Impact Evaluation Report of Activate! 2014
Prepared for the DG Murray Trust

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Contents

List of Tables

Section 1

Introduction

1.1 Context

Policy discussions about issues facing young people in South Africa today are rightly focused on changing *outcomes*. However in order to identify the best policy levers to change perverse outcomes, one doesn't just need an idea of *what* works, but also a good understanding of *why* some interventions work better than others. Unfortunately, this part of the policy making process is largely ignored. The implication is that the *reasons* underlying the behaviours of marginalised young people (what we would technically term as their "preferences") do not matter. In other words, their preferences are taken as exogenous: they are determined outside of, and are substantially unaffected by, the personal and structural aspects that frame their daily lives.

The same can be said of the general lack of attention paid to the relationship between behaviours and outcomes. In other words, the attendant role of *beliefs* to preferences. Yet an individual's beliefs about the preferences and behaviours of others is central to many – if not most – everyday interactions in a polarised society like South Africa. Take for example a marginalised crime-ridden community on the Cape Flats. It is widely known that policing in such neighbourhoods is non-existing at worst; ineffective at best. Non-violent cooperation by members of the community to work with the police can be an effective solution. But it requires collective action on behalf of the members of the community and the beliefs of each individual constituting the community become central here in at least two ways. Firstly, one person's willingness to participate in such an initiative may depend on a second person's willingness to participate, thus making the first individual's decision to participate or not dependent on her *expectation* of who else will follow through on a commitment to get involved. This in turn will depend on the second person's beliefs about a third person, and so on. In other situations ambiguity about what's entailed in the commitment itself can affect the formation and updating of beliefs.

1.2 The Intervention

The Activate! Change Driver's programme is essentially a programme about altering the preferences and beliefs of young people from predominantly marginalised communities in order to spark what might be termed "public innovation". The premise is that in order to be able to affect community-wide improvements to high levels of crime, violence, risky sexual behaviour and (economic) fatalism, one has to first direct effort towards changing underling preferences and beliefs. Public innovation is therefore ultimately about addressing community-wide challenges by supporting and building a network of young leaders who's preferences for altruism, trust, destructive risk-taking, entrepreneurial innovation, patience, and civic engagement are harnessed in a way that leads to better institutional performance. The Activate! training programme engages each of these elements of preferences, through a series of three modularised workshops aimed at activating changes in mindset around self-belief, goal-orientation, creative thinking, problem solving, and resilience. These interventions are theorised to lead to an actualisation of greater pro-social preferences – a necessary condition for public innovation – as well as better outcomes in terms of risk taking and time discounting (tolerance for delayed gratification), greater civic engagement, and ultimately better economic opportunities. The programme is, in many ways, a first of its kind as it offers a wholistic yet measurable definition of social capital that is capable of identifying key behavioural policy levers along the causal chain to improved community governance. The lessons learned from our impact evaluation of this programme therefore have great import for the design of anti-poverty programmes that are aimed at addressing South Africa's "youth problem".

1.3 Scope of the Evaluation

Given the objectives of the programme, it was important to collect information on outcomes that would be good predictors of public innovation. Public innovation requires cooperation, trust in others, altruism, and certain patterns of risk preferences and patience. It also requires active civic and broader political engagement. Finally the theory of change entailed in the rationale for Activate! is that the information→belief updating → outcome causal chain could be important, not just for individual self-actualisation directed towards public innovation, but also individual economic opportunity. Not all of these outcomes can be measured with conventional survey-based methodologies. We therefore designed a suite of incentivised tasks to elicit some of the main behavioural outcomes and coupled these methodologies with extensive surveys administered to the study participants. Thus we used a mixed-methods approach of preference elicitation and outcome measurement – a combination of "task" *and* "ask" methodologies, so to speak. The questions we address in this report are as follows:

- Does the programme alter trust (Section 2)?
- Does the programme alter cooperation (Section 3)?
- Does the programme alter notions of fairness or make participants more efficient bargainers (Section 4)?
- Does the programme alter risk-taking (Section 5)?
- Does the programme alter the extent to which participants discount the future; i.e., does it result in participants becoming more patient (Section 6)?
- Does the programme result in participants becoming more averse to losses (Section 7)?
- Does the programme result in participants becoming more civic-minded and does this correlate with a greater level of community activism (Section 8)?
- Does the programme result in participants obtaining greater private benefits such as increased employment opportunities and/or income (Section 9)?

