaking in public, and managing disagreement and conflict; all of which were specifically discussed by interviewees. We can therefore have good confidence that the survey items used in this analysis do closely reflect the outcomes identified through the interviews. The details of this process are presented and discussed in Chapter 5. The positivist/constructivist debate again becomes live when considering the design and analysis of the interviews on their own. This particular issue is not one relating to the mixing of methods, but is more about qualitative research *per se*, so its discussion is saved for Chapter 5.

In summary, a quasi-experiment, a set of interviews, and an RCT are combined in this thesis to answer three related questions. Although some could see fundamentally conflicting research paradigms in use, this chapter has argued that

each method has been selected because it represents the best way (that is available) of answering the relevant question. When combining findings from different methods, to add depth to the interpretation or to generate and test new hypotheses, care is taken to address the potential difficulties and inconsistencies that could arise. This is a pragmatic empirical research project ('t Hart 2010, p.106).

4 Study 1: The effects of service learning on political participation

4.1 Introduction

This study uses a large quasi-experiment (N=5,486), employing a matched difference-in-differences (DiD) design, with England's National Citizen Service (NCS) to test the effects of service learning on young people's political participation. It finds that NCS has a substantial positive effect on participants overall political participation (3.1pp) and even larger effects on some forms of non-electoral participation (5.4pp on petition signing and 4.9pp on protest attendance).

The literature suggests that self-efficacy might be a key mediator of these effects (Almond & Verba 1989; Condon & Holleque 2013, p.168; Converse 1972; Craig 1979; Craig et al. 1990; Karp & Banducci 2008; Niemi et al. 1991; Pateman 1970; Pollock III 1983; Reinders & Youniss 2006; Vecchione & Caprara 2009; Verba et al. 1995). The idea being that an experience of service-based civic participation will lead to increased civic self-efficacy, which will lead to gains in political self-efficacy, which in turn will lead to more political participation (a self-efficacy 'spillover effect'). To test this theory, the study also estimates the effects of NCS on service-based participation, general civic self-efficacy, and political self-efficacy. Positive effects are identified for the first two of these outcomes (3.7pp and 8pp respectively), but the programme has almost no effect on political self-efficacy. This suggests that the self-efficacy spillover story is not quite right. These findings motivate Study 2 in this thesis; an in-depth exploration of mechanisms and moderating factors. The results also suggest that service learning, while making a valuable contribution, may have even more to offer when it comes to closing the political participation gap between the young and the old, if enhancements are made to existing practice.

4.2 Research questions and hypotheses

This study attempts to answer two questions: i. 'What is the effect of service learning on young people's political participation?' (RQ1); and ii. 'If there is an

effect, how is it produced?' (RQ2). The review of the literature leads to the following hypotheses in response to these questions.

Participants of service learning have a service-based civic mastery experience, which suggests two hypotheses.

- **H1.1:** Service learning will increase general civic self-efficacy.
- **H1.2:** Service learning will increase service-based civic participation.

The general civic self-efficacy developed by this service-based mastery experience will spillover into the political domain.

- **H2.1:** Service learning will increase political self-efficacy.
- **H2.2:** Service learning will increase political participation.

Self-efficacy is a domain-dependent concept (the closer the domain, the more powerful the mastery experience), and service-based participation is more likely to have tangible and immediate perceived results.

- **H3.1:** The effect on general civic self-efficacy will be greater than the effect on political self-efficacy.
- **H3.2:** The effect on service-based civic participation will be greater than the effect on political participation.

4.3 Research design

As with all studies in this thesis, the example of service learning that is used to test these hypotheses is NCS. Take up of NCS is not random. The programme is promoted through a national campaign, and young people choose whether or not to participate. Hypotheses must therefore be made about the mechanisms behind selection (see section 4.6.1) and, for a plausible causal inference to be made, these mechanisms should be controlled for in the analysis. The data available for this study comes from a survey that was issued to all participants of NCS in summer 2019 and to a sample of young people who expressed an interest in NCS but did not participate. This survey collected data on the outcomes of

interest (political participation, service-based participation, political efficacy and civic self-efficacy), as well as a range of demographic characteristics.[9]

This data allows for a combination of difference-in-differences (DiD) analysis and matching to be used to test the study hypotheses. A detailed discussion of this matched DiD design, the assumptions that underpin it, and the plausibility of these assumptions in the case of this study is given below in section 4.6. The matching part of the design selects a subsample of data such that the intervention and comparison groups are as close as possible to each other on their observed characteristics (Stuart & Rubin 2008, p.155). For each outcome tested, the DiD analysis compares the average change over time in the intervention group to the average change over time in the comparison group. In doing so, it removes all confounding caused by factors (both observed and unobserved) that are constant over time. If the parallel trends assumption holds, it also accounts for factors that vary over time and tend to influence both groups similarly (Angrist & Pischke 2009, p.230). So, Study 1 focusses on a single, clearly defined intervention, and combines matching with a DiD analysis to reduce bias in the estimated effects that may be caused by omitted variables and the fact that selection into the intervention is non-random.

A pre-analysis plan (PAP) was registered prior to data access (Taylor, 2020a). However, specifying the most appropriate matching procedure before seeing the data was difficult as the size and composition of the sample was unknown. There are therefore some minor differences between the analytic approach described in the PAP and the actual analysis carried out. These differences are described and justified in Appendix III.

4.4 Sample and data

The total sampling population comprised all young people who were invited to participate in NCS on the Summer 2019 programme and completed an

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[9] Separate to this study, a pre/post survey was commissioned by the UK Government Department for Digital, Culture, Media and Sport (DCMS, the department responsible for NCS) with the aim of evaluating the effects of NCS; hereafter the 'NCS Impact Survey'. Items were added to this survey for the purposes of this study. All sampling and data collection were carried out by a third-party research consultancy on behalf of DCMS. The choice of survey items and all analysis pertaining to the present study was carried out by the researcher.

expression of interest form (EOI). This population was then divided into two parts: those who expressed an interest but did not participate, and those who did participate in the intervention. From these two populations, two groups were sampled for analysis: the intervention group and the comparison group. A census approach was taken to data collection, with the whole population invited to complete a baseline survey that captured demographic and baseline outcome data. All those who completed the baseline survey and consented to being recontacted were sent a post-intervention survey to capture a second round of outcome data.

The intervention group was made up of all participants who met the following criteria: i. they participated in the intervention; ii. they consented to being contacted for surveying; iii. they completed a baseline survey; and iv. they completed a post-intervention survey. The comparison group was initially made up of all individuals who met the following criteria: i. they expressed an interest in participation in NCS; ii. they did not participate in the intervention; iii. they consented to being contacted for surveying; iv. they completed a baseline survey; and v. they completed a post-intervention survey. All individuals who met the criteria in these two groups were sent a combination and emails and letters in the post to invite participation in the surveys. This sampling approach yielded an initial comparison group of 1,118 participants and an intervention group of 4,110 participants.

As the comparison group was relatively small, the research consultancy administering the survey decided to conduct a second round of sampling via an online panel, yielding an extra 258 people. The total sample before matching was therefore 5,486. The inclusion criteria for people from the online panel recruitment were as follows: i. they were 16 or 17 years old (matching the rest of the sample); and ii. they reported not doing NCS.[10] This sample is therefore different in some potentially important ways to the rest of the comparison group. In particular, members of the online panel had not expressed an interest in NCS. For robustness, the analysis is therefore repeated with and without the

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[10] Details of the sampling procedure for the online panel were not recorded by the research consultancy that led the sampling and data collection, but we do know that it was a form of convenience sampling where a group of individuals who met the inclusion criteria were invited by email to complete the two surveys.

inclusion of the online panel sample (see section 4.9.1). This check reveals that the results are not sensitive to this difference in sampling.

Baseline survey data was collected in waves over summer 2019 between 03/06/2019 and 02/09/2019 (ahead of the periodic start dates for the intervention). Follow-up survey data was collected three to four months post-intervention between 11/10/2019 and 12/12/2019. All survey data was collected online. Figure 4.1 summarises the sampling and attrition, with figures that are close to the estimates in the PAP (Taylor 2020a, p.8). (This attrition data was not available for the online panel).

Figure 4.1: Study 1 flow diagram

	Intervention group (NCS)	Comparison group 1 (EOIs)	Comparison group 2 (online panel)
Self-selected into group	n = 98,331	n = 107,008	n = unknown
Completed baseline survey	n = 19,693 (20%)	n = 6,373 (6%)	n = 509 (unknown%)
Completed follow-up survey	n = 4,110 (0.04%)	n = 1,118 (0.01%)	n = 258 (unknown%)
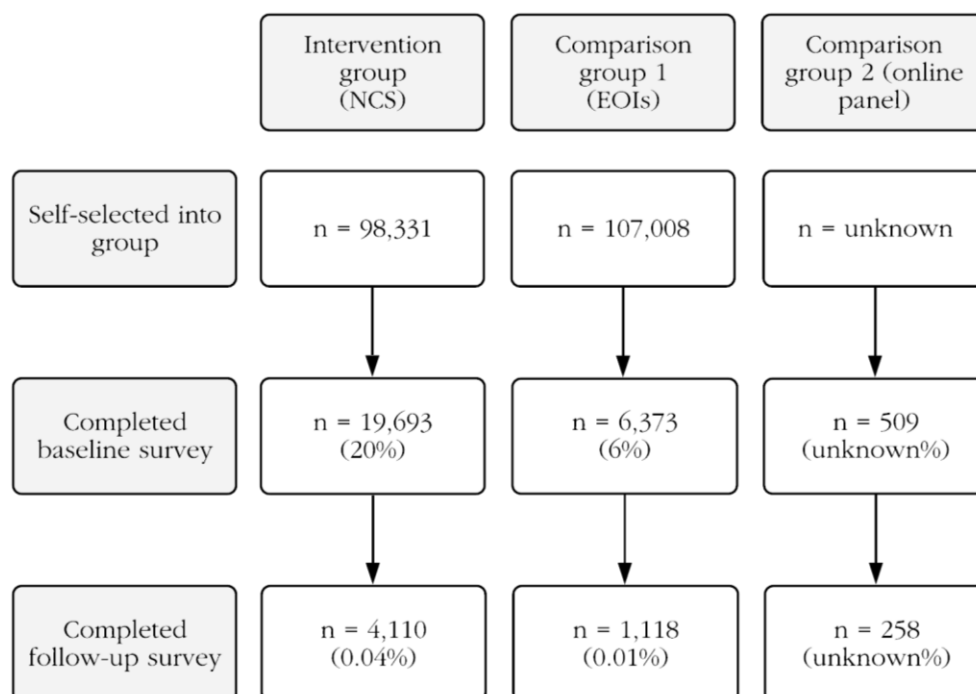
Notes: All participants who completed the follow-up survey are analysed.

This figure shows clear differential attrition between the intervention and comparison group. This is as expected; participants of the programme may be more motivated in general, and more invested NCS, so more likely to complete the surveys. This is only a problem for the internal validity of the study insofar as this motivation is a true confounder (predicting take-up and the outcomes of interest) *and* is not controlled for by the matching and DiD analysis. In the discussion on selection mechanisms below, it is argued that the analysis does at least partially address this issue.

Table 4.1 compares the characteristics of the sample with the sampling population, as well as the wider population of England. The sampling strategy did not aim for statistical representation, but these comparisons help to give a sense of the external validity of the results. They show that, compared to the sampling population, the sample has a smaller proportion of participants from lower socio-economic backgrounds (as measured by free school meal (FSM) eligibility), and is substantially over-represented by female participants. The distribution of ethnic groups remains similar, however. Compared to the wider population of young people of a similar age in England, members of the sample are equally likely to come from disadvantaged socio-economic backgrounds, more likely to be female and more likely to be a member of a minority ethnic group.[11] These are all potentially important differences when considering how the effects estimated in this study might be realised in the wider population of NCS participants, and in the English population more broadly. No hypotheses are made in this study about heterogeneous effects but socio-economic background (Condon & Holleque 2013, pp.170-171; Craig 1979, p.231), gender (Norris et al. 2004; Sloam et al. 2018) and ethnicity (House of Commons Political and Constitutional Reform Committee (HCPCRC) 2014) are known predictors of political participation. Therefore, the average treatment effects estimated in this study may well differ between the sample, the sampling population and the wider population of England, so caution should be taken when generalising the results.

Table 4.1: Sample characteristics compared to the sampling population and the wider population of England

	% of matched sample	% of pre-match sample	% of sampling population	% of England
Eligible for FSM?				
Yes	13*	13*	22*	14**
No / unknown	87*	87*	78*	86**

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[11] The most recent census in England was conducted in 2011. At that point the study cohort were approximately 8 years old. The nearest category in the census data, used to calculate the ethnicity figures for England in this table, is 8-9 years. While this is not a perfect match, it is sufficient here, where the aim is to provide an approximate indication of the differences between the sample and the wider English population. The gender figures for England are based on ONS population estimates for 16- and 17-year olds in 2018 (the latest available figure at the time of writing).

Gender				
Female	67	67	55	49
Male	31	31	44	51
Ethnicity				
Asian	14	14	15	11
Black	6	6	8	5
Mixed	6	6	5	5
White	72	72	69	78
Other	2	2	2	1

Notes: N(matched) = 5,461, N(pre-match) = 5,486.
[*] In the one-year period prior to participation in NCS.
[**] Mean average of state secondary school pupils across 2017, 2018 and 2019.
Source for free school meal data for England: Department for Education, 2019.
Source for ethnicity estimates for England: Office for National Statistics, 2015.
Source for gender estimates for England: Office for National Statistics, 2019.

4.5 Outcome measures

Testing the hypotheses requires the analysis of four outcomes: political participation, political self-efficacy, service-based participation and general civic self-efficacy. For the purposes of this study, items on political participation and political self-efficacy were added to the NCS Impact Survey by the researcher. One item on intention to vote, and a series of items on civic self-efficacy and service-based participation were already present in the survey. Full versions of the baseline and follow-up surveys can be found in Appendices I and II. The items that are analysed from the survey are described below. The outcomes are classified as 'primary' or 'secondary' to indicate their relative status in the study; the former being the main quantity of interest (RQ1), and the latter being factors that might explain the main effect (RQ2). Table 4.2 summarise the outcomes and shows which hypotheses each outcome relates to.

Table 4.2: Mapping outcomes on to the hypotheses

Outcome	**Primary or secondary?**	**Hypotheses**
Political participation	Primary	H2.2 & H3.2
Political self-efficacy	Secondary	H2.1 & H3.1
Service-based participation	Secondary	H1.2 & H3.2
General civic self-efficacy	Secondary	H1.1 & H3.1

4.5.1 Primary outcome

The primary outcome is political participation. To operationalise this, four types of self-reported political participation are measured: voting, contacting a politician, petitioning and protesting. The voting question ($Q019 - Q10$) asks respondents how likely they are to vote in the next election or referendum when legally old enough (on a scale of 1 to 10). The questions on contact, petitioning and protest ask respondents whether they have performed the relevant behaviour in the past three months ($Q014 - Q4$, *options 5, 7, 8 and 9*). These items were adapted from the most age-appropriate survey questions covering political behaviour, that have good validity and reliability, found in the survey for the International Civic and Citizenship Education Study (ICCS) (Schultz et al. 2009, p.188). The ICCS items ask respondents to report their *intention* to participate in a range of political activities. However, for other civic behaviours, the NCS Impact Survey asks respondents to retrospectively report participation in the past three months, so the ICCS items were adapted to fit this model of questioning. This amendment also improves the validity of the measures, as self-reported past behaviour is more accurate than self-reported intentions (Achen & Blais 2015).[12] Minor adaptations to the wording of some ICCS items were also made at the request of the NCS Trust and the Department for Digital, Culture, Media and Sport (DCMS, the government department responsible for NCS), who had the final say over the survey content.[13] For the main analysis, a normalised combined score for all four types of participation is calculated as follows.

$$polpart_i = \frac{\left(\left(\frac{vote_i}{9}\right) + signpet_i + orgpet_i + contact_i + protest_i\right)}{5} \tag{1}$$

where, for individual i:

- $polpart_i$ is the combined political participation score (with range [0,1]);
- $vote_i$ is self-reported intention to vote (on a 10-point scale with range [0,9]);

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[12] This is another feature of this study that sets it apart from the existing literature, which often relies on self-reported intentions for this age group.
[13] See Appendix IV for a full breakdown of all amendments.

- $signpet_i$ is self-reported petition signing in the past 3 months (binary 1 or 0 to indicate 'yes' or 'no');

- $orgpet_i$ is self-reported petition organising in the past 3 months (binary 1 or 0 to indicate 'yes' or 'no');

- $contact_i$ is self-reported contacting of a politician in the past 3 months (binary 1 or 0 to indicate 'yes' or 'no'); and

- $protest_i$ is self-reported protest participation in the past 3 months (binary 1 or 0 to indicate 'yes' or 'no').

Exploratory analysis also estimates the effects on each form of participation separately.

4.5.2 Secondary outcomes

The secondary outcomes analysed are political self-efficacy, general civic self-efficacy and service-based participation. The NCS Impact Survey already contained items relating to the latter two of these variables (*Q018-Q9, rows 1 & 3; Q014-Q4, options 1, 2, 3 & 6*), so items were only added in relation to political self-efficacy (*Q020-Q10a, Q021-Q10b, Q022-Q10c, Q023-Q10d*). The European Social Survey (Saris & Revilla 2012) contains the most age appropriate and robustly tested items on political self-efficacy and these items were added, with very minor adaptations, to the NCS survey as a new question.[14] The questions on political and general civic self-efficacy both have items relating to internal and external self-efficacy. The four questions on service-based participation ask respondents whether they have done any of the following in the past three months: i. helping at a club, group, organisation or place of worship; ii. helping at other organisations; iii. charity fundraising; and iv. helping other people. For the main analysis, combined scores are created for each outcome as follows.

$$poleff_i = \frac{(intpoleff_i + extpoleff_i)}{2} \tag{2}$$

where, for individual *i*:

- $poleff_i$ is the combined political self-efficacy score (with range [0,4]);

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[14] See Appendix IV for adaptations.

- $intpoleff_i$ is the combined internal political self-efficacy score (the mean average of items *Q022-Q10c* and *Q023-Q10d,* based on 5-point Likert scales, with range [0,4]); and

- $extpoleff_i$ is the combined external political self-efficacy score (the mean average of items *Q022-Q10a* and *Q023-Q10b*, based on 5-point Likert scales, with range [0,4]).

and,

$$civeff_i = \frac{(intciveff_i + extciveff_i)}{2} \tag{3}$$

where, for individual *i*:

- $civeff_i$ is the combined civic self-efficacy score (with range [0,4]);
- $intciveff_i$ is the internal civic self-efficacy score (*Q018-Q9, row 3*, based on a 5-point Likert scale, with range [0,4]); and
- $extciveff_i$ is the external civic self-efficacy score (*Q018-Q9, row 1*, based on a 5-point Likert scale, with range [0,4]).

and,

$$serpart_i = \frac{(helpclub_i + helporg_i + raise_i + helpother_i)}{4} \tag{4}$$

where, for individual *i*:

- $serpart_i$ is the combined service-based participation score (with range [0,1]);
- $helpclub_i$ is self-reported helping at a club, group organisation or place of worship in the past 3 months (binary 1 or 0 to indicate 'yes' or 'no');
- $helporg_i$ is self-reported helping at other organisations in the past 3 months (binary 1 or 0 to indicate 'yes' or 'no');
- $raise_i$ is self-reported charity fundraising in the past 3 months (binary 1 or 0 to indicate 'yes' or 'no'); and
- $helpother_i$ is self-reported helping of other people in the past 3 months (binary 1 or 0 to indicate 'yes' or 'no').

Exploratory analysis also estimates the effects on each dimension (internal and external) of each form of self-efficacy, and on each type of service-based participation.

4.6 Analytic approach[15]

4.6.1 What are the likely selection mechanisms?

Take up of the intervention is not random in this study, so hypotheses must be made about the mechanisms behind selection and, for a plausible causal inference to be made, these mechanisms should be controlled for in the analysis or by the way in which units are selected for analysis. Two features have been built into the design of this study to address this. First, intervention effects are estimated using a difference-in-differences (DiD) analysis. This takes into account confounding factors (both observed and unobserved) that are constant over time. If the parallel trends assumption holds, it also means that confounding factors that vary over time, and tend to influence both groups similarly, do not bias the estimated effects of the intervention (Angrist & Pischke 2009, p.230).[16] Second, where confounding variables are observed, they are controlled for by including them in the model that estimates the propensity scores that are used for matching.

NCS is a voluntary programme and all people in the sampling population have expressed an interest in participation. There are three broad factors that could influence whether or not an individual participates in the intervention, after expressing an interest: i. the nature of the invitation; ii. the individual's motivation to participate; and iii. the individual's means of participation. These factors are discussed below, along with an overview of how they are addressed in the analysis.

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[15] See Appendix V for the full analysis code and log file.
[16] The rationale for this is described in more detail in the 'Difference-in-differences' section below.

4.6.1.1 The nature of the invitation

Recruitment and delivery of NCS is conducted by a number of youth organisations across England. While there is some standardisation in the recruitment materials used by these organisations, some variation is also expected in their approach to recruitment, and this could lead to differences in participation rates by recruiting organisation. The recruiting organisation is a time invariant variable so is accounted for by the DiD design.

4.6.1.2 Motivation to participate

Members of the sampling population vary in their motivation to participate in civic activities. This motivation is likely to be higher in the intervention group as compared to the comparison group. It could also be changing at different rates in each group. If the latter is true, the DiD estimate will be biased.[17] However, civic motivation is expected to be strongly correlated with self-reported civic participation (which is observed), and this is controlled for in the propensity score model. The study design also partially addresses this potential for differential motivation by constructing a comparison group from individuals who formally expressed an interest in participation in NCS (as opposed to young people who were unaware of NCS, or those who were aware but showed no formal interest). This effectively adds a first stage of matching to the data on some level of unobserved motivation.

4.6.1.3 Means of participation

Finally, participation is contingent on having the appropriate means. While the participation fee for NCS is relatively low for a residential programme (£50) and is waived for low-income families, there is some research to suggest that cost is still a barrier for some (Mills & Waite 2017, p.17). Financial means is likely to be a time invariant variable for the majority of participants over the course of the study, so is accounted for by the DiD design. Beyond this, it is worth noting that individuals are made aware of the cost of participation prior to the collection of expressions of interest. We know therefore that this information did not prevent

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[17] It is very difficult to know whether the rate of change of motivation is the same in the intervention and comparison group as there is no trend data prior to baseline. This is discussed in more detail in the discussion of the difference-in-differences estimator below.

people in the comparison group from going through the initial stages of application.

If this analysis of the selection mechanisms is comprehensive, it seems that the combination of matching with a DiD analysis accounts for a large part of the possible bias created by these mechanisms.

4.6.2 Why propensity score matching?

Matching is used in this analysis because it avoids the assumptions made by regression modelling about the functional form of the relationship between participation in the intervention, the outcome and the covariates. If the distribution of covariates is substantially different in the intervention group as compared to the comparison group, the results of regression modelling are sensitive to minor changes in the specification (Imbens 2015, p.373). Matching is also preferable to regression modelling because it is a design process that selects units for comparison without using values of the outcome variable so does not, in itself, introduce additional bias (Stuart & Rubin 2008, p.155).

The aim of the matching process is to select a subsample of the data such that the intervention and comparison groups are 'at worst, only randomly different from one another on all observed covariates' (Stuart & Rubin 2008, p.155). There are, however, different approaches to matching and decisions need to be made about the approach that is most appropriate given the data. It was impossible to know before seeing the data, which approach would produce the most balanced total sample. Nonetheless, based on knowledge of the intervention selection process, the estimated sample size, and the available covariates, the PAP specified that propensity score matching (PSM) would be used in the primary analysis. It also specified the decision-making processes for refining the approach and a number of robustness checks, all of which are implemented and reported below.

When the data allows it, exact matching – where units are matched with those in the opposing group that have exactly the same values of all covariates – maximises balance between groups. This approach approximates fully blocked randomisation in experimental studies (King & Nielsen 2016, p.2), maximising the precision of the estimated effects of the intervention by removing all

confounding caused by the covariates. In the case of this study, a large number of the variables are categorical (with five or more categories), continuous or quasi-continuous[18]. Because of this, there would be so few exact matches between the two groups that the resulting matched sample would be too small to support a meaningful estimation of effects. PSM overcomes this problem by estimating a one-dimensional quantity that can be used to match observations with similar scores, rather than trying to match on each individual covariate (Stuart & Rubin 2008, p.159). This quantity is called the 'propensity score' and expresses the probability of selecting into the intervention group, contingent on a set of covariates. Where exact matching approximates fully blocked randomised assignment, PSM approximates complete randomisation, aiming to generate balance in the average values of the observed covariates, rather than at the individual level (King & Nielsen 2016, p.2). PSM therefore results in matches that share similar values of the propensity score, but different values of each covariate.

One limitation of this approach is that it can lead to less balance on the observed covariates and, therefore, less precise effect estimates. However, in this case, this compromise on precision gives a substantially larger sample that leads to more accuracy in the estimates. The issue of imbalance is also partially addressed through the balance checks and re-specification of the parameters used in the matching procedure, as described below. A second limitation of PSM is that the collapsing of the contribution of the covariates into a scalar quantity 'ignores information' relating to the relative importance of those covariates (King & Nielsen 2016, p.8). This issue can be addressed by scaling the covariates prior to estimation of the propensity score, based on their known strength as predictors of the outcome. However, such scaling is not carried out in this case because the study is testing for multiple outcomes. Scaling covariates under these circumstances would lead to different propensity scores for each outcome, so different samples being used to estimate effects for each outcome. This would make the results difficult to interpret because it would be impossible to say whether differences (or similarities) in the observed effects were 'true'

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[18] Composite scores from multiple Likert scales.

differences (or similarities) or were artefacts of the different samples.[19] Weighting covariates in the estimation of the propensity score is therefore avoided, accepting that this may lead to an increase in the variance of the effect estimates.

In summary, PSM is the most appropriate approach to matching for this study, but measures need to be taken to mitigate the limitations in the method. The analytic approach taken – along with these mitigation strategies – is specified in detail in what follows. A set of robustness checks are also carried out (see Annex I to this chapter) to check the sensitivity of the results to a range of alternative specifications. One of these checks employs coarsened exact matching (CEM) instead of PSM, and shows that the results are not sensitive to this choice.

4.6.3 The matching process

Matching follows four steps (Imbens 2015, p.383; Austin 2011): i. estimating the propensity scores; ii. matching on the propensity scores; iii. assessing the unconfoundedness assumption; and iv. assessing post-match balance. These are discussed in turn below. The same matching model is used to construct the sample for analysing all outcomes (primary and secondary).[20]

4.6.3.1 Step 1: Estimating the propensity scores

Propensity scores are estimated for all units in the sample using a logistic regression that estimates the probability of taking up the intervention, with intervention receipt as the dependent variable and a set of baseline covariates as the independent variables (Rosenbaum & Rubin 1984), specified as follows.

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[19] For example, using weighted covariates to estimate the propensity scores for political participation may result in a sample that contains 70% males, and we may observe a positive effect on political participation for this group. Using different covariate weights to estimate the propensity scores for civic self-efficacy may result in a sample that contains 70% females, and we may observe a negative effect on civic self-efficacy for this group. Under such a design, it would be impossible to say whether the observed difference in effects is 'real' or down to the different gender proportions in the samples.

[20] It is possible to vary the matching model by outcome; for example, by having a two-stage estimation process that reduces the calipers for the value of the outcome at baseline. However, this approach is not taken in this case because it would likely result in different samples being used to estimate the effects for different outcomes. Under this design, any observed differences in effects between outcomes would again be consistent with two competing explanations: i. the difference is due to a true difference in effects of the intervention; or ii. the difference is due differences in the samples. Using the same matching model for each outcome will avoid this potential complication in the interpretation of the results.

$$logit(e_i) = \beta_0 + \beta_1 \boldsymbol{X_i} \tag{5}$$

where,

- e_i is the propensity score (the probability of unit i participating in the intervention); and

- $\boldsymbol{X_i}$ is a vector of pre-intervention covariates.

Choosing the covariates ($\boldsymbol{X_i}$) to include in the propensity score estimation is a balancing act. The more covariates that are included, the more accurate the propensity score is, reducing the bias and variance in the estimated effects. However, the more covariates that are included, the harder it is to find overlap in the propensity scores between the intervention and comparison groups (sometimes called 'common support') and the smaller the matched sample becomes. To identify the combination that best balances these competing goals, the following procedure is followed. To begin with, all baseline covariates from the survey are used to estimate e_i. The level of common support is then assessed through three tests (Garrido et al. 2014, pp.1706-1707). First, graphical distributions of e_i in each group are visually assessed, looking for both overlap and good discrimination between groups. Second, the sample is split into quintiles of the propensity score, and the average value of e_i in each quintile is compared for treated and untreated units. If these values differ within a quintile (based on t-tests for significance, $p < 0.05$)[21] then it is split in half and the test is carried out again until balance is achieved. Third, the second balance check is repeated for each individual covariate used to estimate e_i.

These balance checks are not passed when all baseline covariates are included, so the propensity score model is refined by reducing the number of covariates, through the following process, until balance is achieved (Austin et al. 2011; Brookhart et al. 2006; Caliendo & Kopeinin 2008). First, all predictors of take

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[21] Significance tests are dependent on sample size so are not relied upon as the 'final word' on balance. They are used here as an early filter, but post-match balance checks are also performed below by calculating normalised differences (see 'Assessing post-match balance') (Imbens & Rubin 2015, p.311).

up that do not predict the primary outcome are excluded, because they increase the variance and reduce the common support. Second, predictors of the primary outcome that are not predictors of take up are dropped one-by-one, starting with the variable that makes the smallest contribution to the variation in the primary outcome. These variables would reduce the variance of the estimated effect without increasing bias. However, balance is not achieved when any are included, so all are dropped, leaving only the true confounders (variables that predict both take up and the primary outcome). This last set of variables is again reduced one-by-one, based on the size of their joint contribution to the variation in the treatment and the outcome.[22]

4.6.3.2 Step 2: Matching on the propensity score

After the propensity score model is defined following the steps above, treated and untreated units are matched based on the similarity of these scores. The main analysis uses nearest neighbour matching with replacement for this purpose. In this study, the number of observations in the comparison group is substantially lower than the number of observations in the intervention group. The choice of matching estimator takes this fact into account, aiming to maximise the precision and accuracy of the estimated effects. One-to-one matching would reduce the total sample by 50% (from 5,486 to 2,752), which would increase the variance and reduce the accuracy, so is unlikely to yield the best result in this case. Matching with replacement is therefore used, where observations in the comparison group can be matched with more than one treated unit. This increases the matched sample size and, with it, the precision and the accuracy of the estimates (Stuart 2010, p.11).

The use of replacement in matching can also have a negative effect on the variance, however. When replacement is used, the error in the effect for each pair is multiplied by a factor of *k(k-1)*, where *k* is the number of observations in the intervention group that are matched to each observation in the comparison group. It is also possible that simple 'nearest neighbour' matching could result in 'bad matches' (observations with propensity scores that are far apart). To

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[22] This joint contribution is defined as the sum of the r-squared values derived from two separate bivariate regressions: i. the regression of the treatment indicator on the covariate; and ii. the regression of the primary outcome on the covariate.

account for these issues, the following three rules are applied. First, observations in the intervention group are only matched with one observation in the comparison group (because the intervention group is substantially larger than the comparison group). Second, weights are applied to the estimated effect from each pair, based on the number of times that the comparison unit in the pair has been used (Stuart 2010, p.16). Third, calipers are applied so that matches are only made for observations that are sufficiently close in propensity score (Cochran & Rubin 1973). If no match is found within the calipers for a treated unit, that observation is dropped from the analysis. Defining appropriate calipers before seeing the data is difficult (Smith & Todd 2005, p.315), as this again involves a trade-off. Setting them narrowly increases the quality of the matches but reduces the sample size. Setting them widely increases the sample size but reduces the quality of the matches and thus increases the bias. Following the PAP, these calipers are set first at 10% of the standard deviation of the propensity scores and then varied in the sensitivity analysis.[23]

4.6.3.3 Step 3: Assessing the unconfoundedness assumption

Any causal inference that is made in the absence of random assignment must assume that there are no unobserved variables that affect both take up of the intervention and the outcome(s) of interest. This is an untestable assumption as, by definition, these potential confounders are not observed. However, the *plausibility* of the unconfoundedness assumption can be tested. To do this, the effect of the intervention is estimated for a set of 'pseudo-outcomes'; variables that are known to be unaffected by the intervention (Imbens 2015, pp.383-384). In the absence of unobserved confounding variables, this effect will be zero as it is impossible, by definition, for the intervention to influence a pseudo-outcome. If the estimated effect of the test is non-zero, then this counts as evidence that the unconfoundedness assumption does not hold.[24]

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[23] See Abadie & Imbens (2016) for the full derivation and specification of the nearest neighbour matching estimator.

[24] This is, in effect, a type of post-match balance check, just presented in a different way – as a set of 'effects' instead of differences between treatment groups. In this sense, it complements Step 4 in the matching process, which calculates normalised differences between treatment groups for the covariates in the propensity score model.

The variables selected for this purpose are the four baseline measures of service-based participation (defined above in 'Outcomes'). These are chosen because they are not used in the propensity score model, cannot be affected by the intervention, but represent characteristics that can change over time (Imbens 2015, pp.383-384). After matching, the average treatment effect on the treated (ATT) is estimated for each pseudo-outcome.

4.6.3.4   Step 4: Assessing post-match balance

Once the matched sample has been created, balance is checked on the covariates in the propensity score model. This is done by reviewing the magnitude of the differences in the average value of each covariate between the intervention and comparison group. This complements the pre-match balance checks conducted above during the refinement of the propensity score model, and the assessment of the unconfoundedness assumption in the previous step. Rather than reporting simple differences for each covariate, normalised differences are presented to aid comparison between covariates that have different units, and to facilitate comparisons across studies. The normalised difference is defined as the difference in means between two groups, divided by the pooled standard deviation. The estimate of this quantity is given by the following equation (Imbens & Rubin 2015, p.311).

$$\hat{\Delta}_{ct} = \frac{\bar{X}_t - \bar{X}_c}{\sqrt{(s_c^2 + s_t^2)/2}} \tag{6}$$

where:

- $\bar{X}_t$ is the mean value of the variable in the treatment group;
- $\bar{X}_c$ is the mean value of the variable in the comparison group;
- s_t is the standard deviation of the variable in the treatment group; and
- s_c is the standard deviation of the variable in the comparison group.

4.6.4   Estimating the effects of the intervention

To reduce potential bias further, effects are estimated using a difference-in-differences analysis. This process involves taking two differences to account for two sources of potential bias. First, the difference is taken between the values of the outcome variable before and after the intervention, in the intervention

group. This removes any bias from the estimate that is due to factors that are constant over time (for example, the family background and gender of participants), as it compares the outcome at two points in time for the same group of people. However, this 'first difference' is not a good estimate of the effect of the intervention, as it could be confounded by other factors that vary over time (such as individuals' participation in other developmental activities or their sense of self-efficacy). To account for these factors, the before-and-after change in outcomes is also calculated in the comparison group. This is known as the 'second difference'. Finally, the difference is calculated between the first difference and the second difference, giving an estimate of the effect of the intervention.

This is displayed in Figure 4.2, where t_0 is the time that the intervention starts, t_{-1} is a time before that and t_1 is the time that the intervention stops. The unbroken blue and orange lines represent the trajectory of the mean outcome in the intervention group and comparison group respectively.

Figure 4.2: Explaining the difference-in-differences estimator
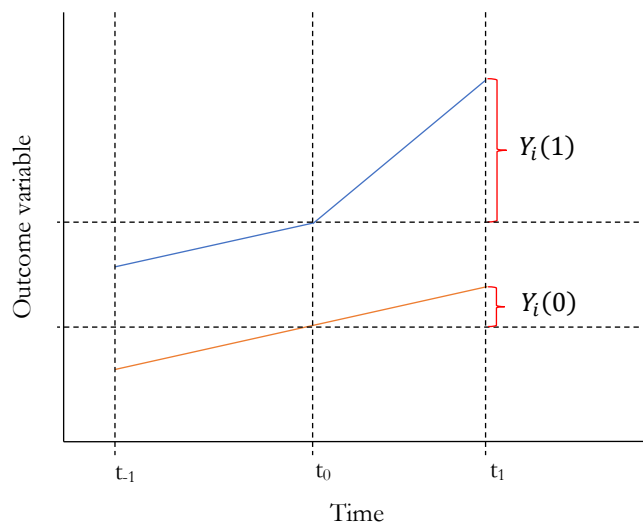
This figure reveals a key assumption behind the difference-in-differences estimator; that the rate of change of the outcome in the absence of the intervention is the same in both groups. The plausibility of this 'parallel trends' assumption is usually assessed by observing the trajectories of the outcome in each group prior to baseline (between t_{-1} and t_0). Unfortunately, this data was not collected as part of the NCS Impact Survey, so it is not possible to test the parallel trends assumption in this study.

To implement the DiD estimator, the average treatment effect on the treated (ATT) is estimated for each outcome of interest in two steps. First, the change in the outcome is calculated for all units (represented by $Y_i(1)$ for the treatment group and $Y_i(0)$ for the comparison group). Then, a weighted difference in mean averages – between the treatment and comparison groups – is calculated based on the matching procedure described above.[25]

4.7 Results

4.7.1 Introduction

Two types of results are presented here. First, sections 4.7.2 to 4.7.4 give the results of the analysis used to select the propensity score model, the key properties of this model, and the properties of the sample that results from matching. These findings show that matching achieves a well-balanced sample. Second, section 4.7.5 presents the estimated effects of NCS on the outcomes of interest. These results provide support for all of the study's hypotheses. The findings are robust to a wide range of alternative specifications, as well as corrections for multiple comparisons, and the results of these robustness checks are reported in Annex I at the end of this chapter.

4.7.2 Estimating and matching on the propensity score

In total, 20 different sets of covariates are tested to estimate the propensity scores, starting with all baseline covariates, then using the rules specified above to remove one variable at a time from the model, and evaluating the common support each time. This analysis brings us to an optimal specification, given the data, that contains the following six baseline variables.

- *bvote*, intention to vote on a scale of 0 to 9.
- *bgcse*, a binary indicator of whether the individual was studying for GCSEs before the summer.
- *bpetition*, a binary indicator of whether the individual organised a petition or event to support an issue in the 3 months prior to baseline.

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[25] See Abadie & Imbens 2016 for the derivation and specification of the nearest neighbour matching estimator and the associated robust standard errors.

- *bcontact*, a binary indicator of whether the individual contacted a politician in the 3 months prior to baseline.
- *bhelp*, a binary indicator of whether the individual helped a non-family member with a university or job application in the 3 months prior to baseline.
- *bvote_nosay*, a binary indicator of whether the individual chose to report their intention to vote.

Table 4.3 reports the parameter estimates for this specification of the propensity score.

Table 4.3: Estimated parameters of the propensity score

	Estimate	(Standard Error)
bvote	-0.063***	(0.014)
bgcse	2.146***	(0.098)
bpetition	-0.243	(0.199)
bcontact	-0.287+	(0.167)
bhelp	-0.502***	(0.102)
bvote_nosay	0.522	(0.814)
Constant	-0.243+	(0.133)

Notes: N = 5,486. Pseudo r-squared = 0.10.
+ p<0.1 * p<0.05, ** p<0.01, *** p<0.001

When the matching procedure is followed, 25 observations are trimmed from the sample as they are not on the common support as defined by the calipers. This leaves a sample of 5,461 units. The magnitude and standard errors of the parameter estimates above remain the same after this trimming. Figure 4.3 presents the common support graphically. This plot shows that a large proportion of treated units (relative to the comparison group) have a propensity score of around 0.8. This means that some control units in this region are used in a relatively large number of matches. This is accounted for in the weighting procedure that adjusts the treatment effect estimates based on the frequency of reuse of comparison units.

Figure 4.3: Kernel density plot of propensity scores by treatment group
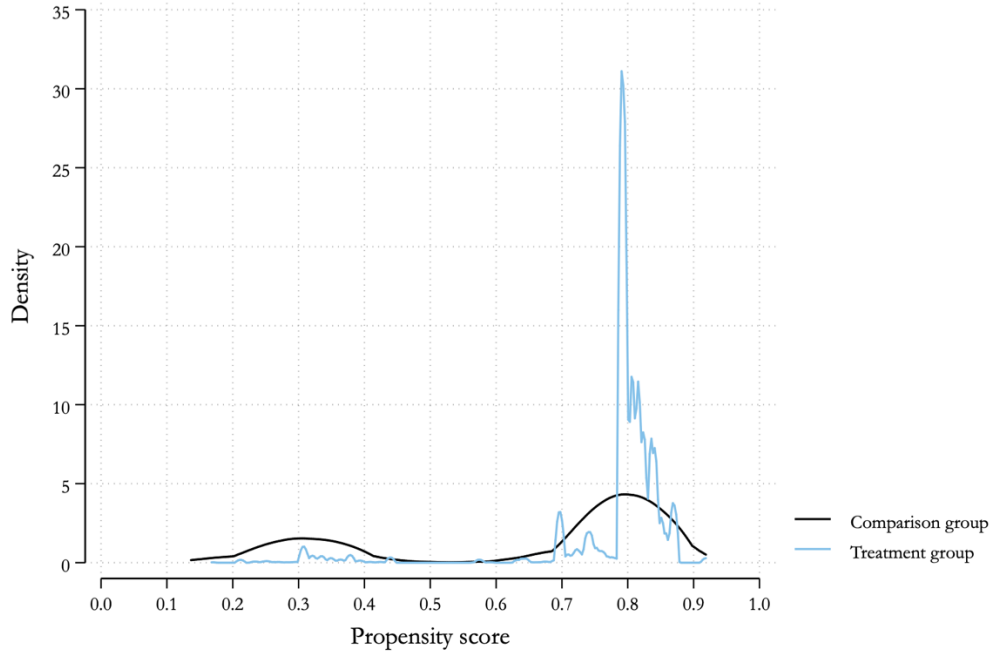
4.7.3 Assessing the unconfoundedness assumption

The ATT is estimated for four pseudo-outcomes – the baseline indicators of service-based participation described in section 4.7.2 above – using the same propensity score model, matching procedure and estimator that is used in the real outcome analysis. The results of these tests are presented in Table 4.4.