1.4 Evaluation Design

The study uses a pipeline randomised control-trial methodology to mitigate the possibility of selection bias. Successful applicants to the programme were randomly assigned to participate either in 2014 or in 2015. As such, the 2015 group forms the control group, whilst the 2014 group constitutes the treatment group. Importantly the outcomes of the control group (both survey-based outcomes as well as behavioural outcomes) were measured *before* the control group embarked on the programme (between January-July 2015). This design mitigates selection bias on two fronts. First the focus only on *successful* applicants means that the study sample will be homogenous with respect to the selection criteria – leadership and innovation potential, as well as a commitment to the public good. Second, by randomising the year of training, the important behavioural markers should be relatively balanced across the treatment and control groups, thus allowing us to attribute any *differences* in these markers that arise between the treatment and the control groups to the intervention itself and not other unobserved factors.

Tables 1.1–1.3 (see the end of this section) presents a check on how well this design worked. In each table, we have conducted a test of differences in means between the treatment and control groups according to the participants randomly assigned treatment status as well as the realised treatment status. At baseline, there were 1012 individuals that were offered places in the programme, roughly equally split between those offered places in 2014 (the treatment group) and those offered places in 2015 (the control group). At endline there were about 610 study participants whose outcomes were successfully measured. Thus take-up of the programme was about 60% (the other 40% accounts for non take-up and attrition). To check that selective take-up and attrition will not bias our estimates, we also conducted tests of differences in means for the final sample of 610 individuals (this group is labelled "Actual" treatment status in the tables).

Table 1.1 reports the results of several demographic variables. Table 1.2 reports the results on self-reported perceptional health markers as well as several variables relating to social cohesion and proximate control. Finally table 1.3 reports the results on educational attainment and the most relevant labour market status variables. As is clearly seen, the final sample of 610 is well balanced on just about every variable reported across the three tables. The one statistically significant difference between the final treated and control samples is educational attainment. To correct for this, we have controlled for educational attainment in all the treatment effects models on which we base our inferences.

A final point worth noting is that there is almost universal compliance with the randomised treatment assignment for the final sample of 610 individuals: only about 5% of individuals did not comply with their randomly assigned status (i.e., they either joined the control group if assigned to the treatment group or vice-versa). Note that non-compliance here generally means that the implementing agency did not comply in about 5% of cases; not that the study subjects themselves had any choice in the matter. Nevertheless, this is a potential threat to identification. Where it makes sense, we present intention to treat (ITT) estimates instead of the standard average treatment effect on the treated (ATET). This involves using the assigned treatment status as an instrumental variable (IV) for actual treatment status (we don't always report this reduced form regression model; suffice it to say that the IV is highly statistically significant with a large magnitude (0.9) that accords with the the 95% compliance rate we observe in the final sample of 610 individuals.)

1.5 Preview of Main Results

Chapter 2: Trust *If the programme changes the beliefs of the participants about the trustworthiness of others, then there is a positive impact on trust behaviour.*

We find heterogeneous treatment effects of the programme on trust. The basic take home message on trust is that the measured impact of the programme seems to go in two different directions depending on whether an individual's perceptions of the trustworthiness of others can be altered by the programme. In practical terms, this means that there is a divergence between how Activators! are responding to the programme. If the programme changes their beliefs about the trustworthiness of others in a positive direction, the programme increases the treatment group's trust by 5%. However, if the programme has no impact on perceptions of the trustworthiness of others (a measure of participants' beliefs), they exhibit *less* trusting behaviour on the order of about 6.5%.

Chapter 3: Cooperation *The programme has no effect on measured cooperation.*

A key aim of the programme is to strengthen the proclivities of Activators towards innovation for the social good. One behavioural measure of such proclivities is the propensity to co-operate with strangers to achieve a mutually desirable outcome, as opposed to simply acting in one's own best interest. To test this, we use a standard version of the classic prisoners dilemma game, but framed it in terms of a decision to make a High or Low contribution to a Common pot. Participants earn a return based on contributions to the common pot. The results of this experiment suggest that whilst there is evidence that the propensity to co-operate is higher amongst the treatment group, this difference is not statistically significant, possibly due to small sample sizes and heterogeneity in behaviour.

Chapter 4: Bargaining *The programme has no effect on building fairness norms*

If part of the social cohesion agenda around Activate! is to build a sense of economic justice and fairness, we would expect this to be reflected by how participants respond to situations that are perceived as being unfair. To test this aspect of behaviour, we used the ultimatum game. The ultimatum game is a strategic interaction involving two individuals randomly assigned to take the role of either Player A or Player B. In this task, Player A is given an initial endowment and asked if they would like to give any to Player B. Player B is then given the option to accept or reject the offer. If Player B accepts the offer, the split is implemented as dictated by A's offer. In the event that Player B rejects the offer made by Player A, both players receive zero and the task ends. The game theoretic prediction in this task is that Player B should accept any offer made by Player A, since receiving a small share of money is better than remaining with none. By backward induction, the prediction is that Player A should make the smallest possible offer, and this should be accepted by Player B. This kind of behaviour would be the textbook economic man prediction of selfish and self-regarding preferences, where concerns over fairness play no role.

Typically, this is not what is observed in this experiment, which has been conducted in hundreds of other studies the world over in vastly different cultural contexts. Instead, player Bs, on average, exhibit a willingness to reject low offers; usually anything below 25-30% of the initial endowment. In turn, it appears that Player As in these tasks anticipate this kind of response, and tend to offer 25% or more in the first instance. Taken together, the international evidence suggests that participants have clear ideas about what constitutes an unfair offer, and that social norms about what is fair does affect choices, even if it comes at a personal cost. Utilising this task allowed us to explore a number of questions; namely, is it the case that subjects in the treatment group make higher offers than those in the control group. If the answer to this question is yes, this could indicate some success of the programme in altering social norms around fairness.

We find that treated subjects make higher offers in the ultimatum game than control group subjects, although the differences are not statistically significant. There is weak evidence that treated females make higher offers than treated males, and control group female subjects. Very few low offers were rejected by the player Bs and there is no statistically significant treatment effect on the probability of rejecting an offer. This is an interesting finding because one would expect that a commitment to fairness requires some rejection of very low offers. However recall that the self-actualisation elements of the programme also aimed to promote entrepreneurial innovation. Viewed from this perspective, this finding suggests that the programme was successful in selecting young people who already exhibited more or less the same level of (high) entrepreneurial potential into the programme but did not manage to effect any further changes on this outcome. It also provides a possible answer to the open question of whether the twin goals of building entrepreneurial mindsets *and* building social cohesion could be achieved within the same programme. At first glance this looks not to be the case. However recall the heterogeneous impact on trust discussed earlier. Viewed from the perspective of a pure investment with a risky outcome (trusting B without know anything about B's preferences) we found that trust increased for people who's beliefs were altered by the programme. This suggests that even though all study participants were equally entrepreneurially minded, the programme did alter the social preferences of a subset of the participants. This suggests that the twin goals need not be in opposition to one another.