Table 4.4: ATT estimates for pseudo-outcomes

	Estimate	(Standard Error)	Normalised difference, $\hat{\Delta}_{ct}$
bhelpclub	0.044**	(0.015)	0.09
bhelporg	0.002	(0.012)	0.007
bhelpother	-0.001	(0.013)	-0.004
braise	0.006	(0.014)	0.015

Notes: N = 5,461. $\hat{\Delta}_{ct}$ has been calculated using the standard deviation of the total sample on common support, as opposed to the pooled standard deviation as defined in Equation 6. It is therefore only an approximation of Cohen's d. + $p<0.1$ * $p<0.05$, ** $p<0.01$, *** $p<0.001$

The results show that participants in the intervention group are 4 percentage points (pp) more likely to have helped at a club, group, organisation or place of worship in the 3 months prior to baseline (p = 0.005, CI [1.3, 7.4]). This suggests some evidence of unobserved confounders. This effect represents a normalised difference of 0.09, which is approximately half the size of the effect on the primary outcome ($\hat{\Delta}_{ct}$ = 0.19). Effects on the other three pseudo-outcomes are

small and not significant at the 95% confidence level. Overall, the tests provide some support for the plausibility of the unconfoundedness assumption. It is possible though, that the effects presented below may be slightly biased due to unobserved confounders. Any bias would probably be in the upwards direction, as helping behaviour is likely to be correlated with most outcomes of interest and is a component part of the composite service-based participation outcome. If there are unobserved confounders that are time-invariant, then the DiD analysis will remove the bias that they cause, but factors that vary over time will only be accounted for if the parallel trends assumption holds.

4.7.4 Assessing post-match balance

After matching, the normalised differences between treatment and comparison groups are calculated for the mean values of the of covariates in the propensity score model. The results are presented in Table 4.5.

Table 4.5: Post-matching balance checks

	Mean		Normalised difference, $\hat{\Delta}_{ct}$
	Treatment group	Comparison group	
bvote	6.910	6.924	-0.006
bgcse	0.957	0.957	0.000
bpetition	0.022	0.016	0.038
bcontact	0.033	0.029	0.018
bhelp	0.084	0.087	-0.008
bvote_nosay	0.000	0.000	0.000
Notes: N = 5,461			

Normalised differences with a magnitude of 0.1 or less indicate a negligible correlation between the covariate and the group (Austin 2009, p.1233). In this case, all normalised differences are at least an order of magnitude smaller than this threshold (-0.000 < $\hat{\Delta}_{ct}$ < 0.038). The post-matching sample is therefore well-balanced across groups on all covariates.

4.7.5 Estimating the effects of service learning

4.7.5.1 Main analysis

The research questions and hypotheses are summarised in Table 4.6.

Table 4.6: Summary of hypotheses

Research questions
What is the effect of service learning on young people's political participation? (RQ1)
If there is an effect, how is it produced? (RQ2)

Hypotheses
H1.1: Service learning will increase general civic self-efficacy.
H1.2: Service learning will increase service-based civic participation.
H2.1: Service learning will increase political self-efficacy.
H2.2: Service learning will increase political participation.
H3.1: The effect on general civic self-efficacy will be greater than the effect on political self-efficacy.
H3.2: The effect on service-based civic participation will be greater than the effect on political participation.

To test these hypotheses, the ATT is estimated for the primary and secondary outcomes and the results are presented in Figures 4.4 and 4.5, and Table 4.7. Table 4.8 presents the same results, but with the estimated mean averages for each outcome at baseline and endpoint, broken down by treatment group. The two types of self-efficacy were measured using 5-point Likert scales, so *diffciveff* and *diffpoleff* can take values between -4 and 4. The two types of civic participation are measured using combined scores that are standardised to take a value between 0 and 1, so *diffserpart* and *diffpolpart* can take values between -1 and 1. To make the results easier to compare, normalised differences (also known as 'effect sizes' or 'Cohen's d') are presented in the final column of the table.[26] The findings below provide strong support for all hypotheses.

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[26] Note that this normalised difference, or 'Cohen's d', has been calculated using the standard deviation of the total sample on common support, as opposed to the pooled standard deviation as defined in Equation 6. This approximation is necessary because it is not possible to calculate the standard deviation in the comparison group, as some units are used more than once to estimate the effects. Calculating the standard deviation of the 'effective comparison group' – i.e. a synthetic dataset containing multiple observations for some individuals – is also not appropriate because duplicated observations are not independent. Assuming such independence would mean that the variance of the mean decreases with the increased sample size, which is incorrect. An approximation is therefore presented here for readers who are interested in getting a rough sense of the effects in terms of standard deviations. However, the findings and the conclusions presented do not rely on this quantity.

Figure 4.4: Effects on participation
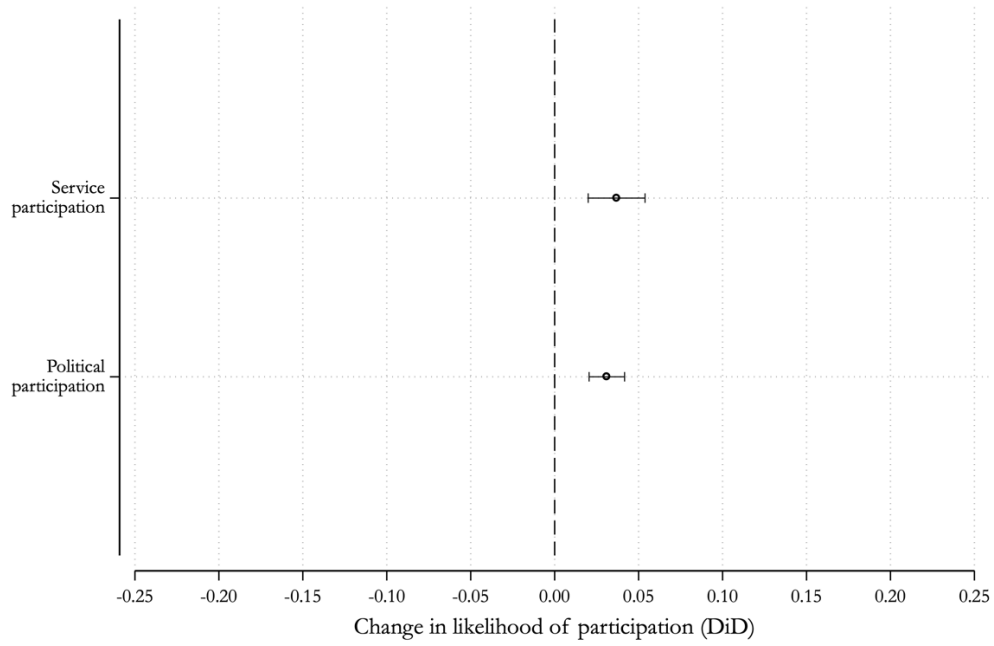
Figure 4.5: Effects on self-efficacy
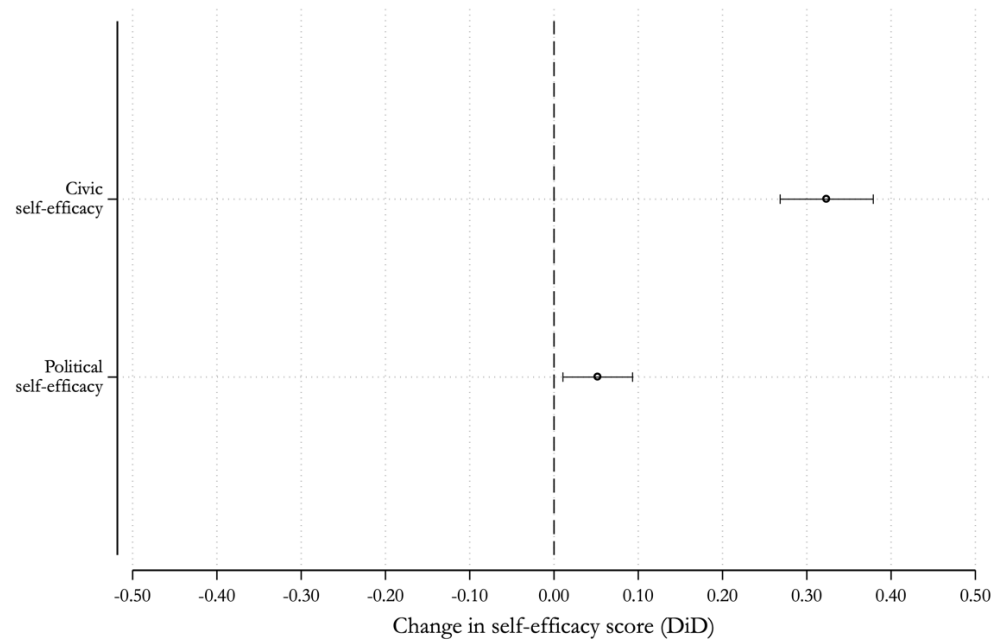
Table 4.7: ATT estimates (DiD) for the outcomes of interest

	Estimate	(Standard Error)	Normalised difference, $\hat{\Delta}_{ct}$
Civic self-efficacy	0.32***	(0.028)	0.39
Service participation	0.037***	(0.009)	0.14
Political self-efficacy	0.052*	(0.021)	0.08
Political participation	0.031***	(0.005)	0.19

The results support H1.1. They show that this particular experience of service learning (NCS) increased participants' sense of general civic self-efficacy by an average of 8pp[27] on the combined scale (p = 0.000; 95% CI [6.7, 9.5]); a 13% increase on the baseline average in the treatment group. Participants of NCS are also estimated to be 3.7pp more likely to participate in service-based civic activities after NCS (p = 0.000; 95% CI [2.0, 5.4]); an 18% increase on the baseline average in the treatment group, in line with H1.2. The results also offer support for H2.1 and H2.2. NCS is estimated to increase political self-efficacy by 1.3pp on the combined scale (p = 0.01; 95% CI [0.27, 2.3]); a 3.9% increase on the baseline average in the treatment group. Participants are also 3.1pp more likely to participate in political activities after NCS (p = 0.000; 95% CI [2.0, 4.1]); a 12% increase on the baseline average in the treatment group. As predicted by H3.1 and H3.2, the service-based effects are greater than their political counterparts in absolute terms. This is particularly clear for the two types of self-efficacy. The difference is less clear when comparing service-based and political participation, where overlapping confidence intervals mean that these effects may in fact be the same size.

Table 4.8 suggests that participants in both the treatment and comparison groups were more engaged in political activities than service-based volunteering prior to NCS. This is possible, but may also be a result of measurement error; particularly as the composite political participation variable contains an intended behaviour (intention to vote), whereas its service-based counterpart does not. These results should also be considered in the context of participation rates in the general population, which show a clear preference for volunteering over political participation amongst young people in England. This is discussed in more detail in the conclusions.

Table 4.8: Estimated averages for the outcomes of interest by treatment group

	Treatment group	Comparison group

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[27] Note that the scales for all the self-efficacy items are not presented as 0 to 100 on the survey. The findings are presented here as percentage points to aid comprehension and comparability.

	Baseline mean	Endpoint mean	Baseline mean	Endpoint mean
Civic self-efficacy	2.415	2.686	2.260	2.209
Service participation	0.210	0.248	0.197	0.199
Political self-efficacy	1.318	1.406	1.268	1.305
Political participation	0.264	0.297	0.262	0.264

Notes: N = 5,461.
+ p<0.1 * p<0.05, ** p<0.01, *** p<0.001

4.7.5.2 *Exploratory analysis*

To explore in more detail what is driving these effects, three pieces of (pre-specified) exploratory analysis are presented below, breaking down the composite indicators into their component parts. First, Figure 4.6 and Table 4.9 show the effects on political participation, broken down by participation type. The variables representing the non-electoral forms of participation are all binary, and the intention to vote scale has been normalised to aid comparability. This analysis suggests that the effect on political participation may be driven disproportionately by an increase in petition signing and protest, whose individual effects are both larger than the average (represented by the combined score, above). However, overlaps in some of the confidence intervals mean that we cannot rule out the idea that some of the effects are in fact the same size.

Figure 4.6: Effects on political participation by type
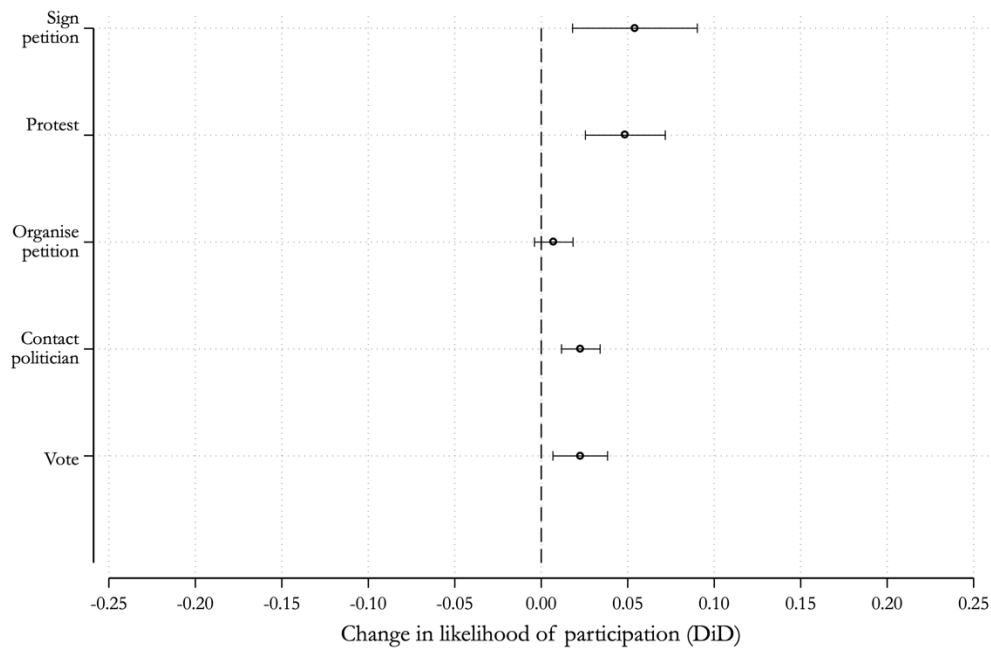
Table 4.9: ATT estimates (DiD) for effects by type of political participation

	Estimate	(Standard Error)	Normalised difference, $\hat{\Delta}_{ct}$
Sign petition	0.054**	(0.018)	0.10
Protest	0.049***	(0.012)	0.14
Organise petition	0.007	(0.006)	0.03
Contact politician	0.023***	(0.006)	0.10
Vote	0.023**	(0.008)	0.09

Notes: N = 5,461. $\hat{\Delta}_{ct}$ has been calculated using the standard deviation of the total sample on common support, as opposed to the pooled standard deviation as defined in Equation 6. It is therefore only an approximation of Cohen's d. + $p<0.1$ * $p<0.05$, ** $p<0.01$, *** $p<0.001$

Second, Figure 4.7 and Table 4.10 show the effects on service-based participation, broken down by participation type. This analysis suggests that NCS has no effect on participants' rate of helping at clubs, groups or places of worship. Helping other people or improving the local area, helping organisations other than clubs, groups or places of worship, and fundraising for charity seem therefore to be driving the overall effect on service-based participation. However, these results are less informative than those on political participation as the 'helping' categories of service-based activity are not well delineated.

Figure 4.7: Effects on service participation by type
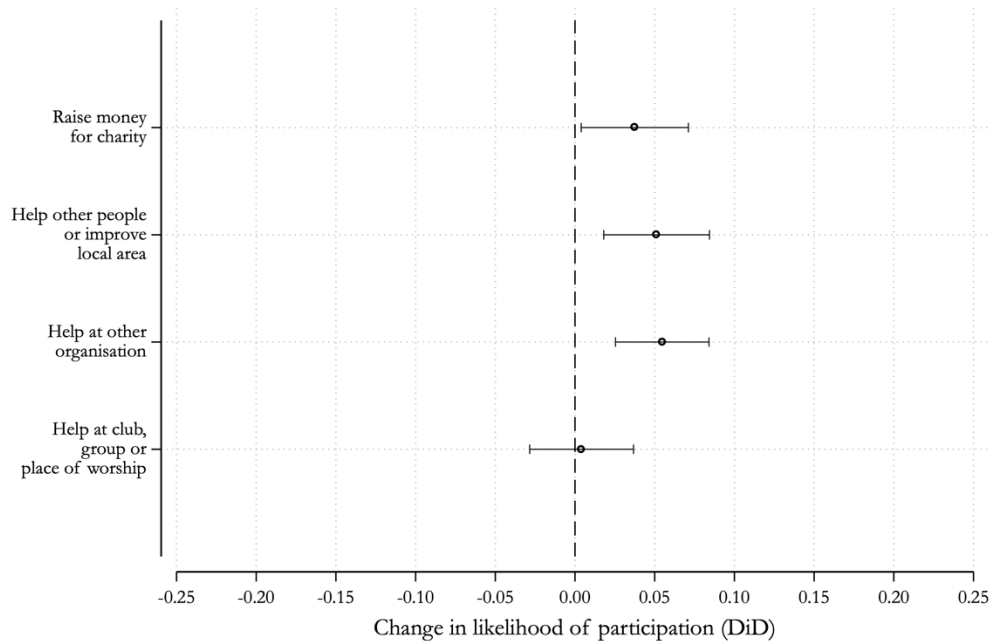
Table 4.10: ATT estimates (DiD) for effects by type of service participation

	Estimate	(Standard Error)	Normalised difference, $\hat{\Delta}_{ct}$
Help others	0.051**	(0.017)	0.10
Help club	0.004	(0.017)	0.01
Help other org	0.055***	(0.015)	0.12
Raise money	0.038*	(0.017)	0.07

Notes: N = 5,461. $\hat{\Delta}_{ct}$ has been calculated using the standard deviation of the total sample on common support, as opposed to the pooled standard deviation as defined in Equation 6. It is therefore only an approximation of Cohen's d. + p<0.1 * p<0.05, ** p<0.01, *** p<0.001

Third, Figure 4.8 and Table 4.11 show the effects on civic and political self-efficacy, broken down by the two dimensions of self-efficacy. This analysis suggests that, in both cases, the effects on self-efficacy are driven more by the internal dimension, with a precisely estimated null effect for external political self-efficacy.

Figure 4.8: Effects on self-efficacy by dimension
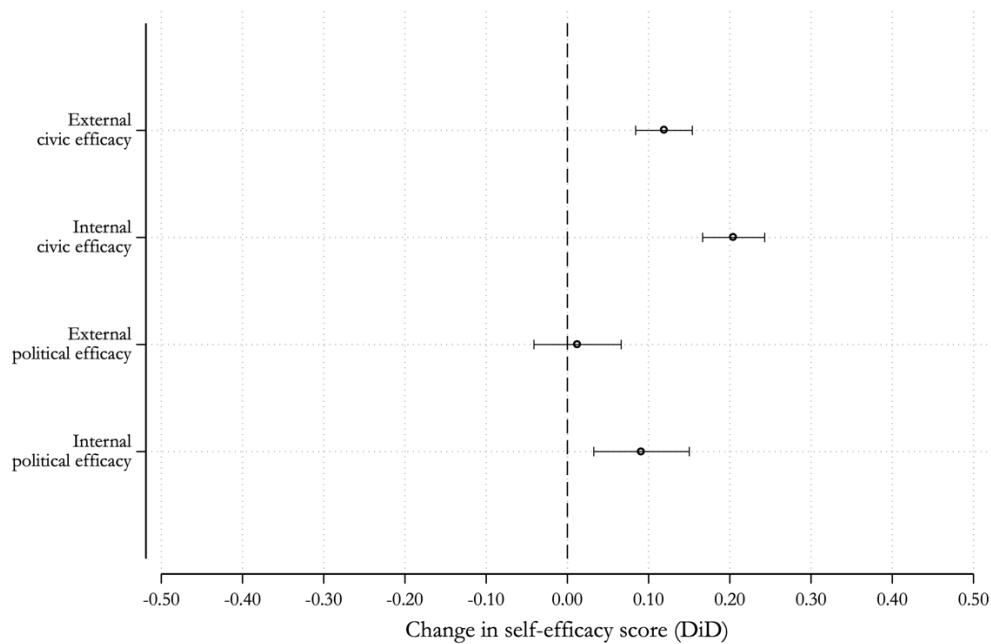
Table 4.11: ATT estimates (DiD) for effects by dimension of self-efficacy

	Estimate	(Standard Error)	Normalised difference, $\hat{\Delta}_{ct}$
External civic efficacy	0.112***	(0.018)	0.23
Internal civic efficacy	0.205***	(0.019)	0.37
External political efficacy	0.013	(0.027)	0.02
Internal political efficacy	0.091**	(0.030)	0.11

4.8 Conclusions

This study set out to answer two questions: i. 'What is the effect of service learning on young people's political participation?' (RQ1); and ii. 'If there is an effect, how is it produced?' (RQ2). It was hypothesised that service learning would have a positive effect on political participation, and that this would be accompanied by positive effects on participants' general civic self-efficacy, service-based civic participation and political self-efficacy. The approach taken to testing these hypotheses makes it the most robust study to date on this topic. The combination of a large sample, a strong matching procedure, a DiD analysis and outcomes measured through reports of actual past behaviour (rather than future intentions) distinguishes it from previous research in this field. The results provide support for all of the study's hypotheses. This experience of service learning (NCS) did increase participants' sense of general civic self-efficacy and service-based participation (H1.1 and 1.2). It also increased participants' sense of political self-efficacy and political participation (H2.1 and 2.2). And, as predicted by H3.1 and 3.2, the general and service-based effects were larger than the political effects.[28] These are important contributions to the literature. Until now, there has been no robust estimate of the effect of service learning on political participation (or the secondary outcomes measured here). This lack of strong evidence has been accompanied by speculation in the literature that service learning could, on average, have a negative effect on political participation. The results of this study suggest that this is very unlikely. The claim that NCS (as a specific intervention) might discourage political participation (Mycock & Tonge 2011, pp.63-64; Bacon et al. 2013; Bulley & Sokhi-Bulley 2014) is now shown to be particularly unfounded in terms of an average effect.

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[28] These results are robust to a wide range of different analytic specifications. However, some evidence of potential upward bias is identified in the testing of the unconfoundedness assumption and, in the absence of trend data prior to baseline, it is impossible rule out plausible time varying confounders like participant motivation (which could be changing at a faster rate in the treatment group). Both of these facts suggest that the effects presented could be over-estimates, but there is no way of confirming or quantifying this.

Not only are the effects on political participation positive, but they also seem quite large; especially for non-electoral participation. Effects of 5.4pp for petition-signing, 4.9pp for protest attendance and 2.3pp for contacting politicians are substantial when we consider that only 25% of 16- to 25-year olds in England are estimated to take part in at least one of these activities in a year (Cabinet Office 2016). If the estimated average effect of NCS on non-electoral political participation were realised in the wider English population of 16-25 year olds, it would make them the second-highest participating age group (as opposed to the second lowest as they are currently). Recent experimental work has suggested that interventions that develop noncognitive skills can have a positive effect on voter turnout (Holbein & Hillygus 2020), but this study's findings on non-electoral participation are particularly new.

The positive effects on both political and service-based participation were predicted to come about because of increases in self-efficacy. There is partial support in the findings for this explanation. The effect on general civic self-efficacy (8pp or a 12% increase on the baseline average in the treatment group) seems quite large when compared with similar interventions targeting similar cohorts. For example, an RCT of a youth social action programme for 16- to 19-year-olds found an effect of 5pp (or a 7% increase on the control group average) on a similar construct (Kirkman et al. 2016, p.23).[29] We can say therefore that service learning substantially increases participants civic self-efficacy as well as their service-based participation. We cannot say that the former causes the latter, but the literature on self-efficacy provides strong evidence that enactive mastery experiences enhance domain-specific self-efficacy, which in turn leads to increased participation in that domain (Bandura 1997, p.79), and this finding is line with that idea.

The efficacy-based explanation suggested in the hypotheses for the increase in political participation seems less secure. The effect of NCS on political self-efficacy is the smallest of all the outcomes measured (1.3pp or a 3.9% increase on the baseline average in the treatment group). So, it does not look like gains

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[29] The nearest outcome in this study was labelled 'problem solving'. This outcome was composed of some of the same survey items used to construct the general civic self-efficacy measure in the current study, but also included some items relating to skills for solving problems.

in civic self-efficacy are spilling over much into the domain of political self-efficacy. This is contrary to what some have suggested (Condon & Holleque 2013, p.168; Reinders & Youniss 2006). The exploratory analysis on the two dimensions of self-efficacy (internal and external) provides some clues as to why this might be the case. This analysis shows that NCS has no effect on participants' external political self-efficacy. After taking part in service learning, young people are no more likely to believe that the political system is responsive to their actions. This is probably because service learning is focused on non-political forms of participation, whose outcomes are also non-political. Participants do experience an increase in their sense of internal political self-efficacy. This finding provides support for the idea in the literature that some of the capabilities developed through non-political civic participation are transferable (Verba et al. 1995; Holbein & Hillygus 2020). It could be that a spillover is occurring only on this internal dimension. However, the effect on internal political self-efficacy is still small so seems unlikely to account for the relatively large increase that we see in political participation. All this seems to confirm the suspicion held by some that the 'simple political spillover thesis' is problematic (Ayala 2000, p.101). Study 2 investigates the question of causal mechanisms in more detail and concludes that civic and political self-efficacy, while important factors in general for political participation, are not doing the heavy lifting when it comes to the causal relationship between service learning and political participation.

As well as making significant contributions to the academic literature, the findings from this study have important implications for policy and practice. They tell us that service learning can be an effective way of increasing political participation amongst young people. They also point to an opportunity for a larger effect. NCS (like other programmes of service learning) currently focusses on giving young people an experience of service-based civic participation. One result of this is a larger effect on service-based participation over political participation, post-service (an increase of 18% vs. 12%). With rates of formal and informal volunteering already very high amongst 16- to 25-year olds (49% and 69% respectively (Cabinet Office 2016)), and rates of political participation very low among this age group, we might question why national, government-funded programmes do not focus more on the political. The results here suggest that there is a great opportunity to develop these programmes in ways that

increase young people's political self-efficacy, which should in turn increase their political participation even more. In particular, the findings point to a need for enactive mastery experiences of political participation on-programme, that have clear and positive outcomes and, therefore, a positive effect on participants' external political self-efficacy.

4.9 Annex I: Robustness checks

Four categories of robustness check are carried out on the primary analysis for Study 1: i. variations on the sample; ii. alternative specifications *of* nearest neighbour PSM; iii. alternatives *to* nearest neighbour PSM; and iv. coarsened exact matching (CEM) instead of PSM. This includes all checks specified in the PAP (Taylor 2020a, p.19), and three additional checks that have been added for extra security after seeing the data (see Appendix III for an explanation of these additions). The results of these checks provide strong support for the findings from the primary analysis.

4.9.1 Variations on the sample

Estimates of the effects are presented in Table 4.12 based on six different variations on the sample: (1) the sample used in the primary analysis; (2) the sample that results from applying wider calipers (0.2SD of the propensity score); (3) the sample that results from applying narrower calipers (0.05SD of the propensity score); (4) the sample that results from trimming observations with extreme values of the propensity score (Imbens 2015, pp.393-394); (5) the sample that results from excluding the online panel that was used to boost the comparison group; and (6) the sample that results from excluding the individuals who responded "Don't know" or "Prefer not to say" to any items used to construct the outcome measures.[30]

Table 4.12: Testing sensitivity of results to variations on the sample

	(1) Primary	(2) Wide	(3) Narrow	(4) Trimmed	(5) No Panel	(6) No Abs.
Civic self-efficacy	0.32***	0.32***	0.32***	0.32***	0.33***	0.32***
	(0.028)	(0.028)	(0.028)	(0.028)	(0.029)	(0.028)
Service participation	0.037***	0.037***	0.036***	0.037***	0.038***	0.037***
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Political self-efficacy	0.052*	0.052*	0.053*	0.052*	0.053*	0.055*
	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)
Political participation	0.031***	0.031***	0.031***	0.031***	0.034***	0.031***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
N observations	5,461	5,463	5,409	5,461	5,209	5,320

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[30] In the primary analysis, zeros are imputed for responses of this type.

This analysis shows that the results are not sensitive to a wide range of sample definitions. All revised point estimates and standard errors are either identical (to 2 s.f.) or very close to those calculated in the primary analysis.

4.9.2 Alternative specifications of nearest neighbour PSM

Next, three different specifications of nearest neighbour matching are compared: (1) that used in the primary analysis; (2) matching without replacement; and (3) a different combination of covariates is used to estimate the propensity scores. For the third of these specifications, the covariates used are those specified in the PAP as the best-guess model before seeing the data, based on *ex ante* knowledge of the intervention, the outcomes and the probable selection mechanisms (Taylor 2020a, pp.13-14). This model includes: the baseline values of the outcome variables, gender, and free school meal (FSM) status (an indicator of socio-economic status).[31] The results of these checks are presented in Table 4.13.

Table 4.13: Testing sensitivity of results to different specifications of nearest neighbour matching

	(1) Primary	(2) No Replace	(3) Diff Covars
Civic self-efficacy	0.32***	0.35**	0.39***
	(0.028)	(0.107)	(0.032)
Service participation	0.037***	0.032	0.028**
	(0.009)	(0.037)	(0.010)
Political self-efficacy	0.052*	0.007	0.063*
	(0.021)	(0.092)	(0.026)
Political participation	0.031***	0.027	0.025***
	(0.005)	(0.026)	(0.006)
N observations	5,461	1,442	5,468

Notes: N = 5,461.
+ p<0.1 * p<0.05, ** p<0.01, *** p<0.001

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[31] The PAP also specified that the region of England in which the individual participated would be an important variable. However, this variable was not present in the dataset that was shared with the researcher.

The two alternative specifications to the primary analysis produce some different results. When matching without replacement is performed, four out of five of the effects lose their significance and the point estimate for political self-efficacy reduces by an order of magnitude. However, there is a reason that this specification was not chosen for the primary analysis; when untreated units are only allowed to be matched with a single treated unit, the number of treated units that are on the common support to drops to 66. This procedure discards 4,044 units from the analysis (74% of the total sample and 98% of the treated sample). The alternative to dropping these units (but maintaining matching without replacement) is to broaden the calipers so that worse quality matches are allowed, but this reduction in the quality of matches would substantially increase the bias in the estimates. When the propensity scores are distributed as they are in this study, matching with replacement is the natural choice (Dehejia & Wahba 2002, p.154). The primary analysis – using matching with replacement – only discards 25 units so retains substantially more information and, while the reuse of some untreated units could introduce bias, this issue is accounted for by a weighting regime that adjusts the estimates based on the frequency of reuse. For these reasons, the results of the primary analysis are preferred.

When the alternative covariate model is used to estimate the propensity score, the results are similar to those from the primary analysis, but the point estimates differ slightly. This alternative model was tested in the refinement process described in section 4.6.3.1 above. These tests revealed imbalance on five of the variables used to predict the propensity scores. For this reason, the model was rejected and the results from the primary analysis are preferred (though model (3) does provide broad support for the primary analysis anyway).

4.9.3   Alternatives to nearest neighbour PSM

The primary analysis matches each treated unit with the untreated unit that has the closest propensity score (its 'nearest neighbour'), within the calipers. In the following set of robustness checks, three alternatives to nearest neighbour PSM are implemented: radial matching, kernel matching and inverse probability weighting (IPW). Radial matching matches each treated unit with *all* untreated units that lie within the calipers (Dehejia & Wahba 2002, pp.153-154). Kernel matching produces a counterfactual for each treated participant, based on a weighted average of all members of the comparison group, where the weights

are dependent on the distance between the participant and each untreated unit (Smith & Todd 2005, pp.316-317). Inverse probability weighting calculates the difference between two weighted averages (in the treatment and comparison group), with weights based on the propensity scores (Hirano & Imbens 2002, pp263-264). Table 4.14 presents the results.

Table 4.14: Testing sensitivity of results to alternatives to nearest neighbour matching

	(1)	(2)	(3)	(4)
	Primary	Radial	Kernel	IPW
Civic self-efficacy	0.32***	0.32***	0.32***	0.33***
	(0.028)	(0.028)	(0.028)	(0.028)
Service participation	0.037***	0.038***	0.039***	0.037***
	(0.009)	(0.009)	(0.009)	(0.008)
Political self-efficacy	0.052*	0.056**	0.061**	0.048*
	(0.021)	(0.021)	(0.021)	(0.021)
Political participation	0.031***	0.033***	0.036***	0.033***
	(0.005)	(0.006)	(0.006)	(0.005)
N observations	5,461	5,485	5,486	5,486

Notes: Standard errors are in brackets.
+ $p < 0.1$ * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

In general, these tests provide strong support for the primary analysis. The magnitude of the effect on political self-efficacy varies slightly, with kernel weighting estimating a larger effect and IPW estimating a smaller effect. However, both of these alternative point estimates lie well within the confidence interval (CI[0.011, 0.093]) estimated in the primary analysis. We can conclude therefore that the results from primary analysis are not sensitive to a range of alternative approaches to matching within PSM.

4.9.4 Coarsened exact matching

The final check of the robustness of the primary analysis is to use coarsened exact matching (CEM) instead of PSM. PSM collapses the covariates used to predict uptake into a single score for each individual. CEM, in contrast, matches individuals based on the exact values of each covariate after they have been coarsened into categories. In this case, the coarsening process is straightforward, as all but one of the variables used to estimate the propensity scores in the primary analysis are binary. The only variable that needs coarsening is baseline intention vote (*bvote*). This was measured on a 1 to 10 Likert scale, with no

obvious cut-offs, so the automatic cut-off algorithm in Stata's `cem` command is used to define the coarsening categories here (Blackwell et al. 2010). Table 4.15 presents the estimates produced after CEM.

Table 4.15: Testing sensitivity of results to an alternative to PSM

	(1)	(2)
	Primary	CEM
Civic self-efficacy	0.32***	0.32***
	(0.028)	(0.028)
Service participation	0.037***	0.038***
	(0.009)	(0.009)
Political self-efficacy	0.052*	0.047*
	(0.021)	(0.022)
Political participation	0.031***	0.033***
	(0.005)	(0.005)
N observations	5,461	5,417

Notes: Standard errors are in brackets.
+ p<0.1 * p<0.05, ** p<0.01, *** p<0.001

Again, strong support is found for the results from the primary analysis. The sample is smaller under CEM, as expected with a stricter matching procedure, but only 69 units (1% of the total sample) are lost. After these units are dropped, the $\mathcal{L}_1$ statistic – a multivariate measure of global imbalance (Iacus et al. 2012, p.7) – is effectively zero, indicating almost perfect covariate balance (up to coarsening) (Blackwell et al. 2010, p.6). The estimates produced by CEM therefore seem robust and are either exactly the same (to 2 s.f.) or very similar to those produced by the primary analysis.

4.9.5 Multiple comparison corrections

This study estimates three groups of treatment effects: primary, secondary and exploratory effects. For every estimation made, there is a chance that a statistically significant result will be found by chance, i.e., a 'false discovery' will be made. The more statistical comparisons that are made, the greater the chance of making such a false discovery. To account for this, the Benjamini-Hochberg procedure (Benjamini & Hochberg 1995) is applied to adjust the significance threshold for each test, based on the number of comparisons made in the group, increasing the burden of proof for each additional comparison. This procedure

involves ordering the p-values for a group of estimates. All estimates with p-values that satisfy the following inequality are considered to be significant:

$$p_k \leq \frac{k}{m} \alpha \qquad (7)$$

where:

- p_k is the k-th smallest p-value;
- m is the number of comparisons in the group; and
- α is the pre-defined significance threshold (in this case $\alpha = 0.05$).

Table 4.16 presents the results when this correction is applied to the significance thresholds for the three groups of tests in Study 1.

Table 4.16: Corrections for multiple comparisons (Study 1)

	p-value	Family	k	Sig. at α = 0.05?	Correction to α	Sig. after correction
Political participation	8.02e-09	Primary	1	Yes	0.050	Yes
Civic self-efficacy	1.69e-30	Secondary	1	Yes	0.017	Yes
Service participation	1.93e-05	Secondary	2	Yes	0.033	Yes
Political self-efficacy	1.38e-02	Secondary	3	Yes	0.050	Yes
Protest	3.70e-05	Explore 1	1	Yes	0.010	Yes
Contact politician	5.78e-05	Explore 1	2	Yes	0.020	Yes
Sign petition	3.23e-03	Explore 1	3	Yes	0.030	Yes
Vote	5.15e-03	Explore 1	4	Yes	0.040	Yes
Organise petition	2.03e-01	Explore 1	5	No	0.050	No
Help other org	2.57e-04	Explore 2	1	Yes	0.013	Yes
Help others	2.47e-03	Explore 2	2	Yes	0.025	Yes
Raise money	2.87e-02	Explore 2	3	Yes	0.038	Yes
Help club	8.03e-01	Explore 2	4	No	0.050	No

This shows that the results in the main analysis are not sensitive to this correction for multiple comparisons.

5 Study 2: Developing a theory

5.1 Introduction

This study aims to identify the causal mechanisms that link a service learning experience to an individual's future political participation, as well as the factors that moderate this effect. Study 1 begins to answer the question of mechanisms by estimating the effects of NCS on civic and political self-efficacy. These tests suggest that a self-efficacy spillover effect – from the general civic domain to the political – is unlikely to account for the relatively large effects that NCS seems to have on political participation. However, this analysis is limited to the outcomes measured in the survey, and to the necessarily reductive quantitative indicators used to measure these outcomes, so is unable to say any more on the matter. It is also unable to tell us whether the self-efficacy effects that are observed are causally linked to political participation. Indeed, no quantitative research design is capable of definitively testing causal relationships between the outcomes of an intervention. This is because the observed proximal outcomes, such as increased general civic self-efficacy, could be systematically related to other unobserved variables that influence political participation (Montgomery et al. 2016). For these reasons, the strongest experimental design always leave us with a limited picture of the processes that link an activity to an outcome.

Qualitative methods can help to improve inferences about causal processes because they allow the in-depth exploration of individual cases. This supports the investigation of factors that go beyond the limited number of variables in a quantitative dataset. It is also an approach does not rely on reductive measurement tools such as closed question surveys (Brady & Collier 2004, p.9). This present study therefore adopts a qualitative design that explores NCS participants' perceptions of causal mechanisms and moderating factors in depth, and in a small number of cases.

The findings make two important general contributions to the literature on service learning and young peoples' political participation: i. they reveal substantial heterogeneity in the effects of service learning on political participation; and ii. they add important detail to our understanding of *how* these effects are produced. Two substantive results within these general findings may

be particularly surprising to some. First, an increase in social self-efficacy seems to be the most powerful mediating mechanism. Second, gains in service self-efficacy can lead to losses in political self-efficacy and, therefore, a reduction in political participation for some individuals. Preventing such losses – and encouraging positive spillovers – is difficult and requires new activities and conditions to be present in service learning. These contributions to the literature are also important to policy makers and practitioners who want to increase youth political participation through programmes of service.

Together, Study 1 and Study 2 develop a broad and detailed picture of the relationship between service learning and political participation; with the former identifying the causal effect and the latter giving us a greater depth of insight into how this effect is produced.

5.2 Existing theory

The aim of this study is to begin to fill a theoretical gap in the literature on the causal relationship between service learning and political participation. Because of this gap, the research necessarily involves an inductive process, with a theory being developed from the new data collected. However, while there was a lack of detailed and well-tested theory in this domain prior to data collection, the review of the literature does provide some starting points for a theory. These theoretical starting points are described as part of pre-analysis plan (PAP) for this study that was registered prior to data collection (Taylor, 2020b). The purpose of this was to mitigate the risk that the existing literature would influence data collection and analysis in an implicit and unsystematic way. This initial theory is summarised below as a logic model (Coldwell & Maxwell 2018) in Figure 5.1. This model informs the data collection as well the analysis, which employs 'folk Bayesian' reasoning (McKeown 2004, p.158); using these prior assumptions about the causal relationship to assess the new data collected. The approach taken to data collection and analysis is described in more detail below under 'Research design' and 'Analytic approach'.

Section 2.2 of the literature review lays out what is already known about how service learning increases political participation, and these details are not repeated here. In summary, no detailed and comprehensive theory on the causal

mechanisms that might connect service learning to political participation has been identified in the literature. However, the little that has been said on the topic, combined with the wider research on voluntary associations and political participation suggests that, if service learning has a positive effect on political participation, it might do so via one or more of three types of mechanism: i. by developing key skills; ii. by increasing the motivation to participate via an increased sense of self-efficacy and/or social responsibility; and/or iii. by giving access to new networks. The detail on the activities that trigger theses mechanisms are even more limited. Implicitly, there seem to be three types of activity within a service learning experience that could lead to the mechanisms in question: i. non-civic personal development activities that focus on skills relevant to civic participation; ii. non-political civic participation activities; and iii. reflection activities that aim to support a positive cognitive appraisal of (i) and (ii), and that encourage participants to reflect on the systemic causes of the issues being addressed through voluntary service, and the role of government in addressing these issues. Finally, when it comes to factors that might moderate the effects, no strong evidence has been identified at all.

Figure 5.1 gives a pictorial summary of the theory suggested by the literature. The arrows show the causal pathways that might link the activities to mechanisms and the mechanisms to the primary outcome. They show that each of the three categories of activity could trigger multiple mechanisms, and that some mechanisms are thought to act directly on political participation, whereas for others to work, a chain of outcomes is required. This initial theory requires development. First, nuance needs to be added to the description of the outcome. Like the theory of Civic Voluntarism, it attempts to provide a general explanation of all kinds of political participation. In doing so it provides a set of conditions whose necessity and sufficiency must vary by participation type (Aldrich 1997, p.423). Second, detail and clarity need to be added to all components in the chain that are thought to lead to this outcome. The broad existing literature on volunteering and service learning lacks clarity on the activities, mechanisms and moderating factors that might lead to an increase in political participation, as well as the causal pathways that link these parts together. This study aims to add clarity to these components by exploring in-depth the experiences of young people who have participated in NCS. As well

as assessing the plausibility of the initial theory then, the study aims to develop it into a more detailed and complete explanation.

Figure 5.1: Initial logic model of service learning and political participation
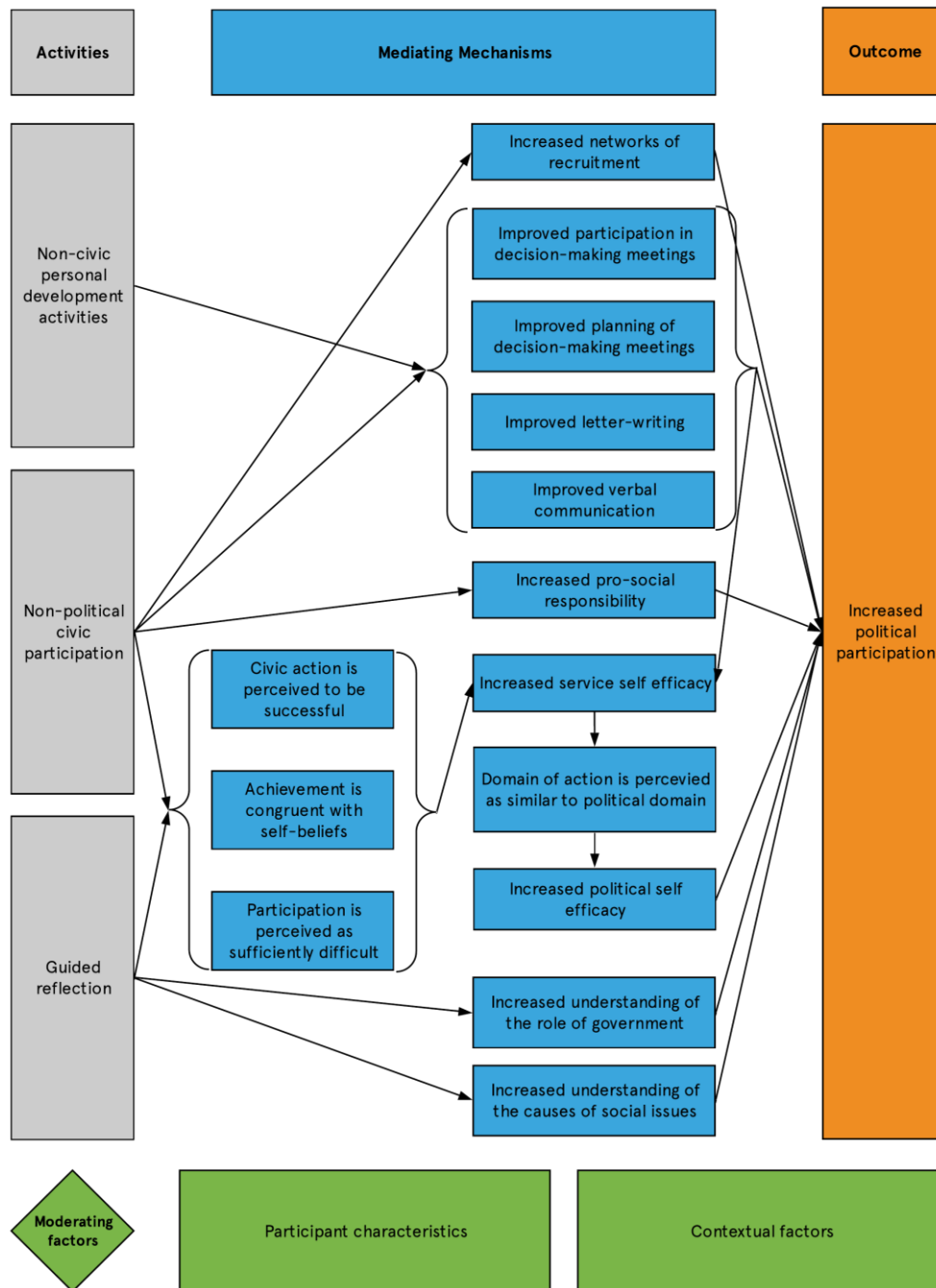
5.3 Research questions

The research question for this study is: How does service learning influence participants' future political participation? This question is addressed in three parts:

- **RQ2.1:** What are the mechanisms in a service learning experience that lead to a change in participants' future political participation?
- **RQ2.2:** Which activities trigger these mechanisms?
- **RQ2.3:** What are the factors that moderate the effect of service learning on participants' future political participation?

5.4 Research design

The study employs 'ideational process tracing' (Jacobs 2015, p.43), using in-depth interviews with 27 graduates of NCS. These interviews cover the graduates' participation in democratic activities, their motivations for such participation and the role, if any, that NCS has played in this regard. The analysis looks for explanations for each individual's behaviour and makes comparisons across cases to develop a theory. Three factors influenced this choice of design: i. the nature of the research question; ii. the nature of the theory under development; and iii. the available sources of evidence. These factors are discussed in turn below.

5.4.1 The nature of the research question

The research question has two important characteristics that influence the design. First, it covers a topic that is not well understood. As the preceding discussion and the literature review have shown, some broad responses to the question can be inferred from the existing literature, but these ideas are mostly based on observed correlations of outcomes with a range of different activities. Study 1 adds to this quantitative literature by providing strong quasi-experimental evidence on the causal effects of service learning, and by showing that the postulated self-efficacy spillover effect – from the general civic domain to the political – is unlikely to account for the relatively large effects that NCS seems to have on political participation. But the number of constructs explored in this study are necessarily limited by the method of data collection (a large-scale survey), and the depth to which they are explored is also limited, as they have been reduced to quantitative indicators to allow for statistical aggregation. Second, the research question is about complex causal processes involving individuals' attitudes and beliefs. A topic like this requires an in-depth, qualitative exploration of participants experiences and their beliefs about their subsequent behaviour (Ritchie et al 2014, p.37). Such exploration has the potential to take

us beyond observations of coinciding outcomes, to detailed explanations of why such outcomes coincide, and under what conditions.

The 'process tracing' literature provides the best developed framework for the qualitative study of causal processes. Bennett and Checkel define process tracing as, 'the analysis of evidence on processes, sequences, and conjunctures of events within a case for the purposes of either developing or testing hypotheses about causal mechanisms that may explain the case' (2015, p.7). This is a commonly-used definition, partly rooted in a tradition that applies the language of quantitative research to qualitative methods (see for example King et al. 1994). In some cases, such an application of quantitative language has provided helpful challenge to the field of qualitative methods, but in other cases – such as this definition – it is problematic (Brady & Collier 2010). In Bennett and Checkel's definition, the language of 'hypothesis testing' implies two things: i. a prediction of the kind, 'X causes Y'; and ii. a definitive test of that prediction. As discussed above, however, there is no definitive 'test' for causal processes (or 'mechanisms') because they cannot be experimentally assigned. To speak about testing hypotheses in the field of process tracing is therefore misleading and it is more appropriate to talk about 'identifying' rather than 'testing for' potential causal processes (McKeown 2010, pp.164-165); a convention that this study follows.[32] The study engages in this process of identification by combining deductive and inductive approaches to data collection. It deductively examines the 'observable implications' of the initial theory (Bennett & Checkel 2015, p.7), and it inductively identifies additional mechanisms and moderators.

5.4.2 The nature of the theory under development

The theory under development is 'ideational' in the sense that it aims to provide a largely cognitive explanation for a set of behaviours (Jacobs 2015, p.43). For an ideational theory to hold, evidence needs to be found that: 'decision-makers [possess] particular cognitions (a measure of the independent variable); those cognitions [shape] their choices (evidence of a mechanism of influence); and those cognitions [are] not simply reducible to material features of the

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[32] The language of 'identification' is obviously used fairly extensively in quantitative causal inference, especially experimentation. The word clearly means something different as it is used here, in the context of qualitative process tracing.

circumstances of choice (evidence of exogeneity of the independent variable)' (Jacobs 2015, p.45). This is the evidential challenge for this study.

5.4.3   The available sources of evidence

Three sources of information are typically available to support ideational process tracing: i. intrapersonal reflections gained from interviews with the subjects of the research; ii. texts that offer evidence of an individual's ideas (for example, publicly available minutes from a relevant meeting); and iii. texts that offer evidence on the external forces influencing an individual's decision-making (such as the voting record of a Member of Parliament). The second and third of these sources are commonly available in studies of elite decision-making, but rarely available when citizens are the subjects of research. This is the case in this study, where the only available source of evidence is interviews with people who have participated in a programme of service learning.

While interviews are often the most useful source of information in ideational process tracing, the absence of other sources poses difficulties that need to be addressed in the design and analysis. Effectively measuring an individual's ideas through interview is hard because it relies on sufficient recall and self-knowledge from the interviewee. In the absence of other sources that could be used for triangulation, these issues are addressed as far as possible through careful sampling and interviewing. The absence of evidence of external forces is assumed to be less problematic in this study, as citizens are unlikely to be subject to strategic incentives in the way that elite decision-makers are. Where political elites often make decisions that are inconsistent with their expressed ideas for strategic reasons, such as the exchange of votes, the subjects of this study (young graduates of a service learning programme) are much more likely to behave in ways that are consistent with their expressed ideas because they are not subject to the same strategic incentives as politicians. Research participants are also assured anonymity to further reduce this risk.

5.5   Sampling strategy

The aim of the sampling strategy is to capture the range and diversity of experiences that support a comprehensive response to the research question (Ritchie et al. 2014, pp.116-117). To achieve this, a 'stratified purposive sample'

(Ritchie et al. 2014, p.114) is created, where participants are selected by the researcher based on key demographic characteristics, stratified by their level of political participation post-NCS.

5.5.1 Creating the sample frame

The population of interest for this study is graduates of the NCS programme. NCS has been chosen because it is the largest programme of service learning in the UK and, as the government-funded programme of national voluntary service, has particular policy significance. For a different study, the Department for Culture Media and Sport (DCMS) – the government department with oversight for NCS – commissioned a survey of young people participating in NCS. This survey was issued to all participants of the NCS Summer 2019 programme (approximately 100,000 young people). At the end of this survey, consent was sought from respondents for their name and contact details to be passed on to researchers working with NCS Trust, for the purposes of future research. This process led to a dataset containing 3,186 records. Members of this dataset made up the sampling population for this study.

This initial dataset only contained participants' first name, last name, and contact details; not enough detail to select an appropriate sample for interviews. To address this, a screener survey (Appendix VI) was issued by email (Appendix VII) to all members of the dataset, inviting recipients to be considered for the study, asking them to share some demographic information, and to complete a brief survey on their democratic engagement. As an incentive to complete this survey, respondents were entered into a prize draw with a single prize of £200 of online shopping vouchers. Details of these survey items and justification for their inclusion are described in 'Selection criteria', below. Those participants who completed the screener survey in full made up the sample frame. This process means that members of the sample frame had to be sufficiently motivated to complete the NCS Impact Survey, provide the initial consent and then complete the screener survey; something that about 3% of the sampling population did.[33] The sample frame is therefore unlikely to be representative of the total population from which it was drawn (the summer 2019 cohort of NCS).

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[33] Figure based on NCS Trust's internal participation number for summer 2019 (98,331, unpublished at the time of writing).

However, the sampling strategy for this study did not aim for statistical representation. The attrition that happened during the creation of the sample frame would have only been a limiting factor if it had led to a dataset that did not contain the number and diversity of participants required by the selection criteria. Fortunately, this was not the case. In the event, 1,316 individuals completed the screener survey, which was a large enough pool to meet the selection criteria and quotas described below.

5.5.2 Selection criteria

The criteria used to select participants for interview are chosen to support the goal of accessing a comprehensive range and diversity of experiences (Ritchie et al. 2014, pp.116-117). To achieve a sample size that is practically manageable, the number of criteria used for selection is restricted and ranked in terms of importance to the research question, creating a list of primary and secondary criteria. The approach taken to these problems is described below.

The aim of this study is to understand how service learning influences political participation. Addressing this question requires interviewing individuals that have participated politically after NCS. While this does not guarantee finding individuals whose political participation was influenced by NCS, it would be impossible to find such individuals without having people in the sample who meet this criterion. To identify the factors that moderate the effects of service learning on political participation, it is also necessary to interview people who have lower levels of political participation, as well as those who have not participated at all. The primary sampling variable is therefore the level of political participation of the individual post-NCS. This is measured through participants' self-reported political participation in the screener survey, taking a combined score across all political participation questions.[34] The questions used to assess respondents' level of political participation are the same as those used in Study 1. They are, however, combined in a slightly different way here to support sampling, with extra weight given to the forms of participation that require people to self-organise (organising a petition and contacting a politician). The variable used for sampling is as follows.

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[34] See Appendix VI for all survey items.

$$polpart\_sample_i$$
$$= \left(\left(\frac{vote_i}{10}\right) + signpet_i + (2 * orgpet_i) + (2 * contact_i)\right.$$
$$\left. + protest_i\right) \tag{8}$$

where, for individual i:

- $polpart\_sample_i$ is the combined political participation score (with range [1,7]);
- $vote_i$ is self-reported intention to vote (on a 10-point scale with range [1,10]);
- $signpet_i$ is self-reported petition signing in the past 3 months (binary 1 or 0 to indicate 'yes' or 'no');
- $orgpet_i$ is self-reported petition organising in the past 3 months (binary 1 or 0 to indicate 'yes' or 'no');
- $contact_i$ is self-reported contacting of a politician in the past 3 months (binary 1 or 0 to indicate 'yes' or 'no'); and
- $protest_i$ is self-reported protest participation in the past 3 months (binary 1 or 0 to indicate 'yes' or 'no').

The sample frame is ranked based on this indicator and an equal number of individuals is selected from the upper, middle and lower third. For meaningful comparisons to be made between these three groups in the analysis, they need to be similar in composition in relation to other variables that could affect political participation. Without this similarity, it would be more difficult to isolate differences in experiences of NCS from differences due to these other variables. The literature suggests that there are three main demographic variables that predict political participation: gender (Norris et al. 2004; Sloam et al. 2018), socio-economic status (Condon & Holleque 2013), and ethnicity (HCPCRC 2014). These variables are used as secondary selection criteria.[35] There is no

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[35] The PAP specifies including one additional selection criterion: whether or not the participant took part in any political engagement activities during their service learning experience. These types of activities are rare, and not generally the focus of the civic participation carried out during service learning programmes. However, there is some variation in the delivery of NCS around England and some versions of the programme do include political engagement activities, whereas others do not. This factor could plausibly influence participants' future political participation, so was identified in the PAP as a secondary criterion. Unfortunately, NCS was

reason to suggest an interdependency between any of these variables in relation to their effect on political participation, so no nesting is required (Ritchie et al. 2014, p.132). Table 5.1 summarises these primary and secondary selection criteria.

Table 5.1: Selection criteria

Variable	Importance	Indicator	Source
Level of political participation	Primary	Self-reported participation	Screener survey
Gender	Secondary	Self-reported gender	Screener survey
Socio-economic status	Secondary	Self-reported Free School Meal status	Screener survey
Ethnicity	Secondary	Self-reported ethnicity	Screener survey

The number of participants required to reach analytical saturation – the point at which adding more participants offers no extra analytical value – is unknown. Some researchers have tried to provide general guidelines for sample sizes for qualitative interviews, which tend lie between 12 and 60 (Adler & Adler 2012; Ragin 2012; Ritchie et al. 2014) but others suggest that as little as 2 participants can be sufficient (Creswell 2017). Such guidelines are unhelpful due to their wide range, and the actual number required will depend on the characteristics of the particular study, including: the research question, the heterogeneity of the population, the number of selection criteria, and the extent to which nesting of criteria is needed (Ritchie et al. 2014, pp. 117-118). The amount of time available to interview, transcribe and analyse the data is also an important limiting factor. In the case of this study, there was time available to include a maximum of 30 people in the sample. To begin, 27 individuals were selected for interview to meet the following quotas (Table 5.2) and achieve approximate balance between groups on the secondary criteria.

Table 5.2: Target sampling quotas

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unable to provide the data required to use this as a selection criterion, so it was dropped from the list. To address this limitation in the sampling, interviewees were asked if they took part in any democratic engagement activities during NCS and this factor was then explored as part of the interviews.

	High participation	Moderate participation	Low participation
Gender			
Male	Min. 4	Min. 4	Min. 4
Female	Min. 4	Min. 4	Min. 4
Socio-economic status			
FSM	Min. 4	Min. 4	Min. 4
Non-FSM	Min. 4	Min. 4	Min. 4
Ethnicity			
BAME	Min. 4	Min. 4	Min. 4
White	Min. 4	Min. 4	Min. 4
Total	9	9	9

Selecting 27 (rather than 30) interviewees up front left open the option of selecting a further 3 individuals to fill particular gaps in the data identified after the first round of interviews. In the event, a second round of sampling was not deemed necessary as there were no obvious gaps in responses from any particular group.

5.5.3 Recruitment

Targeted purposive sampling yielded a sample of 27 participants that met all sampling quotas. This took four rounds of selection and invitations. In the first round, 27 people were selected from the sample frame to meet the quotas. These individuals were sent an email inviting them to take part in an interview, and offering £35 of online shopping vouchers as an incentive. Unresponsive individuals were sent a second email and, if necessary, called once on the telephone. This process yielded 19 participants. Three more rounds of targeted recruitment, following the same contact strategy (up to two emails and telephone call), were then carried out until all quotas were satisfied. In total 104 people were invited to participate, and 46 people accepted this invitation (44%).[36] This resulted in the sample summarized in Table 5.3. The figures in brackets represent the percentage of people in each category who accepted an invitation to participate.

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[36] 19 out of the 46 were placed on a waiting list but not interviewed.

Table 5.3: Number of recruited participants and response rate by sampling category

	High participation	Moderate participation	Low participation	Total
Gender				
Male	4 (67%)	5 (100%)	4 (26%)	13 (52%)
Female	5 (83%)	4 (54%)	5 (26%)	14 (41%)
Socio-economic status				
FSM	4 (57%)	5 (64%)	5 (22%)	14 (32%)
Non-FSM	5 (100%)	4 (71%)	4 (67%)	13 (75%)
Ethnicity				
BAME	4 (57%)	4 (62%)	5 (46%)	13 (55%)
White	5 (100%)	5 (67%)	4 (20%)	14 (39%)
Total	9 (75%)	9 (65%)	9 (26%)	

Notes: N = 27. Figures in brackets show the response rate for each category. 'FSM' = has been eligible for free school meals. 'BAME' = black and other minority ethnic groups.

This shows that those who reported higher levels of political participation were easier to recruit, with 75% accepting the invitation in the 'High' category and 25% in the 'Low' category. Another substantial difference in recruitment rates is seen between those from lower and higher socio-economic groups (indicated by prior eligibility for free school meals). 32% of those who reported prior eligibility for free school meals accepted an invitation to participate versus 75% of those who had never been eligible. While the figures also show that participants from BAME backgrounds were harder to recruit than their white peers, the hardest participants overall to recruit were low participating and white.

These differences are perhaps unsurprising. *A priori*, people who are more motivated to participate politically will be more motivated to participate in an interview about political participation. And socio-economic status and ethnicity were deliberately chosen as sampling criteria because they are known to be correlated with participation (Achen 2002; Beck and Jennings 1982; Condon & Holleque 2013; HCPCRC 2014; Hooghe and Stolle 2004; Norris et al. 2004; Sloam et al. 2018; Uhlaner et al. 1989; Verba et al. 1995). The differing recruitment rates do not reflect a weakness of the sampling and recruitment strategy as all quotas were met. However, the trends noted here were partially reflected in the levels of enthusiasm for and engagement with the interviews themselves. Those groups that were harder to recruit were also sometimes harder to engage in interview. In particular, it was sometimes harder to elicit

detailed reflections from interviewees that had reported lower levels of political participation in the screener survey. The sampling strategy accounted for this possibility by setting and meeting quotas based on these characteristics that were associated with lower engagement; thus increasing the chances that there would be some well-engaged interviewees in each group. It also left open the possibility of recruiting additional participants in certain groups if it was deemed that more information was needed from those groups (in the event it was not). In addition, all transcripts were qualitatively coded to indicate the individual's level of engagement with the interview, and this is used as a factor in the analysis. This process of categorisation reveals that there are interviewees from all the 'difficult to recruit' groups described above who engaged deeply in the interviews. It also shows that only 7 out of the 27 participants engaged at a very low level, and even some of these interviews generated useful insights.

5.6 Method

As argued above, the research question requires an in-depth, qualitative exploration of participants' experiences of service learning and their beliefs about their subsequent behaviour. As the most effective way to gather this type of intrapersonal information, in-depth interviews are used to collect the data for this study (Rubin & Rubin 2012, p.3).

There is a lot of debate about the way that data is generated in interviews. On one side of this debate are those who believe that, 'knowledge is waiting in the subject's interior to be uncovered' (Kvale & Brinkman 2009, p.48), unaffected by the interviewer's presence or line of questioning. This position is often described as a 'positivist' one. On the other side, there are those who believe that knowledge is necessarily jointly constructed by the interviewer and interviewee in the course of their interaction (see for example, Holstein & Gubrium 2011). The most extreme 'constructivist' position believes not only that the interviewer will influence the attitudes and beliefs of the interviewee (and therefore the findings), but that there is no such thing as attitudes and beliefs that are independent of a particular social interaction. A full discussion of this debate is beyond the scope of this paper, but it is important to note here that the latter position seems untenable, as it requires us to believe that it is impossible to generate data from interviews with any amount of stability,

reliability or validity (Ritchie et al. 2014, p.179); i.e. that if a similar interview were conducted with the same individual, it would likely yield substantively different results, and it would be conceptually impossible to say which results were a truer reflection of reality. For the purposes of this study, it was assumed that the interviewer would unavoidably influence the interviewee to some degree, but that this effect could be mitigated – through the way that the interviews were structured and executed – to access interviewees' independently held attitudes and beliefs. It was not assumed that interviewees consciously held all of these attitudes and beliefs prior to interview. Indeed, one of the aims of the interviews was to encourage new intrapersonal reflections. This epistemological position aligns the study with an established body of pragmatic qualitative research (see for example, Evans et al. 2015; Kvale & Brinkman 2009; Lofland et al. 2006; Miller & Glassner 1997; Ritchie et al 2014; and Rubin & Rubin 2012).

In-depth interviewing can take different forms. There is an implicit assumption in this study that service learning can be a formative experience in an individual's life; i.e. that it has effects over-and-above the longer-term processes of political socialisation and the effects of other interventions. To understand the nature of a service learning experience, and the factors that moderate its effects, it is therefore necessary to situate it in the participant's wider influences and experiences. To do this, interviews draw on life and oral history approaches (see, for example, Chamberlayne et al. 2000; Riessman 2008), tracing the interviewees' political motivations and behaviours over the course of their lives, seeking to identify the main influencing factors, and the role that NCS has or has not played within this wider context. While a semi-structured interview guide is used (see Appendix IX) to help ensure that a substantial part of each discussion covers the role of NCS, taking a partly narrative approach means that the interviewee's personal story influences the direction of the conversation. In practice this meant that not all interviews followed the same order, not all questions in the guide were asked in all cases, and some questions were asked that were not in the guide. These variations are intentional and specified in the PAP. The interview guide is designed to support this approach to interviewing by including both questions and a checklist for the interviewer. The latter is designed to support a more natural conversation, with the interviewer crossing off topics when they are discussed and forming questions on topics that have not been

discussed in ways that fit with the specific conversation. Taking this approach, as opposed to a more structured one, takes more time and can make the analysis of transcripts more difficult, but it also makes it more likely that the complexities and details of each individual's experiences are properly explored (Riessman 2008, pp.23-24).

All interviews were conducted on the telephone and lasted for approximately one hour. This decision was made based on the time constraints of the study and the geographically scattered sample. While face-to-face interviews are often assumed to be more effective than telephone interviews, there is limited evidence to support this view (Irvine 2010; Irvine et al. 2012) as well as research that suggests that some interviewees find it more comfortable to speak on the telephone (Nicholls et al. 2012) and that telephone interviews can be effective for narrative interviewing (Holt 2010). The exploratory research conducted at the beginning of this PhD also used telephone interviews to good effect. The alternative would have been to restrict the sample frame to London, where the researcher is based and could feasibly conduct all interviews in person. However, this would have significantly reduced the range and diversity of the sample. Given this, and the lack of strong evidence to suggest that face-to-face interviewing would improve the quality of the data collected, the telephone approach seems preferable.[37]

5.7 Analytic approach

Good qualitative research requires analysis during both the data collection and formal analysis phases (Ritchie et al. 2014, p.276). This is true for the current study, where conducting effective semi-structured interviews requires real time analysis, in order to properly probe responses. Initial findings from each interview are also used to make additions and adaptations to the interview guide in subsequent cases. Perhaps because of this 'ongoing' approach to analysis that is often necessary in qualitative research, the analytic approach is rarely specified prior to data collection. However, to be transparent about the formal phase of analysis for this study, three elements of the analysis are specified in a PAP

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[37] In the event, the interviews all took place during the first period of 'lockdown' in England due to the coronavirus pandemic. The restrictions placed on social contact during this period meant that face-to-face interviews would not have been permitted anyway.

(Taylor 2020b): i. the approach to formally analysing interview data; ii. the approach to making inferences; and iii. the approach to generalisation. These elements are summarised here.

5.7.1 Analysing the data

A thematic analysis is carried out across the interview data, following a three-stage process. In the first stage, transcripts are coded by research question – 'mechanisms' and 'moderators' – for the purpose of 'topic coding' (Saldana 2009). This first stage is a 'low-inference and descriptive' process of data management (Ritchie et al. 2014, p.272). In the second stage, data is coded by themes that responded to the research questions. These themes are identified both deductively and inductively (Braun & Clarke 2006, p.83), using constructs from the literature where the evidence supports this, and creating new constructs where it does not. Summary case studies are also written for each participant at this stage. This means that the results of the second stage analysis can be seen within and across cases, which is crucial for drawing meaningful inferences (Ritchie et al. 2014, p.305). In the third stage, the themes are refined by reviewing their relation to each other, grouping them into conceptual categories where possible, and ensuring that they comprehensively cover the data (Ritchie et al. 2014, p.280).

There are some researchers who advocate for using at least two analysts in the process of coding in qualitative analysis, but this is rarely appropriate (Ritchie et al. 2014, p.278). The aim of this study's analysis is to make sense of the data and its relation to the research question, not provide a 'definitive' set of codes that all analysts would agree upon. In this study, the data generated is complex, influenced by the researcher during the collection process, ambiguous and, therefore, open to interpretation by the analyst. In cases like these, it is possible that two different analysts could generate similar findings that are organised differently (by categorising themes differently, for example). It is also possible for competing interpretations to be produced, both of which could be supported by the raw data. These are inevitable consequences of the design, not weaknesses to be addressed by trying to reconcile the views of multiple analysts.

In quantitative content analysis, which aims to identify the number of times a particular concept appears in a set of texts, the use of multiple analysts and the

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idea of inter-rater reliability *is* relevant because the aim of the analysis is more modest, and objectivity is easier to achieve. In complex qualitative analysis however, the introduction of a second analyst can decrease the quality of the analysis, especially if that individual lacks a depth of knowledge of the topic and is not involved in the data collection. Only one analyst (the researcher) is therefore used for this study and the quality of the analysis is assured by focussing on three principles (Ritchie et al. 2014, p.280): i. that it is grounded in the data; ii. that it comprehensively covers the data; and iii. that the approach is transparent. These principles are evidenced in the reporting below, and also through the publication of the PAP and all (anonymised) transcripts.

5.7.2   Making inferences

Section 5.2 argues that the literature contains some broad initial answers to the research question. When this is the case, some researchers have suggested that a Bayesian approach should be taken to making inferences, even in qualitative research (Abell 2009; Humphreys & Jacobs 2012; Rohlfing 2013). To do this formally involves specifying three quantities: i. the probability that a proposition derived from a theory is true; ii. the probability of finding a particular piece of evidence, if the proposition is true; and iii. the probability of finding a particular piece of evidence, if the proposition is false. These three quantities can then be used to calculate the probability that a proposition is true in light of the evidence. The appeal of this approach is that it 'asks the researchers to make specific and public assumptions that they must make implicitly for process tracing to work' (Bennett & Checkel 2015, p.297). All qualitative researchers with prior assumptions implicitly apply elements of Bayesian logic to their analysis by (qualitatively) weighting different pieces of evidence according to their evidential value, but often do so in a post-hoc manner without a formal framework for the weighting process. This makes it harder to assess evidence systematically and to manage researcher bias.

Though appealing in general, such a formal application of Bayesian reasoning in this study is considered inappropriate for three reasons. First, it is impossible to estimate with any precision or known level of certainty the required probabilities. This is partly due to the state of the current evidence, which lacks specificity and precision, but also the field of enquiry. While some fields, such as the natural sciences, do contain simple and reliable deterministic theories supported by

detailed quantitative evidence, the social sciences do not. To make quantitative estimates of the probabilities in question would therefore be misleading (Bennett & Checkel 2015, p.297). Second, the method of data collection and the topic of study are so complex that it would be impossible to *ex ante* list all of the observable implications of the initial theory. Third, Bayesian logic does not cover the inductive side of process tracing, which uncovers new potential explanations, rather than assessing the plausibility of existing ones. To account for these limitations of Bayesian reasoning, while recognising its value, a compromise is taken in this study, using Collier's framework of 'process tracing tests for causal inference' (2010, p.825).[38] The table below summarises these four tests, that are ordered in terms of their inferential strength, with 'straw-in-the-wind' tests being the weakest and 'doubly decisive' ones being the strongest. Evidence is classified into one of these categories depending on how *uniquely* it supports a certain explanation (versus alternative ones) and how much *certainty* there is in the inference that we can draw from it (Bennett & Checkel 2015, p.17). This combination of uniqueness and certainty then tells us the extent to which the evidence confirms one proposition and rules out others. It also tells us what we can reasonably conclude in the absence of different types of evidence.

Table 5.4: Process tracing tests for causal inference

		LEVEL OF UNIQUENESS	
		Low	High
		1. Straw-in-the-Wind	**3. Smoking-Gun**
		a. Passing: Affirms relevance of hypothesis, but does not confirm it.	**a. Passing:** Confirms hypothesis
LEVEL OF CERTAINTY	Low	**b. Failing:** Hypothesis is not eliminated, but is slightly weakened.	**b. Failing:** Hypothesis is not eliminated, but is somewhat weakened.
		c. Implications for rival hypotheses: Passing *slightly* weakens them Failing *slightly* strengthens	**c. Implications for rival hypotheses:** Passing substantially weakens them. Failing somewhat strengthens them.

[38] As noted in section 5.4, the language of 'hypotheses', 'tests' and 'causal inference' can be misleading in the context of process tracing and will be avoided in the analysis. It is used here because that is the standard language used in this part of the literature.

	2. Hoop	4. Doubly Decisive
High	**a. Passing:** Affirms relevance of hypothesis, but does not confirm it.	**a. Passing:** Confirms hypothesis and eliminates others.
	b. Failing: Eliminates hypothesis.	**b. Failing:** Eliminates hypothesis.
	c. Implications for rival hypotheses: Passing somewhat weakens them. Failing somewhat strengthens them.	**c. Implications for rival hypotheses:** Passing eliminates them. Failing substantially strengthens.

Source: Adapted from Collier (2010, p.825)

The analysis applies these principles in this study by looking for the following two indicators of *uniqueness* in the interview data: i. the specificity of interviewees' descriptions (are they clearly talking about, for example, self-efficacy, or could they be describing something else?); and ii. the absence of rival explanations (with absence after specific interviewer prompting being given even greater probative value). The following four indicators are used to support the assessment of *certainty* in the findings: i. whether or not a description of a causal process is accompanied by a concrete example; ii. the amount of prompting that led to a proposition; iii. the existence of evidence that supports more than one part of a causal chain (for example, evidence that suggests participants developed service-based self-efficacy AND evidence that they developed political self-efficacy WHEN they saw the two domains as similar); and iv. whether or not the evidence supports an element of the initial theory (and is therefore supported by the wider literature).

In this study, no 'doubly decisive' (unique and certain) evidence is produced in favour of a particular causal process. This is as expected. The strongest evidence for an explanation that could (hypothetically) appear in the data would be that all interviewees gave the same explicit description of a causal processes that they thought had acted upon them; that they told the same, detailed story of how service learning has affected their political participation. This is an unlikely outcome and did not occur in practice. But even if such a consistent and detailed account had arisen from the interviews, such evidence would not count as doubly decisive as there are inherent limitations in the method of data collection that prevent this. Thinking about the 'uniqueness' criterion, there is often

ambiguity and sometimes contradiction in what people say, such that multiple interpretations are possible. Thinking about the 'certainty' criterion, it is impossible to say with certainty that an individual's expressed ideas are true reflections of their psychological states – due to potential issues with self-reflective accuracy and honesty – so evidence from interviews will always fall short of this standard.

5.7.3 Generalisation

This research aims to generate findings that are applicable beyond the cases studied, but the purposive approach taken to sampling has important implications for the type of generalisations that can be made. The aim of this sampling method is to capture the range and diversity of experiences in relation to the research questions, to help build a more detailed and comprehensive theory of behaviour change in this context. Importantly, the sampling strategy does not aim to generate a sample that is statistically representative of either the study population or any wider population. As such, the prevalence of a reported experience in the qualitative findings 'tells us nothing about the prevalence within [either] population' (Ritchie et al. 2014, p.329). Furthermore, the interview method used does not collect data in the structured way that is necessary for quantitative aggregation. This is in contrast, for example, to structured questionnaires, which collect responses in fixed categories that can be aggregated. The reporting of frequency counts in relation to qualitative findings is therefore carefully avoided as such counts are at best uninformative and at worst misleading. If successful, the sampling strategy therefore supports the generation of findings that cover the range and diversity of experiences in the sampling population, but no generalisations are made about the prevalence of these experiences.

5.7.4 Exploratory quantitative analysis[39]

The results of the qualitative analysis described above reveal some surprises. These surprises prompt a return to the quasi-experimental dataset for some further quantitative analysis. This analysis was not specified in the PAP for Study 2, because it is based on its findings. However, before carrying out the analysis

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[39] See Appendix XII for the full analysis code and log file.

described here, an analysis plan was registered online as an addendum to the Study 1 PAP (Taylor 2020c). The findings from Study 2 suggest that social self-efficacy is a more powerful mediating mechanism than political self-efficacy or general civic self-efficacy in this context. They also suggest that social anxiety, and social and communication skills are important mechanisms. This generates four quantitative hypotheses as follows.

- **H4.1:** NCS increases social self-efficacy more than general civic self-efficacy.
- **H4.2:** NCS increases social self-efficacy more than political self-efficacy.
- **H4.3:** NCS decreases social anxiety.
- **H4.4:** NCS increases social and communication skills.

Fortunately, the quasi-experimental dataset contains good indicators for all of these newly identified outcomes. These hypotheses are therefore tested by returning to the quasi-experimental dataset and estimating average treatment effects, following the same matching and estimation procedures as in Study 1. The new outcomes are operationalised as follows.

- A measure of **social self-efficacy** is created by combining survey items that ask respondents to report their confidence (on a 5-point Likert scale) in meeting new people, working with other people in a team, being a leader of a team, speaking in public, and managing disagreement and conflict. Formally, this involves taking the mean average score of the following survey items: (*Q028-Q15, row 1*), (*Q028-Q15, row 3*), (*Q028-Q15, row 4*), (*Q028-Q15, row 5*), (*Q028-Q15, row 6*), (*Q028-Q15, row 7*). This variable can take values between 0 and 4 inclusive.
- **Social anxiety** is measured by an item (*Q034-Q21*) that asks respondents to report how anxious they felt yesterday on a 11-point Likert scale. This variable can take values between 0 and 10 inclusive.
- A measure of **social and communication skills** is created by combining survey items that ask respondents to report how easily (on a 5-point Likert scale) they get along with people, they can tell if

someone says one thing and means another, they can notice if someone in a group feels awkward, and they are able to see things from another person's viewpoint. Formally, this involves taking the mean average score of the following survey items: (*Q030-Q17, row 2*), (*Q030-Q17, row 3*), (*Q030-Q17, row 4*), (*Q030-Q17, row 5*)

- **General civic self-efficacy** and **political self-efficacy** are measured in the same way as is in the analysis for Study 1.

Care is taken when looking for appropriate matches between the constructs described by interviewees and the variables in the quantitative dataset, and good results are achieved in this respect. In the case of social skills and social self-efficacy (the two most complex constructs), new composite variables are created that closely reflect the range of ideas described by interviews in these categories. We can therefore have good confidence that the survey items used in this analysis do closely reflect the outcomes identified through the interviews.

5.8 Results

5.8.1 Introduction

This study aims to improve our understanding of how service learning increases political participation (RQ2). This question has three main parts to it: i. 'What are the mechanisms in a service learning experience that lead to a change in participants' future political participation?' ii. 'Which activities trigger these mechanisms?'; and iii. 'What are the factors that moderate the effect of service learning on participants' future political participation?'. These questions are interrelated. It is impossible to properly understand the role that one component plays in the causal chain – be it a mechanism, activity or moderator – without understanding how it relates to the other parts. For example, describing a certain type of skill-development as a mechanism is only fully informative as part of the theory if you say which particular activities contribute to that skill-development, and which specific factors moderate its effects on political participation. For this reason, the detailed findings that follow this introduction are not separated into findings about mechanisms, findings about activities, and findings about moderating factors. Instead, they are structured around the potential mechanisms that are identified in the analysis. The evidence for each of these potential mechanisms is presented in turn, alongside the specific activities and

moderating factors that relate to them. After the detailed findings are described, a revised version of the logic model is then presented (section 5.8.7) to give a pictorial overview of the main results.

Four categories of mechanism are identified in the analysis: i. capabilities; ii. attitudes and beliefs; iii. knowledge; and iv. networks of recruitment. In total, seventeen individual potential mechanisms have been identified across these four categories. In the results that follow, the evidence that each of these constructs is a true mediator of the relationship between service learning and political participation is presented in two parts; first, whether there is evidence of the construct as a proximal outcome (a change during or shortly after the experience, but prior to any change in political behaviour), and second, whether there is evidence to suggest that this proximal outcome contributes to the outcome of interest (an increase in political participation).

The findings suggest a high level of heterogeneity in effects. For some of the outcomes, positive, null and negative effects are identified. There is evidence that twelve of the proximal outcomes identified are true mediators. Some of the outcomes identified as mechanisms seem to directly affect political participation. Others have an indirect effect; i.e. there is at least one additional step in the causal chain prior to an increase in political participation.[40] Table 5.5 summarises these findings.

Table 5.5: Summary of findings on mediating mechanisms

Potential mechanism	Evidence as proximal outcome	Evidence as mechanism	Evidence of no effect	Evidence of negative effect	In literature?
Capabilities					
Social interaction and communication skills	Strong	Moderate	No	No	Partly
Teamworking skills	Moderate	None	No	No	Partly
Planning and organisation skills	Weak	None	No	No	Partly

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[40] This extra layer of complexity is not displayed in Table 5.5, but is captured in the detailed findings and the revised logic model.

Attitudes and beliefs					
General self-efficacy	Weak	Weak	No	No	No
Social self-efficacy	Strong	Moderate	No	No	No
Perceived success of social interactions	Strong	Moderate	No	No	No
Service self-efficacy	Moderate	Moderate	No	Yes	Yes
Perceived success of service	Moderate	Moderate	Yes	Yes	Yes
Perceived importance of issue addressed through service	Moderate	Moderate	No	No	No
Perceived similarity between service and political participation	Moderate	Moderate	Yes	No	Yes
Political self-efficacy	Moderate	Moderate	Yes	Yes	Yes
Prosocial responsibility	Moderate	Weak	Yes	No	Yes
Self-esteem	Strong	None	No	No	No
Networks of recruitment					
Peer-to-peer	Moderate	Moderate	Yes	No	Yes
Through service	None	None	Yes	No	Yes
Knowledge					
Basic awareness of issues	Moderate	Weak	Yes	No	Partly
How politics works	Moderate	None	Yes	No	Partly

The terms 'weak', 'moderate' and 'strong' used in Table 5.5 are relative, qualitative descriptions of the strength of the evidence collected in this study. They are broad labels applied by the researcher, based on an assessment of the evidence, using the uniqueness and certainty criteria described in section 5.7.2. The purpose of this presentation is to provide an overview of the results to help orient the reader. These descriptions of evidence strength should be read in the context of the study design and the inherent limits on our ability to make causal inferences about mediating mechanisms.

The evidence relating to these outcomes is presented below, accompanied by descriptions of the specific activities and characteristics of the programme that seem to lead to the results (RQ2.1 and 2.2). These activities fall into four categories: i. non-civic personal development activities; ii. service-based civic participation; iii. guided reflection and positive feedback; and iv. political and

issue-based discussions. Perhaps surprisingly, it is the non-civic personal development activities – i.e. the activities that have nothing, on the face of it, to do with civic or politics – that seem to be to be among the most powerful triggers of the most important mechanisms.

The findings on the factors that moderate the effects identified (RQ2.3), are also integrated into the findings on mechanisms, to make it clear which specific effect is under moderation in each case. Two categories of moderator are identified: i. characteristics of the participants prior to the service learning experience; and ii. characteristics of the service learning experience itself. In total, sixteen moderating factors are identified across these two categories. These are summarised in Table 5.6 alongside the outcomes that they seem to influence.

Table 5.6: Summary of findings on moderating factors

Moderator	Outcomes influenced by moderator
Participant characteristics prior to service learning	
Social interaction and communication skills	Social interaction and communication skills
Critical thinking	Prosocial responsibility
Prosocial responsibility	Service self-efficacy; Prosocial responsibility
Issue interest	Teamworking skills; Service self-efficacy
Political interest	Social interaction and communication skills; Social self-efficacy; Service self-efficacy; Political knowledge
Political motivation	Social interaction and communication skills; Social self-efficacy; Service self-efficacy
Social self-efficacy	Social interaction and communication skills; Social self-efficacy
Political self-efficacy	Service self-efficacy
Openness to others' views	Social interaction and communication skills; Teamworking skills
Political knowledge	Social self-efficacy; Political knowledge
Other commitments / priorities	All
Characteristics of the service learning experience	
A safe and supportive environment	Social self-efficacy
Legal and safety restrictions on service	Service self-efficacy
Sociodemographic diversity of cohort	Social interaction and communication skills

Diversity of cohort in terms of political engagement	Networks of recruitment
Depth and focus of political knowledge components	Political knowledge

As with the summary of mechanisms, this table is only intended to provide a snapshot of the findings on moderators to help orient the reader. The detailed findings on moderating factors are complex. Whether or not a particular factor was at play, the combination of factors at play, and the influence that these factors have often varies by individual. The presentation of the results attempts to account for this complexity in two ways. First, wherever outcomes or moderators are observed to have different effects in different circumstances, the full range and diversity of effects is described, categorising these experiences where possible, along with the relevant circumstances (Ritchie et al. 2014, p. 386). Second, brief case studies are presented throughout the findings to try to capture some of the residual complexity that is lost in the (necessary) processes of summary and categorisation carried out in the main analysis.

The moderating factors identified are also unlikely to comprise an exhaustive list. A factor is only included in the list if there is specific qualitative evidence of it affecting a specific outcome. It is plausible that some of these moderators influenced other outcomes, but inferences in the absence of specific reports from participants, or clear implicit evidence, have been avoided. It should also be noted that a number of other factors are identified as influencing interviewees' political participation in general, but with no evidence that they were moderating the effects of service learning.[41] These factors are also omitted from the findings. All names used below are pseudonyms.