Chapter 5: Risk *The programme causes participants to be relatively less risk averse.*

An additional question of interest to the programme implementers was to examine whether the programme has an impact on risk and time preferences. Behavioural economists typically measure risk preferences by offering subjects a series of incentivised lottery choices, and use the outcomes of these decision tasks to estimate risk parameters. We employed this methodology to measure each study subject's degree of relative risk aversion. This is usually a number that varies from a small negative number to a small number greater than 1. A value of 1 represents someone who is risk-neutral. A value less than one represents someone who is risk-averse. Finally a value of 1 represents someone who is risk neutral (in other words, someone who would be indifferent between a lottery that has an expected value that is equal to a guaranteed amount of money). Controlling for race, gender and node of training we estimate a risk co-efficient of 0.95, which is indicative of risk neutrality. Whilst all subjects display risk aversion, treatment group subjects are significantly *less* risk averse than control group subjects. Females subjects are significantly more risk averse than males, but this propensity does not differ by treatment status. Finally, there are no significant differences in estimated levels of risk aversion between or within province.

Chapter 6: Discounting *The programme causes participants to become relatively more patient.*

To elicit the study subjects level of impatience, we offered a series of incentivised choices, where they had to choose between smaller monetary payoffs in the near future as opposed to larger payoffs at a more distant time horizon. The results suggest an annualized discount rate of 75% (compared to 78% for the 2013 cohort) and that there are significant differences between treatment and control group subjects. Specifically, treated subjects exhibit significantly less impatience than control group subjects on average, especially treated subjects in the Western Cape. Additionally, treated subjects who have had an HIV test display even greater willingness to delay gratification, whilst the converse holds true for control group subjects.

Chapter 7: Loss Aversion *The programme has a significant effect on increasing aversion to losses.*

Risk aversion, as conventionally measured in Economics is a very restrictive concept that employs some very strong assumptions about how people choose between gambles. Loss aversion is a weaker concept that sometimes is more applicable to certain types of contexts. Formally it is defined as the tendency for individuals to respond more strongly to losses than equal sized gains relative to a reference point. Loss aversion can be present in risky and riskless choices (Kahneman and Tversky, 1979; Tversky and Kahneman, 1991). An example of loss aversion in risky choices is when individuals reject small gambles that have a positive expected value but may involve losses (Rabin, 2000; Fehr and Goette, 2007). Since we explored the risk preferences of treatment and control group subjects, we chose to supplement this work with a simple loss aversion experiment involving a risky choice. We adopted the experimental procedure used by Gachter et al (2007) because of its simplicity, and simply changed the monetary payoffs accordingly. In this task, participants are offered a series of 20 coin flips which each involve a 50-50 chance of a fixed gain of R100, and losses that vary from R20 to R115. For each of the twenty choices, participants had to simply indicate if they wanted to flip the coin or not. One of the big challenges in analysing this data (as with the risk data) is the non-monotonicity of choices exhibited by subjects. In others words, subjects make multiple switches between wanting to flip the coin as opposed to not, and this does not proceed linearly with the expected value of the gamble. Just under 30% of subjects exhibit this non-monotonicity of choices, meaning that 71% of subjects make a single switch only. Be that as it may, we devised two methods of estimating loss aversion to see if this non-monotonic switching behaviour presents a confound to our analysis but we found that not to be the case.

We found that treated subjects are more loss averse than control subjects, although these differences lack significance once selection effects are controlled for. Females are significantly more loss averse than males, but there is no differential treatment effect in this domain.

Chapter 8: Civic Engagement *Project involvement by treated subjects after a year of exposure to the programme increases relative to the control group (with no exposure). Treated subjects report significantly higher levels of civic participation in some domains (attending and organising community meetings, and engaging politicians) compared to control group subjects. There are no strong treatment effects in terms of organisational involvement and leadership. There are no treatment effects evident on the diversity of network resources, although treatment group subjects report improved access to financial contacts at endline relative to baseline.*

Chapter 9: Labour Market Outcomes *We find no statistically significant effects on labour force participation. On the other hand, there is some evidence that the programme did have an effect on labour supply.*

The programme causes women to increase their monthly hours worked by about 59%. This is a very large effect. We expect that this could be attributed to elements of the programme that might be having a strong empowering effect differentially by gender.