5.8.2 Capabilities

Three sets of capabilities are identified as proximal outcomes of NCS that could lead to an increase in political participation post-service: i. social interaction and communication skills; ii. teamworking skills; and iii. planning and organising

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[41] These factors are: baseline networks of recruitment, baseline political attitudes (political trust, feelings of representation by current political parties, the perceived effort required for participation), baseline level of political socialisation, sense of identity, previous experience of political participation, and having 'more important' commitments.

skills. They are presented below in order of the strength of evidence that supports each as a true mediating mechanism. The evidence supporting social interaction and communication skills is the strongest, with a range of participants suggesting, without prompting, that they have developed in this area and some suggesting that this has in turn increased their propensity to participate politically. The second most secure finding under capabilities relates to teamworking skills, with unprompted reports of this outcome from some participants again. Third is planning and organisational skills. While no participants reported developing this final set of skills without prompting, some concrete and detailed examples of this outcome were identified. These latter two outcomes are identified without an explicit link being made to political participation.[42] A theme that cuts across all the results on capabilities is the importance of non-civic activities for capability-building; allowing for mastery experiences whose lessons are applied later in the programme to the civic domain.

5.8.2.1 *Social interaction and communication skills*

There is strong evidence to suggest that some participants developed their social interaction and communication skills during NCS, with a range of participants reporting improvements in this area without prompting, with certainty, and using concrete examples that linked the outcome to a relevant activity. There is also some evidence to suggest that these increased capabilities may lead to increased political participation in some cases.

This category of capabilities includes technical communication skills and non-technical ones (that are sometimes referred to as 'soft skills'). Two technical communication skills are identified as outcomes for some participants: public speaking and debating. Some participants received formal workshops on these topics during the programme; learning skills like how to construct an argument or deliver a persuasive speech. Others reported developing their public speaking skills through their civic participation project work, which often required participants to deliver a pitch for financial support to a panel of adults, or

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[42] The findings relating to these two outcomes are included here – whereas other proximal outcomes that showed no link to political participation are not mentioned – because they do not relate to constructs identified in the literature as potential mechanisms.

through less formal activities on the programme like talent shows. For some, this was the first time that they had learned about and/or practiced these skills. The level and breadth of skills that participants had prior to NCS was the key moderating factor here. For example, Grace reported being a competent communicator in a range of ways prior to her service learning experience. NCS filled a debating-skills gap for her but had no effect on any other technical communication skills. In another example, James reported a complete ceiling effect on communication skills, reporting that NCS did not teach him any new communication skills that he had not already mastered in the military cadets.

The soft skills that were described in relation to communication and social interaction all relate to participants' ability to interact with people with different views and/or those from different backgrounds. Participants reported being exposed to peers with different ethnicities, sexual identities, socio-economic status, views about society, and levels of political interest and knowledge. Three characteristics of the programme were identified as encouraging this social mixing. First, all participants were grouped in diverse teams and given a common goal (to plan and deliver their own civic participation project). Second, these groups had to spend a substantial and unbroken period of time together. The programme lasted three or four weeks, with two weeks of residential living. Third, all groups participated in formal teambuilding activities that required them to interact and work together. One participant said that this intense period of time together led to the sharing of personal stories and the exposure of personal 'vulnerabilities'. This, for her, set the experience apart from school, where a similar mix of people might exist, but very little social mixing takes place, with schoolmates preferring to stick together in groups of 'people like them'.

> *"The fact that we were all on the same…team and we were all in the same boat and that…there was no hierarchy… I just feel like it was quite a nice environment to learn about other people and their backgrounds." Olivia*

Five interrelated capabilities were identified as sometimes resulting from this exposure to differences: i. increased empathy; ii. increased ability to listen to different points of view; iii. increased patience; iv. increased ability to make new friends; and v. reduced prejudice. These capabilities were often developed in combination. For example, one participant believed before NCS that people who identified as transexual might have a mental illness. He described interacting

with trans and gay people for the first time on NCS, improving his ability to communicate and changing his views.

> *"So like…normally I wouldn't go out into like the LGBTQ+ community…[but at NCS, I chatted] to people who are part of that… [D]uring NCS I was…good mates with this gay lad, we just chatted about what it is actually like being a gay lad at 16."*
> *James*

For James, this positive effect seemed to be enabled by an openness to others' views that he held prior to NCS. For example, he reported having friends before NCS who had attended a rally held by the far-right leader Tommy Robinson and, while he did not agree with these views or attend the rally himself, he expressed interest and did not see this as a barrier to these friendships. He also described visiting his local Labour club to find out about their activities, purely out of curiosity. This openness and curiosity seemed to act as enablers to the improved communication and reduced prejudice that he developed on NCS.

Participants also described becoming better at interacting with peers from different socio-economic backgrounds or those with different levels of engagement with the programme.

> *"[They say] don't judge a book by its cover but no one actually listens - especially in my age [group]… [W]hatever [people] present themselves [as] is what you think they are. So a lot of the people from my wave, they appeared a certain way like wearing tracksuits, not making an effort…so I was a bit like, 'Okay…these people don't really care.' But then afterwards I saw that not every activity suits everyone and how they got engaged in other things."* Priya

This type of observation was made in both directions. Participants who considered themselves to be from more deprived backgrounds (in terms of wealth and quality of schooling) also described improving their ability to communicate with new peers from more privileged backgrounds. To achieve these outcomes, a cohort of participants needs to be diverse in terms of its sociodemographic characteristics, as well as its composition of views about society and politics.

While the main focus of these developments in soft skills was on peer-to-peer interaction, some participants also described increases in their abilities to interact with adults; for example, elderly people in care homes that they were supporting through volunteering, or members of the public that they spoke to on the street as part of fundraising activities for their projects.

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NCS explicitly tries to develop these soft skills, partly as ends in themselves and partly to encourage greater social mixing post-programme (NCS 2020). However, there is some evidence to suggest that these gains in social interaction and communication skills also led to increased political motivations for some. One participant, Olivia, was convinced that this had been the case for her.

Case study: Olivia

Olivia lives in a town/city in the East of England, comes from a comfortable background with two professional parents and identifies as white. She reported a very high level of interest in politics. Prior to NCS, she had taken part in petitions, written to politicians, and been on protests. She has also been an active participant in youth parliamentary activities, is studying politics at A Level and has an ambition to become a Member of Parliament, and maybe Prime Minister, when she is older.

Despite her very high levels of political interest and participation prior to NCS, Olivia still believes that her service learning experience increased her motivation to participate in politics.

"I think [NCS] helped me...because…people that I was on [the programme] with were from…everywhere around [county name]. I think it really opened my eyes to see how other people lived and to see how other people weren't as engaged as I was… [I]t's [enabled] me to…see how I can…influence other people. It's [helped] me see…how I can help young people and how I can make them more engaged. [I]t has also made me…see…I won't call it the dark side of society, but the parts of society that the media doesn't focus on – as in the young from the deprived backgrounds that don't really have that knowledge." Olivia

For Olivia, being exposed to less politically engaged people than her, and meeting people from lower SES backgrounds added to her already-high motivation to be politically active, and she stuck by this story in the interview after probing. She did also suggest however, that this effect was not likely to be realised in the short term because, having just transitioned to post-16 education, she was focussing on her studies and deliberately reducing the amount of time that she spent on other activities (including political participation).[43]

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[43] Other commitments (usually studies) were also described by other participants as a factor that moderated their political participation.

Olivia's high levels of political interest and motivation prior to NCS seem to play a moderating role here; without this, her increased communication skills would be unlikely to make a difference to her political participation. And there is also evidence that this mechanism was present as an outcome for some, without leading to an increase in political participation. For example, one participant, Katie, reported increases in her social interaction and communication skills, but was also sure that NCS had not increased her motivation to participate in political activities. This seemed to be because her level of self-efficacy in this domain ('social self-efficacy' hereafter) prior to NCS was very low, as was her political motivation, and the proximal effects of her service learning experience were not large enough to lead to a change in political behaviour. There was a final category of participants who did link improved social interaction and communication skills to their future political participation, but not in terms of quantity. Participants in this category described being more considered in their political views, having a better understanding of the range and diversity of experiences in society, and being better at influencing; all as a result of the exposure to diverse peers on the programme. For them, these were outcomes that might change the *way* that they engaged in political activities in the future, but not *how much* they participated.

This discussion highlights two themes that reappear throughout the analysis. First, there is substantial heterogeneity in the sample, both in terms of the characteristics of participants prior to their service learning experience, and the effects that they derived from it. Some reported an increase in their abilities to interact and communicate with others as a result of NCS, some did not. Of those who did, some reported that this acted as a mediating mechanism that led to an increase in their political participation and some did not. Second, the factors that explain these differences are complex. This first discussion of cases reveals that, at the least, baseline capabilities, openness to others' views, social self-efficacy, other commitments, political interest and political motivation may all play a part in moderating the effects of this single proximal outcome.

5.8.2.2   Teamworking skills

The second set of capabilities identified as potential mechanisms were teamworking skills. These are closely related to the social interaction and communication skills discussed above. However, they are presented here as a

separate category as they relate specifically to carrying out collective action, whereas the former need not.

> *"[I]t was multiple 16 year olds giving opinions about what we should do as the group. So, I mean there was quite a lot of locking heads at the start between all of us because we all had different views on what we wanted to do." Josh*

During NCS, a range of different tasks are given to participants to address as a team over the course of the programme. In Phase 1, during the outward bound week, participants carry out teambuilding activities like raft building; in Phase 2, teams live together residentially and have to plan, shop for and cook meals together; and in Phase 3, they are asked to plan and deliver a civic participation project as a team.

Through these repeated group-based tasks, some participants described gradually developing one or more of three capabilities. The first of these was the ability to build positive working relationships. For some groups, being asked to work together led to tensions and disagreements. This provided participants with the opportunity to learn how to resolve these differences and build relationships that allowed them work more effectively together. The chance to practice collaborating repeatedly over an extended period of time seemed to be an important factor that influenced this outcome. For some groups, it took a few weeks for good peer relationships to develop, with participants seeing the benefits of this only when reaching their civic participation project in Phase 3.

> *"[In Phase 1] there was times where…there was a bit of an argument or a bit of tension in the group…when things weren't working properly. So, it highlighted where there would be issues and then how we could solve them… [By Phase 3], we were three weeks into the programme…so we knew each other a lot better… [T]he first week it didn't go as well, so, I guess we did improve our skills by the time we got to that point… [H]aving better relationships definitely helped. I think…we had done a lot of things that…prepared us for getting to week three." Hannah*

As with James' improved social interaction and communication skills described above, Hannah's improved ability to work with others seemed to be moderated by an openness to the views of others prior to NCS. Elsewhere in her interview, she described regular, respectful political debate in her family, even with relatives who hold very different political views. The second capability identified in this category was the ability to collaborate to achieve a common goal. Here, participants described learning the importance of the division of labour and taking on or assigning roles that suited the individual.

"We did this…pretend rescue mission. We had…a map and we had to…find this person that'd collapsed somewhere else around the area. So, then…we had to…give out different roles and someone had to have the map, someone had to lead the team and make the decisions. And…when we got the person they had to be… administered tablets once every two minutes, so someone had to have the timer and someone had to administer. So, [we learned to give] out different roles and [make] decisions." Hannah

Again, participants described honing this skill through non-civic activities and then applying and refining it on their civic participation projects, for example when delivering collective fundraising pitches.

The third skill identified in the teamwork category was group decision-making. Some participants learned to more effectively make collective decisions through informal discussion, as well as through formal processes like voting or assigning a leader with decision-making powers. Learning to compromise in group decision making was also identified as an outcome for some.

But while a range of participants described these three dimensions of teamwork as proximal outcomes, none related these outcomes directly to an increased motivation to participate in political activities, even when specifically prompted by the interviewer to consider this. For one participant, Josh, for example, the key determinant of his political participation was his interest in the issue at hand. Without this interest, his newly developed skills of collaboration made no difference to his political motivations.

5.8.2.3 *Planning and organisation skills*

The final capability-based outcome identified as potentially linked to political participation was planning and organisation. The evidence for this outcome was weaker than for the prior two, as no interviewee mentioned it without prompting from the interviewer, and no direct connection was made between this outcome and participants' future political participation. However, the programme did clearly require participants to plan and organise, and planning capability does appear as a potential mechanism in the literature, so it is considered here. The collective tasks described in the section on teamwork above all required some element of planning prior to execution, and the complexity of this planning increased as the programme progressed. Participants' civic participation projects required the most thought and coordination as they involved a series of activities over a number of days. Examples of these projects included planting/renovating gardens at care homes, fundraising for charities, and collecting supplies for food

banks. In relation to these activities, participants described developing their abilities to write formal plans, task lists and risk assessments, to assign roles to tasks (as described above), and to troubleshoot problems that arose during project implementation.

> *"[NCS helped with my planning and organising] because for our social action plan we had to…write out a bunch of stuff… We were able to call different places, and then we'd…write down a list of places we needed to go to in town to check if we could volunteer there or ask questions and things like that. So I think we organised it really well and that was actually quite helpful because then…if I wanted to do something similar now I'd be able to follow the same sort of steps that I did before." Titi*

In summary, three capability-based proximal outcomes were identified from the analysis that could have influenced some participants' future political participation: i. social interaction and communication skills; ii. teamworking skills; and iii. planning and organising skills. The evidence for the first of these outcomes being a true mechanism is the strongest, with a full causal chain being described by some participants with social interaction and communication skills sitting in between their service learning experience and their subsequent political participation. The evidence for the latter two outcomes is weaker as they only appear as proximal outcomes in the analysis, but they do both relate to mechanisms that are hypothesised in the literature.

5.8.3 Attitudes and beliefs

5.8.3.1 *Self-efficacy*

Four types of self-efficacy[44] were identified as proximal outcomes for some participants: i. general self-efficacy; ii. social self-efficacy; iii. service self-efficacy; and iv. political self-efficacy. However, the evidence of causal chains that connect these outcomes to political participation was limited. There was no

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[44] Though participants did not use this word themselves, the term 'self-efficacy' is used here because it is more precise than alternative words like 'confidence' and helps to situate the findings in an established body of literature. As Bandura notes: '[T]he construct of self-efficacy differs from the colloquial term "confidence." Confidence is a nondescript term that refers to strength of belief but does not necessarily specify what the certainty is about. I can be supremely confident that I will fail at an endeavor. Perceived self-efficacy refers to belief in one's agentive capabilities, that one can produce given levels of attainment. A self-efficacy assessment, therefore, includes both an affirmation of a capability level and the strength of that belief. Confidence is a catchword rather than a construct embedded in a theoretical system. Advances in a field are best achieved by constructs that fully reflect the phenomena of interest and are rooted in a theory that specifies their determinants, mediating processes, and multiple effects. Theory-based constructs pay dividends in understanding and operational guidance. The terms used to characterize personal agency, therefore, represent more than merely lexical preferences' (Bandura 1997, p. 382).

strong evidence that an increase in general self-efficacy led to an increase in political self-efficacy or political participation. There was stronger evidence that social self-efficacy could be a true mediator for those with high baseline levels of political interest and participation. There was also some limited evidence of developments in service self-efficacy having positive knock-on effects on political self-efficacy and political participation, but clearer evidence of no effects for some and even negative effects for others. These findings expose some further heterogeneity in effects and offer more insights into potential moderating factors. The details of each form self-efficacy as a proximal outcome and as a mediator are discussed in turn below.

There was evidence to suggest that some participants' **general self-efficacy** increased as a result of the programme. These participants described these increases either as a result of participating in NCS as an overall experience – which some were nervous about – or after taking part in outward bound activities that were new and frightening. Doing these activities revealed to the participants that they were capable of things that they previously considered to be out of their reach. Consequently, they felt more confident in their ability to try new things in general. These mastery experiences were sometimes encouraged by vicarious experiences; i.e. seeing peers doing the activities helped some to believe that they could do them too.

> *"[D]uring the adventure week, [there was] a bunch of things we did…[that] I thought I would not do…[W]e did…body boarding and…before that time I'd never swam in the…sea and I didn't think I'd be able to do [it]. I was scared of…fish and jelly fish and things like that. And I did see a jelly fish but I wasn't scared…everyone else was doing it and I was thinking if they are doing it I can do it too. I just joined in and it was really fun… I pushed myself a bit more than I would have done before… I was really…proud of myself and I feel like if I could do that then I can do anything." Titi*

There was some limited evidence to suggest that this increase in general self-efficacy could lead to increased political self-efficacy and political participation. After prompting, one participant, Emily, described being more confident about doing new things in general and suggested that this could extend to political participation, but she was not sure on this last point.

There was strong evidence to suggest that a diverse range of participants developed their **social self-efficacy** as a result of their service learning experience. 'Social self-efficacy' in this context refers to individuals' beliefs in their social interaction and communication skills (their internal social self-

efficacy), and their beliefs that applying these skills can achieve positive outcomes (their external social self-efficacy). Increases in internal social efficacy were sometimes linked to the increases in capability described in the preceding section. Participants who described developing and applying these skills – interacting with different types of people (new adults and diverse peers) and in new ways (socially, presenting, debating, and influencing) – also described an increased belief in their abilities. Some participants also gave examples of their external social self-efficacy increasing when applying these skills yielded positive results. These 'results' took the form of praise from peers for a talent show performance, making new friends, and getting positive feedback from adults when pitching for support for their civic participation projects.

Three features of NCS were identified as supporting these increases in social self-efficacy. First, the programme offered a lot of mastery experiences in this domain. All participants were placed in teams for a prolonged and intense period of time and had to build relationships with new peers accordingly. Some also took part in formal debates and group presentations, and interacted with members of the public, service managers and service users as part of their civic participation projects. Second, these experiences forced engagement with new types of people, which was challenging for some. And third, those who overcame these challenges described feeling supported by staff and being in a safe environment to try new things.

> *"NCS proper helped me with my anxiety… I made so many friends I still keep in contact with and I wouldn't have been able to do that unless I did [NCS]. [The staff] wouldn't…push us to do something we didn't want to do but they strongly suggested we get out there and do something. And so we [were] put into teams [and] we [had] to talk to someone." Lucy*

For some participants, like Lucy, these gains did not seem likely to transfer into increased political participation in the near future, due to low baseline levels of political knowledge. Lucy explicitly said that she did not feel that she knew enough about the issues and the political parties in England to participate. There was however evidence that, for some, these increases in social self-efficacy did have a knock-on effect on their political self-efficacy and participation. Priya provided one of the clearest cases of this type.

Case study: Priya

Priya lives in a town/city in the West Midlands, has been eligible for free school meals and identifies as Asian. She has been politically active from a young age, and this participation has been strongly linked to her religious and cultural community. She attends her local place of worship on a regular basis where she has learned about the history and politics of her religion and participated in a youth board which, among other things, gave her the opportunity to meet and question a number of prominent politicians. Her dad is an elected representative within the place of worship, giving her further insights into the democratic process. She is also politically active outside of her religious community, and is particularly engaged in petitioning, sometimes mobilising others to participate. Through these experiences, Priya has developed a passion for democracy and, in particular, minority rights. All of this began before NCS.

Even though she has a lot of experience of political participation relative to her peers in the sample, Priya still feels that her service learning experience had a positive effect on her general and social self-efficacy, which in turn led to an increase in her political self-efficacy and the type of participation that she felt capable of in the future.

> *"[NCS] pushed me out of my comfort zone. Before NCS I would never, ever have jumped off a waterfall… And [I had to speak] to people that I've never known before… [If] I want to get to the higher rankings [in politics], someone that's [in the public eye], then [I] need to have these skills or qualities… [NCS] has motivated me to get involved in…more public politics because it's shown me that I do… actually…have the skills." Priya*

So, while she had a strong motivation to participate in political activities prior to NCS, these self-efficacy effects seem to have increased the likelihood that Priya will engage in more challenging political activities – like becoming an elected representative – in the future.

As an example of someone who was very highly engaged in politics prior to NCS, Priya's case is similar to Olivia's, presented above in 'Capabilities'. Both reported an increased motivation to participate in politics after their service learning experience, but they differ slightly in their descriptions of the causal chain that led to this. For Olivia, an increase in her capabilities led directly to an increase in her motivation. For Priya, there was an extra step; an increase in her

ability to communicate and interact socially led to an increase in her social self-efficacy, which in turn increased her motivation.

Similar effects to Priya's were identified among other participants who also had high baseline interest and participation in politics, and there was also evidence from one case that social self-efficacy could be a decisive factor for civic participation for some.

> *"[C]onfidence is the main thing I struggled with. I've always wanted to volunteer but the confidence was never really there… [I]f I saw that someone needed help I wouldn't go up and offer it to them – not because I'm an awful person – I just wouldn't be able to talk to them." Jess*

For this young person, her lack of social self-efficacy was, according to her, the only thing that was holding her back from civic participation prior to NCS. Developing this self-belief through service learning, made her more likely to participate. While the type of participation that she describes is clearly non-political, this example does highlight how significant social self-efficacy can be for some young people in determining their behaviour. Further evidence of this significance was seen in the analysis of participants with very low baselines on this outcome. A range of participants in this category reported gains in their social self-efficacy but also explicitly stated that these gains were not big enough for them to feel confident in engaging in political activities, which they perceived as generally requiring communicating with new people. This variation in the sample suggests that whether increased social self-efficacy leads to increased political participation depends at least upon a participant's prior political interest and motivation, as well as her level of social self-efficacy prior to participation.

Service self-efficacy was also identified as a proximal outcome for some. Participants described developing their internal self-efficacy in this domain, with delivery of their service-based projects proving to some that they were capable of performing the necessary tasks, and that these activities were not as difficult as they first imagined. Some also explicitly described an increase in their sense of collective internal efficacy; i.e. that they could carry out civic tasks in collaboration with others. External efficacy development was also identified, with a range of participants describing an increased belief in their ability to make a difference through their service. Three features of the experience were identified as supporting this belief. First, receiving positive feedback from

beneficiaries – for example, residents of a care home enjoying their renovated garden, or managers of services showing gratitude for young peoples' volunteering efforts – offered evidence to participants that they had made a positive contribution. Second, the presence of a clear and important need for support could enhance this belief. For example, one participant, Titi, was part of a group that organised donations to a local food bank. When delivering these donations, she and her teammates met a mother who was visiting the foodbank for food and clothes for her baby. For Titi, seeing first-hand the severity of the need at the foodbank added to her sense that she was able to make a valuable contribution. Third, the financial results of charity fundraising efforts acted as concrete evidence for some that they were able to make a difference. However, some participants described this final piece of 'proof' of efficacy as a second order one, because raising money was seen as a less direct contribution.

> *"[We packed bags with books, lunch boxes, drinks bottles and school supplies for children in developing countries]. [W]e were able to pack them ourselves as well so not only we were able to see what we accomplished, we were able to take a part in what we accomplished. So like some [charities] just take the money and unless you go to that place and you see the improvements, you're only told about it." Josh*

There were also some participants who reported negative effects on their service self-efficacy for one of two reasons. First, some young peoples' civic participation projects were not as successful as they had hoped. For example, one participant described her team as dysfunctional. The group split during the civic participation phase of the programme, which led to a project that was poorly planned and executed. Second, some participants came up against external barriers that prevented them from performing the service that they saw as most valuable. For example, some participants wanted to work directly with vulnerable groups such as homeless people or those with special educational needs, which they were not allowed to do because of their age. These failures of execution and confrontations with external barriers led to some feelings of reduced service self-efficacy.

Where increases in service self-efficacy *were* felt, some participants described positive effects on their political self-efficacy and future participation, some (explicitly) described no effects, and some even described negative effects. In the first category, participants described a causal chain starting with their civic participation experience during service learning, that led to an increased sense of service self-efficacy, that in turn led to an increased sense of political self-

efficacy. Some suggested, after prompting, that this chain may end up leading to an increase in their political participation, though none of these individuals had yet participated in a political activity after NCS and all expressed uncertainty about this final step. Hannah was a good example of a case like this.

Case study: Hannah

Hannah lives in a rural area in the East of England, has been eligible for free school meals and identifies as white. She describes herself as very interested in politics – particularly issues related to poverty, welfare benefits, the NHS, and food banks – as these are all things that she has experienced personally. She had a good experience of citizenship education at school, and enjoys discussing political issues with her parents at home. She had high general and social self-efficacy prior to NCS, seeing herself as intelligent, able to argue her point and listen. However, she also describes herself as "not that motivated" to engage in political activities. She has not participated in anything political beyond signing the occasional petition that has come to her through social media. This may in part be down to her low political self-efficacy. She wasn't sure how to participate politically and doubted that she would be listened to even if she did (mainly because of her age).

Hannah felt an increased sense of service self-efficacy after her service learning experience and hypothesised that this could positively influence her political participation in the future.

> *"I think it's showed me that I can make a bit of a difference in my community… [W]ith…the fundraising…I felt like I achieved quite a lot. [W]e did…a raffle and I was in charge of getting all the…resources and…I…wrote out loads of…emails and I got so much back…from businesses…donating stuff to me. I…felt that was quite an achievement and I realised that…I could do stuff and make a bit of a difference. I think it shows that you can do something. So, then I probably would be a bit more inclined to figure out what I can do because…I was worried…that I wasn't going to get any emails back, I wasn't going to get any resources back. But actually, I got more from it than I thought I would. So, maybe that would apply to like political activities… I'm still…not sure. But I think…having that memory and that experience…might have helped to…make me feel a bit more like I could. [If it was a political issue that was] extremely important to me; if it's something that really directly affects me and people I care about, then I think that would probably tip me over the edge." Hannah*

This case also re-emphasises the importance of issue interest as a moderating factor. Hannah's report suggests that her enhanced service self-efficacy will only lead to an increase in her political participation if the issue in question is

relevant enough. As with Josh (described above), this moderator seems to be a deciding factor. Elsewhere in her interview, Hannah also expressed a general scepticism about the ability of young people to influence politicians and political institutions.

Hannah's was the clearest articulation of service self-efficacy leading to increased political participation, but it comes with substantial equivocation. Next were participants who felt a positive effect on their service self-efficacy but explicitly reported no subsequent effect on their political self-efficacy or participation. Three reasons were identified for this. First, for some, their characteristics prior to NCS seemed to play a strong role. There was a group of participants who came to the experience with a low interest in politics, low political self-efficacy and low levels of prior participation, accompanied by little or no political socialisation in the home. These participants described a small effect of NCS on their service self-efficacy, but this was far from enough to overcome their very low levels of political motivation. Second, for others, a perceived lack of similarity between service and political participation was the barrier; meaning that their gains in service self-efficacy were, in their minds, unrelated to any feelings that they had about their ability to engage effectively in politics. Third, there were participants who came to their service learning experience with such high levels of interest in – and knowledge about – political participation that their sense of political self-efficacy was not affected. These participants shared in common strong stories of political socialisation and civic education, whose effects were not altered by a service learning experience that was perceived as having very little to do with politics. For example, Oliver was very knowledgeable about climate change and the failure of political institutions to address the problem to date. He was cynical about the likelihood of these institutions changing in the future, and sceptical about the ability of political activism to create change. These detailed and well-reasoned thoughts were unchanged by his service-based experience of civic participation on the programme, even though he reported increases in his service self-efficacy.

Finally, there were those who described positive effects on their service self-efficacy but some negative effects on their political self-efficacy and participation. For these participants, their service learning experience had contributed to a preference for service-based civic participation. This was based

on a view that political change is harder to achieve because it requires support from more people (including influential people) and takes longer. During these discussions, some also expressed the view that political protest makes no positive contribution and often has negative effects.[45] For these participants, their positive experience of service-based participation with its directly observable positive outcomes contrasted with and reinforced these views about politics. There were participants from a range of backgrounds, with varying levels of political interest in this category, but this effect was most clearly described by a participant who was highly politically engaged prior to NCS.

Case study: Tom

Tom lives in a town/city in the West Midlands, has not been eligible for free school meals and identifies as white. He had a lot of experience of political participation prior to NCS, including close links to the Labour Party through his mum, who was a local councillor and had run to be an MP on two occasions. He supported her by campaigning on the doorstep from a young age, took part in protests with her, had experience of petitioning and had met senior politicians on numerous occasions. In interview, he displayed a high level of critical thinking, which seems to have substantially moderated the effects his service learning experience.

Tom described his experience of service-based participation on NCS as having a positive effect on his motivation to take part in similar action in the future. In part, his experience made him more aware of his own privilege relative to the least well-off, but it also enhanced his sense of service self-efficacy; seemingly at the cost of his political self-efficacy.

> *"I think I'd prefer to do social action than political action [in the future]. [M]y experience in politics is that political action doesn't necessarily work because they say if a petition reaches 100,000 signatures then it will get debated in parliament. The debate can quite often just be a couple of words or not that long and just saying we realise your point, there's nothing we can do. Whereas social action can have an effect. [NCS helped to show me] the effect people have when they do social action projects. The effects that I had…on the people we were helping was more profound than I thought it would be." Tom*

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[45] This last point was related to not just to participants sense of efficacy, but also their sense of social responsibility. The latter idea is picked up again below in the discussions of social responsibility as a mechanism and a moderator.

Tom asserted this new preference for non-political participation, even though he also saw the limitations of this type of action in terms of the scale of its impact. He knew that he and his team had only made a small difference to the homeless people that they were supporting and, at other points in the interview, he described the large, long-term impact that political action can have, but he still expressed a preference for service when given the choice.

5.8.3.2 *Prosocial responsibility*

Some participants' sense of prosocial responsibility seemed to be affected by their experience, but there was limited evidence that this led to an increased motivation to participate politically. There was stronger evidence to suggest that, for those participants whose sense of responsibility was altered, there was a positive effect on their motivation to participate in service-based activities, possibly at the cost of political participation for some. This effect seems to be closely connected to enhanced feelings of service self-efficacy and self-esteem, derived from positive experiences of service on the programme.

When discussing the balance of responsibilities between citizens and the government, some participants described feeling a greater sense of personal responsibility for solving social and environmental problems as a result of NCS. These young people had come into the programme believing that the government was almost solely responsible for these matters and left feeling a greater sense of personal civic duty. This feeling either came from a message that they had picked up from marketing materials and other communications – one participant mentioned the programme's 'No We Can' slogan – or from experiences of giving direct help to people through voluntary service. In this latter category, participants described feeling more responsible for helping others after becoming aware of their relative privilege (compared to homeless people that they had met through their project, for example) and becoming more aware of pressing needs in their communities.

There were, however, also a range of participants who reported no effect on this outcome after direct questioning. These participants felt differing levels of prosocial responsibility but were all clear that, whatever their view, NCS did not influence it. For these participants, this null effect seemed to come from of a lack of explicit discussion of the matter during the programme.

Participants who did describe increases in their prosocial responsibility sometimes showed greater levels of critical thinking during their interviews, so perhaps did not need such a discussion in order to derive the effects described above. However, there was no strong evidence of any increased sense of prosocial responsibility leading to an increase in political participation. One participant suggested that she may participate civically more as result of feeling more responsible, but she had not done anything yet and did not single out political participation. Others explicitly linked their new sense of personal responsibility to a preference for non-political participation. For example, Tom's new-found preference for volunteering and charitable giving was partly based on an argument about efficacy (as described in the case study above), but also on a new sense that he should not expect the government to solve everything, and that he had a duty to start making a more direct contribution. Individuals' sense of social responsibility prior to NCS also seemed to play an important moderating role here. A range of participants expressed socially conservative views that manifested in an aversion to political protest, which they perceived as socially irresponsible, illegal and inconveniencing others.

> *"I don't want to take part in protesting… I feel like I'm then on the wrong side… [A] protest is physically going against the thing that's normal and I don't like doing illegal or wrong things, I just quite like to get things done in my own way… Say instead of demanding a pay rise through a protest or equality through protest, I'd…go straight to the highest person I could talk to."* Oliver

5.8.3.3   Self-esteem

Feelings of increased personal responsibility were sometimes described alongside feelings of enhanced self-esteem. A diverse range of participants – including those with very high and very low levels of civic interest and participation – related feeling good about themselves, proud of the help they had given, and a sense of being needed and appreciated as a result of their service. For some, these ideas contrasted with feelings that they had prior to their service learning experience of being too young to make a meaningful contribution to society.

> *"It made me feel important… [I]t just highlighted how as young people we can make a difference and we don't need to feel useless because I think that's a common feeling... When in reality, like the old people seeing us for a day…they told me how much of a difference that made to their happiness... So it's just…made us feel needed."* Ellie

Two characteristics of voluntary service reappeared here as important factors: direct contact with beneficiaries in need, and an immediate sense of

achievement. This self-esteem effect seemed to further reinforce the effects of service self-efficacy and prosocial responsibility described above in developing a preference for service-based participation over political participation for some participants.

5.8.4 Knowledge

Two types of political knowledge were identified as proximal outcomes for some participants: i. a new basic awareness of social and/or environmental issues; and ii. increased knowledge of how politics works. The gains described in these two areas were marginal, and there was no strong evidence to suggest that either led to an increased motivation to participate politically. These apparently small and null effects seem to be down to a lack of in-depth knowledge-based discussion on the programme. Participants' prior knowledge – developed at school, through political socialisation, and through the media – also seems to play a role in moderating the effects. Even those with moderate-to-low political knowledge reported learning nothing new on the topic during NCS.

5.8.4.1 Awareness of social and environmental issues

Some participants with low levels of prior knowledge and interest reported slight increases in their basic awareness of social and/or environmental issues. For example, one participant described becoming aware of issues relating to equal pay and workers' rights for the first time during NCS. These developments occurred during semi-structured discussions facilitated by a youth worker during the second and third phases of the programme, or through completely informal discussions that occasionally occurred in groups during unstructured social time. There was weak evidence to suggest that these marginal gains led to an increased motivation to participate politically. One participant suggested that her increased awareness might make her more likely to participate in political activities in the future. However, at other points during her interview the same individual expressed very low levels of political interest and motivation, had not engaged in any political activities prior to or after her service learning experience, and was unsure as to whether she would ever participate (even in low effort activities like signing a petition).

Others expressly stated that NCS had no effect on their issue knowledge. Two reasons were identified for this lack of effect. First, some participants' prior level

of knowledge was too high. Some had extensive prior knowledge of some issues; for example, a deep understanding of climate change and its causes, developed through GCSE subjects like Geography combined with personal research. But even those with moderate-to-low levels of knowledge, picked up from mass media and personal experience, claimed to have learned nothing new – even about the issue that they addressed through their civic participation project. Second, some participants suggested that, where social or environmental issues were discussed on the programme, the focus of those discussions was either on participants' opinions or on what they could do to make a difference through volunteering, rather than on learning new facts.

5.8.4.2   How politics works

The second category of knowledge development identified related to how politics works. The gains described here were also slight. Some had vague recollections of workshops that described how government works in the UK, but were unable to recall any specific details, even after probing. Others described informal discussions facilitated by their youth worker about ways that a citizen can participate, reporting that this was the first time that they had thought about protest or contacting an MP before. One participant, Tom, described developing a deeper understanding of who holds power and influence and who is marginalised in politics.

> *"There was one thing we learnt: that…quite a lot of the stuff is done [in politics] to help the people who have the ability to effect…politicians; [for example] people who are older; they are people who are in power themselves. [S]ome people aren't necessarily represented. So…government is quite self-serving… [T]he people who are in parliament are all quite similar, similar ages…all generally quite rich and…they are out there to help the people who fund them for their next election; for people who voted for them rather than…the people who didn't." Tom*

However, as we have seen above, Tom's service learning experience seems to have led to preference for non-political participation, and the new knowledge that he describes may have contributed to this (though he did not say so explicitly). For the participants who described more basic effects on their understanding of how politics works, no evidence was identified that this led to an increased motivation to participate politically either. It is also worth noting that Tom's high level of political interest and knowledge at baseline has likely moderated the effect on his knowledge here. Without this prior interest and understanding, he may not have learned and retained the new knowledge that he

reports. Conversely, a lack of such interest and understanding on the part of other participants may partly explain the lack of new knowledge gained.

As with the issue-based knowledge, there was also a wide range of participants who reported no positive effect on their understanding of how politics works. This was either because the topic was not covered at all (or at least they could not remember it being covered), or because the level at which it was discussed was lower than their prior knowledge (gained through Citizenship classes, political socialisation, prior participation experiences and the media).

5.8.5 Networks of recruitment

A small positive effect on participants' networks of recruitment into political activities was identified from the interviews. Where it occurred, this effect was driven by peer-to-peer communication, rather than any information provided by NCS. One participant who was highly politically engaged prior to NCS described sharing information about political participation opportunities and relevant social media accounts with her less-engaged peers.

> *"[T]here was another person on…the same wave of NCS as me who did similar [political] stuff and sometimes we would talk about that and other people would get involved in our conversations and they'd be interested in how…to get involved in what we do. So we would just give them…the information that we have…like the Instagram accounts that publish stuff about when the next strike's going to be, who it's going to be run by and…just stuff like that." Ellie*

This participant was unable to say whether any of these peers took up any new opportunities as a result of this information sharing. However, she did describe making a similarly politically engaged friend on NCS who took her on a protest about a second Brexit referendum. She gave this as a concrete example of a political event that she would not have attended – because of a lack of interest in the issue – had it not been for this invitation.

There were also reports from a wide range of participants, however, that they developed no new networks from NCS that were relevant to political participation. Any positive effect in this area seems therefore to have been moderated by the chance composition of the cohort; i.e. whether there were any participants who were politically engaged and influential enough to effectively share relevant information.

5.8.6 Exploratory quantitative findings

These qualitative results reveal some surprises. They suggest that social self-efficacy might a more powerful mediating mechanism than political self-efficacy or general civic self-efficacy in this context. They also suggest that social anxiety, and social and communication skills are important mechanisms. This generates four quantitative hypotheses, summarised in Table 5.7.

Table 5.7: Summary of exploratory hypotheses

Hypotheses
H4.1: NCS increases social self-efficacy more than general civic self-efficacy.
H4.2: NCS increases social self-efficacy more than political self-efficacy.
H4.3: NCS decreases social anxiety.
H4.4: NCS increases social and communication skills.

To test these hypotheses, the ATT is estimated for the relevant outcomes and the results are presented in Figure 5.2 and Table 5.8. The three types of self-efficacy and the social and communication skills were measured using 5-point Likert scales, so *diffsoceff*, *diffciveff, diffpoleff* and *diffsoccom* can take values between -4 and 4. Social anxiety is measured on an 11-point Likert scale so *diffanx* can take values between -10 and 10. To make the results easier to compare, normalised differences (also known as 'effect sizes' or 'Cohen's d') are presented in the final column of the table.[46] The findings below provide partial support for the hypotheses. All estimates are robust to corrections for multiple comparisons (presented in Annex I to this chapter).

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[46] Note that this normalised difference, or 'Cohen's d', has been calculated using the standard deviation of the total sample on common support, as opposed to the pooled standard deviation as defined in Equation 6. This approximation is necessary because it is not possible to calculate the standard deviation in the comparison group, as some units are used more than once to estimate the effects. Calculating the standard deviation of the 'effective comparison group' – i.e. a synthetic dataset containing multiple observations for some individuals – is also not appropriate because duplicated observations are not independent. Assuming such independence would mean that the variance of the mean decreases with the increased sample size, which is incorrect. An approximation is therefore presented here for readers who are interested in getting a rough sense of the effects in terms of standard deviations. However, the findings and the conclusions presented do not rely on this quantity.

Figure 5.2: Effects on self-efficacy
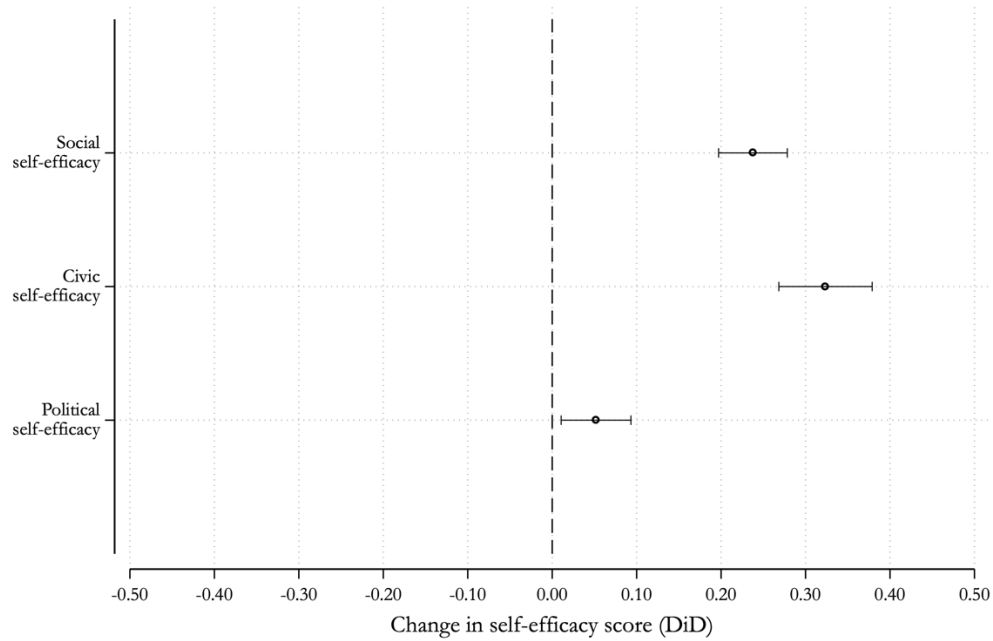
Table 5.8: ATT estimates (DiD) for the outcomes of interest

	Estimate	(Standard Error)	Normalised difference, $\hat{\Delta}_{ct}$
Civic self-efficacy	0.32***	(0.028)	0.39
Political self-efficacy	0.052*	(0.021)	0.08
Social self-efficacy	0.24***	(0.021)	0.37
Social anxiety	-0.51***	(0.114)	-0.16
Social and communication skill	0.045	(0.053)	0.02

Notes: N = 5,461. $\hat{\Delta}_{ct}$ has been calculated using the standard deviation of the total sample on common support, as opposed to the pooled standard deviation as defined in Equation 6. It is therefore only an approximation of Cohen's d. + p<0.1 * p<0.05, ** p<0.01, *** p<0.001

The results do not support H4.1. They show that there is no detectable difference in the size of the effect on social self-efficacy as compared to civic self-efficacy, with the confidence intervals for these two estimates overlapping. There is support or H4.2, however. NCS does seem to increase participants sense of social self-efficacy more than it does their political self-efficacy. As predicted by H4.3, NCS also decreases participants social anxiety. Interestingly – and contrary to the prediction in H4.4 – there seems to be no effect on participants' social and communication skills. So, the observed gain in social self-efficacy and decline in social anxiety may be driven more by participants' *perceptions* of their abilities, rather than their *actual* abilities in this domain. These

quantitative findings, when read alongside the qualitative results, provide further support for the idea that social self-efficacy is an important part of the explanation for *how* service learning increases political participation.