Table 1.1: T-tests: of difference in characteristics I

	Assigned		Actual	
Gender (1=Male)	-0.00468	(-0.14)	-0.0398	(-0.95)
African (1=yes)	-0.00313	(-0.21)	0.0135	(0.93)
Coloured (1=yes)	0.00501	(0.41)	-0.00682	(-0.50)
Indian (1=yes)	-0.00348	(-0.48)	-0.00664	(-1.32)
White (1=yes)	0.00161	(0.33)	0	(.)
English	-0.00576	(-0.38)	-0.0167	(-1.12)
Afrikaans	0.00000480	(0.00)	-0.0145	(-1.13)
IsiZulu	-0.0122	(-0.41)	-0.0182	(-0.48)
IsiXhosa	0.0345	(1.32)	0.0534	(1.62)
Sesotho	0.0118	(0.60)	0.00629	(0.26)
Tshivenda	-0.00230	(-0.24)	0.00431	(0.30)
IsiTsonga	-0.0180	(-1.30)	-0.0145	(-0.81)
SiSwati	0.00578	(0.52)	-0.00126	(-0.08)
Setswana	-0.0263	(-1.30)	-0.00925	(-0.35)
Sepedi	0.00326	(0.14)	0.0182	(0.61)
IsiNdebele	0.00739	(0.98)	-0.0117	(-1.04)
Other	0.00185	(0.49)	0.00386	(1.08)
Married (1=yes)	-0.00184	(-0.21)	0.00494	(0.62)
Living with a Partner (1=yes)	0.000927	(0.13)	0.00269	(0.24)
Widow/Widower (1=yes)	0.00208	(0.95)	0	(.)
Divorced or Seperated (1=yes)	-0.000230	(-0.07)	0.000539	(0.11)
Never Married (1=yes)	-0.000931	(-0.08)	-0.00817	(-0.56)
Completed matric (1=yes)	-0.0167	(-0.53)	0.0385	(0.95)
Completed some type of tertiary training after matric (1=yes)	-0.0136	(-0.45)	-0.0534	(-1.36)
Observations	1012		610	

* $p < 0.01$, ** $p < 0.05$, *** $p < 0.10$.

Std. errors reported in parenthesis, robust to individual clustering.