5.8.7   Revised logic model

The results are summarised in a revised version of the logic model in Figure 5.3. This diagram shows the activities, mechanisms, causal pathways, and moderators that were identified as present for some participants in the study. If an item appeared in the pre-study model – because it was identified in the literature as potentially important – but found no support in the findings, then it is not included in the revised model. If a construct is identified in the results as a proximal outcome, but there is no evidence to show it acting as a true mediator, then it is also excluded from the model. Mechanisms that are supported by at least 'moderate' evidence (as defined in Table 5.5), are coloured in darker blue. Those supported by weaker evidence are coloured in lighter blue.

There are six key differences between this model and the model that was created based on the pre-study literature (Figure 5.1). First, an additional class of activities has been identified – political and issue-based discussion. Participants described taking part in both formal and informal discussions about social and environmental issues, sometimes with a political angle and, for those with more limited baseline knowledge, this raised awareness of some issues for the first time, which may have sometimes been a politically motivating factor (though the evidence for this mechanism was relatively weak). Second, receiving positive feedback from mastery experiences – in the form of praise from peers and staff, and thanks from the beneficiaries of service – has also been identified as an important additional activity in triggering enhanced self-efficacy. The guided reflection category of activities has been expanded to include this new idea. Third, there are some subtle differences in the factors identified as important in the cognitive appraisal that precedes a development in service-based self-efficacy. Where the literature suggests that the *perceived difficulty* of the service activities undertaken is key (Bandura, 1997), the interviews suggested that it was the *perceived importance* of these activities that made the difference. There was also no evidence suggesting that participants' achievements needed to be congruent with their existing beliefs about their capabilities, so this factor is dropped from the model. Fourth, the capabilities identified in the literature as important were

either not present in the findings as mechanisms or, if they were, they carried different emphases. Mechanisms in the initial model relating to planning and organisational skills, are dropped in the revised model, and the communication skills are redescribed. More is said about this in the conclusions below. Fifth, two new forms of self-efficacy have been added as mechanisms: general self-efficacy and social self-efficacy (the latter being supported by stronger evidence). Sixth, a detailed set of moderators has been added in two parts: participant characteristics at baseline, and characteristics of the experience. The mechanisms that these factors act upon, and the ways in which they do so, are too complex to show in the diagram. These, as with the whole picture, should be read alongside the summary tables presented in the introduction to the findings (Tables 5.5 and 5.6), and with the narrative findings themselves.

Figure 5.3: Revised logic model of service learning and political participation
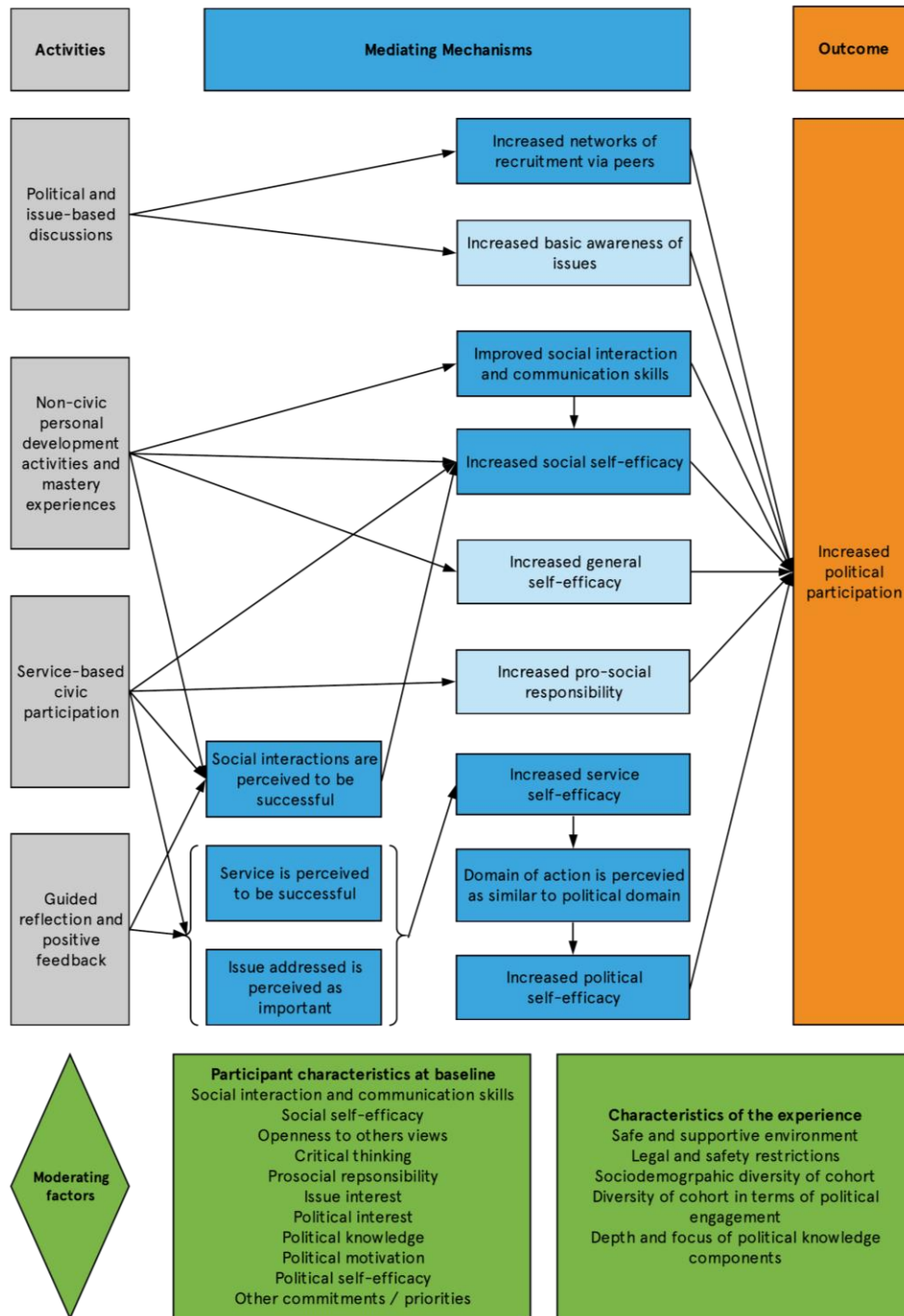
5.9 Conclusions

Study 1 in this thesis suggests that, on average, service learning can have a positive effect on young people's future political participation. This present study (Study 2) adds to our understanding by explaining the mechanisms by which this effect is produced, the activities that trigger these mechanisms and the factors that moderate the effect. The findings also suggest that the average treatment effects estimated in Study 1 mask substantial heterogeneity. This

heterogeneity may in part explain the disagreements in the literature as to whether service learning has a positive, null or negative effect on political participation. In one sense, all of these positions may be right, and the disagreement may be driven by a lack of robust and precise analysis to date. The findings here suggest that even the same programme of service learning can have positive, null and negative effects, depending on the individual, her characteristics prior to service learning, and her experience of the intervention itself. Where previous research has made unhelpful generalisations about treatment effects, supported by weak research designs, this study, as part of a mixed-method project, does the opposite. It adds to the literature by revealing heterogeneity in effects, and the conditions under which these different effects are experienced.

The interviews for the study drew on life and oral history approaches; tracing the interviewees' political motivations and behaviours over the course of their lives, seeking to identify the main influencing factors, and the role that NCS did or did not play within this wider context. To do this, the interviews began with a discussion that gave participants the opportunity to explain the key influences on their political views, knowledge and participation. No direct questions were asked about NCS during this part of the interview, and no participant raised it as an influencing factor during this phase. Instead, they described influences that are familiar in the general literature on political participation; factors like political socialisation from family and friends, civic education in school, political trust, political self-efficacy, feelings of representation by current political parties, the perceived effort of taking part, and prior experiences of participation. The fact that NCS was not mentioned during this part of the interview is perhaps surprising when we consider that it was a recent experience related to civic participation and citizenship. Interviewees were also told at the beginning of the interview that the topic of the study was the relationship between NCS and political participation. This suggests that, at least relative to the other factors identified, NCS was not a formative experience.

However, Study 1's findings suggest that there is a significant and substantial positive effect on average and, after explicit questioning and probing, a range of participants did report positive effects from NCS on their political participation, and a set of mechanisms have been identified that might explain these effects.

These mechanisms fall into one of four categories: i. capabilities; ii. attitudes and beliefs; iii. knowledge; and iv. networks of recruitment. These categories are broadly in line with the theory inferred from the pre-study literature, but there are substantial additions and differences in the details. In total, seventeen proximal outcomes have been identified across these four categories. Three of these outcomes – social interaction and communication skills, social self-efficacy, and networks of recruitment – stand out as having the strongest evidence to suggest that they can be true mediators (under the right circumstances). For two further outcomes – service self-efficacy and political self-efficacy – there is weaker and conflicting evidence that they may also play a mediating role. These five outcomes are discussed below in relation the literature.

5.9.1   Social and communication skills and self-efficacy

There is relatively strong evidence to suggest that some participants developed their social interaction and communication skills, and their social self-efficacy during NCS. A range of interviewees reported improvements in these areas without prompting, with certainty, and using concrete examples that linked the outcomes to relevant activities. These reported gains in skill and self-efficacy were also linked by some to increases in political participation.

On the face of it, these findings are supported by the literature, which suggests that civic participation experiences can develop communication skills, which can in turn contribute to an increase in political participation (Verba et al. 1995, pp.304-313). There are, however, important differences between the literature and the findings from this study. Verba et al. suggest that participants of civic voluntarism see improvements in their abilities to participate in decision-making meetings, speak publicly and write letters. The findings from this study support the first two of these outcomes. However, this study suggests that – when it comes to mediators of political participation – the more important social interaction and communication skills developed through service learning are not technical, but 'soft'. The outcomes in this field that seemed to make the biggest difference were increased empathy, increased ability to listen to different points of view, increased patience, increased ability to make new friends, and reduced prejudice. The literature also says nothing about social self-efficacy as a mechanism, but this was described as a decisive factor by some participants. The

exploratory quantitative analysis suggests that this *perception* of abilities is perhaps more important than the abilities themselves. This analysis estimates that NCS has a substantial ATT (equivalent to 6pp) on participants' social self-efficacy, but no average effect on their social or communication skills.

These findings are not only new but, perhaps, surprising. The communication skills identified by Verba et al. are clearly things that are often needed for effective political participation. The softer skills and social self-efficacy identified by this study have a less obvious connection to politics. However, the analysis does reveal a clear logic. In particular, a range of participants described feelings of social anxiety, which were partly alleviated by their NCS experience. This finding is again supported by the exploratory quantitative analysis, which estimates another substantial ATT (equivalent to -5pp) on participants' level of anxiety. Participants interviewed associated political activities with social interaction and felt more able to participate when their sense of social self-efficacy was increased. These results are also supported by the latest experimental literature in this field. Recent work by Holbein and Hillygus links general noncognitive development programmes (not specifically service learning programmes) to electoral turnout, and suggests that generalised self-efficacy, empathy and emotional control are key mediators (2020, p.107).

The result makes further sense when we consider that young people are particularly sensitive to the social world; their sense of self-worth is more closely tied to the opinions of others and they fear social exclusion more than adults do (Blakemore 2019, pp.36-39). However, while a range of young people described feeling increases in their social self-efficacy, it seems that this outcome only had a positive knock-on effect on political participation for those with a high level of interest and participation in politics prior to NCS. The results in section 5.8.4 suggest that this type of service learning lacks the activities required to substantially enhance the knowledge of, and interest in, politics that seems to be a pre-requisite here. If you do not arrive on the programme with this knowledge and interest, you are unlikely to develop it during your experience and,

consequently, your likely gains in social efficacy will not support gains in political participation.[47]

There is a more tangential part of the literature that these results also relate to. The soft communication skills and self-efficacy identified as mechanisms here, are clearly important for social integration. And there is evidence from research on adult civic engagement that strong social integration, and the feeling of belonging that comes with it, enhances democratic participation (Putnam 1995; Putnam 2001). This idea is also supported by findings from educational research, showing that participation in drama clubs, music groups and religious organisations can be as predictive of political behaviour in young adulthood as participation in community service (MacFarland & Thomas 2006). Researchers have hypothesised that this is not just a selection effect. They argue that these non-civic activities are communal, and therefore require social interaction, which enhances political participation (MacFarland and Thomas 2006, p.418). This present study provides some evidence to support these broader theories of the connection between social integration and political participation. However, the analysis above has tried to reveal how complex the observed effects in this study are – relying as they do on a confluence of multiple activities, outcomes and moderating factors – so the findings should not be read simply across into other domains of activity, such as the completely non-civic associations described by Putnam and others.

The details discussed in this section are important because they improve our theoretical understanding of the relationship between non-political and political civic participation, but also because they provide crucial information for those wanting to design service learning experiences that do have a positive effect on political participation. For participants to have achieved the social and communication outcomes identified in this study, some important conditions had to be met. The cohort of young people had to be diverse in terms of its sociodemographic characteristics, as well as its composition of views about society and politics. The experience also had to be structured around teams of

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[47] There is evidence from the interviews that some participants, with low baseline knowledge, were made aware of some basic facts relating to social/environmental issues and/or how politics works. The evidence that this led to increased political participation, however, was weak. This result contradicts suggestions made by some researchers (Eyler & Giles 1999).

young people – supporting collective rather than individual action – and it needed to offer multiple social interaction and communication mastery experiences in a safe and supportive environment.

Importantly, these conditions are unlikely to be common to all programmes of service learning. Nor are most of the activities that participants' described as supporting their social and communication skills and self-efficacy; activities that were either part of the week-long outward bounds phase of the programme, or the second week of residential living and skills-building workshops. Both of these phases of NCS are predominantly *non-civic*, focussing instead on a combination of structured and unstructured personal development activities, centred around social mixing with people from diverse backgrounds. These are features of NCS, and other informal programmes of service learning, that are unlikely to be replicated in programmes linked to formal curricula (those in schools or universities). Though detail is often lacking in the descriptions of their activities, programmes of service learning based in formal education usually have a combination of a knowledge-based curriculum (taught in the classroom) and a period of voluntary service. As far as it is possible to see in the literature, they lack the group-based personal development activities that seem to be so key in driving the effects of NCS on political participation. Being based in formal educational settings like schools and universities also means that they are likely to lack the socio-economic diversity identified as important in this study, and that is deliberately engineered in NCS. What the informal approach – exemplified here by NCS – lacks is a high-quality knowledge and critical thinking component that helps participants to contextualise their service experience. Adding these activities to NCS would increase the chances that the strong social and communication effects identified by this study are transferred into increased political participation for more participants.

5.9.2 Service self-efficacy and political self-efficacy

The findings identify service self-efficacy as a proximal outcome for some, with some limited evidence of positive knock-on effects on political self-efficacy and political participation, but also clearer evidence of no effects for some and even negative effects for others. If service learning is going to increase a participant's likelihood of political participation via service self-efficacy, a three-step process has to take place. In step 1, participation in service learning needs to increase

service self-efficacy. In step 2, this has to lead to an increase in political self-efficacy. And in step 3, this leads to an increase in political participation. The literature suggests that step 1 requires perceived success from service, and that step 2 relies on perceived similarity between the domains of service and politics. However, the limited evidence available prior to this study made it hard to know how likely it was that such a process would take place.

The results from Study 1 suggest there is a substantial average effect on participants' general civic self-efficacy. This is a similar concept to their service self-efficacy, and the results from this present study seem to confirm that a positive effect is felt in this specific domain as well. So, step 1 in the chain seems to be satisfied.[48] However, the analysis from the interviews suggests that step 2 in the chain – a transferal of self-efficacy to the domain of politics – is very hard to achieve. This experience of service learning did not leave participants with a sense that the domains of service and political action were similar. Indeed, some participants explicitly stated the opposite view. This latter finding is also supported by the Study 1 analysis, which estimates a very small effect of NCS on political self-efficacy. This suggests that the guided reflection that is identified in the literature as a key supporting activity (Billig 2000; Melchior et al 1999; Walker 2000) was either absent or not focussed enough to support the necessary cognitive appraisal in this case. Three other factors that moderate this self-efficacy spillover process are also identified in the results: i. political interest at baseline; ii. political self-efficacy at baseline; and iii. legal and safety barriers. Participants who saw no effect in step 2 reported levels of political interest and political self-efficacy prior to NCS that were either too low or too high. This idea is also supported by Holbein and Hillygus' new research on the effects of noncognitive skills programmes. Not only do they identify political interest as a key moderating factor, but they also find, in a large observational dataset, that those with medium levels of political interest see the largest effect from

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[48] A detail in the theory at stage 1 has been amended, however. Where the general literature on self-efficacy suggests that, along with perceived success, the *perceived difficulty* of the service activities undertaken is key (Bandura, 1997), the interviews suggested that it was the *perceived importance* of these activities that made more of a difference. This idea is supported by a service learning intervention study that suggests that service experiences that bring participants into contact with people in more serious need (for example people who are homeless) have a greater effect on political participation (Reinders & Youniss 2006, p.6).

noncognitive programmes on their electoral turnout (2020, p.92). Individuals with very high or very low levels of interest see smaller effects.

Relatively strong evidence has been presented in the results to suggest that enhanced service self-efficacy can lead to lower political self-efficacy and less political participation. The case study of Tom provides a very clear story of this. His service learning experience contributed to a newfound preference for service-based participation over political forms of action. This provides support for the argument in the literature that service-based participation can come to be seen as particularly effective – *by its very nature* – because activities such as volunteering with the elderly or cleaning up a park have immediate and directly observable positive outcomes (Hornsey et al. 2006, p.1702; Walker 2000). It also supports the idea that, if service-based participation is to avoid this negative effect, it needs to be accompanied by a very specific type of knowledge development and reflection. Walker suggests that this process should encourage participants to reflect on the systemic causes of the issue being addressed through voluntary service, and the role of government in addressing the issue (Walker 2000, pp.646-647). As noted above, these components are either missing or lack the necessary focus in the case of NCS. Indeed, for Tom, the discussions during NCS of politics and the role of government led to an increased cynicism about the political system that reinforced his feelings about the superior efficacy of service.[49]

This is perhaps the type of effect envisaged by critics of NCS (Mycock & Tonge 2011, pp.63-64; Bacon et al. 2013; Bulley & Sokhi-Bulley 2014). Where Study 1 shows these criticisms to be ill-founded in terms of average effects, the present study suggests that they do have a basis in the experiences of some individuals. In general, however, this analysis reinforces the findings on mechanisms from Study 1, which finds NCS to have a substantial positive average effect on participants general civic self-efficacy but a negligible one on their political self-efficacy. The difficulties described in the qualitative analysis in transferring self-

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[49] For some participants who expressed a new-found preference for service, this idea of superior efficacy also seemed to be accompanied by a new sense of social responsibility – that emphasised the responsibility of individuals (rather than the government) in addressing social/environmental issues – and an increase in self-esteem that came from direct contact with (and gratitude from) the beneficiaries of their service.

efficacy between these domains (even though they may seem close at first glance), combined with the experiences of null and negative effects on political self-efficacy expressed by some interviewees, provide a strong explanation as to why this pattern exists in the quantitative data. These are again important new findings for theory and practice. For theory, they reveal the complexity and perils involved in trying to encourage self-efficacy 'spillover effects' between domains. For practice, they show how precisely focussed the knowledge and reflection components of an intervention need to be if such spillovers are to have a good chance of occurring.

5.9.3   Networks of recruitment

A small positive effect on participants' networks of recruitment into political activities is identified from the interviews. Where it occurred, this effect was driven by peer-to-peer communication, rather than through information provided by NCS. Importantly, this peer-to-peer communication, when it did it occur, again happened in unstructured social time, further reinforcing the importance of informal and non-civic activities in driving some of the observed effects. There were also reports from a wide range of participants of no new networks relevant to political participation being formed. Any positive effect in this area seems therefore to have been moderated by the chance composition of the cohort; i.e. whether there were any participants who were politically engaged and influential enough to effectively share relevant information.

These findings are supported by the wider literature. Of the three factors identified in the theory of Civic Voluntarism – capabilities, motivation and networks of recruitment – the supporting evidence is weakest for the third (1995, pp.443-444). However, Verba et al.'s survey data also show that recruitment efforts most often come through personal connections, and that personal requests are most effective in mobilising participation (1995, p.141). While this finding is not particularly new, it does suggest that current service learning practice may need to change to maximise the chances that this type of recruitment will take place.

5.9.4 Summary

This study makes two important general contributions to the literature on service learning and young peoples' political participation: i. it reveals substantial heterogeneity in the effects of service learning on political participation; and ii. it adds substantial detail to our understanding of *how* these effects are produced. Two substantive results within these general findings may be particularly surprising to some. First, an increase in social self-efficacy seems to be the most powerful mediating mechanism. Second, gains in service self-efficacy can lead to losses in political self-efficacy and, therefore, a reduction in political participation for some individuals. Preventing such losses – and encouraging positive spillovers – is difficult and requires new activities and conditions to be present in service learning. These contributions to the literature also have crucial significance to policy makers and practitioners who want to increase youth political participation through programmes of service.

5.10 Annex I: Robustness checks

The exploratory quantitative analysis in this study estimates five treatment effects. To account for this, the Benjamini-Hochberg procedure (Benjamini & Hochberg 1995) is applied to adjust the significance threshold for each test, as in line with the robustness check carried out for Study 1 (section 4.9.5)

Table 5.9 presents the results when this correction is applied to the significance thresholds for the three groups of tests in Study 1.

Table 5.9: Corrections for multiple comparisons (Study 2)

	p-value	Family	k	Sig. at $\alpha = 0.05$?	Correction to α	Sig. after correction
Civic self-efficacy	1.69e-30	Exploratory	1	Yes	0.010	Yes
Social self-efficacy	2.32e-30	Exploratory	2	Yes	0.020	Yes
Anxiety	6.84e-06	Exploratory	3	Yes	0.030	Yes
Political self-efficacy	1.40e-02	Exploratory	4	Yes	0.040	Yes
Social and communication skills	4.00e-01	Exploratory	5	No	0.050	No

This shows that the results in the exploratory analysis are not sensitive to this correction for multiple comparisons.

6 Study 3: Encouraging political participation post-service

6.1 Introduction

Studies 1 and 2 are descriptive, in the sense that they investigate the effects of an existing policy. Study 3 is more generative in the sense that it develops and tests new interventions, with the aim of producing the first direct evidence of how best to encourage more political participation post-service (RQ3). There is a large amount of evidence to suggest that 'Get Out the Vote' (GOTV) campaigns can increase voter turnout, and to tell us what form and content a message should take to be most effective in this context (Gerber & Green 2000; Gerber et al. 2008; Middleton & Green 2008; Nickerson 2006; Nickerson 2008). There is less evidence, however, on the effects of such campaigns on non-electoral political participation, and none at all when the target population is young people who have participated in service learning. This is an important gap in the literature. Both non-electoral participation and programmes of service learning are on the rise, and non-electoral participation makes an important contribution to a strong democracy (Han 2016, p. 296). Programmes such as NCS are uniquely placed to encourage democratic participation post-service, but there is no direct evidence to suggest how this could be done most effectively. This study addresses this final gap in the literature, using a large (N=227,372) three-arm randomised controlled trial to test the effects of three different email messages on young people's participation in political letter writing.

Opportunities for young people to participate in formal political activities are scarce, as is behavioural data on participation. Controlled experiments in this field are therefore challenging. To address these challenges in this study, the researcher worked with NCS Trust (the body responsible for delivery of NCS) to design and run a political letter writing competition for graduates of NCS. To give a valid measure of the outcome of interest, the competition was designed to offer young people an opportunity for genuine political participation; i.e. an 'activity that is intended to or has the consequence of affecting, either directly or indirectly, government action' (Verba et al. 1995, p.9). To support this aim, the competition was run in partnership with the UK government Department

for Culture, Media and Sport (DCMS). The competition asked participants to submit a policy idea for reducing loneliness amongst young people, with a particular focus on the role of digital technology. It offered winners the chance to meet with the government minister and civil servants responsible for this policy area. Creating an opportunity for participation in this way allowed the random assignment of invitation messages to a large sample of research participants, and the monitoring of two outcomes at the individual level: whether a young person expressed interest in participation (by clicking a link to find out more), and whether s/he submitted a letter to the minister.

The study tests the hypotheses that messages that prime a sense of civic self-efficacy, and those that draw on certain aspects of recipients' social identities, will be more effective than a plain invitation to participate. Three emails were developed to test these claims. The results of the experiment show that the theory-informed messages perform no better than a plain invitation in encouraging participants to submit a letter. There is also some evidence to suggest that the plain invitation is slightly more effective in sparking initial interest in participation. These findings are contrary to the hypotheses. There are two possible explanations for these results: i. the theories behind the interventions are wrong; or ii. the theories are valid, but the interventions do not effectively implement them. Both of these explanations seem to have a role to play. The discussion of this trial, combined with the findings from Study 1 and Study 2, suggests that at least four factors might explain the results. First, using a person's sense of general civic self-efficacy to encourage political participation is very hard to do (among this population at least), and does not seem to have worked here. Second, it does not look like graduates of NCS have incorporated a strong sense of being an active citizen into their identities. Third, email does not appear to be an effective tool for mobilising political participation among this group of young people. And fourth, the control condition – which also encouraged participation, by design – did have potentially favourable characteristics: its simplicity; and its earlier mentioning of the competition element of the activity. These plausible strengths in the control condition, when combined with the possible failures in theory identified in the theory-informed messages, may have made it as (or slightly more) effective.

6.2 Theory

Section 2.4 of the literature review concludes that there is no direct evidence to say how best to encourage political participation post-service, and the details of this part of the review are not repeated here. In summary, the GOTV literature is well-developed, but focuses only on voting, has not been tested on the population of interest for this thesis, and has produced findings that do not seem transferable as they focus on modes of contact that are not viable for youth civic engagement. However, combining insights from the review on mechanisms (section 2.2), and the wider literature on social mobilisation suggests two avenues that might be fruitful, when aiming to encourage political participation post-service. The first is to appeal to the sense of enhanced civic self-efficacy that people may feel after their service learning experience. The second is to use identity as a lever; focusing in particular on participants' sense of youth and of being active citizens.

The review of the self-efficacy literature suggests that a messaging intervention encouraging political participation post-service that draws on this idea, in this context, should encourage a recipient to do three things: i. consider the increased sense of self-efficacy that she may have developed in the non-political domain of civic participation; ii. link these gains in the non-political domain to the political one (encouraging a spillover effect); and iii. believe that her political actions could make a difference (that the system will respond).

The discussion of the literature at the intersection of identity, service learning and political participation suggests that members of the study population are likely to identify as 'young people' and – under the right conditions – as 'active citizens'. A messaging intervention that leverages these identities needs to raise their salience by naming them before the call to action. It should then encourage recipients of the message to: i. feel a sense of belonging to NCS as a social group; ii. consider NCS participants as a social group that is civically active; and iii. consider young people as a social group that is unjustly treated and marginalised by society and politics.

6.3 Interventions

Three messaging interventions were developed to answer the research question, based on these theories. These messages were sent to participants with an invitation to participate in a political letter writing competition, run by NCS Trust. To give a valid measure of the outcome of interest, the competition offered young people an opportunity for genuine political participation; i.e. participation 'that is intended to or has the consequence of affecting, either directly or indirectly, government action' (Verba et al. 1995, p.9). To support this aim, it was run in partnership with the UK government Department for Culture, Media and Sport (DCMS). The competition asked participants to submit a policy idea for reducing loneliness amongst young people, with a particular focus on the role of digital technology. It offered winners the chance to meet with the government minister responsible for this policy area (Diana Barran). The runners up were offered the chance to have their letters published online.

The interventions were delivered in email form. The evidence from GOTV experiments suggests that mass emails are generally ineffective in mobilising voters (Bennion & Nickerson 2011; Nickerson 2007). The evidence relating to non-electoral participation is more positive, however. Emails can be effective in mobilising campaign supporters to write to their MPs (Townsley et al forthcoming), and the effectiveness of such emails can vary according to their content (Han 2016, p.300).

Email is used as the method of communication in this study for three reasons. First, the study's aim is to test the content of messages, not their method of delivery. In doing so, it aims to improve our theoretical understanding of the relationship between service learning and political participation, and the type of message that is most effective in enhancing this relationship. Second, email was considered preferable to SMS (the only alternative in this case) for this purpose as it allows for a higher word count, and therefore a clearer implementation of different theories. Third, email is the main mode of communication used by NCS to offer post-programme opportunities to its graduates. There is therefore practical value in attempting to optimise email communications in this case.

All study participants were sent an invitation email, which gave some basic information about the competition and invited them to click a hyperlink to find out more. This hyperlink took participants to a web page explaining the terms of the competition and how to enter. This webpage was common across intervention groups. Reminder emails were also sent 11 days after the first message.[50] These emails contained the same content as the invitation emails, with the addition of the words 'Reminder' in the subject line and the phrase 'two weeks to go!' added to the header of each message. The only variation between trial arms was in the opening paragraph of each email. See Appendix XIII for the full emails. For full details of the competition, including the information page that recipients were directed to from the emails, and the terms and conditions, see Appendix XIV. For the letter submission pages, see Appendix XV.

6.3.1 Intervention 1 (self-efficacy)

The first email (I1) aimed to increase recipients' political self-efficacy by encouraging them to: i. consider the increased sense of self-efficacy that they may have developed in the non-political domain of civic participation, as a result of NCS; ii. link these gains in the non-political domain to the political domain, (encouraging a 'spillover' of self-efficacy between these two domains); and iii. believe that their actions could make a difference (that the system will respond). The part of the email that was designed to leverage self-efficacy read as follows.

> *'Through NCS, you proved that you can make a difference in your community. You have the skills and you've seen that change can happen. Now make a difference by having your say with the government.'*

Table 6.1 breaks this message down by theoretical component.

Table 6.1: Theoretical components of Intervention 1

Text	Theory
Through NCS, you proved that you can make a difference in your community.	Priming external efficacy in the domain of non-political civic participation

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[50] The pre-analysis plan specified that reminder emails would be sent 2 weeks after the first message (Taylor 2020d, p.6). This was changed to 11 days during the trial to avoid a clash with another, unforeseen and unrelated communication that needed to be sent by NCS Trust.

You have the skills...	Priming internal efficacy in the domain of non-political civic participation
...you've seen that change can happen.	Priming external efficacy in the domain of non-political civic participation
Now make a difference by having your say with the government.	Encouraging spillover to the political domain

6.3.2 Intervention 2 (identity)

The second email (I2) aimed to draw on recipients' sense of social identity by encouraging them to: i. feel a sense of belonging to NCS as a social group; ii. consider NCS citizens as a social group that participates in civic activities; and iii. consider young people as a social group that is unjustly treated/marginalised by society/politics. The part of the email that was designed to leverage identity read as follows.

'As an NCS grad you've already proven that you are a changemaker. Too often young people are ignored in politics and society. We want to change that, and we need people like you to help.'

Table 6.2 breaks this message down by theoretical component.

Table 6.2: Theoretical components of Intervention 2

Text	Theory
As an NCS grad...	Encouraging a sense of belonging to NCS as a social group
...you've already proven that you are a changemaker.	Labelling NCS citizens as a social group that participates in civic activities
Too often young people are ignored in politics and society. We want to change that and we need people like you to help.	Highlighting the marginalisation of youth

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6.3.3 Intervention 3 (control)

The third email (I3) acted as the control group. In the place of the theory-informed motivational messages in interventions 1 and 2, some plain informational text of a similar length was included, as follows.

> *'We're writing to let you know about an exciting competition that we're running for graduates of NCS. Take a look at the details below and we hope you'll consider taking part.'*

6.4 Research question and hypotheses

The research question for this study is: 'What is the most effective way to encourage political participation post-service?' (RQ3). The literature suggests that, when it comes to civic participation, self-efficacy and identity are particularly important factors for young people who have taken part in service learning. This leads to the following two hypotheses.

- **H5.1:** I1 (self-efficacy) will be more effective than I3 (no motivational message) in encouraging participation.
- **H5.2:** I2 (identity) will be more effective than I3 (no motivational message) in encouraging participation.

While there is evidence to suggest that I1 and I2 will be more effective than no motivational message at all (I3), the literature and the other studies in this thesis reveal complexities in the relationship between service learning, self-efficacy and political participation. It is possible that a substantial proportion of the study population will not experience the spillover effect that is described above. There may even be some whose experience of non-political civic participation has led them to see political participation as particularly ineffective. For this latter group, a message that encourages recipients to consider issues of efficacy before choosing whether to participate may even have a backfire effect. Furthermore, there is a lack of experimental evidence showing that self-efficacy-based messaging interventions can have positive effects in similar domains. The identity-based intervention, on the other hand, does not seem to suffer from these theoretical complexities and there is strong experimental evidence to show that similar interventions in similar domains have had positive effects. This leads to Hypothesis 5.3.

- **H5.3:** I2 will be more effective than I1 in encouraging participation.

The interventions were not expected to influence participation equally for all people. In particular, people from low socio-economic status (SES) backgrounds were expected to be more affected by the interventions than those from higher SES backgrounds. This is because the latter group is likely to have had fewer experiences of political socialisation, have less resources that support participation and have received fewer invitations to participate politically in general, as compared to their peers from higher SES backgrounds (Condon & Holleque 2013, pp.170-171). (See also Craig 1979, p.231). When such a deficit of other political advantages is experienced, attitudinal traits such as self-efficacy play a larger role in determining a young person's political participation (Condon & Holleque 2013). This leads to Hypothesis 5.4.

- **H5.4:** I1 and I2 will be more effective for participants from low-SES backgrounds.

6.5 Research design

The best method for making unbiased estimates of causal effects is a randomised controlled trial (RCT) (Halperin & Heath 2017, p.150). The use of intervention and control groups allows the comparison of effects on people who have had an experience versus those who have not, and random assignment of participants to different conditions ensures that the estimated effects are not subject to omitted variable bias (Burtless 1995, p.68). The purpose of this study is to examine the relative effects of three interventions. A three-arm RCT is therefore employed. The full research design and analytic approach was specified in a pre-analysis plan (PAP) and registered prior to collecting outcome data (Taylor 2020d).

6.5.1 Outcome measures

The outcome of interest is political participation, defined as 'activity that is intended to or has the consequence of affecting, either directly or indirectly, government action' (Verba et al. 1995, p.9). Examples of political participation under this definition include voting, signing a petition, contacting a public

official, protesting, joining a political party, or taking part in a public consultation. In this case, study participants were invited to attempt to influence a government minister by sending a letter. The primary outcome is defined as whether or not a letter was submitted. This activity is a demanding one, and the means of encouragement (two emails) were limited. As such, a secondary outcome – whether or not email recipients clicked a button to find out more about the participation opportunity – is also measured. This is a less effortful behaviour, but still an indicator of interest in political participation.

Table 6.3: Outcome measures

Outcome measures	Data collected	Point of collection
Did the participant submit a letter? (primary outcome)	Binary 'yes/no' for each participant	1 week after competition closes
Did the participant click through from the email to 'find out more' (secondary outcome)	Binary 'yes/no' for each participant	1 week after competition closes

6.5.2 Sampling

The aim of the sampling strategy is to maximise statistical power by creating a sample from the population of interest that is as large as possible. This was especially important due to the uncertainty in the expected effect sizes (see section 6.5.6 on power calculations below). The population of interest for this study is graduates of the NCS programme. NCS has been chosen because it is the largest programme of service learning in England and, as the government-funded programme of national voluntary service, has particular policy importance. Approximately 600,000 young people have completed NCS to date.[51] In 2019, the programme had a reached a scale such that 7% of 16-17 year olds in England did NCS in that year.[52] NCS graduates therefore represent a substantial proportion of the English population in their age group, and the aim is to continue to grow this number (NCS Trust 2019).

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[51] Figure calculated by adding published participation figures up to the beginning of 2019 (NCS Trust 2019, p.6) to NCS Trust's internal participation number for summer 2019 (98,331, unpublished at the time of writing).
[52] Figure calculated using NCS's internal participation number for 2019 and the Office for National Statistics (ONS) population estimate for 16- and 17-year olds in the same year (ONS 2020).

The sampling frame for the study was all NCS graduates for whom the NCS Trust held a record on their participant database. Participants were considered eligible for the trial if they met all three of the following criteria: i. they graduated from NCS in 2017, 2018 or 2019 (the cohorts for whom NCS holds a high proportion of complete records); ii. they had an operational email address (as tested by NCS Trust in their last mailout prior to randomisation); and iii. NCS Trust had identified a lawful basis to contact them by email to invite participation in the competition (for example, they had not opted out of communications from the Trust).

This sample included 227,372 individuals. Table 6.4 compares the characteristics of the sample with the sampling population and the wider population of England. The sampling strategy did not aim for statistical representation, but these comparisons help to give a sense of the external validity of the results. They show that the sample very closely matches the sampling population in terms of the proportion eligible for free school meals (FSM, an indicator of SES), the gender balance, and the mix of ethnicities. Compared to the wider population of young people of a similar age in England, members of the sample are more likely to come from disadvantaged socio-economic backgrounds, more likely to be female and more likely to be a member of a minority ethnic group.[53] These are all potentially important differences when considering how the effects estimated in this study might be realised in the wider population of England. If H5.4 holds (that the interventions will be more effective for young people from lower SES backgrounds), the interventions may be more effective for trial participants than they would be for the wider English population. No hypotheses were made about heterogeneous effects by gender or ethnicity, but it is possible that the identity-based intervention – that is designed to resonate with recipients' sense of marginalisation from the political process – will be more effective for female participants and those from ethnic minority backgrounds. Gender (Norris et al. 2004; Sloam et al. 2018) and ethnicity (HCPCRC 2014) are

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[53] The most recent census in England was conducted in 2011. At that point the study cohort were aged 8-11 years old. The nearest category in the census data, used to calculate the ethnicity figures for England in this table, is 10-14 years. While this is not a perfect match, it is sufficient here, where the aim is to provide an approximate indication of the differences between the sample and the wider English population. The gender figures for England are based on ONS population estimates for 16- and 17-year olds in the relevant years (2016, 2017 and 2018). A mean average has been calculated across the three years.

known predictors of campaign-based political activities such as contacting a politician, and these two groups face discrimination that may lead to increased feelings of marginalisation.

Generalising the findings of this study beyond young people is not possible. The evidence suggests that attitudinal traits such as self-efficacy are substantially stronger predictors of participation amongst younger people (Denny & Doyle 2009, p.30) and the identity-based intervention directly addresses 'young people' as a group.

Table 6.4: Sample characteristics compared to the sampling population and the wider population of England

	% of sample	% of sampling population	% of England
Eligible for FSM?			
Yes	18*	17*	13**
No / unknown	82*	83*	87**
Gender			
Female	56	56	49
Male	44	44	51
Ethnicity			
Asian	16	15	9
Black	8	8	5
Mixed	5	5	4
White	67	68	80
Other	2	2	1

Notes:
* In the one-year period prior to participation in NCS (2017, 2018 or 2019).
** Mean average of state secondary school pupils across 2017, 2018 and 2019.
Source for free school meal data for England: Department for Education, 2019.
Source for ethnicity estimates for England: Office for National Statistics, 2015.
Source for gender estimates for England: Office for National Statistics, 2019.

6.5.3 Randomisation

Randomisation was carried out at the individual level, stratified by FSM eligibility – an indicator of SES, which is a known predictor of political participation (Achen 2002; Beck and Jennings 1982; Condon & Holleque 2013; HCPCRC 2014; Hooghe and Stolle 2004; Norris et al. 2004; Sloam et al. 2018; Uhlaner et al. 1989; Verba et al. 1995). FSM eligibility is also used to conduct the subgroup analysis required to test H5.4. Stratifying the randomisation in this way

maximises the statistical power of this hypothesis test. Randomisation was blocked, so that participants were assigned to one of the three conditions with equal probability. Participants were blind to assignment.

6.5.4   Attrition

The flow diagram below summarises the trial process and attrition. It shows that, of the 227,372 individuals that were randomised, none were lost before analysis. This was as expected. If a participant clicked to find out more and/or entered the competition, their outcomes were automatically recorded. If they did not click or submit a letter, they were assigned zeros for those outcomes. Attrition could only have occurred if data were lost due to an administrative error or if participants withdrew from the study. Neither of these things happened.

There were however, a relatively large number of individuals who did not receive at least one email (c.9,000 per arm) due to various types of email 'bounce'. Section 6.9.1 on non-compliance, below gives a detailed breakdown of email receipts and open rates, and checks whether the effects estimated in the main analysis are sensitive to these factors.

The NCS participant database contained approximately 299,000 individuals at the point of randomisation. 71,268 of these were excluded from the study prior to randomisation because they did not meet the inclusion criteria.
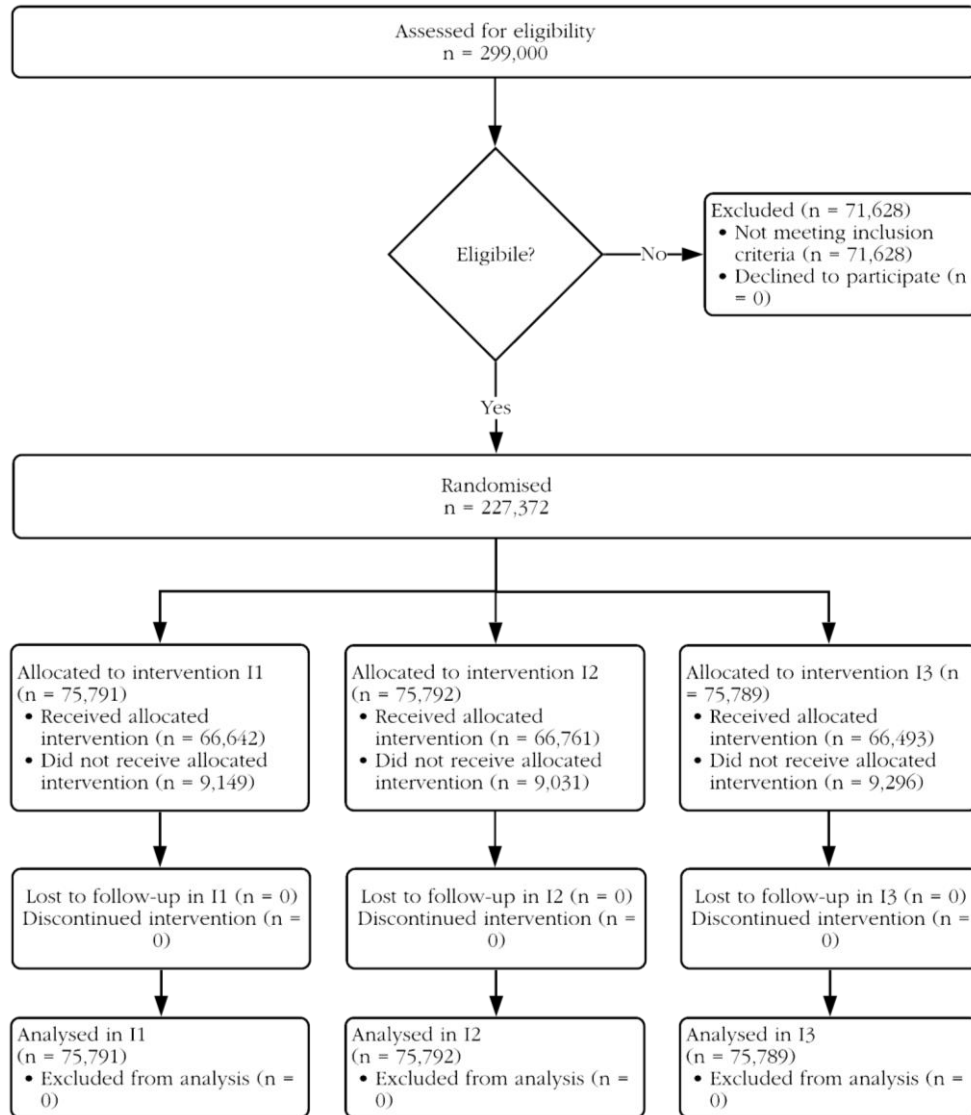
Notes: In the diagram, 'received allocated intervention' is a count of participants who received at least one of the invitation or reminder emails. See section 6.9.1 on non-compliance for a detailed breakdown and analysis of email receipts. 'Analysed' refers to the main analysis, which was conducted on an intention-to-treat basis.

6.5.5   Balance checks

Balance checks are carried out on FSM eligibility and three other variables that have been found to predict political participation (gender, age and ethnicity) (Achen 2002; Beck and Jennings 1982; HCPCRC 2014; Hooghe and Stolle 2004; Norris et al. 2004; Sloam et al. 2018; Uhlaner et al. 1989; Verba et al. 1995). To assess balance, the magnitude of the differences between each combination of groups is calculated for each covariate (Table 6.7). A common alternative is to report whether differences between groups are statistically significant at a certain level of confidence (often $p < 0.05$ in the social sciences). This approach is not particularly helpful because it only tells us whether the sample is large enough to

detect a difference, and leaves open the question as to whether any observed differences – and the associated bias – can be addressed through simple covariate adjustment (the approach taken in the analysis for this study) (Imbens & Rubin 2015, p.311). Rather than reporting simple differences for each covariate, normalised differences are presented to aid comparison between covariates that have different units, and to facilitate comparisons across studies. The normalised difference is defined as the difference in means between two groups, divided by the pooled standard deviation. The estimate of this quantity is given by the following equation (Imbens & Rubin 2015, p.311).

$$\hat{\Delta}_{ct} = \frac{\bar{X}_t - \bar{X}_c}{\sqrt{(s_c^2 + s_t^2)/2}} \tag{6}$$

where:

- $\bar{X}_t$ is the mean value of the variable in the treatment group;

- $\bar{X}_c$ is the mean value of the variable in the control group;

- s_t is the standard deviation of the variable in the treatment group; and

- s_c is the standard deviation of the variable in the control group.

The distribution of covariate categories across the three intervention groups is reported in Table 6.5.

Table 6.5: Distribution of covariates by treatment group

	Group 1 (N₁ = 75,791)	Group 2 (N₂ = 75,792)	Control (N_c = 75,789)
Eligible for FSM?			
Yes	13,670 (18%)	13,670 (18%)	13,669 (18%)
No	58,592 (77%)	58,593 (77%)	58,592 (77%)
Unknown	3,529 (4.7%)	3,529 (4.7%)	3,528 (4.7%)
Gender			
Female	42,292 (56%)	42,224 (56%)	42,300 (56%)
Male	33,216 (44%)	33,293 (44%)	33,232 (44%)
N/A	197 (0.26%)	164 (0.22%)	165 (0.22%)
Other	86 (0.11%)	111 (0.15%)	92 (0.12%)
Ethnicity			
Asian	12,033 (16%)	11,982 (16%)	12,299 (16%)

	Black	6,226 (8.2%)	6,332 (8.4%)	6,319 (8.3%)
	Mixed	3,912 (5.2%)	3,911 (5.2%)	3,921 (5.2%)
	N/A	1,130 (1.5%)	1,140 (1.5%)	1,181 (1.6%)
	Other	1,456 (1.9%)	1,502 (2.0%)	1,427 (1.9%)
	White	51,034 (67%)	50,925 (67%)	50,642 (67%)
Year				
	2017	24,891 (33%)	24,887 (33%)	24,688 (33%)
	2018	25,503 (34%)	25,505 (34%)	25,715 (34%)
	2019	25,397 (34%)	25,400 (34%)	25,386 (34%)

Notes: N = 227,372**.** The 'Year' variable indicates the year in which the individual took part in NCS, used as an indicator of age (individuals are 16-17 years old at the time of participation). Figures in brackets are the percentage of the relevant intervention group that has that characteristic.

The mean and standard deviation of each covariate (transformed into binary indicators) is given in Table 6.6.

Table 6.6: Mean and standard deviation of covariates by treatment group

	Group 1 (N₁ = 75,791)		Group 2 (N₂ = 75,792)		Control (N_c = 75,789)	
	Mean	(S.D.)	Mean	(S.D.)	Mean	(S.D.)
FSM	0.19	(0.39)	0.19	(0.39)	0.19	(0.39)
White	0.67	(0.47)	0.67	(0.47)	0.67	(0.47)
Female	0.56	(0.50)	0.56	(0.50)	0.56	(0.50)
Year_2017	0.33	(0.47)	0.33	(0.47)	0.33	(0.47)
Year_2018	0.34	(0.47)	0.34	(0.47)	0.34	(0.47)
Year_2019	0.34	(0.47)	0.34	(0.47)	0.33	(0.47)

Notes: All variables are binary indicators, so mean averages represent proportions of the group. The 'Unknown' category in FSM is coded as missing in the dataset, so the reported means and S.D.s are of the non-missing sample.

The normalised differences in means of each covariate (transformed into binary indicators) is give in Table 6.7, below. Normalised differences with a magnitude of 0.1 or less indicate a negligible correlation between the covariate and assignment to treatment group (Austin 2009, p.1233). In this case, all normalised differences are 10 times smaller than this threshold (-0.01 < $\hat{\Delta}_{ct}$ < 0.01). The sample is therefore well-balanced across treatment groups on all covariates.

Table 6.7: Normalised differences between treatment groups

	Normalised differences, $\hat{\Delta}_{ct}$		
	Group 1 v Group 2	Group 1 v Group 3	Group 2 v Group 3
FSM	0.00	0.00	0.00
White	0.00	0.01	0.01
Female	0.00	0.00	0.00
Year_2017	0.00	0.01	0.01
Year_2018	0.00	-0.01	-0.01
Year_2019	0.00	0.00	0.00

6.5.6   Power calculations

Ex ante power calculations were conducted for the primary outcome. Minimum detectable effect sizes (MDESs) are given below based upon the following assumptions: i. the total sample size is 227,372 (approximately 75,790 per arm); ii. no attrition was expected (see section 6.5.4 above); iii. the unit of randomisation is an individual; iv. the outcome variable is binary; v. the baseline rate of the primary outcome was estimated to lie between 0.1 and 2%; vi. the proportion of variance explained by pre-treatment covariates was unknown and no relevant literature existed to support a good estimate so, to be conservative, the contribution of pre-treatment covariates was estimated to be zero; vii. power (the probability of rejecting the null hypothesis when it is false) was set at 0.8; alpha (the probability of rejecting the null hypothesis when it is true) was set at 0.05. Table 6.8 presents a range of scenarios, based on these assumptions.

Table 6.8: MDESs by baseline rate of primary outcome

Baseline rate of primary outcome (%)	Estimated MDES (pp)	Estimated MDES (Cohen's h)
0.1	0.05	0.01
0.2	0.07	0.01
0.5	0.11	0.01
1.0	0.15	0.01
2.0	0.21	0.01

Estimating the true effect size is difficult as no experimental evidence has been identified for similar interventions, making similar types of comparisons, with a similar sample. Two RCTs that tested the relative effects of different email

messages on petition-signing found effects of between 5 and 9 percentage points (Han 2016, pp. 301-302). However, the population for these studies (medical professionals in the United States) was quite different to that in the present study, as was the target behaviour (petition signing rather than letter writing). A large part of the experimental political participation literature focuses on the effects of different communication methods (telephone calls, door-knocking, emails etc.) on voter turnout. Effect sizes here range from 1 to 10 percentage points (Bond et al. 2012, p.295), but this is when the control group receives no contact at all, which is not the case in this study, where a comparison is being made between different types of message and effect sizes are therefore expected to be smaller. A large online experiment testing the relative effects of different Facebook messages found a 0.39 percentage point difference in voter turnout between a group that received a theory-informed message based on social influence and a group that received a simple informational message (Bond et al. 2012, p.296). The mode of delivery in this experiment was not the same as the current study, nor was the population, the theory or the outcome variable. However, this is the smallest effect size identified in the literature and the MDESs estimated above are substantially smaller than 0.39 percentage points. This study appears therefore to be well-powered.

6.5.7 Data collection

All data was collected by NCS Trust. At the beginning of the study, a dataset containing the FSM eligibility, gender, ethnicity and year of NCS participation was shared with the researcher. This was used to randomly assign participants to one of the three experimental conditions, and then returned to NCS Trust who sent the emails and collected the outcome data. At the end of the study, NCS Trust shared a second dataset with the researcher containing the outcome data and administrative data for the compliance analysis. Table 6.9 summarises the data collected.

Table 6.9: Summary of data collected

	Purpose	Collection Point
Covariates		
FSM eligibility	Stratification variable	Baseline
	Balance checks	
	Control variable	

	Sub-group analysis	
Gender	Stratification variable Balance checks Control variable	Baseline
Ethnicity	Stratification variable Balance checks Control variable	Baseline
Year of graduation from NCS	Stratification variable Balance checks Control variable	Baseline
Outcome data		
Letter submitted?	Estimating effect of interventions	1 week after competition closed
Link to 'find out more' clicked?	Estimating effect of interventions	1 week after competition closed
Administrative data		
Study ID	Uniqueness check Randomisation	Baseline and 1 week after competition closed
Which email was sent to the individual?	Compliance check	1 week after competition closed
Email received?	Compliance check	1 week after competition closed
Email opened?	Compliance check	1 week after competition closed

6.6 Analytic approach[54]

Table 6.10 presents a summary of the study hypotheses.

Table 6.10: Summary of hypotheses

Research question
What is the most effective way to encourage political participation post-service?
Hypotheses
H5.1: I1 (self-efficacy) will be more effective than I3 (no motivational message) in encouraging participation.
H5.2: I2 (identity) will be more effective than I3 (no motivational message) in encouraging participation.
H5.3: I2 will be more effective than I1 in encouraging participation.
H5.4: I1 and I2 will be more effective for participants from low-SES backgrounds.

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[54] See Appendix XVI for the full analysis code and log file.

The analytic approach is described below. H5.1, H5.2 and H5.3 are tested by estimating the effects of the interventions on the primary and secondary outcomes (see 'Primary outcome analysis' and 'Secondary outcome analysis' below). H5.4 is tested by comparing the estimated effects for participants who are eligible for free school meals (FSM) with those who are not (see 'Heterogeneous intervention effects' below). Eligibility for FSM is based on a child's household income and is a widely-used indicator of socio-economic status in England. It is not perfect – missing out some children who may rightly be considered to be living in relative poverty – but it correlates strongly with other indicators of deprivation (Illie et al. 2017). This analysis is carried out for both the primary and secondary outcomes.

6.6.1 Primary outcome analysis

The following model is used to estimate the effects of I1 and I2 on the primary outcome. Analysis is conducted on an intention-to-treat (ITT) basis, including all complete cases in the sample using the following model.

$$Y_i \sim bernoulli(p_i) = \beta_0 + \beta_1 T_i + \beta_2 X_i \tag{7}$$

where:

- Y_i is a binary indicator of response (equal to 1 if the participant entered the competition and 0 if not);
- p_i is the probability that a given participant enters the competition;
- T_i is a dummy variable representing intervention assignment; and
- X_i is a vector of pre-treatment covariates that predict political participation: FSM, gender, ethnicity, and year of participation in NCS (a proxy for age) (Achen 2002; Beck and Jennings 1982; HCPCRC 2014; Hooghe and Stolle 2004; Norris et al. 2004; Sloam et al. 2018; Uhlaner et al. 1989; Verba et al. 1995).

6.6.2 Secondary outcome analysis

The same model is used to estimate the effects of I1 and I2 on the secondary outcome, where Y_i is a binary indicator of response (equal to 1 if the participant clicked the link to find out more and 0 if not).

6.6.3 Heterogeneous intervention effects

Heterogeneous intervention effects are estimated for both the primary and secondary outcomes by testing for interaction effects (Wang & Ware 2013) using the following model.

$$Y_i \sim bernoulli(p_i) = \beta_0 + \beta_1 T_i + \beta_2 X_i + \beta_3 Z_i + \beta_4 T_i \cdot Z_i \qquad (8)$$

where:

- Y_i is binary indicator of response (primary or secondary outcome);
- p_i is the probability that a given participant performs the target behaviour (primary and secondary outcome);
- T_i is a dummy variable representing intervention assignment; and
- X_i is the vector of pre-treatment covariates used in the primary analysis (excluding FSM); and
- Z_i is a binary indicator of FSM eligibility (equal to 1 if the participant reported being eligible for FSM and 0 if not).

In this model, β_1 denotes the effect of the intervention for participants who have not been eligible for FSM (Z=0) and $\beta_1 + \beta_4$ denotes the effect of the intervention on those who have been eligible for FSM (Z=1). The difference in intervention effects between the two groups is therefore denoted by β_4 (Wang & Ware 2013, p.3).

6.7 Results

6.7.1 Introduction

The raw results for the number of entries (the primary outcome) and the number of people who clicked to find out more (the secondary outcome) are presented in Tables 6.11 and 6.12. The baseline response rate for the primary outcome was approximately 0.1%, so *ex ante* power calculations suggest that the study was powered for a MDES of 0.05 percentage points. The analysis that follows is grouped by hypothesis, rather than primary and secondary outcomes, as different hypotheses require comparisons between different combinations of the three trial arms. This analysis shows that none of the differences between groups are statistically significant at the 95% confidence level. The results for the

secondary outcome are marginally significant (p < 0.1). They suggest that, if there are any true differences between the interventions, they are very small and the control email is the most effective. The findings are robust to a wide range of alternative specifications, and the results of these robustness checks are reported in Annex I at the end of this chapter.

Table 6.11: Raw results for primary outcome

	# entries	Group N	% of group
I1 (Self-efficacy)	55	75,736	0.073%
I2 (Identity)	59	75,733	0.078%
I3 (Control)	66	75,723	0.087%
Notes: N = 227,372			

Table 6.12: Raw results for secondary outcome

	# clicks	group N	% of group
I1 (Self-efficacy)	701	75,736	0.93%
I2 (Identity)	704	75,733	0.93%
I3 (Control)	762	75,723	1.01%
Notes: N = 227,372			

6.7.2 Comparing interventions to the control (H5.1 and H5.2)

H5.1 and H5.2 predicted that the emails based on the theories of self-efficacy and identity (I1 and I2 respectively) would both encourage more political participation than the control email (I3). Table 6.13 presents the findings of the tests for these hypotheses.

Table 6.13: Average intervention effects for H5.1 and H5.2, logistic regression models

	Primary outcome		Secondary outcome	
	(1)	(2)	(3)	(4)
	Basic model	Inc. covariates	Basic model	Inc. covariates
I1 (Self-efficacy)	-0.182	-0.186	-0.084	-0.088[+]
	(0.183)	(0.185)	(0.053)	(0.053)
I2 (Identity)	-0.112	-0.131	-0.080	-0.091[+]
	(0.179)	(0.183)	(0.053)	(0.053)
Constant	-7.045***	-7.427***	-4.590***	-4.798***
	(0.123)	(0.282)	(0.036)	(0.078)
N observations	227,372	216,507	227,372	216,786
Pseudo r-squared	0.000	0.033	0.000	0.037

Notes: The reference group for comparisons – the control group (I3) – is omitted from the table. Point estimates are presented as log-odds. Standard errors are in brackets. Covariates (FSM, gender, ethnicity and year) are omitted from the table but included in the models as labelled. The 'Basic model' in each case includes only the outcome variable and assignment.
[+] p<0.1 * p<0.05, ** p<0.01, *** p<0.001

Point estimates in the table are presented as log-odds. These figures represent the proportional change in response rate between the intervention group and the control group. So, the effect of the self-efficacy email (I1) on the competition entry rate is estimated to be -19% (p = 0.316; 95% CI [-55%, 18%]). The effect of the identity email on the competition entry rate is estimated to be -13% (p = 0.474; 95% CI [-49%, 23%]). These effects correspond to differences of -0.015pp and -0.011pp respectively.

The effect of the self-efficacy email (I1) on the proportion of people who clicked to find out more is estimated to be -8.8% (p = 0.097; 95% CI [-19%, 1.6%]). The effect of the identity email on the proportion of people who clicked to find out more is to be -9.1% (p = 0.087; 95% CI [-20%, 1.3%]). These effects correspond to differences of -0.086pp and -0.088pp respectively.

These results suggest that both interventions had no effect on the primary outcome. Recipients of I1 and I2 were no more likely to enter the competition than recipients of I3. The estimated effects on the secondary outcome are marginally significant (p < 0.1), suggesting that the effects are either zero or very slightly negative. If there *was* a real effect, recipients of I1 and I2 were slightly less likely to click to find out more than recipients of I3. H5.1 and H5.2 are therefore shown to be false.

Figure 6.2: Average intervention effects, competition entry rate

n = 216,507
** p < 0.01, * p < 0.05, + p < 0.1
Primary analysis, controlling for fsm, gender, ethnicity and age
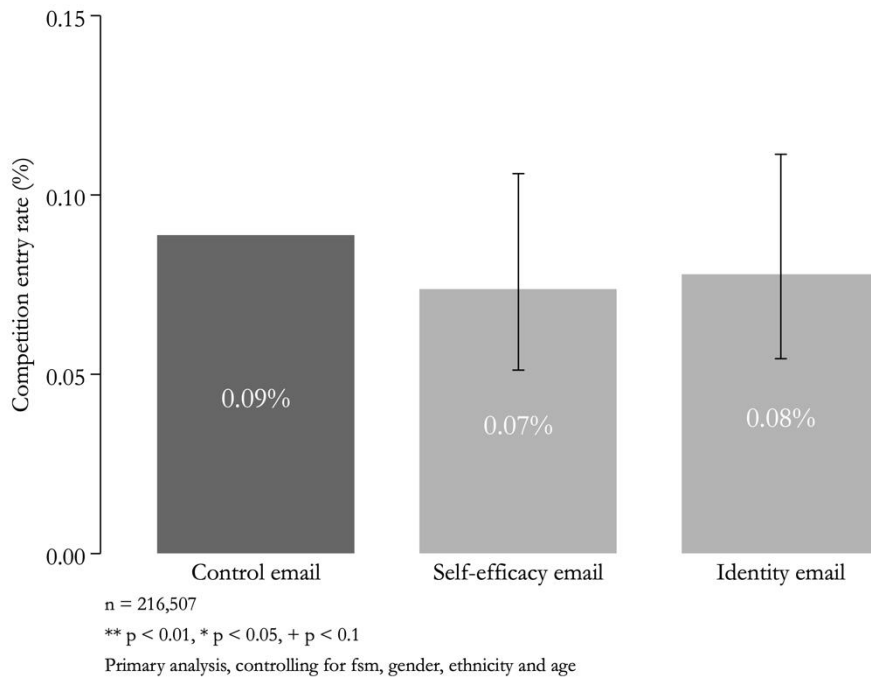
Figure 6.3: Average intervention effects, rate of clicks to find out more

n = 216,786
** p < 0.01, * p < 0.05, + p < 0.1
Secondary analysis, controlling for fsm, gender, ethnicity and age
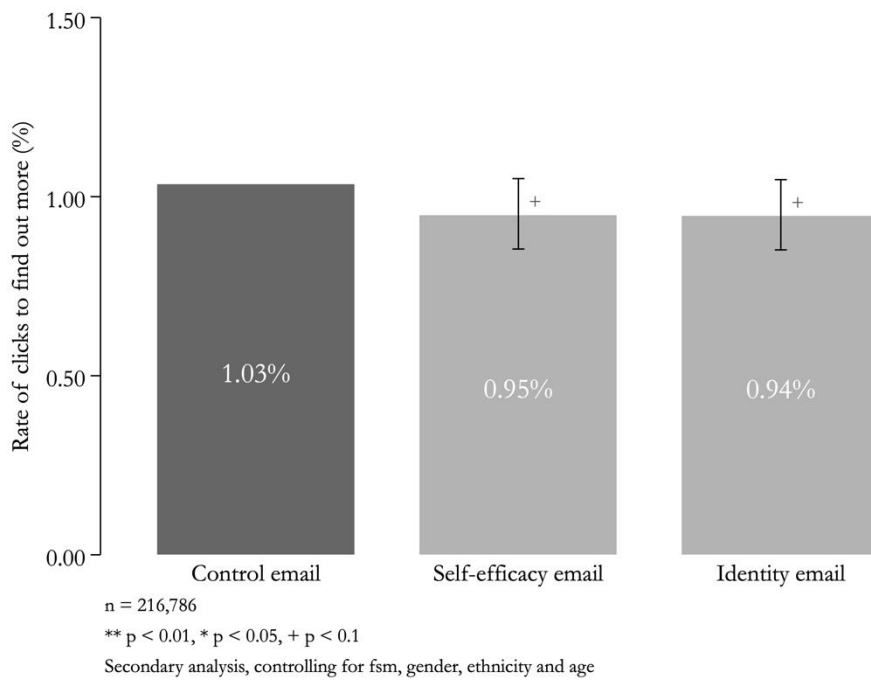
6.7.3 Comparing interventions to each other (H5.3)

H5.3 predicted that the email based on the theory of identity (I2) would encourage more political participation than the one based on self-efficacy (I1). Table 6.14 presents the findings of the tests for this hypothesis.

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Table 6.14: Average intervention effects for H5.3, logistic regression models

| | Primary outcome | | Secondary outcome | |
| | (1) | (2) | (3) | (4) |
	Basic model	Inc. covariates	Basic model	Inc. covariates
I2 (Identity)	0.070	0.055	0.004	-0.003
	(0.188)	(0.186)	(0.054)	(0.055)
I3 (Control)	0.182	0.186	0.084	0.088
	(0.183)	(0.186)	(0.053)	(0.053)
Constant	-7.228***	-7.614***	-4.674***	-4.887***
	(0.135)	(0.289)	(0.038)	(0.079)
N observations	227,372	216,507	227,372	216,786
Pseudo r-squared	0.000	0.033	0.000	0.037

Notes: The reference group for comparisons – the self-efficacy email (I1) – is omitted from the table. Point estimates are presented as log-odds. Standard errors are in brackets. Covariates (FSM, gender, ethnicity and year) are omitted from the table but included in the models as labelled. The 'Basic model' in each case includes only the outcome variable and assignment. The control group (I3) is included in the table for completeness but is not referenced in hypothesis H5.3.
$^{+}$ $p<0.1$ * $p<0.05$, ** $p<0.01$, *** $p<0.001$

The effect of the identity email (I2) on the competition entry rate, relative to the self-efficacy email (I3), is estimated to be 5.5% (p = 0.774; 95% CI [-32%, 43%]). The effect on the proportion of people who clicked to find out more is estimated to be -0.3% (p = 0.959; 95% CI [-11%, 10%]). These effects correspond to differences of 0.004pp and -0.003pp respectively. These results suggest that there was no difference in effect between I1 and I2 on either the primary or secondary outcome. Recipients of I2 were no more likely to participate in the competition – or to click to find out more – than recipients of I1. H5.3 is therefore shown to be false.

6.7.4 Testing for heterogeneous intervention effects (H5.4)

H5.4 predicted that the two theory-based emails would be more effective for people from low-SES backgrounds. Table 6.15 presents the findings of the tests for this hypothesis.

Table 6.15: Average intervention effects for H5.4, logistic regression models interacting FSM with assignment

| | Primary outcome | | Secondary outcome | |
| | (1) | (2) | (3) | (4) |
	Basic model	Inc. covariates	Basic model	Inc. covariates
I1 (Self-efficacy)	-0.214	-0.209	-0.093	-0.087
	(0.208)	(0.208)	(0.060)	(0.060)
I2 (Identity)	-0.123	-0.120	-0.110^{+}	-0.110^{+}
	(0.202)	(0.203)	(0.061)	(0.061)

Yes*Self-efficacy	0.127	0.116	0.004	-0.008
	(0.466)	(0.466)	(0.128)	(0.128)
Yes*Identity	-0.060	-0.061	0.073	0.067
	(0.474)	(0.474)	(0.127)	(0.127)
Constant	-7.026***	-7.424***	-4.604***	-4.794***
	(0.139)	(0.287)	(0.042)	(0.080)
N observations	216,786	216,507	216,786	216,786
Pseudo r-squared	0.000	0.033	0.001	0.037

Notes: 'Yes' means that an individual has been entitled to free school meals. The reference groups for comparisons – the control group (I3) and FSM=No – are omitted from the table. Point estimates are presented as log-odds. Standard errors are in brackets. Covariates (gender, ethnicity and year) are omitted from the table but included in the models as labelled. The 'Basic model' in each case includes only the outcome variable, assignment, FSM and FSM interacted with assignment.

$+$ $p<0.1$ * $p<0.05$, ** $p<0.01$, *** $p<0.001$

The interaction effect for the self-efficacy email (I1) on the competition entry rate is estimated to be 12% (p = 0.804; 95% CI [-80%, 103%]. The same effect on the proportion of people who clicked to find out more is estimated to be -7.8% (p = 0.952; 95% CI [-26%, 24%]). The equivalent interaction effects for the identity email are estimated to be -6.1% for the primary outcome (p = 0.897; 95% CI [-99%, 87%]) and 6.7% for the secondary outcome (p = 0.598; 95% CI [-18%, 32%]). These results suggest that there was no difference in effects for people from low-SES backgrounds. H5.4 is therefore shown to be false.

6.8 Conclusions

The research question for this study was, 'What is the most effective way to encourage political participation post-service?' (RQ3). It was hypothesised that a message that primed recipients' sense of civic self-efficacy, and one that appealed to their sense of identity would both be more effective than a plain invitation. It was also predicted that the identity message would be more effective than the self-efficacy one, and that both theory-informed messages would be more effective for young people from low-SES backgrounds. The study design was robust – a large scale RCT – and the implementation was sound. The findings are contrary to all hypotheses. Neither of the theory-informed emails were more effective than the control in encouraging political participation, the identity message was no more effective than the self-efficacy message, and there was no difference in effects for participants from low-SES backgrounds. If there are any differences at all between groups, the control email may be slightly more effective in encouraging initial interest. These findings are

broadly robust to a range of robustness checks (see Annex I at the end of this chapter).

The results are consistent with two possible conclusions: i. the theory-informed emails *did* activate a sense of self-efficacy or identity as intended, but this had no effect on the outcomes of interest (a failure of the theories); or ii. the theory-informed emails *did not* activate a sense of self-efficacy and identity as intended (a failure of implementation). Unfortunately, due to the time lag between administering the interventions and measuring the primary outcome, it was not feasible to conduct a manipulation check (such as a post-intervention survey) that would have helped with the interpretation of the results.[55] The plausibility of the two possible conclusions is therefore discussed below in relation to the literature and the findings from the other two studies in this thesis.

6.8.1 Failure of the theories

6.8.1.1 Intervention 1 (self-efficacy)

The self-efficacy-based email was based on three key assumptions: first, that service learning enhances non-political self-efficacy; second, that this increase in non-political self-efficacy can lead to an increase in political self-efficacy; and third, that priming a sense of political self-efficacy can lead to increased political participation. The review of the literature suggests that the third of these assumptions is the most secure. There is an extensive literature demonstrating a correlation between political self-efficacy and political participation ($r = .20$ to .50) (Almond & Verba 1989; Converse 1972; Craig 1979; Craig et al. 1990; Karp & Banducci 2008; Niemi et al. 1991; Pateman 1970; Pollock III 1983; Vecchione & Caprara 2009; Verba et al. 1995) and there is some experimental evidence that political self-efficacy can be manipulated (John & Sjoberg 2020; Tedesco 2007). If the theory has failed then, it is the first or second assumption (or both) that are most likely to be flawed. On the first, outside of this thesis the evidence on

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[55] A survey administered after the primary outcome was measured would have been completed weeks after the emails were first read. This delay would have resulted in very low response-rates and, for those who did respond, a poor measure of the hypothesised mechanism. A survey administered directly after the emails were first read, and prior to measurement of the primary outcome, would likely have influenced the outcome itself. At the very least, it would have been impossible to say whether any observed effects were a result of the interventions or the survey. NCS Trust was also reluctant to introduce extra friction into the process that may have reduced overall response rates to the competition.

a causal relationship between service learning and civic self-efficacy is limited. However, Study 1 in this thesis has provided strong evidence of a substantial positive effect on general civic self-efficacy, so the intervention is very unlikely to have fallen at this first hurdle.

The second assumption is less secure. The results from Study 1 and Study 2 both suggest that a transferal of self-efficacy to the domain of politics is very hard to achieve. The quantitative analysis estimates a very small effect of NCS on political self-efficacy, and the qualitative analysis suggests that NCS did not leave participants with a sense that the domains of service and political action were similar. Indeed, some interviewees explicitly stated the opposite view. This complexity in the relationship between self-efficacy and service learning, and the potential for null or backfire effects was also recognised in the theory and hypotheses in the PAP for this present study (Taylor, 2020d, pp.4-10). The results of this study provide further evidence that this second assumption may not hold. It may instead be true that service learning contributes to a belief that political participation is harder and less effective than service-based participation, and this may explain why a message that was focussed on efficacy had a null or small negative effect.

6.8.1.2   Intervention 2 (identity)

The identity-based email relied on four key assumptions: first, that participants identified as young people; second, that they also identified as active citizens; third, that they saw young people as group that is unfairly marginalised; and fourth, that priming participants to identify with these groups can lead to increased political participation. The first of these assumptions seems reasonable as a matter of fact. The third is well supported by existing research. Section 1.1 of the introduction described how, as a result of their lower rate of participation in politics, young people suffer materially, and have fewer of their preferences expressed in government policy (Birch et al. 2013, p.14; Ipsos Mori, 2016). There is also evidence to suggest that a substantial proportion of young people in the UK *feel* discriminated against because of their age (without any priming) (Willow et al. 2007). So, it seems fairly safe to assume that a large proportion of participants saw young people as a group that is unfairly marginalised. The fourth assumption is fairly well-grounded in the evidence on social mobilisation. We know that appealing to identity can be an effective way of encouraging

political participation (Bryan et al. 2011; Kessler & Milkman 2017), although these particular identities have never been tested in this way.

The second assumption – that NCS graduates self-identify as active citizens – is less secure. While there is some evidence that participants of NCS can develop a sense of attachment to the social group of NCS graduates (Panayiotou et al. 2017), the qualitative analysis from Study 2 in this thesis suggests that any such attachment is unlikely to be based on a sense of active citizenship. The Study 2 interviews specifically asked participants a series of questions on the topic of identity and the results from this were clear. Participants' strongest memories of the programme rarely related to experiences of civic participation, and no evidence was identified to suggest that they left the programme with strong sense of self as an active citizen (unless they already thought of themselves in these terms prior to NCS). This is contrary to what some other researchers have suggested about service learning in general (Flanagan et al. 1999, p.3; Youniss et al. 1997, p.625) but this prior research is not convincing (as the literature has demonstrated), and the findings in the case of NCS seem clear on this point. If the theory did fail for the identity intervention, it is most likely because graduates of NCS have no strong sense of being a civic changemaker.

6.8.2 Failure of the implementation

The alternative explanation for the findings is that the theories behind the interventions were valid, but the implementation was flawed. There are three main possibilities here: i. that recipients did not read the intervention text (properly); ii. that they did read it, but it did not activate self-efficacy or identity as intended; or iii. that they read it, it worked as intended to some extent, but the control message was as (or more) effective in terms of the primary and secondary outcomes. These possibilities are discussed in turn below.

First, it is possible that recipients did not read the intervention text at all, or read it, but did not engage properly with its contents. There is only a small amount of experimental evidence in the field of political participation that shows differential effects between email messages and, where effects have been observed, the settings and messages have been very different. Han (2016) showed that the contents of an email can have substantial effects on political participation, with the best performing email in one study outperforming the

control by 6pp. Participants in Han's study clearly read the messages in detail, and the contents of these messages affected their behaviour. However, there are a number of important differences between Han's study and the present trial. First, the study population was made up of medical doctors who were members of a professional body. This population was above-averagely educated and used to reading emails and other texts in detail. Second, the act of participation that was being encouraged was very different, with recipients being asked to sign a petition in support of a health care policy. This issue was directly related to their professional lives, and this fact is likely to further encourage close reading of the message.[56] These characteristics are in contrast to the sample in this study, which was made up of a diverse group of young people who should not be expected to respond so positively to email interventions. In this sense, they may be similar to recipients of mass emails that aim to mobilise voters, who are generally unresponsive to such communications, or may even react negatively to them (Bennion & Nickerson 2011; Nickerson 2007). The efficacy of email as a tool for the mobilisation of volunteers among university students has also been questioned (John et al. 2019). So, it is plausible that recipients in the trial did not engage (properly) with the content of the messages because they were delivered in email form.

Second, it is possible that participants did read the intervention messages, but that the text did not activate self-efficacy or identity as intended. Where previous interventions have been shown to enhance self-efficacy, they have not been delivered by email. For example, interactive websites, which give more detailed political information, interspersed with short quizzes have been shown to increase political efficacy for some groups (John & Sjoberg 2020; Tedesco 2007). And where interventions have been successful in leveraging identity to encourage voting, they have been delivered by survey, on the eve of an election (Bryan et al. 2011). The study populations were, again, very different in all of these cases, and individual characteristics (such as partisanship) have been shown to substantially moderate effects (John & Sjoberg 2020). This again suggests that a high level of interest in the content of the message may be required for interventions such as these to be effective.

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[56] The intervention text was also based on a different theory (personal goal affirmation).

Finally, it may the case that the intervention texts were read, and worked as intended to some extent, but that the control condition was just equally (or slightly more) effective in encouraging interest and participation. In some studies, researchers opt for pure control conditions (where participants receive nothing), or placebo treatments (where participants receive content that is completely unrelated to the outcomes of interest). It is trivially true that either of these condition-types would have led to little or no participation in the letter writing competition (which was not publicly advertised), so would not have been appropriate in this case. Han's experiments (2016) took a similar approach to the control condition as in this study, but instead of attempting to make the control message informational only, she opted to replace the treatment text with a series of ideological rhetorical questions. It could be argued that this approach might be expected to produce a negative reaction from recipients. The control condition in this study was simple and gave recipients a clear sense of the purpose of the email from the outset, as follows.

> *'We're writing to let you know about an exciting competition that we're running for graduates of NCS. Take a look at the details below and we hope you'll consider taking part.'*

This contrasted with the intervention texts which began with more general motivational messages. Simplicity and clarity have been shown to be important factors in effective messaging across a range of policy domains (Sunstein 2013), and the findings of this study provide some support for this idea. The control message also emphasised the competition element of the activity in the first line of the email, which was presumably a motivating factor for some entrants and, while it was clearly described in both intervention emails as well, it was not the first thing that was mentioned in either. Writing a completely 'plain' or atheoretical message is, of course, an impossible task, so this kind of issue is always a risk with this type of study design.

All three of these explanations about implementation failure are plausible, but the evidence seems strongest in support of the first. If the null /negative findings were a result of a failure of implementation, this is likely because the mode of communication – email – did not match the characteristics and preferences of the study population. Participants did not engage with the email content in enough depth for the interventions to have a chance of working.

6.8.3 Summary

The literature review (section 2.4) and hypotheses for this study (section 6.4) explicitly acknowledged the difficulties involved in developing effective interventions in this context. The study population and the type of participation being encouraged were very different to anything in the existing literature. The two theories tested in this study – relating to self-efficacy and identity – were chosen because they related to specific characteristics of the study population and context (as described in section 2.4), but they had not been tested in this way before. Two broad explanations for the null/negative results have been discussed: a failure in the theories and a failure in implementation. Both are plausible, and the lack of manipulation check – coupled with limited associated literature – make it difficult to be sure which combination of these factors led to the results in this case. However, the discussion here, combined with the findings from Study 1 and Study 2, suggests that at least four factors might explain the results. First, encouraging a spillover from a sense of service-based or general civic self-efficacy to a sense of political self-efficacy is very hard to do (among this population at least), and does not seem to have happened here. Second, it does not look like graduates of NCS have incorporated a strong sense of being an active citizen into their identities. Third, email does not appear to be an effective tool for mobilising political participation among this group of young people. And fourth, the control condition – which also encouraged participation, by design – did have potentially favourable characteristics; its simplicity and earlier mentioning of the competition. These plausible strengths of the control, when combined with the possible failures in theory, may have made it as (or slightly more) effective than the theory-informed interventions.

6.9 Annex I: Robustness checks

Two categories of robustness check are carried out on the primary analysis for Study 3: i. non-compliance analysis; and ii. rare events analysis. This includes all checks specified in the PAP (Taylor 2020d, pp.16-18). The results of these checks provide strong support for the findings from the primary analysis.

6.9.1 Non-compliance analysis

6.9.1.1 True non-compliance

Non-compliance, in the sense that a participant receives a different email to the one that she is assigned, is unlikely to have occurred in this study. However, there are two possible ways that it could have happened: i. through participants sharing emails between study arms; or ii. through an administrative error in the mailout process. It is not possible to identify cases of the first type in the data. If someone clicked, or entered the competition, through a link that was shared with them by a trial participant, the outcomes would have been registered against that participant. This risk was mitigated by accepting only one competition entry per hyperlink, but it could not be prevented entirely and, if it happened, the data does not show it. To check for cases of the second type (an error in the mailout), a report was generated after the two mailouts showing which email was sent to each study participant. This was compared with the intervention assignment dataset using a unique participant ID for matching. No differences were identified between these two lists, so we can be confident that emails were sent as assigned.

6.9.1.2 Bounce backs

There were, however, some study participants who did not receive one or more of the emails, due to issues such as deactivated email addresses and full inboxes. These issues are collectively referred to as 'bounce backs'.[57] All participants were programmed to be sent an invitation email and reminder email. Table 6.16 summarises the number of participants who received both emails, at least one email, only the invitation email, only the reminder email, or neither. It shows that 88% of the total sample received at least one email.

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[57] For a full description of bounce back types and the protocols followed by the mailout software, see: https://sforce.co/2yPM0EI.

Table 6.16: Email receipt by treatment group

	Control email	Self-efficacy email	Identity email	Total
Both	65,311 (29%)	65,408 (29%)	65,547 (29%)	196,266 (86%)
At least one	66,493 (29%)	66,642 (29%)	66,761 (29%)	199,896 (88%)
Invitation only	1,137 (1%)	1,209 (1%)	1,187 (1%)	3,533 (2%)
Reminder only	45 (0%)	25 (0%)	27 (0%)	97 (0%)
Neither	9,296 (4%)	9,149 (4%)	9,031 (4%)	27,476 (12%)

Notes: N = 227,372. Figures in brackets are percentages of N.

Following the process specified in the PAP (Taylor 2020d, p.16), bounce back analysis begins by testing whether the proportion of bounce backs is significantly different by trial arm. These tests are carried out using the following model.

$$Y_i \sim bernoulli(p_i) = \beta_0 + \beta_1 T_i \tag{9}$$

where:

- Y_i is a dummy variable indicating whether an individual received at least one email; and
- T_i is a dummy variable representing intervention assignment.

Receiving at least one email has been chosen as the definition of intervention receipt. This is because it indicates whether an individual received any intervention at all, and is therefore the most conservative definition. The logistic regression in Equation 9 is run twice, with a different reference group each time, to test the significance of differences between all three groups. These tests reveal a significant difference (p = 0.037) in email receipt between I2 (the identity email) and I3 (the control). Under these circumstances, the PAP specifies that upper and lower bounds should be calculated for each estimated intervention effect (Taylor 2020d, p.16). However, the large proportion of zeros in the overall sample (for both the primary and secondary outcome), combined with the large number of people who did not receive at least one email, means that this approach would result in unhelpfully wide (and implausible) bounds.[58] For this

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[58] For example, calculating the upper bound for the effect of I1 would involve imputing ones in group I1 and zeros in the two other groups, for all bounce backs. This would increase the raw

reason, a deviation from the PAP is made here. Instead of calculating bounds, individuals who did not receive at least one email are dropped, and intervention effects are re-estimated using this reduced sample. The different rates of bounce backs noted above means that this procedure may introduce bias, so the results should be interpreted with caution. However, there is no plausible mechanism that could link bounce backs to assignment or the outcomes, so it is likely that observed difference in rates between two of the groups is down to chance.

Table 6.17 presents the results from this analysis for the hypothesis tests for H5.1 and H5.2, alongside the ITT estimates from the main analysis above. When the sample is reduced, the direction and magnitude of all effects remain the same (to 1 d.p.) and the standard errors are also very similar. These findings provide further support for the main results, suggesting null effects for both interventions on the primary outcome, and small negative effects on the secondary outcome.

Table 6.17: Comparing intervention effects for ITT and bounce back analysis for H5.1 and H5.2

| | Primary outcome | | Secondary outcome | |
	ITT	Received at least one	ITT	Received at least one
I1 (Self-efficacy)	-0.186	-0.190	-0.088[+]	-0.092[+]
	(0.185)	(0.186)	(0.053)	(0.053)
I2 (Identity)	-0.112	-0.135	-0.091[+]	-0.094[+]
	(0.179)	(0.183)	(0.053)	(0.053)
Constant	-7.427***	-7.282***	-4.798***	-4.652***
	(0.282)	(0.283)	(0.078)	(0.078)
N observations	216,507	190,742	216,786	191,004
Pseudo r-squared	0.033	0.031	0.037	0.035

Notes: The reference group for comparisons – the control group (I3) – is omitted from the table. Point estimates are presented as log-odds. Standard errors are in brackets. Covariates (FSM, gender, ethnicity and year) are omitted from the table but included all models.
[+] p<0.1 * p<0.05, ** p<0.01, *** p<0.001

Table 6.18 presents the bounce back analysis results for the hypothesis test for H5.3. When the sample is reduced, the direction of the results remains the same, and magnitude and standard errors are very similar. These findings again provide

difference in competition entries between I1 and I3 (the control) from 11 to 9,138, which is clearly a meaningless result.

further support for the main results, suggesting a null effect for H5.3 on the primary and secondary outcomes.