Difference is control mean minus treatment mean

Table 1.2: T-tests: of difference in characteristics II

	Assigned		Actual	
Excellent health (1=yes)	-0.0387	(-1.17)	0.00150	(0.04)
presenthealth==Very good	0.0590**	(2.05)	0.0472	(1.29)
presenthealth==Good	-0.0422	(-1.46)	-0.0583	(-1.57)
presenthealth==Fair	0.0201	(1.58)	0.00909	(0.56)
presenthealth==Poor	0.00185	(0.49)	0.000524	(0.10)
exercise==Never	0.0219	(1.01)	-0.0180	(-0.65)
exercise==Less than once a week	-0.00203	(-0.10)	0.00750	(0.30)
exercise==Once a week	-0.0196	(-0.85)	-0.0352	(-1.25)
Exercise 3 or more times a week (1=yes)	0.0110	(0.45)	0.0307	(0.99)
Don't drink alcohol (1=yes)	-0.0331	(-1.08)	0.0150	(0.38)
Smoke cigarettes (1=yes)	-0.0122	(-0.58)	-0.0357	(-1.28)
Smoke cigarettes in the past (1=yes)	0.0205	(0.85)	0.0198	(0.62)
alcohol==I have never drank alcohol	-0.0447	(-1.51)	0.0122	(0.32)
alcohol==I no longer drink alcohol	0.0155	(0.67)	0.00207	(0.07)
alcohol==I drink very rarely	0.0461	(1.44)	0.0186	(0.46)
alcohol==Less than once a week	0.00350	(0.15)	0.0134	(0.50)
alcohol==One or two days a week	-0.0217	(-1.38)	-0.0441**	(-2.10)
alcohol==Three or four days a week	0.00139	(0.24)	-0.00227	(-0.29)
On a day that you do drink, how many standard drinks do you have?	0.107	(0.41)	0.0881	(0.26)
HIV test (1=yes)	-0.0182	(-0.80)	-0.0338	(-1.17)
Have you ever engaged in unprotected sex? That is sexual intercourse where you a	-0.0331	(-1.02)	0.00493	(0.12)
Friend/Neighbour unlikely to return wallet (1=yes)	0.0243	(0.77)	0.0346	(0.85)
Stranger unlikely to return wallet (1=yes)	0.0641**	(2.07)	0.0499	(1.25)
Strong preference to stay in neighbourhood (1=yes)	0.00346	(0.12)	0.0153	(0.41)
Strong preference to leave neighbourhood (1=yes)	0.0563**	(2.29)	0.0435	(1.34)
High or very high chance of staying in neighbourhood (1=yes)	0.0128	(0.43)	0.0538	(1.41)
Happy or very happy with life (1=yes)	-0.0115	(-0.39)	-0.00768	(-0.20)
Totally in control or control over most things in life (1=yes)	-0.00354	(-0.13)	0.0270	(0.81)
Little or no control over most things in life (1=yes)	-0.0142	(-0.67)	-0.0263	(-0.96)
Observations	1012		610	

* $p < 0.01$, ** $p < 0.05$, *** $p < 0.10$.

Std. errors reported in parenthesis, robust to individual clustering.

Difference is control mean minus treatment mean

Table 1.3: T-tests: of difference in characteristics III

	Assigned		Actual	
Biological mother alive (1=yes)	-0.0241	(-0.94)	-0.0369	(-1.14)
Biological father alive (1=yes)	0.0370	(1.11)	0.0453	(1.06)
Assume that you become unemployed, what is the absolute lowest monthly take-home	0.811	(0.00)	-145.4	(-0.42)
What is the highest level of education you have successfully completed?	-0.194	(-0.82)	-0.536***	(-1.81)
Are you currently enrolled in any school, classes or correspondence courses of a	-0.0200	(-0.61)	-0.0328	(-0.78)
Driver's license (1=yes)	0.0104	(0.36)	-0.0179	(-0.49)
Advanced computer literacy (1=yes)	0.00116	(0.04)	-0.0281	(-0.69)
Basic computer literacy (1=yes)	-0.0126	(-0.40)	0.0228	(0.57)
Not computer literate (1=yes)	0.0115	(0.93)	0.00524	(0.33)
Unemployed and actively searching for work (1=yes)	0.0198	(0.89)	0.00720	(0.26)
Working for pay (1=yes)	0.0179	(0.60)	0.0117	(0.30)
Full time student (1=yes)	-0.0423	(-1.56)	0.0150	(0.42)
Unpaid volunteer (1=yes)	-0.0205	(-1.16)	-0.0338	(-1.37)
Observations	1012		610	

* $p < 0.01$, ** $p < 0.05$, *** $p < 0.10$.

Std. errors reported in parenthesis, robust to individual clustering.

Difference is control mean minus treatment mean

Section 2

Trust

2.1 Background

Trust is a vital ingredient in social exchange, and without which, the contractual and informational incompleteness characterizing real world markets, would be undermined in their functioning and efficiency (Arrow, 1973). By reducing transactions costs, higher trust may be associated with higher co-operation, particularly in resolving social dilemmas (Messick and Brewer, 1983, Coleman, 1990), and may even enhance economic growth and the performance of a society's institutions (Knack and Keefer, 1997; Coleman, 1990). However, in segmented societies, trust may depend on group affiliation, with trust being inversely related to the social distance between groups (Zak and Knack, 2001; Bouckaert and Dhaene, 2003; Akerlof, 1997). Thus, while inter- and intra-group trust may affect the economic success or failure of the society as a whole, it may also affect the relative economic outcomes for different groups within that society (Fershtman, and Gneezy, 2001). High status individuals may be able to capture a larger share of the surplus, and earn significantly more than low status individuals especially if individuals exhibit a preference for trading with high-status, and by extension, more trustworthy, individuals (Ball *et al*, 2001).