Table 6.18: Comparing intervention effects for ITT and bounce back analysis for H5.3

	Primary outcome		Secondary outcome	
	ITT	Received at least one	ITT	Received at least one
I2 (Identity)	0.055	0.055	-0.003	-0.002
	(0.186)	(0.192)	(0.055)	(0.055)
I3 (Control)	0.186	0.190	0.088+	0.092+
	(0.186)	(0.186)	(0.053)	(0.053)
Constant	-7.614***	-7.472***	-4.887***	-4.745***
	(0.289)	(0.289)	(0.079)	(0.079)
N observations	216,507	190,742	216,786	191,004
Pseudo r-squared	0.033	0.031	0.037	0.035

Notes: The reference group for comparisons – the self-efficacy email (I1) – is omitted from the table. Point estimates are presented as log-odds. Standard errors are in brackets. Covariates (FSM, gender, ethnicity and year) are omitted from the table but included in all models. The control group (I3) is included in the table for completeness but was not referenced in hypothesis H5.3.
+ p<0.1 * p<0.05, ** p<0.01, *** p<0.001

The results for the sub-group analysis for participants from low-SES backgrounds follows a similar pattern to the other hypothesis tests. As Table 6.19 shows, the analysis on the subsample produces estimates that are very similar to those produced by the ITT analysis.

Table 6.19: Comparing intervention effects for ITT and bounce back analysis tests of H5.4

	Primary outcome		Secondary outcome	
	ITT	Received at least one	ITT	Received at least one
I1 (Self-efficacy)	-0.214	-0.213	-0.093	-0.090
	(0.208)	(0.208)	(0.060)	(0.061)
I2 (Identity)	-0.123	-0.125	-0.110+	-0.110+
	(0.202)	(0.203)	(0.061)	(0.061)
Yes*Self-efficacy	0.116	0.118	-0.008	-0.007
	(0.466)	(0.467)	(0.128)	(0.128)
Yes*Identity	-0.061	-0.055	0.067	0.071
	(0.474)	(0.474)	(0.127)	(0.127)
Constant	-7.424***	-7.278***	-4.794***	-4.648***
	(0.287)	(0.287)	(0.080)	(0.080)
N observations	216,507	190,742	216,786	191,004
Pseudo r-squared	0.033	0.031	0.037	0.035

6.9.1.3   Opens

There were also some people who received emails but did not open them. Email subject lines were held constant across trial arms, so people who did not open at least one email did not receive their assigned intervention. Table 6.20 summarises the number of participants who opened both emails, at least one email, only the invitation email, only the reminder email, or neither. It shows that 31% of the total sample opened at least one email.

Table 6.20: Email opens by treatment group

	Control email	Self-efficacy email	Identity email	Total
Both	9,383 (4%)	9,434 (4%)	9,665 (4%)	28,482 (13%)
At least one	23,463 (10%)	22,952 (10%)	24,004 (11%)	70,419 (31%)
Invitation only	8,049 (4%)	7,422 (3%)	7,830 (3%)	23,301 (10%)
Reminder only	6,031 (3%)	6,096 (3%)	6,509 (3%)	18,636 (8%)
Neither	52,326 (23%)	52,839 (23%)	51,788 (23%)	156,953 (69%)

Notes: N = 227,372. Figures in brackets are percentages of N.

The same procedure as the bounce back analysis is followed for open rates. First, significance tests are run using the model specified in Equation 9, but with Y_i this time indicating whether an individual opened at least one email. These tests reveal significant differences in open rates between all groups ($0.000 < p < 0.004$). However, as with the bounce back analysis, upper and lower bound estimation would be unhelpful here. The procedure applied in the bounce back analysis – i.e. dropping individuals who did not open at least one email – is therefore repeated here and the likelihood of bias being introduced is again small; all emails had the same subject line so there is no possible mechanism linking assignment or the outcomes to the open rate.

Table 6.21 presents the results from the analysis on the subsample of participants who opened at least one email for the hypothesis tests for H5.1 and H5.2, alongside the ITT estimates from the main analysis. When the sample is reduced, the direction of all effects remains the same, and the magnitudes are similar. The confidence interval for the effect of I1 on click rates broadens

slightly, so that the effect is no longer marginally significant. The confidence interval for the effect of I2 on click rates narrows slightly, so that this is now significant at the 95% confidence level. These results broadly support the findings of the main analysis and, in particular, provide further support for the finding that the identity intervention had a small negative effect on the secondary outcome.

Table 6.21: Comparing intervention effects for ITT and email open analysis for H5.1 and H5.2

| | Primary outcome | | Secondary outcome | |
	ITT	Opened at least one	ITT	Opened at least one
I1 (Self-efficacy)	-0.186	-0.172	-0.088[+]	-0.074
	(0.185)	(0.186)	(0.053)	(0.054)
I2 (Identity)	-0.112	-0.149	-0.091[+]	-0.113[*]
	(0.179)	(0.183)	(0.053)	(0.054)
Constant	-7.427***	-6.150***	-4.798***	-3.507***
	(0.282)	(0.283)	(0.078)	(0.079)
N observations	216,507	67,636	216,786	67,732
Pseudo r-squared	0.033	0.023	0.037	0.024

Notes: The reference group for comparisons – the control group (I3) – is omitted from the table. Point estimates are presented as log-odds. Standard errors are in brackets. Covariates (FSM, gender, ethnicity and year) are omitted from the table but included all models.
[+] $p<0.1$ * $p<0.05$, ** $p<0.01$, *** $p<0.001$

Table 6.22 presents the results of the email opens analysis for the hypothesis test for H5.3. When the sample is reduced, the direction of the effects of I2 relative to I1 remain the same. There is a substantial change in magnitude of the effect on the secondary outcome, but it (and the primary outcome effect) remains insignificant. These results provide further support for the null findings in the main analysis.

Table 6.22: Comparing intervention effects for ITT and email open analysis for H5.3

| | Primary outcome | | Secondary outcome | |
	ITT	Opened at least one	ITT	Opened at least one
I2 (Identity)	0.055	0.023	-0.003	-0.039
	(0.186)	(0.192)	(0.055)	(0.055)
I3 (Control)	0.186	0.172	0.088[+]	0.074
	(0.186)	(0.186)	(0.053)	(0.054)
Constant	-7.614***	-6.322***	-4.887***	-3.581***

	(0.289)	(0.289)	(0.079)	(0.080)
N observations	216,507	67,636	216,786	67,732
Pseudo r-squared	0.033	0.023	0.037	0.024

Notes: The reference group for comparisons – the self-efficacy email (I1) – is omitted from the table. Point estimates are presented as log-odds. Standard errors are in brackets. Covariates (FSM, gender, ethnicity and year) are omitted from the table but included in all models. The control group (I3) is included in the table for completeness but was not referenced in hypothesis H5.3.
⁺ p<0.1 * p<0.05, ** p<0.01, *** p<0.001

The results for the sub-group analysis for participants from low-SES backgrounds follows a similar pattern to the equivalent bounce back analysis. The direction of the effects are all the same in the new analysis, and all effects remain small and insignificant at the 95% confidence level, with very broad confidence intervals (Table 6.23).

Table 6.23: Comparing intervention effects for ITT and email open analysis for H5.4

	Primary outcome		Secondary outcome	
	ITT	Opened at least one	ITT	Opened at least one
I1 (Self-efficacy)	-0.214	-0.197	-0.093	-0.073
	(0.208)	(0.208)	(0.060)	(0.061)
I2 (Identity)	-0.123	-0.135	-0.110⁺	-0.123⁺
	(0.202)	(0.203)	(0.061)	(0.061)
Yes*Self-efficacy	0.116	0.126	-0.008	-0.001
	(0.466)	(0.467)	(0.128)	(0.130)
Yes*Identity	-0.061	-0.079	0.067	0.048
	(0.474)	(0.475)	(0.127)	(0.129)
Constant	-7.424***	-6.147***	-4.794***	-3.503***
	(0.287)	(0.287)	(0.080)	(0.081)
N observations	216,507	67,636	216,786	67,732
Pseudo r-squared	0.033	0.023	0.037	0.024

Notes: 'Yes' means that an individual has been entitled to free school meals. The reference groups for comparisons – the control group (I3) and FSM=No – are omitted from the table. Point estimates are presented as log-odds. Standard errors are in brackets. Covariates (gender, ethnicity and year) are omitted from the table but included in all models.
⁺ p<0.1 * p<0.05, ** p<0.01, *** p<0.001

6.9.1.4 Conclusions

The results of the non-compliance analysis provide comprehensive support for the findings from the ITT analysis. The direction of all effects remains the same across the new estimates and most maintain very large confidence intervals. The ITT analysis revealed marginally significant (p < 0.1) small negative effects of I1 and I2 on click rates, compared to the control group. When these effects are re-estimated on the sample of individuals who received at least one email, these

estimates, and their confidence intervals, remain almost identical. However, when the sample is further reduced to only those participants who opened at least one email (so truly received the interventions), the confidence interval on the effect of I1 broadens such that it is no longer even marginally significant. Conversely, the effect of I2 becomes significant (p < 0.05). This suggests that effect of I2 may be the only true effect observed on the click rates.

6.9.2 Rare events analysis

When a binary dependent variable contains a large number of zeros and a small number of ones, logistic regression based on maximum likelihood estimation (MLE) can underestimate the probability of the event in question, and overestimate the variance (King & Zeng, 2001, pp.146-147). The analysis carried out above may be vulnerable to these issues. Out of the 227,372 people in the study dataset, only 180 people entered the political letter writing competition. In other words, the primary outcome variable contains 227,192 zeros and 180 ones. The potential bias that this might lead to can be corrected for in a number of ways. 'Penalised maximum likelihood estimation' (PMLE), proposed by Firth (1993), is used here as it is easiest to implement and produces numerically very similar results to the alternatives (King & Zeng, 2001, p.148). Effects for all hypothesis tests are re-estimated using PLME and compared with the results of the MLE used in the main analysis, with the results presented in Tables 6.24, 6.25 and 6.26.

Table 6.24: Comparing intervention effects for rare events analysis for H5.1 and H5.2

	Primary outcome		Secondary outcome	
	(1)	(3)	(4)	(6)
	MLE	PMLE	MLE	PMLE
I1 (Self-efficacy)	-0.186	-0.186	-0.088+	-0.088+
	(0.185)	(0.185)	(0.053)	(0.053)
I2 (Identity)	-0.131	-0.130	-0.091+	-0.091+
	(0.183)	(0.182)	(0.053)	(0.053)
Constant	-7.427***	-7.388***	-4.798***	-4.795***
	(0.282)	(0.280)	(0.078)	(0.078)
N observations	216,507	216,786	216,786	216,786

Notes: The reference group for comparisons – the control group (I3) – is omitted from the table. Point estimates are presented as log-odds. Standard errors are in brackets. Covariates (FSM, gender, ethnicity and year) are omitted from the table but included in all models. Two different estimators are used to estimate effects for both outcome variables: the maximum likelihood estimator (MLE), used in the main analysis, and the penalised maximum likelihood estimator (PMLE).
+ p<0.1 * p<0.05, ** p<0.01, *** p<0.001

Table 6.25: Comparing intervention effects for rare events analysis for H5.3

	Primary outcome		Secondary outcome	
	(1)	(2)	(3)	(4)
	MLE	PMLE	MLE	PMLE
I2 (Identity)	0.055	0.055	-0.003	-0.003
	(0.186)	(0.190)	(0.055)	(0.055)
Constant	-7.614***	-7.573***	-4.887***	-4.884***
	(0.289)	(0.286)	(0.079)	(0.079)
N observations	216,507	216,786	216,786	216,786

Notes: The reference group for comparisons – the self-efficacy email (I1) – is omitted from the table. Point estimates are presented as log-odds. Standard errors are in brackets. Covariates (FSM, gender, ethnicity and year) are omitted from the table but included in all models. Two different estimators are used to estimate effects for both outcome variables: the maximum likelihood estimator (MLE), used in the main analysis, and the penalised maximum likelihood estimator (PMLE).
$^+$ $p<0.1$ * $p<0.05$, ** $p<0.01$, *** $p<0.001$

Table 6.26: Comparing intervention effects for rare events analysis for H5.4

	Primary outcome		Secondary outcome	
	(1)	(3)	(4)	(6)
	MLE	PMLE	MLE	PMLE
I1 (Self-efficacy)	-0.214	-0.207	-0.093	-0.087
	(0.208)	(0.206)	(0.060)	(0.060)
I2 (Identity)	-0.123	-0.119	-0.110$^+$	-0.106$^+$
	(0.202)	(0.202)	(0.061)	(0.061)
Yes*Self-efficacy	0.116	0.117	-0.008	-0.008
	(0.466)	(0.458)	(0.128)	(0.128)
Yes*Identity	-0.061	-0.054	0.067	0.067
	(0.474)	(0.464)	(0.127)	(0.127)
Constant	-7.424***	-7.424***	-4.794***	-4.790***
	(0.287)	(0.287)	(0.080)	(0.080)
N observations	216,507	216,786	216,786	216,786

Notes: 'Yes' means that an individual has been entitled to free school meals. The reference groups for comparisons – the control group (I3) and FSM=No – are omitted from the table. Point estimates are presented as log-odds. Standard errors are in brackets. Covariates (FSM, gender, ethnicity and year) are omitted from the table but included in all models. Two different estimators are used to estimate effects for both outcome variables: the maximum likelihood estimator (MLE), used in the main analysis, and the penalised maximum likelihood estimator (PMLE).
$^+$ $p<0.1$ * $p<0.05$, ** $p<0.01$, *** $p<0.001$

None of the estimated intervention effects are sensitive corrections to the maximum likelihood estimator made for rare events. This is likely to be because, while there were a relatively small number of competition entrants as compared to non-entrants, the overall sample was large, and the bias associated with rare

events tends to zero as N tends to infinity (King & Zeng 2001, p.148). The rare events analysis therefore supports the findings of the main analysis.

6.9.3 Missing data analysis

Only one variable in the data, FSM eligibility, contained missing values. For this variable, 10,586 values were missing, which represents 4.7% of the sample. When less than 5% of data is missing, little bias is likely to be introduced to estimated treatment effects (Schulz & Grimes 2002, p.784), so no further sensitivity analysis is conducted here based on missing data. This is in line with the procedure specified in the PAP (Taylor 2020d, p.17).

7 Summary and conclusions

7.1 Results

This thesis has aimed to improve our understanding of the relationship between service learning and political participation by answering three questions.

- **RQ1:** What is the effect of service learning on young people's political participation?
- **RQ2:** If there is an effect, how is it produced?
- **RQ3:** What is the most effective way to encourage political participation post-service?

RQ1 has been addressed by Study 1, RQ2 has been addressed by Studies 1 and 2 in combination, and RQ3 has been addressed by Study 3. The findings in response to these three questions are summarised below.

7.1.1 RQ1: What is the effect of service learning on young people's political participation?

Study 1 has shown that service learning – as exemplified by NCS – can have a substantial positive effect on young people's political participation post-service. Participants are 3.1pp more likely to participate in political activities after NCS ($p = 0.000$; 95% CI [2.0, 4.1]); a 12% increase on the baseline average in the treatment group. This average effect across participation types is made up of a 5.4pp effect on petition-signing, a 4.9pp effect on protest attendance, a 2.3pp effect on contacting a politician, and a 2.3pp effect on intention to vote.

7.1.2 RQ2: If there is an effect, how is it produced?

The combined results of Studies 1 and 2 in response to this question are summarised in a logic model in Figure 5.3. This model suggests that there are four key types of activity in this type of service learning that contribute to its positive effects on political participation: i. political and issue-based discussions; ii. non-civic personal development activities and mastery experiences; iii. service-based civic participation; and iv. guided reflection and positive feedback. These activities can trigger a set of mechanisms that fall into one of four categories: i. capabilities; ii. attitudes and beliefs; iii. knowledge; and iv. networks of

recruitment. The causal pathways in the model show that each of the four categories of activity could trigger multiple mechanisms, and that some mechanisms are thought to act directly on political participation, whereas for others to work, a chain of outcomes is required. The effectiveness of service learning on political participation is moderated by a complex set of participant characteristics, as well as characteristics of the experience.

Figure 7.1: Revised logic model of service learning and political participation
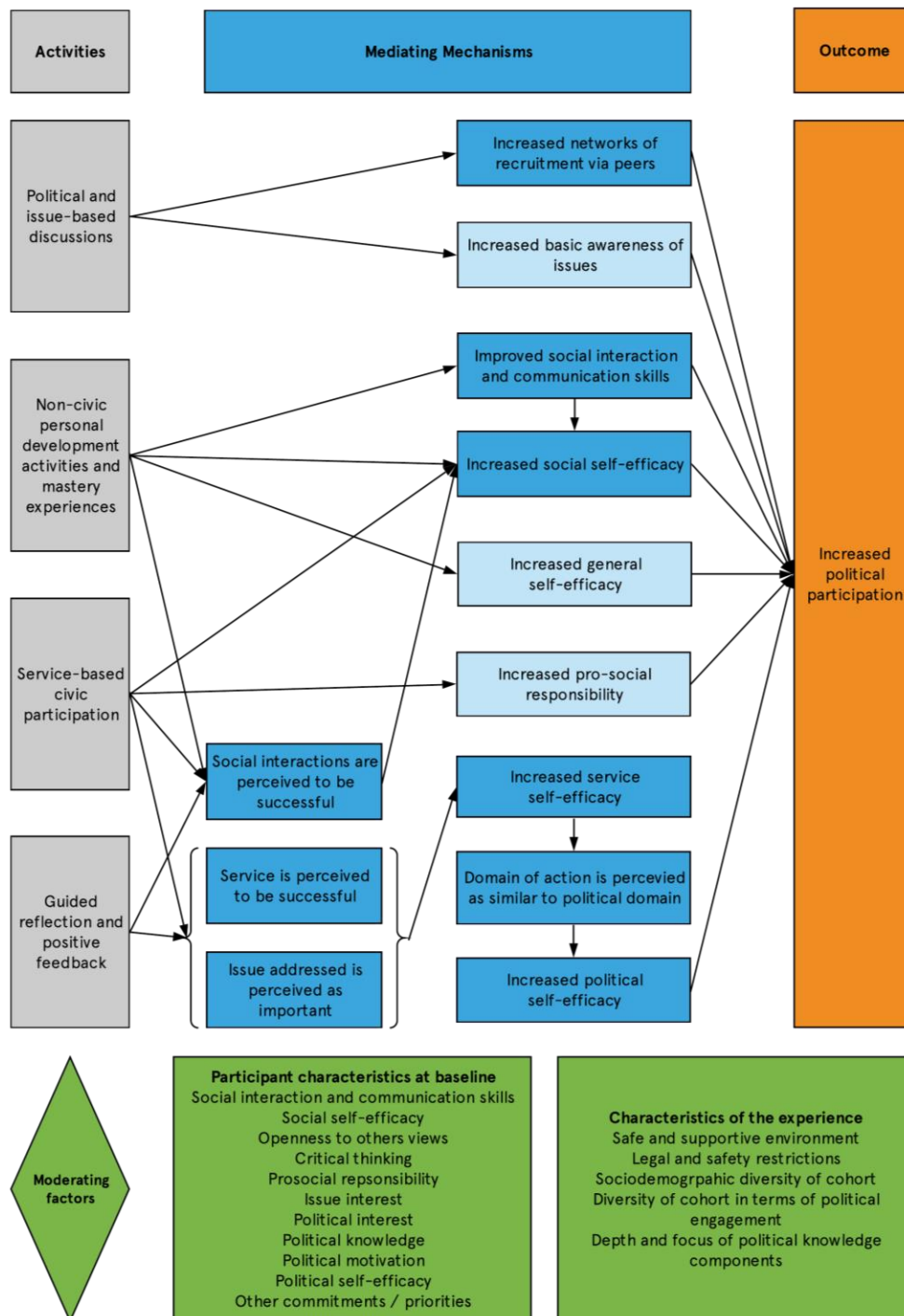
The findings that sit behind this diagram are detailed and complex, so cannot be summarised in full here. Two substantive results within these general findings have been highlighted in this thesis because they may be particularly surprising to some. First, an increase in social self-efficacy seems to be the most powerful mediating mechanism. Second, gains in service self-efficacy can lead to losses in political self-efficacy and, therefore, a reduction in political participation for some individuals. Preventing such losses – and encouraging positive spillovers – is difficult and requires new activities and conditions to be present.

7.1.3   RQ3: What is the most effective way to encourage political participation post-service?

Study 3 aimed to answer this question by testing the relative effects of three messages that encouraged graduates of NCS to participate politically post-service. The first message was based on the theory of self-efficacy, the second on theories of identity, and the third was intended to be a 'plain' invitation (as a control). Neither of the theory-informed messages was more effective than the control message in encouraging political participation. The effect of the self-efficacy email on the competition entry rate was estimated to be -19% ($p = 0.316$; 95% CI [-55%, 18%]). The effect of the identity email on the competition entry rate was estimated to be -13% ($p = 0.474$; 95% CI [-49%, 23%]). These effects correspond to differences of -0.015pp and -0.011pp respectively. The effect of the self-efficacy email on the proportion of people who clicked to find out more is estimated to be -8.8% ($p = 0.097$; 95% CI [-19%, 1.6%]). The effect of the identity email on the proportion of people who clicked to find out more is to be -9.1% ($p = 0.087$; 95% CI [-20%, 1.3%]). These effects correspond to differences of -0.086pp and -0.088pp respectively.

The marginal effect of the self-efficacy email on the competition entry rate for participants from low-SES backgrounds is estimated to be 12% ($p = 0.804$; 95% CI [-80%, 103%]. The same effect on the proportion of people who clicked to find out more is estimated to be -7.8% ($p = 0.952$; 95% CI [-26%, 24%]). The equivalent marginal effects for the identity email are estimated to be -6.1% for the primary outcome ($p = 0.897$; 95% CI [-99%, 87%]) and 6.7% for the secondary outcome ($p = 0.598$; 95% CI [-18%, 32%]). These results suggest that there was no difference in effects for people from low-SES backgrounds.

7.2 Contributions to the literature

7.2.1 A robust estimate of effects

Until now, there has been no robust estimate of the effect of service learning on political participation (or the secondary outcomes measured here). This lack of strong evidence has been accompanied by speculation in the literature that service learning could, on average, have a null or negative effect on political participation (Newmann & Rutter 1983; Smith 2006; Kahne 2013). The results of this thesis suggest that this is very unlikely. The claim that NCS (as a specific intervention) might discourage political participation (Mycock & Tonge 2011, pp.63-64; Bacon et al. 2013; Bulley & Sokhi-Bulley 2014) is now shown to be particularly unfounded in terms of an average effect.

Recent experimental work has suggested that interventions that develop noncognitive skills can have a positive effect on voter turnout (Holbein & Hillygus 2020). The results from this thesis support this idea, and its findings on non-electoral participation are particularly new. It is important, however, to remember that these effects have been estimated for one specific programme of service learning; NCS. NCS is an informal education programme, and some of the characteristics that are particular to this type of service learning seemed to play key roles in bringing about the observed positive effects. It is not clear that the same effects would be seen for the service learning programmes that are linked to formal education.

7.2.2 Contradicting the self-efficacy spillover explanation

Prior to this thesis, there seemed to be strong evidence in the literature that, if service learning did have a positive effect on political participation, a key part of the explanation would be down to intermediate effects on participants' self-efficacy. In particular, a spillover effect was hypothesised where gains in service-based self-efficacy would lead to gains in political self-efficacy which, in turn, would lead to gains in political participation (Condon & Holleque 2013, p.168; Reinders & Youniss 2006). The combined findings from the three studies in this thesis suggest that this spillover effect is very hard to create, and does not happen for the average participant. Study 1 estimates that NCS has a small effect on participants' sense of *internal* political self-efficacy (equivalent to 2.3pp), and no effect at all on their *external* political self-efficacy.

In fact, relatively strong evidence has been presented in Study 2 to suggest that enhanced service self-efficacy can lead to lower political self-efficacy and less political participation. This provides support for the argument in the literature that service-based participation can come to be seen as particularly effective – *by its very nature* – because activities such as volunteering with the elderly or cleaning up a park have immediate and directly observable positive outcomes (Hornsey et al. 2006, p.1702; Walker 2000). It also supports the idea that, if service-based participation is to avoid this negative effect, it needs to be accompanied by a very specific type of knowledge development and reflection. Walker suggests that this process should encourage participants to reflect on the systemic causes of the issue being addressed through voluntary service, and the role of government in addressing the issue (Walker 2000, pp.646-647). Study 3 confirms the difficulty in inducing a positive spillover between these domains by finding a null effect of an intervention that is based precisely on this idea. All this seems to confirm the suspicion held by some that the 'simple political spillover thesis' is problematic (Ayala 2000, p.101).

7.2.3 A refined theory

If a spillover from service-based self-efficacy to political self-efficacy is not part of the explanation, then what is? Study 2, provides the first direct and detailed explanation of how service learning increases political participation. This theory, as a whole, is therefore a significant contribution in its own right. However, two parts of the theory stand out as particularly new to the literature. First, while there were some clues in the research about mechanisms prior to this thesis, almost nothing was said about the factors that might moderate the effects. Study 2 has identified 16 moderating factors, and has identified the specific mechanisms that these factors act upon (mapped out in Table 5.6). Second, the findings on social and communication skills, and social self-efficacy as mechanisms were particularly surprising.

On the face of it, the idea that communication skills are important mediators is supported by the literature, which suggests that civic participation experiences can develop communication skills, which can in turn contribute to an increase in political participation (Verba et al. 1995, pp.304-313). There are, however, important differences between the literature and the findings from this thesis.

Verba et al. suggest that participants of civic voluntarism see improvements in their abilities to participate in decision-making meetings, speak publicly and write letters. The findings from this thesis support the first two of these outcomes. However, Study 2 suggests that – when it comes to mediators of political participation – the more important social interaction and communication skills developed through service learning are not technical, but 'soft'. The outcomes in this field that seemed to make the biggest difference were increased empathy, increased ability to listen to different points of view, increased patience, increased ability to make new friends, and reduced prejudice. The literature also says nothing about social self-efficacy as a mechanism, but this was described as a decisive factor by some participants. The exploratory quantitative analysis conducted at the end of Study 2 also suggests that this *perception* of abilities is perhaps more important than the abilities themselves. This analysis estimates that NCS has a substantial effect (equivalent to 6pp) on participants' social self-efficacy, but no average effect on their social or communication skills.

These findings are not only new but, perhaps, surprising. The communication skills identified by Verba et al. are clearly things that are often needed for effective political participation. The softer skills and social self-efficacy identified by this study have a less obvious connection to politics. However, the analysis does reveal a clear logic. In particular, a range of participants described feelings of social anxiety, which were partly alleviated by their NCS experience. This finding is again supported by the exploratory quantitative analysis, which estimates another substantial ATT (equivalent to -5pp) on participants' level of anxiety. Participants interviewed associated political activities with social interaction and felt more able to participate when their sense of social self-efficacy was increased. These results are also supported by the latest experimental literature in this field. Recent work by Holbein and Hillygus links general noncognitive development programmes (not specifically service learning) to electoral turnout, and suggests that generalised self-efficacy, empathy and emotional control are key mediators (2020, p.107). These result makes further sense when we relate them to the literature on adolescent development, which shows that young people are particularly sensitive to the social world; their sense of self-worth is more closely tied to the opinions of others and they fear social exclusion more than adults do (Blakemore 2019, pp.36-39). The findings on social and communication skills and self-efficacy also

offer a new spin on the idea that increased social integration leads to more political participation (Putnam 1995; Putnam 2001; MacFarland & Thomas 2006).

7.3 Limitations

All three studies in this thesis were well-executed, in the sense that the sampling and data collection strategies worked as intended. So, there are no limitations in the findings that arise from issues of research implementation. There are, however, limitations that are inherent in the designs of each study and, while Chapter 3 of this thesis argues that the mixing of methods partly accounts for these limitations, they warrant some discussion here.

Study 1 has two main limitations. First, it was not possible to randomly assign participants into the intervention and comparison groups. This means that it is possible that the estimated effects are biased due to unobserved confounding factors. The design attempts to address this issue through a combination of matching and a DiD analysis. We can be confident that this has reduced some bias in the estimates, because the DiD estimator accounts for all time invariant differences between groups, and the variables used for matching are all predictors of the take up and the outcome (see the discussion on selection mechanisms in section 4.6.1, and the process used for refining the propensity score model in section 4.6.3.1). However, some evidence of potential upward bias is identified in the testing of the unconfoundedness assumption (section 4.7.3) and, in the absence of trend data prior to baseline, it is impossible rule out plausible time varying confounders like participant motivation (which could be changing at a faster rate in the treatment group). Both of these facts suggest that the effects presented could be over-estimates, but there is no way of confirming or quantifying this. Second, a lot of researcher choice was involved in the approach to matching and analysis. To mitigate against the risk that these choices have influenced the results, a pre-analysis plan (PAP) was registered prior to receiving the data, specifying in detail the approach that would be taken (Taylor 2020a). This plan was closely followed, with all (minor) deviations reported and justified (Appendix III). Further to this, a wide range of robustness checks have been carried out to test whether the reported results are sensitive to the choices

made (Annex I to Chapter 4). These checks provide strong support for the findings from the primary analysis.

Study 2 attempts to make inferences about causal mechanisms (and moderators). It does this by using participant interviews that aim to access interviewees' independently held attitudes and beliefs. This method is limited in two ways. First, the interviewer has an unavoidable effect on the data that is generated (Holstein & Gubrium 2011). This effect was mitigated against by the way that the interviews were structured and executed (as argued in section 5.6). However, it was not assumed that interviewees consciously held all of these attitudes and beliefs prior to interview. Indeed, one of the aims of the interviews was to encourage new intrapersonal reflections. Second, where ideational process tracing with elites can often draw on a range of datatypes, the inferences drawn in Study 2 are almost exclusively based on interview data. The quality of this data is limited by interviewees' ability to recall the relevant experiences, accurately reflect on the effect of these experiences on their subsequent attitudes and behaviour, and to be honest with the interviewer. Again, this issue was partly addressed in the structure and execution of the interviews, but also in the approach to analysis, which uses a clear and explicit framework for making inferences that assesses the uniqueness of, and certainty in, the explanations given. So, the extent to which we have confidence in the findings from Study 2 depends strongly on the extent to which we have confidence in the quality of the interview process, and in the robustness of the analysis. To help readers assess these two things, the design was specified in detail in a PAP (Taylor 2020b), which is not the norm in qualitative research, and the coding framework and transcripts have been published in full (see appendices X and XI).

Study 3 was limited by the fact that it had no manipulation check. This makes the null results harder to interpret. The results are consistent with two possible conclusions: failure of the theories, or a failure of implementing the theories. Unfortunately, due to the time lag between administering the interventions and measuring the primary outcome, it was not feasible to conduct a manipulation check (such as a post-intervention survey) that would have helped with the interpretation of the results. A survey administered after the primary outcome was measured would have been completed weeks after the emails were first read. This delay would have resulted in very low response-rates and, for those who

did respond, a poor measure of the hypothesised mechanism. A survey administered directly after the emails were first read, and prior to measurement of the primary outcome, would likely have influenced the outcome itself. At the very least, it would have been impossible to say whether any observed effects were a result of the interventions or the survey. NCS Trust was also reluctant to introduce extra friction into the process that may have reduced overall response rates to the competition. The interpretation of the results from this study is therefore reliant on relating them to the existing literature and the other two studies in this thesis. This discussion does help to assess the plausibility of the range of possible explanations, but it remains difficult to say what the true explanation is for the null effects.

7.4 Possibilities for future research

7.4.1 Unanswered questions from this thesis

This thesis has fallen short of achieving two of its aims. First, while a good theory has been produced in Chapter 5, it is still lacking in detail in terms of the primary outcome. At the moment, the model groups all forms of political participation into one. In doing so, it is unable to say whether the identified mechanisms apply equally to all forms of participation. Study 1 suggests that there may be bigger effects on some forms over others, but data collected in this thesis has been unable to explain these differences. Future research could investigate whether there are particular mechanisms that relate to certain forms of political participation. Second, the third research question in the thesis – how best to encourage political participation post-service – remains open. While Study 3 produced some ideas about what *not* to do, it has not told us much about what might work best. More research is needed on this, testing new theories, and new modes of communication.

7.4.2 New questions

At least four new questions come out of the thesis. First, Study 2 has identified a long list of complex moderating factors. Some of these could be tested for quantitatively in a new experiment. This experiment would require a dataset that contained the appropriate variables (matching the identified moderators), and a sample that is large enough to detect heterogenous effects. The obvious place to start here would be in testing whether service learning is more effective (*vis*

political participation) for participants with higher levels of political interest and motivation at baseline (the key moderator identified for the social self-efficacy mechanism). Second, Study 1 has estimated effects for one specific programme – NCS – and it seems as if the key activities that led to these effects were ones that are particular to *informal* service learning of this type. NCS begins with a wide range of non-civic activities (outward bound, independent living, and skills-building workshops) that support personal development and capability-building, and give mastery experiences whose lessons can be applied later in the programme to the civic domain. It also engineers cohorts to be demographically diverse, and structures itself around group-based activities that encourage intense social mixing. Study 2's findings suggest that these characteristics are central to triggering the mechanisms that lead to the observed effects on political participation. A follow-up study might ask whether these effects are observed in formal educational programmes of service learning (those attached to curricula in schools and universities), that are unlikely to share all of these features. Attempts have been made to answer this question before but, as Chapter 2 has argued, these attempts have lacked appropriate designs. This new research could apply a similar design to Study 1 or, if random assignment is feasible, use an RCT. Third, the exploratory analysis in Study 2 involved the creation of a new composite scale to measure 'social self-efficacy', a construct identified in the qualitative analysis. Future research could examine the construct validity of this scale.

Fourth, now that we know that service learning can have a positive effect, the obvious follow-up question from the policy and practical perspectives is, 'What can be done to maximise this effect?'. Answering this question could involve testing variants of NCS against each other. While participants cannot be randomly assigned into NCS as a whole (hence the quasi-experimental design of Study 1), they could be randomised into different versions of the programme, so there is the potential for RCTs to be used to answer this new question. The findings from Study 2 suggest that NCS may benefit from two additions: i. the inclusion of a political mastery experience that increases participants' political self-efficacy (bypassing the need for a spillover effect from the non-political domain); and ii. the addition of activities that support participants to reflect on – and understand – the systemic causes of the issues that they are addressing, and the role of government in this context.

7.5 Conclusion

This thesis has drawn two sets of results that are new to the academic literature, and useful for policy and practice. First, it has shown that service learning can have a substantial effect on young people's political participation. It can increase the chances that they will vote when they are old enough, and it can lead to greater participation in non-electoral influencing activities in the shorter term. These latter effects might be quite large. Effects of 5.4pp for petition-signing, 4.9pp for protest attendance and 2.3pp for contacting politicians are substantial when we consider that only 25% of 16- to 25-year-olds in England are estimated to take part in at least one of these activities in a year (Cabinet Office 2016). If the estimated average effects of NCS on non-electoral political participation were realised in the wider English population of 16-25-year-olds, it would make them the second-highest participating age group (as opposed to the second lowest as they are currently). Second, a detailed explanation of how these effects are achieved has been developed. To create this new knowledge, a mixed-methods approach has been taken, and this mixing of methods has strengthened the inferences made. A large amount of new data has also been collected; some of which has now been placed in the public domain for other researchers to use.

Knowing that these positive effects can be induced, and understanding how they are created, is important. Young people are underrepresented in politics, and they may suffer materially because of this. A lot of resources have been invested in service learning to help address this issue but, until now, we did not know whether it could work. Now that we know that it can, there is a strong argument for more resources to be put in, so that more young people have the chance to benefit, and so that we can assess whether population-level effects can be achieved.

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9 Appendices

9.1 Appendix I: Baseline survey for Study 1

The baseline survey is available to download from the OSF project page for the thesis: https://osf.io/23k4n/.

9.2 Appendix II: Follow-up survey for Study 1

The follow-up survey is available to download from the OSF project page for the thesis: https://osf.io/23k4n/.

9.3 Appendix III: Summary of deviations from the Study 1 PAP

Estimating effects: The PAP specified that effects would be estimated by using a regression model after the matched sample had been created. Unfortunately, the Stata command used to estimate effects (`teffects psmatch`) does not allow for the inclusion of covariates in the second stage regression. The alternative to using `teffects psmatch` would have been to manually programme the specified matching and effects estimation. Taking this alternative approach, while estimating the standard errors correctly, is operationally very difficult, and was considered too resource intensive for the relatively small benefit of having covariates in the second stage regression. In the event, all effects were fairly precisely estimated, so this does not seem to be a big issue.

Region variable: The PAP specified that the dataset would contain a variable indicating the region of the country in which the participant was eligible for NCS. In the event, this variable was not present in the dataset so could not be used. However, this is a time invariant factor, so would be accounted for in the DiD analysis.

Balance checks: The PAP only specified that covariate balance would be checked after matching. Instead, balance was also assessed before matching. These additional checks were added to increase the robustness of the selection process for the propensity score model.

Use of untreated units: The PAP specified that untreated units would be used a maximum of two times for matching. Instead, this cap was not applied, but weights were applied in the effect estimation procedure to account for the number of times that an untreated unit was used. This was necessary due to the distribution of propensity scores across the two groups and is standard practice in nearest neighbour matching.

Assessing the unconfoundedness assumption: The PAP specified that the baseline variable for the primary outcome would be used as the pseudo-outcome for this test. However, components of this composite variable were used in the propensity score model, so could not be used in this test as a pseudo-outcome

as well. Instead, the baseline values of the service participation outcomes are used (with multiple variables being used to increase the robustness of the test).

Post-match balance checks: The PAP specified that these would be carried out through significance tests. However, this is not best practice, and not what has been done in the other experiment in this thesis, so normalised differences are compared instead. Standard errors and significance stars are presented in this analysis for completeness.

Exploratory analysis: The PAP did not specify that effects would be estimated for each individual type of service-based participation. These effects were estimated for completeness, but were fairly uninformative, as the categories of participation were not well-delineated in the survey.

Additional robustness checks: Three additional robustness checks – that were not specified in the PAP – are carried out for added security of seeing the data. First, extreme values of the propensity are trimmed. Second, the online panel sample is excluded. Third, for individuals who replied 'don't know/prefer not to say' to an outcome variable, zeros are imputed in the primary analysis, and an indicator for this is added to the dataset. As a robustness, these individuals are dropped from the analysis.

Amended robustness checks: The procedure for correcting for multiple comparisons specified in the PAP, suggested that these corrections would only be applied if *four or more* comparisons were made within a group (e.g. primary or secondary analysis). This was an error. The Benjamini-Hochberg procedure specifies that corrections should be made when *two or more* comparisons are made within a group. In this thesis, the Benjamini-Hochberg procedure is applied correctly.

9.4 Appendix IV: Study 1 survey item amendments

Table 9.1, summarises the changes made to the validated survey items used in this study, to enable them to fit into the NCS Impact Survey.

Table 9.1: Summary of survey item amendments

Wording of original item	Wording of amended item	Source of original
Collecting signatures for a petition	Collected signature for a petition	ICCS
Contacting a politician	Contacted a politician (e.g. an MP or councillor)	ICCS
Taking part in a peaceful march or rally	Attended a public meeting or rally, taken part in a public demonstration or protest	ICCS
Do you think that you could take an active role in a group involved with political issues?	How able do you think you are to take an active role in a group involved with political issues?	ESS
How much would you say the political system in the UK allows people like you to have a say about what the government does?	How much would you say that the political system in the UK allows people like you to have a say in what the government does?	ESS
And how much would you say that the political system in the UK allows people like you to have a direct influence on politics?	And how much would you say that the political system in the UK allows people like you to have an influence on politics?	ESS

Notes: ESS = European Social Survey. ICCS = International Civic and Citizenship Education Study.

9.5 Appendix V: Study 1 analysis code and log

The analysis code and log is available to download from the OSF project page for the thesis: https://osf.io/23k4n/.

9.6 Appendix VI: Study 2 screener survey

Please complete the survey below.

Filling out the survey should take about 2 minutes.

If you complete the survey, you will be automatically entered into a prize draw with the chance to win £200 of Amazon vouchers.

Please make sure that you read the information sent to you by email about this survey before completing it.

Thank you.

Gender

Which of the following describes how you think of yourself?

> Male
>
> Female
>
> In another way (other specify)
>
> Prefer not to say

DOB

What is your date of birth?

> Date of birth

Ethnicity

What is your ethnic group?

White (including English, Welsh, Scottish, Northern Irish, Irish, Gypsy or Irish Traveller or any other White background)

Black (including Black British, African, Caribbean and any other Black background)

Asian (including Asian British, Pakistani, Bangladeshi, Chinese and any other Asian background)

Mixed (including White and Black Caribbean, White and Black African, White and Asian, any other Mixed or Multiple ethnic groups)

Don't know

Prefer not to say

FSM

Have you been eligible for Free School Meals at any point in the last year?

Yes

No

Don't know

Prefer not to say

UCL

Are you a student at University College London (UCL)?

Yes

No

Prefer not to say

Ways given help

Have you given your time to help in <u>any</u> of the following ways <u>outside of school or college hours</u> since your summer NCS experience (i.e. after summer 2019)? Please select all that apply.

Organised a petition or event to support a local or national issue

Attended a public meeting, rally, or taken part in a public demonstration or protest

Signed a paper petition or an online/e-petition

Contacted a politician (e.g. an MP or a councilor)

None of these

Don't know *Fixed *Exclusive*

Prefer not to say *Fixed *Exclusive*

9.7 Appendix VII: Screener survey invitation email

Dear [first_name],

I'm writing to invite you to take part in some research that I'm doing about the National Citizen Service (NCS). This is a University College London (UCL) research project, funded by the NCS Trust.

At the end of NCS last year, you completed a survey and agreed to being re-contacted by researchers working with NCS Trust. This is why I am contacting you today.

This study is looking at what influences young peoples' engagement with politics. It involves completing a short survey and, if selected, you will be invited to take part in a 1 hour telephone interview about your NCS experience and your views on politics.

Filling out the survey should take about 2 minutes, and all survey respondents will be entered into a prize draw with the chance to win £200 of Amazon vouchers. If you're then selected for an interview, you will also be given £35 in Amazon vouchers as a thank you for taking part.

The deadline for filling out the survey and entering the prize draw is midnight on Sunday 23rd February 2020.

See below for more information about the study and feel free to email me if you have any questions.

If you want to take part, click on the link below to take the survey: [survey-link]

If the link above doesn't work, try copying the link below into your web browser: [survey-url]

This link is unique to you and shouldn't be forwarded to others.

Thanks for taking the time to read this.