Given the increasing interest in understanding the role of social capital, and trust in particular, in explaining economic outcomes, much attention has been placed on how one goes about measuring trust. Growing evidence suggests that survey questions are unable to adequately capture social capital or trust levels. (Ben-Ner, 1999, Glaeser *et al*, 2000). For example, evidence from Glaeser *et al* (2000) indicates that trust and trustworthiness in experiments cannot be predicted by responses to a social capital questionnaire, with the exception of survey responses on trust in strangers. If anything, the standard trust questions may be better at eliciting information about trustworthiness rather than trust. In other words, there is a divergence between what people say they will do in a survey and the way they actually behave in incentive compatible experiments, perhaps indicating that self-reported motivations or behaviour reflect expectations about society as a whole, as opposed to information about an individual attribute. (Glaeser *et al*, 2000). This has spurred a large number of social scientists to collect behavioural trust measures through the use of the trust game.

The trust game studies generalized trust between strangers in a one shot interaction, where an individual displays trust if their action increases their vulnerability to another, whose behaviour they cannot control. (Kollock, 1994, Zak and Knack, 2001). In other words, given contractual incompleteness and asymmetric information, trust is an expectation on the part of the individual taking an action that they will not be exploited by their trading partner, even though the incentive structure facing the latter suggests otherwise (Harvey, 2001).

In the trust game (Berg *et al*, 1995), the proposer is given an endowment and asked what portion (if any) of this endowment they would like to pass on to their partner (the trustee), who is at a separate location. The offer by the first mover is doubled before passing it on to the second mover, who must then decide how much, if anything, to send back to the first mover. The amount sent by the first mover is an indication of trust, while the amount returned by the second mover is an indication of reciprocity or trustworthiness. While the efficient outcome in this strategic interaction implies that the proposer should send his entire endowment to the trustee, the subgame perfect equilibrium prediction is that no transfer of resources will occur at all. Yet, there is now a substantial body of evidence indicating that proposers do in fact send non-negligible amounts to their partners, who return at least as much as the initial endowment sent. (Berg *et al*, 1995; Fehr *et al*, 1993; Fehr *et al*, 1997; Guth *et al*, 1994, Scharleman *et al*, 2001, Glaeser *et al*, 2000, Croson and Buchan, 1999).

It is worth noting that there is increasing debate concerning whether the trust game, in fact, measures trust at all. Camerer (2002) argues that even though the game is devoid of the kinds of social sanctions, or contractual arrangements that characterize many trust relationships in real world interactions, the trust game measures "pure trust", that is, trust between strangers, where agents may be able to observe each other but may not necessarily interact in the future. Of course, the amount sent in the trust game could also be confounded by altruism (Cox 2004), or could be explained by betrayal aversion (Bohnet, Greig, Herrmann and Zeckhauser 2008, Fehr 2009) or a range of other factors (Ben-Ner and Halldorsson 2010, Ermisch, Gambetta, Laurie, Siedler and Noah Uhrig 2009). We abstract from these specific issues because our reading of the literature is that these factors seem highly contextually dependent.