Patrick Taylor

Doctoral Researcher, University College London

Study Information

UCL Research Ethics Committee Approval ID Number: 11259/004

Title of Study: The National Citizen Service and Democratic Participation

Department: University College London, Department of Political Science, School of Public Policy

Researcher: Patrick Taylor, patrick.taylor.16@ucl.ac.uk

Principal Researcher: Dr Lucy Barnes, l.barnes@ucl.ac.uk

You are being invited to take part in a PhD research project. Before you decide, it is important for you to understand why the research is being done and what participation will involve. Please take time to read the following information carefully and discuss it with others if you wish. If you would like more information please direct your queries to UCL rather than NCS Trust. The best person to contact with questions in the first instance is Patrick Taylor at UCL (patrick.taylor.16@ucl.ac.uk).

1.   What is the project's purpose?

This study is funded by the National Citizen Service (NCS) Trust and aims to understand what influences young peoples' engagement with politics.

2.   Why have I been chosen?

You have been chosen because you are a graduate of NCS and you have previously given your consent to be contacted for research purposes. You gave this permission when you completed a survey at the end of NCS last year. This survey was run by independent researchers, Kantar, who have now shared your details with us.

3. Do I have to take part?

It is up to you to decide whether or not to take part. If you do decide to take part, you can withdraw at any time without giving a reason and this won't disadvantage you in any way.

4. What will happen to me if I take part?

If you decide to take part, you need to click the hyperlink in my email above to complete a short survey to register your interest. After the survey closing date (midnight on Friday 23rd February 2020), we will select about 30 survey respondents, and these people will be invited to take part in a follow-up interview. If you are selected for this, you will be sent another email with more information, shortly after the survey closing date. At this point, you can decide whether or not you wish to take part in an interview. Completing the survey does not commit you to taking part in an interview.

5. What are the possible benefits of taking part?

As a thank you for filling out the survey, you'll be entered into a prize draw to win £200 of Amazon vouchers. If you're then selected for an interview, you will also be given £35 in Amazon vouchers as a thank you for taking part.

6. What if something goes wrong?

This is a UCL research project. If you have any concerns or complaints about the research, you can contact the Principal Researcher (Dr Lucy Barnes), using the details above. If you would like to contact someone at NCS Trust about the research, you can contact Victoria Harkness (Head of Research and Evaluation) at victoriaharkness@ncstrust.org.uk. If you feel your complaint has not been handled properly, you can also contact the Chair of the UCL Research Ethics Committee at ethics@ucl.ac.uk.

7. Will my taking part in this project be kept confidential?

All the information that we collect about you in the registration survey will be kept strictly confidential. You will not be able to be identified in any reports or publications from this research.

8. What will happen to the information I give you in the survey?

The information that you give in the survey will be used to select people for interviews. There are no right or wrong answers – we just want to speak to people from as wide a range of backgrounds as possible and will use your responses to help us do this. All of your personal data will be stored securely and deleted 3 months after the end of the research (currently estimated to be 31st December 2021).

9. Local Data Protection Privacy Notice

The controller for this project will be University College London (UCL). The UCL Data Protection Officer provides oversight of UCL activities involving the processing of personal data, and can be contacted at data-protection@ucl.ac.uk. This 'local' privacy notice sets out the information that applies to this particular study. Further information on how UCL uses participant information can be found in our 'general' privacy notice, which you can read here: https://www.ucl.ac.uk/legal-services/privacy/ucl-general-research-participant-privacy-notice. The information that is required to be provided to participants under data protection legislation (GDPR and DPA 2018) is provided across both the 'local' and 'general' privacy notices.

The categories of personal data used will be as follows: name, email address, mobile telephone number, gender, ethnicity and free school meal status. The lawful basis that would be used to process your *personal data* will be the performance of a task in the public interest. The lawful basis used to process *special category personal data* will be for scientific research.

<i>Your personal data will be processed so long as it is required for the research project.</i> If we are able to anonymise the personal data you provide we will undertake this, and we will minimise the processing of personal data wherever possible. If you are concerned about how your personal data is being processed, or if you would like to contact us about your rights, please contact UCL in the first instance at data-protection@ucl.ac.uk.

10. Who is organising and funding the research?

This research is organised by researchers at UCL and funded by the NCS Trust.

Thank you for reading this information sheet and for considering taking part in this research study.

Prize draw terms and conditions

1. Promoter

This free prize draw for The National Citizen Service ("NCS") and Democratic Participation research (the "Prize Draw") is being organised by University College London ("UCL").

2. Eligibility

This prize draw is open to any NCS participant who receives an invitation from UCL to take part in the screener survey for this research (the "Survey") and who participates by completing the online survey.

3. Promotion period

Entries will be accepted during the survey period which starts on Friday 7th February 2020. The closing date for entries is midnight on Sunday 23rd February 2020 (the "Closing Date"). UCL reserves the right to extend the survey period.

4. Entry

Entry into the Prize Draw is initiated by clicking the 'Submit' button found at the end of the Survey. No purchase or payment is required to enter the Prize Draw.

5. Limitation of entries

Only one Survey and therefore entry per eligible participant will be accepted into the Prize Draw. Automated Prize Draw entries, bulk Prize Draw entries or third party Prize Draw entries will be disqualified.

6. Prizes

There is one prize available: a £200 Amazon voucher. UCL reserves the right to substitute prizes of equal or greater value.

7. Draw

Prize Draw winners will be selected at random and drawn from eligible Survey entries. The first eligible participant selected in the Prize Draw will receive £200 worth of Amazon vouchers. The draw will be conducted within one month of the Closing Date. Notwithstanding the foregoing, UCL reserves the right to postpone the Prize Draw until a later date should the survey period be extended beyond the Closing Date. All decisions made by UCL are final and binding.

8. Notification

Prize Draw winners will be notified within six weeks of the Closing Date (unless the research survey period is extended beyond the Closing Date as described above). Notification will only be made to Prize Draw winners who will be contacted by the email address used to invite you to participate in this research. Prizes will be administered by UCL.

UCL will make all reasonable efforts to contact the Prize Draw winner. If the Prize Draw winner does not respond within 4 weeks of being contacted, UCL reserves the right to offer the prize to the next eligible entrant selected at random from the Survey entries that were received before the Closing Date. UCL does not accept any responsibility if a Prize Draw winner is not able to take up the prize.

9. Use of personal information for this prize draw

UCL will use your name and contact details for the purpose of contacting you should you win the prize. Personal data used for the purposes of the Prize Draw will not be given to any third party. All personal data shall be processed by UCL in line with their privacy policy which can be found, via the following link: https://www.ucl.ac.uk/legal-services/privacy/ucl-general-research-participant-privacy-notice.

10. Liability

UCL is not responsible for late, lost, misdirected, mechanically reproduced, mis-delivered, incomplete, illegible, or unintelligible Survey entries, messages or post; unavailable network connections; failed, incomplete, garbled, or delayed computer transmission; online failures; hardware/software or other technical malfunctions or disturbances; or any other communications failures or circumstances affecting, disrupting or corrupting the Prize Draw in any manner.

UCL is not responsible for any inaccuracy in the contact details supplied by the participant that may result in the inability of UCL to contact that participant. UCL is also not responsible for any damage to the entrant's computer occasioned by participation in the Survey or the Prize Draw.

11. Jurisdiction

These rules will be governed by English law and the exclusive jurisdiction of the English courts.

9.8 Appendix VIII: Interview invitation email

Dear [First Name],

Thank you for registering your interest in this research by completing the survey I sent you a few weeks ago. I'd now like to invite you to take part in a telephone interview (or using an online alternative like Skype) about your experiences of the National Citizen Service (NCS) and your views on political participation (e.g. voting).

If you agree to take part, the interview would last for about 60 minutes.

As a thank you for your time, you'll receive a £35 Amazon voucher.

See below for more information about the study and feel free to email me if you have any questions.

If you'd like to take part, just reply to this email to let me know and I'll be in touch to set up a time for a call.

Best wishes,

Patrick Taylor
Doctoral Researcher, University College London

Study Information

UCL Research Ethics Committee Approval ID Number: 11259/004

Title of Study: The National Citizen Service and Democratic Participation

Department: University College London, Department of Political Science, School of Public Policy

Researcher: Patrick Taylor, patrick.taylor.16@ucl.ac.uk

Principal Researcher: Dr Lucy Barnes, l.barnes@ucl.ac.uk

You are being invited to take part in a PhD research project. Before you decide, it is important for you to understand why the research is being done and what participation will involve. Please take time to read the following information carefully and discuss it with others if you wish. Ask if there is anything that is not clear or if you would like more information.

1.   What is the project's purpose?
This study is funded by the National Citizen Service (NCS) Trust and aims to understand what influences young peoples' engagement with politics.

2.   Why have I been chosen?
You have been chosen because you are a graduate of NCS and you have expressed an interest by completing the survey that I sent you. I'm hoping to speak to about 30 people who have graduated from NCS, to get a range of views.

3.   Do I have to take part?
It is up to you to decide whether or not to take part. You can withdraw at any time without giving a reason and this won't disadvantage you in any way.

4.   What will happen to me if I take part?
If you decide to take part, you will be interviewed for about 60 minutes on the telephone (or using an online alternative like Skype) about your experiences of NCS and your views on political participation (e.g. voting). After the interview, you may be invited to a second interview to follow up on some of the discussion. If you are invited for a second interview, it will be up to you to decide if you want to do it. If you decide afterwards that you do not want the interview(s) to be used in the research, you can let the researcher know within two months of your last interview and the recording(s) will be deleted and not used.

5.   Will I be recorded and how will the recorded media be used?
An audio recording will be made of your interview(s). This recording will be used only for analysis for the research. No other use will be made of it without your written permission, and no one outside the project will be allowed access to the original recordings. The recording will be transcribed and anonymised (so

you will not be identifiable from it) and this 'redacted' version of the interview will be made publicly available to other researchers.

6.   What are the possible disadvantages and risks of taking part?

We don't think that there are any particular disadvantages or risks of taking part in this study. However, if you want to discuss any issues after the interview with someone who will listen, then you might want to go to: https://www.themix.org.uk/.

7.   What are the possible benefits of taking part?

As a thank you for your time, you'll receive a £35 Amazon voucher for your participation. If you're invited for a second interview, you'll receive a £20 Amazon voucher (as this will be a shorter, 30 minute, conversation). We also hope you will find the interview interesting and useful as a way of reflecting upon some of your NCS experiences.

8.   What if something goes wrong?

If you have any concerns or complaints about the research, you can contact the Principal Researcher (Dr Lucy Barnes), using the details above. If you would like to contact someone at NCS about the research, you can contact Victoria Harkness (Head of Research and Evaluation) at victoriaharkness@ncstrust.org.uk. If you feel your complaint has not been handled properly, you can also contact the Chair of the UCL Research Ethics Committee at ethics@ucl.ac.uk.

9.   Will my taking part in this project be kept confidential?

All the information that we collect about you during the course of the research will be kept strictly confidential. You will not be able to be identified in any reports or publications from this research.

10. Limits to confidentiality

Please note that confidentiality will be maintained as far as it is possible. If during our conversation I hear anything which makes me worried that someone might be in danger of harm, I might have to inform relevant agencies of this. Also, an external agency will be used to transcribe the audio recording of your interview,

but this agency will not have access to your other personal data. This agency has been vetted by UCL and a clear contract is in place to protect your data.

11. What will happen to the information I give you in the interview(s)?

The results of this research will be published as part of a PhD thesis and will be used by NCS Trust to help improve the programme for future years. If you'd like to receive a copy of this thesis, please email Patrick Taylor (details above). All of your personal data will be stored securely and deleted 3 months after the end of the research (currently estimated to be 31st December 2021). An anonymised, written version of your interview will be made available to other researchers via UCL's website at the end of the study.

12. Local Data Protection Privacy Notice

The controller for this project will be University College London (UCL). The UCL Data Protection Officer provides oversight of UCL activities involving the processing of personal data, and can be contacted at data-protection@ucl.ac.uk. This 'local' privacy notice sets out the information that applies to this particular study. Further information on how UCL uses participant information can be found in our 'general' privacy notice, which you can read by clicking here. The information that is required to be provided to participants under data protection legislation (GDPR and DPA 2018) is provided across both the 'local' and 'general' privacy notices.

The categories of personal data used will be as follows: name, email address, mobile telephone number, gender, ethnicity and free school meal status. The lawful basis that would be used to process your *personal data* will be the performance of a task in the public interest. The lawful basis used to process *special category personal data* will be for scientific research.

Your personal data will be processed so long as it is required for the research project. If we are able to anonymise the personal data you provide we will undertake this, and we will minimise the processing of personal data wherever possible. If you are concerned about how your personal data is being processed, or if you would like to contact us about your rights, please contact UCL in the first instance at data-protection@ucl.ac.uk.

13. Who is organising and funding the research?

This research is organized by researchers at UCL and funded by the NCS Trust.

9.9 Appendix IX: Interview guide

Topic	Purpose	Approx. time (mins)
Introductions and background	• Make sure the participant understands what the research involves • To get verbal consent	5
Participant introduction	• Help the participant feel at ease • Build rapport • Begin to identify basic contextual information	5
Current interests in political participation	• Establish participant's political interest and motivation	15
Interviewee-led questions: tracing development of political interest and motivation (or lack thereof)	• Get participant's explanation for development of her/his political interest and motivation	15
Theory-led questions: testing the logic model and role of NCS	• Assess plausibility of initial theory against participant's experiences	15
Wrapping up	• Capture any thoughts the participant has not had a chance to express • Get consent for potential follow-up interview	5

Introductions and background

Intro to me:

- Paddy – researcher at UCL
- Working with NCS Trust as part of a PhD project

Information sheet:

- Did you read the information about the study in my email?
- Did you have any questions?
- I want to run through the important bits anyway
- Important that you understand what the research involves

You do not have to do the interview:

- If we get to the end of the info and you decide not to take part, that's fine.
- *For UCL students only: You are under no special obligation to participate because you are a UCL student.*

Aim of this research: What influences young peoples' engagement with politics?

You have been chosen because:

- You've done NCS
- You consented to being contacted
- Interviewing about 30 people to get a range of views

The interview:

- Approx. 60 mins
- Topics we'll cover:
  o Your views on democracy and political participation
  o How you think you developed these views
  o Your experience of NCS
- No right or wrong answers – just interested in your opinions
- If you want to skip any questions, just say so and we can – you still get your voucher
- If you decide afterwards that you don't want me to use the interview, let me know within 2 months of today and I'll delete it and not use it

What I'll do with the results:

- PhD thesis - eventually available from UCL library and open access website
- Transcript made available in UCL's Digital Collections repository, with Open Access

Anonymity and privacy:

- The transcript of this interview will be anonymised before putting in the Digital Collection
- Won't be identifiable in any reports

Would you like me to send you a copy of the research when it is complete?

Do you have any questions?

Recording:

- Would like to take an audio recording of the interview and take notes
- Is that okay?

If yes, begin recording

Can you please say your full name?

And can you confirm that you're happy to take part in the interview as I've described it to you?

Participant introduction

Where do you live?

What's it like round there?

What are you up to at the moment? (6th form, college, work?)

How's it going? Missing school / glad to be doing something new?

What are your plans for after? (Uni, work…?)

How did you find NCS?

Current interests in and understanding of political participation

Issue interest:

- What issue did you tackle for you NCS project?
- Was that something that you were particularly passionate about?
    - Why?
    - Are there any other issues that you care about?

Type of civic participation:

- Have you ever done anything outside of NCS to tackle those issues? Probe:
    - Finding out about issue – e.g. online, reading news
    - Discussion
    - Action
- How did you get into that kind of thing?

Have you been following the news about climate change and the protests that have been happening?

- What do you think about those protests?

Have you been following the news about Brexit?

- What do you think about that?

What comes into your mind when I say 'politics'?

What does it mean to you to participate in politics? (What things can people do?)

There are four classic things that people can do if they want to take part in politics:

- Vote
- Write to their MP or councillor
- Petition
- Protest

I'd love to get your views on these four things.

Go through each activity in turn:

- What do you think about it? Probe:
  - Worthwhile?
- Have you done it? / Would you do it? Probe:
  - What makes you say that?

Interviewee-led questions: tracing development of political interest and motivation (or lack thereof)

I'm really interested to know how you've developed your views on politics, particularly those political activities we just discussed: voting, contacting your politician, petitioning, protest.

(Include other activities in this list if interviewee has identified them as political).

Early ideas (primary school age?)

Can you remember when you first became aware of politics? Probe:

- How old were you?
- Where were you (home, school, somewhere else)?
- How did you become aware of it (a person, something you read, something you saw on tele…)?
- If no specific moment, tell me roughly when and how…

Can you remember what you thought about politics at that age? Probe:

- Something you were interested in? Why?
- Something you felt able to participate in? Why?
- Something you wanted to participate in? Why?

Developing ideas (from then to now)

If early ideas are similar to those expressed at start of interview:

So your ideas have stayed fairly fixed about this over the years. Why do you think that is?

If early ideas are different to those expressed at beginning of interview:

So your ideas have developed quite a bit over the years. Why do think that is? Probe:

- What kind of things have influenced you do you think?
- Which experiences/influences have been most powerful in shaping your views?

Theory-led questions: testing the logic model and role of NCS

<u>Mechanisms</u>

Capability

Do you feel like you have the skills and knowledge that you need to participate in political activities? Probe:

- What things do you think you need?
- What things do you have?
- What things are you missing?

Does this make you more/less likely to participate? Probe:

- Why?
    - Directly? (i.e. Already motivated and feel need for a specific skill)
    - Increased motivation? (i.e. Having those skills makes me want to use them)

Opportunity

Do you feel like you have the opportunities that you need to participate in political activities? Probe:

- What things are you missing?
- Do you know the right people / organisations? / Do you have the right contacts?

Does this influence your participation? Probe:

- How?
- Can you give an example?

Motivation

Do you feel motivated to participate in political activities? Probe:

- What's behind your motivation / lack of motivation?

Pro-social responsibility:

- How did your social action project make you feel?
- Who do you think is responsible for solving social/environmental problems? Probe:
    - Do you feel any responsibility?
- What makes you feel like this?
- Where do you think this feeling has come from?

Political self-efficacy:

- Do you feel able to participate in political activities?
- Do you feel like your participation could make a difference?
- What makes you feel like this?

- Where do you think this feeling has come from?

Civic self-efficacy:

- Do you feel able to make a difference in non-political ways?
  - How?
- What makes you feel like this?
- Where do you think this feeling has come from?

Knowledge

Understanding of role of government:

- Do you feel like you understand the government's role in solving social and environmental problems?

Understanding of causes of issues:

- Do you feel like you understand the causes of the social/environmental issues that interest you? (Refer back to issues mentioned – e.g. NCS project)

Identity

Political identity:

- What kind of person gets actively involved in politics? Probe:
  - Describe them to me
  - Maybe think about people you know
- Do you consider yourself to be that kind of person?:
  - Why?
  - Why not?

NCS identity:

- Do you feel part of a group people that did NCS?
  - Is there anything that binds you together?
  - Or not really?
- What kind of person does NCS?
- Do you think that people feel like a different kind of person after doing NCS?
  - Any new beliefs?
  - Any new values?

Activities

Has NCS had an influence over any of these things? Probe:

- Skills and knowledge
- Opportunities
- Motivation
- Understanding of role of gov
- Understanding of causes of social problems

Which particular bits of NCS led to these changes? Probe:

- How did they lead to the changes you've described?
- Did you do any political activities during NCS?

Domain similarity/difference:

- How similar/different do you think your NCS social action project was from political action?
- What makes you say that?
- Has NCS changed the way you think about political action at all?

Did you do any political engagement activities during NCS (e.g. BtB workshop)?

<u>Moderators</u>

One of NCS's aims to encourage young people to participate more in political activities.

You've described the effect it has/hasn't had on you. Thinking about this…

- What kind of person do you think it would have a positive effect on?
- What kind of person do you think it wouldn't have an effect on?
- What does the NCS experience need to look like to create a positive effect?
- What kind of NCS experience would lead to no effect (or a negative effect)?

Wrapping up

Anything else you'd like to share?

Any questions?

Once I've completed the interviews and analysed people's responses, I might like to come back and ask a few more questions. Would it be okay for me to contact you again? ***This doesn't commit you to another interview.***

Theory tick list

<u>Mechanisms</u>

Capability

Skills:

- Planning/decision-making meetings
- Communication – verbal and written

Knowledge:

- Understanding of role of government
- Understanding of causes of social issues

Opportunity

Networks:

- People
- Organisations

Motivation

Pro-social responsibility

Political self-efficacy

Civic self-efficacy

Identity

<u>Activities</u>

Non-civic personal development

Non-pol social action

Guided reflection

<u>Moderators</u>

Participant characteristics

Context

9.10 Appendix X: Study 2 qualitative coding framework

The table below presents the full set of codes produced during the analysis of interview transcripts for Study 2. If a code has subcategories, then these are displayed as higher levels, to the right. For example, 'Public speaking' is a subcategory of 'Social interaction / communication', which is a subcategory of 'Capabilities', which is a subcategory of 'Mechanisms'. The constructs in the table were refined by reviewing their relation to each other, grouping them into conceptual categories where possible, and ensuring that they comprehensively cover the data, to support the findings presented in Chapter 5. Appendix XI presents the interview transcripts, with these codes applied, giving direct access to the excerpts that were coded with these constructs.

Level 1	Level 2	Level 3	Level 4	Level 5	Level 6
Mechanisms	Capabilities	Social interaction / communication	Public speaking		
			Not enough		
			Debating		
			With people with different views and backgrounds		
		Planning and organising	Positive		
		Teamwork	General		
			Collaborating to achieve a collective goal		
			Building positive relationships		
			Group decision-making	Positive	
		No effect in general			
	Motivation	Self-efficacy	Encouraging preference for non-pol participation	Not	
			Communication / social interaction	Increased but not enough	
				Helped by safe environment	
				Exposure to different types of people	
				Pushed out of comfort zone	
			Civic	Negative effect	
				No effect	
				Positive effect	And explicit description of knock-on to pol eff
					But no knock-on to pol eff

	General	Increased but not enough		
		Linked to outward bound		
		Negative effect		
	Political	No effect	Ceiling effect	
			Domains perceived as different	
			General no	
		Positive effect	Linked to civic self-efficacy	
			Opportunity to engage disengaged peers	
	Pro-social responsibility	No effect	Ceiling effect	
		Positive effect		
	Self-esteem	No effect		
		Positive effect	Positive feedback from social action	
			Treated equally by youth workers	
Networks of recruitment	No effect	Peers		
	Not perceived as important			
	Positive effect	Organisations		
		Peers		
Knowledge	Issue awareness	Positive effect		
	How government works	No effect		
		Positive effect		
		Some effect but not helping with polpart		
	Causes of issues	No effect		
	Ways a citizen can participate	No effect		
Identity	NCS grad	Not		
	Attachment to group			
	Marginalised youth			
	Active citizen			
	No effect			
	Powerful youth			
	Youth			
Moderators	Civic education / knowledge	Moderate		
		Lack of		
		Linked to religious community		
		High		
		Not effective / poot quality		
		Not enough		
		Simulated democracy		
	Baseline maturity	Life experience		
		Lack of		
		Level of critical thinking	High	
		Baseline openness to other views		
		Outward looking		
		Taking studies seriously		

	Baseline self-efficacy	General	High	But not enough	
			High	Helping to get over perceived difficulty of polpart	
			Low		
			Service learning not need for capabilities or networks		
		Political	External	Low but still motivated	
			External	Low	
			External	High	
			External	Moderate	
			Varies by participation type		
			Internal	Moderate	
			Internal	Low	
			Internal	High	
			Internal	Varies by participation type	
			Lack of		
		Social interaction / communication	High	But not enough	
			Low		
	Baseline sense of social responsibility / justice	Preventing protest			
		Lack of			
		High			
		Respect for the law			
	Baseline capabilities	Communication / social interaction			
		Leadership			
	General enjoyment of experience	Negative			
		Neutral			
		Positive			
	Main motivation for taking part	Fun			
		Social action	Lack of interest		
			Positive		
		Socialising			
	Quality of relationships with peers and staff on prog				
Fixed participant characteristics	Gender				
	Ethnicity				
	Age				
	Socio-economic status				
Description of NCS social action	Missed that part of prog				
Description of pol engagement on NCS					
Alternative political outcomes	Becoming more locally focussed				
	Improving quality (not quant) of pol participation				
Other factors effecting	Political socialisation	Media			
		Friends / peers	Not enough		

political participation			
	Lack of		
	Literature		
	Family	Negative	
		Moderate	
		Lack of	
		Positive	
	Pol active family and wider network not enough		
	Pol active friends not enough		
	Religious / cultural group		
	Teachers		
	Feeling represented by a political party		
	Other commitments / priorities at this stage of life	Studies	
		Sport	
		Socialising / fun	
		Work	
	Perceived effort		
	Baseline networks of recruitment	Linked to large urban centres	
		Lack of	
		Linked to college / school	
		Linked to community centres	
		Linked to family	
		Linked to friends	
		Linked to religious group	
		Linked to student climate movement	
		Linked to youth groups	
		Not enough	
		Perceived as difficult to engage with	
	Political trust	Lack of	
		Some	
	Previous experience of civic participation	Lack of	
		Non-pol	
		Pol	
	Sense of identity / belonging	Politics for old white people	But overcame this
		Politically active	
		Politicians vs. activists	
		NCS	
		Politics for those affected	
		Youth	
	Baseline political interest and motivation	Like to social status quo	
		High interest in politics but less in participation	
		Interest insufficient	
		Issue interest / awareness	Linked to personal experience
			Important

	Lack of interest
	High public salience
	Linked to personal passion
	Motivating when external efficacy low
	Nothing feels urgent / important enough
Driven by supporting friends (not the issue)	
Not put off by complexity of politics	
Personal passion for activism / democracy	
Politics irrelevant	

9.11 Appendix XI: Study 2 transcripts

All anonymised transcripts are available to download from the OSF project page for the thesis: https://osf.io/23k4n/.

Two versions of each transcript are available; one that shows the application of the coding framework, and one that is clean.

Transcripts are named by the pseudonym used for each interviewee.

9.12 Appendix XII: Study 2 exploratory quantitative analysis code and log

The analysis code and log are available to download from the OSF project page for the thesis: https://osf.io/23k4n/.

9.13 Appendix XIII: Study 3 interventions

9.13.1 Intervention 1 (self-efficacy)

9.13.1.1 Invitation email

OUR MAKE A CHANGE COMPETITION IS ON!

Hi Aabid,

Through NCS, you proved that you can make a difference in your community. You have the skills and you've seen that change can happen. Now you can make a difference by having your say with the government.

The challenge
We're asking NCS grads to write a short letter to the government with an idea they think could help tackle the growing issue of loneliness amongst young people in our society, particularly during this time of isolation.

The winning prize
From the letters we receive, we'll pick a winner, plus three runners up, to meet with a government minister in London to discuss your proposals. Now there's something you don't get to do every day! The best of the rest will be published online.

For the full challenge and to find out how to enter, click below (the link's unique to you, so please don't share it with anybody else).

FIND OUT MORE

View this email in your browser

OUR MAKE A CHANGE COMPETITION: TWO WEEKS TO GO!

Hi Aabid,

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FIND OUT MORE

Privacy notice

You have been sent this email using the contact details you sent us when you signed up to NCS.

We take your privacy and data security very seriously. For more information on how your data will be used, take a look at the competition terms and conditions.

Your anonymous data may also be used by researchers to improve our communications.

Privacy statement | Unsubscribe | wearencs.com

Copyright © 2020 NCS
National Citizen Service Trust is a not-for-profit organisation incorporated by Royal Charter and established to shape, support, champion and lead a thriving National Citizen Service.

National Citizen Service Trust is registered in England and Wales with Royal Charter Body number RC000894.
Our registered office is at NCS Trust, The Pembroke Building, Kensington Village, Avonmore Road London, Kensington Village, W14 8DG, UK

9.13.2 Intervention 2 (identity)

9.13.2.1 Invitation email

OUR MAKE A CHANGE COMPETITION IS ON!

Hi Aabid,

As an NCS grad you've already proven that you are a changemaker. Too often young people are ignored in politics and society. We want to change that and we need people like you to help.

The challenge
We're asking NCS grads to write a short letter to the government with an idea they think could help tackle the growing issue of loneliness amongst young people in our society, particularly during this time of isolation.

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View this email in your browser

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FIND OUT MORE

9.13.3 Intervention 3 (control)

9.13.3.1 Invitation email

NCS

OUR MAKE A CHANGE COMPETITION IS ON!

Hi Aabid,

We're writing to let you know about an exciting competition that we're running for graduates of NCS. Take a look at the details below and we hope you'll consider taking part.

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For the full challenge and to find out how to enter, click below (the link's unique to you, so please don't share it with anybody else).

FIND OUT MORE

9.14 Appendix XIV: Study 3 competition information

9.14.1 Information page

The Make a Change Competition

This is your chance to change the country for the better – and have your voice heard by the government.

Your challenge

Write a letter that answers the following question:

In its strategy for tackling loneliness, the government recognises that digital technology can be used to help tackle loneliness; which is particularly relevant during this time of isolation. However, it can also make the problem worse for some people.

In what ways do you think digital technology can contribute to loneliness amongst young people? What do you think can be done to change this, and what can the government do to help?

The prize –

Tackling loneliness is an important priority for the government, and we know they're keen to hear what NCS grads have to say.

The winner, along with three runners-up, will win a trip to London (all expenses paid, of course) to present their letter in person to a government minister. It's a fantastic opportunity to meet – and share your ideas with – those working at the heart of government to deliver change. You'll also get to visit the Houses of Parliament in Westminster!

The best of the rest will be published online by NCS Trust. And we plan to share your ideas with the Department for Digital, Culture, Media & Sport (DCMS) – one of the government departments responsible for their work on loneliness.

On the judging panel is a team of NCS graduates, NCS staff and officials working at the DCMS. All entries will be judged anonymously.

Here's what you need to know... –

We're all about making things fair and transparent so there are a few rules to follow. To make sure you're eligible, you must...

1. Submit your letter by 23:59 on Sunday 3rd May 2020.
2. Make sure your letter clearly addresses the question set out above. It's about making positive change for the better, not just a chance to rant!
3. Submit your letter only via the online competition form (your unique link below).
4. Stick to a maximum of 3,000 characters, including spaces (that's about one page of text). Anything over this may not be assessed by the judges.
5. Avoid putting your name – or anything that might identify you, or anyone else – in your entry. It's so we can judge them fairly. It's ok however to include any personal experiences. Don't worry, we can identify our winners from your unique link.
6. Use plain text (bold, italic and underline etc won't show up).
7. Avoid any offensive or abusive language.
8. Address your letter 'Dear Minister'. NCS is overseen by the Department for Digital, Culture, Media & Sport (DCMS). Find out more about the DCMS here.

Please do get in touch if you would like to enter the competition, but you feel that you may need additional support to submit a written letter.

Entering the competition means you've accepted our Terms and Conditions. Please make sure you click here to read them.

Don't forget....

This isn't an essay-writing competition – make it read well and check your spelling, but it's more about your ideas and experiences rather than fancy language. Be honest and be creative!

A lot's been written about the issue of loneliness and how it can be tackled. It'll be useful to read it, but remember we want to hear some original ideas! You can find the government's full strategy for tackling loneliness here.

Ready to get started?

Write your letter in a separate document first. When you're ready to enter, come back to this page, click the 'Submit Letter' button below and copy and paste it in.

You can close this page for now. To get back to here, just click the link in the email we sent you.

Good luck!

SUBMIT LETTER

9.14.2 Terms and conditions

9.14.2.1 Page 1

NCS

NCS Make a Change Competition: Terms and Conditions

By entering the competition, all entrants will be deemed to have accepted these Terms and Conditions and agreed to be bound by them.

How to enter

Letters should be submitted into the online entry form. You can access this from the email from NCS Trust that invited you to take part in the competition. All links are unique to you. You should not forward your email onto anyone else.

The competition is free to take part in. If you are eligible to take part, you will have received an invitation email from NCS Trust. Only previous graduates of NCS Trust have been invited to participate.

Competition opening and closing date

The competition will be open to letter submissions on 6th April 2020. The competition will close at 23:59 on the 3rd May 2020. No submissions entered after this time will be considered.

Entry restrictions

Competition entrants must be NCS graduates who attended NCS either in 2017, 2018, or 2019. If you are not an NCS graduate of either the 2017, 2018, or 2019 programmes, then you may not take part in this competition.

Entrants must submit their letters by 23:59 on Sunday 3rd May 2020.

Make sure your letter clearly addresses the question. It's about making positive change for the better, not just a chance to rant!

Submit your letter only via the online competition form. We ask you to stick to a maximum of 3,000 characters, including spaces (that's about one page of text). Anything over this may not be assessed by the judges.

Don't put your name – or anything that might identify you, or anyone else – in your entry. It's so we can judge them fairly. It's ok however to include any personal experiences. Don't worry, we can identify our winners from your unique link.

Use plain text (bold, italic and underline etc won't show up).

Avoid any offensive or abusive language.

Address your letter 'Dear Minister'. NCS is overseen by the Department for Digital, Culture, Media & Sport (DCMS).

Prize details

The winner and three runners up will be invited to Westminster, London, to discuss their ideas with a government minister and/ or the government officials that are responsible for this policy area. We will also be offering these winners the chance to visit the Houses of Parliament in Westminster, which lies at the heart of our democratic system.

We'll cover all expenses for travel and food for the day (following NCS Trust expenses policy), for both for the winner and runners-up. We will also cover expenses for one responsible adult to accompany the winner and each runner-up.

NCS Trust will also be publishing a summary of all the best entries, and plans to share your great ideas with the Department for Digital, Culture, Media & Sport.

The prize is non transferable, and cannot be exchanged for a cash equivalent.

Deciding the winners

Letters will be read and ranked by a team of independent judges working separately. Letters will be judged on their overall quality, according to the judges' opinions.

Letters will be assessed using a process called 'comparative judgement'. This approach to assessment is recognised for its fairness and reliability in academia and by the Office of Qualifications and Examinations Regulation (Ofqual) - the government organisation responsible for regulating qualifications, examinations and assessments in England.

Letters will be labelled with an anonymous ID so that judges will not know the identity of the entrant.

There will be two stages to the judging: one completed by graduates of NCS (who will not themselves be competition entrants) and a second stage conducted by staff at NCS Trust and the Department for Culture, Media and Sport (DCMS).

The competition winners will be notified by email by 7th August 2020. Winners will have 3 weeks to respond. After this time, if we have not received a response, NCS Trust reserves the right to select new winners as necessary.

Privacy and data protection

You have been contacted to take part in this competition as someone who attended NCS in 2017, 2018 or 2019, and who has previously shared their contact information with the Trust. We will be processing your personal data in the public interest, in line with NCS Trust's objectives and as a competition that encourages democratic engagement. This is in line with NCS Trust's Privacy Policy, which you can read by clicking here.

The email sent to you inviting you to take part in the competition is unique to you. It will allow NCS Trust to identify each entrant. It will not be possible for those involved in the judging process to know who the letter has been written by, in order that letters can be judged fairly. Competition entrants should not include any information in their letter which could be used to identify themselves or others.

Your personal data as a competition entrant will be kept until 6 months after the announcement of the winners and runners up.

If a competition entrant does not wish to lose their letter, then it is advised that they save a copy on their own device prior to submitting their entry into the competition.

Competition winners may be asked if they would be willing to consent to participate in promotional activities relating to the prize, but are free to withdraw their consent at any time. Similarly, any competition entrants may be asked if they are willing to have their name attributed to letters, should NCS Trust choose to publish them. Consent will be sought from the relevant individuals before we do this.

Your anonymous data may also be used by researchers to improve our communications.

Competition holder

You have been contacted to take part in this competition by the National Citizen Service Trust, Pembroke Building, Avonmore Road, Kensington, London W14 8DG.
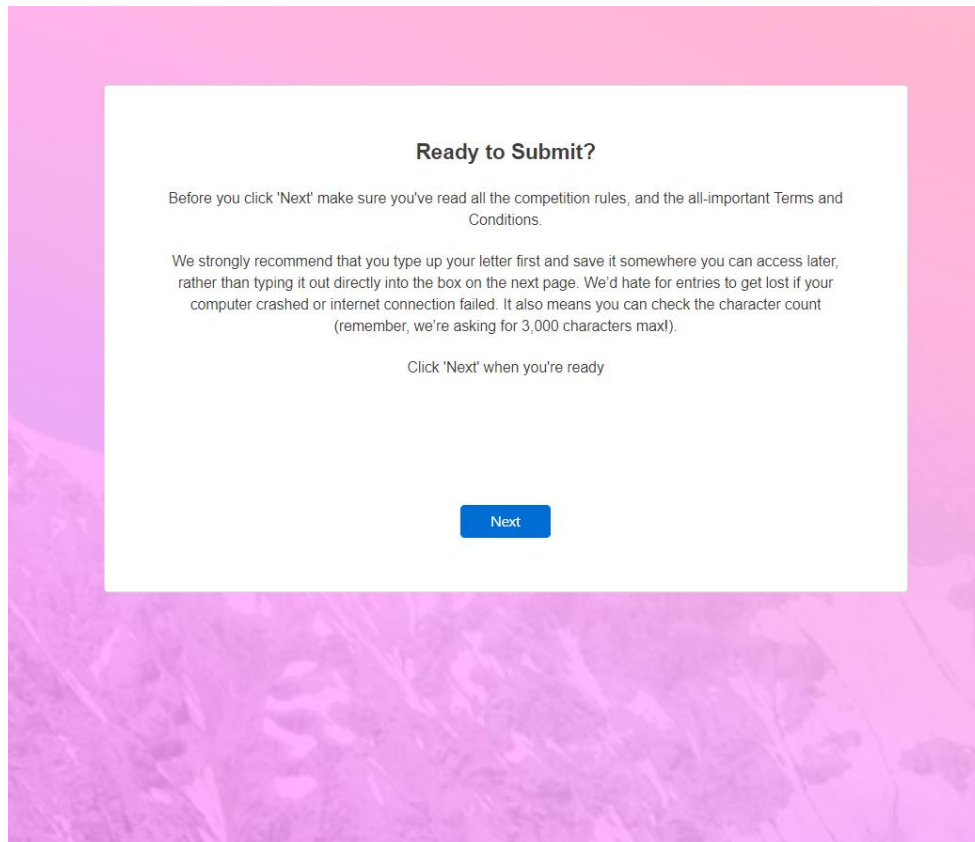
Still have questions?

For more information about the competition and how to take part, feel free to email us at competition@ncstrust.org.uk and we'll try our best to help.

Alternatively you can send us a letter to:

Competitions
National Citizen Service Trust,
Pembroke Bldg,
Avonmore Rd,
Kensington,
London W14 8DG

9.15 Appendix XV: Study 3 competition submission pages

9.15.1 Page 1

Ready to Submit?

Before you click 'Next' make sure you've read all the competition rules, and the all-important Terms and Conditions.

We strongly recommend that you type up your letter first and save it somewhere you can access later, rather than typing it out directly into the box on the next page. We'd hate for entries to get lost if your computer crashed or internet connection failed. It also means you can check the character count (remember, we're asking for 3,000 characters max!).

Click 'Next' when you're ready

Next

9.15.2 Page 2

Submit your entry

Paste your letter into the box below.

Use plain text. Things like bold, italic and underline won't show up.

We ask you to stick to a maximum of 3,000 characters, including spaces (that's about one page of text). Anything over this may not be assessed by the judges. You can check this on Microsoft Word using the 'word count' feature.
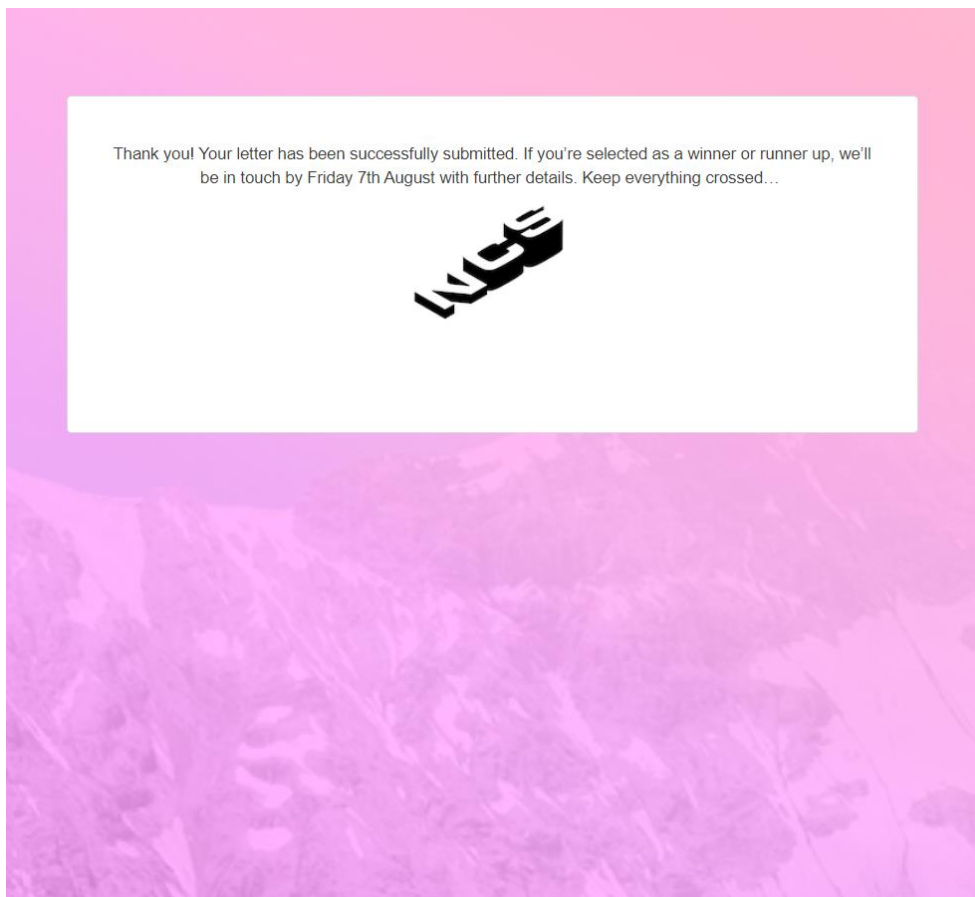
Enter answer...

* Confirm you have read and accepted the Terms and Conditions of the NCS Make a Change Competition, and click 'Finish' to submit your letter.

☐ I Confirm

Previous   **Finish**

9.15.3 Page 3

Thank you! Your letter has been successfully submitted. If you're selected as a winner or runner up, we'll be in touch by Friday 7th August with further details. Keep everything crossed…

9.16 Appendix XVI: Study 3 analysis code and log

The analysis code and logs are available to download from the OSF project page
for the thesis: https://osf.io/23k4n/.