Individuals may differ in their trust levels because of differing beliefs about the trustworthiness of others or different abilities to elicit trustworthy behaviour from others (Glaeser et al, 2000). These differences may be exacerbated in segmented societies where group affiliation based on some individual attribute such as race, ethnicity or gender, is particularly salient. Individuals may be less likely to trust outsiders, and more prone to stereotyping, especially where outsiders can be easily identified, by costlessly observable cues such as race and gender (Chandra, 2003; Cornell, 1996). Negative stereotypes may affect the performance of members of those groups about whom the stereotype exists (Hoff and Pandey, 2003; Steele, 2002). [1]

2.2 Methodology

2.2.1 Measuring Trust

We employ a standard trust/investment game (Berg *et al.* 1995) to elicit trust. In this decision making task, two individuals (most often strangers) need to make a snap decision to interact with each other in order to increase their individual and collective welfare. This experimental design more or less simulates a type of market exchange where asymmetric information and contractual incompleteness leads people to make strategic decisions on whether to trust a stranger or not.

The structure of the game is as follows: there are two players, Player A and B, and each are given R50 by the experimenter. Following that, Player A is asked whether he or she would like to send any of their monies to Player B. Player A is told that they can send any amount, including R0 and R50, in the increments of R1. Player A is also told that whatever amount they choose to do send will be doubled by the experimenter before passing it to Player B. The next stage of the game involves Player B making a decision about responding by sending some of their (variable) total back to A. Although the sub-game perfect equilibrium dictates that Player A should send zero, the socially optimal outcome requires Player A to transfer all of the initial endowment to Player B and for the latter to return 50% of the doubled amount. Of course, Player A would only make a transfer if he/she has an expectation of getting at least half of the doubled amount in return. As the name suggests, the amount that Player A sends is an experimentally induced measure of the extent to which A trusts people in general (i.e., perfect strangers) whilst the money Player B transfers back is considered as trustworthiness/reciprocity.

2.2.2 Evaluating Impact

An unbiased assessment of the impact of a program or intervention requires that some type of inference be made about what the outcomes of the participants of the program would have been had they not participated. Denote with y_1 an outcome of individual i when she is exposed to the program and denote y_0, as the outcome

[1] There is a vast literature connecting trust to discrimination. For instance, Fershtman and Gneezy (2001), focusing on ethnic discrimination, showed systematic mistrust towards men of Eastern origin in Israeli Jewish society. Similarly, Wilson and Eckel (2006) found that minority groups in the United States are trusted less in strategic settings, and Haile, Sadrieh and Verbon (2008), Simpson, McGrimmon and Irwin (2007), and Naef and Schupp (2009), all point to some evidence on higher rates of trust towards lighter-skinned individuals. Similarly, in post-apartheid South Africa, both Ashraf, Bohnet and Piankov (2006) and Burns (2012) found that Black subjects are trusted less compared to White subjects.

when she is not been exposed to the programme. The question we wish to answer is: what difference did the programme have on the outcome of interest (which in this case is the level of trust of the participants)? In other words, what is the difference $\Delta = y_1 - y_0$? The problem is that either y_1 or y_0 is observed for each individual, making it impossible to know Δ.

Let $D = 1$ denote individuals who participate in the program and let $D = 0$ denote individuals who are not participants of the program. Outcomes for both $D = 1$ and $D = 0$ individuals are observed. Further, let $\mathbf{x}$ denote a vector of observed characteristics of the individual (which contains variables such as race, gender, age, language and so forth). The most basic parameter of interest to be estimated is the so called "average treatment effect on the treated":

$$\begin{aligned} \text{ATT} &= \text{E}(\Delta|\mathbf{x}, D = 1) \\ &= \text{E}(y_i - y_0|\mathbf{x}, D = 1) \\ &= \text{E}(y_1|, \mathbf{x}, D = 1) - \text{E}(y_0|\mathbf{x}, D = 1) \end{aligned} \tag{2.1}$$

The presence of the second term in the last line of equation 2.1 summarizes the key problem that must be solved. Without an adequate counterfactual to an individual's observed status, the presence of the second term in the above equation means that the estimated impact suffers from selection bias. Randomisation is one mechanism by which we can mitigate selection bias. In this study, we employed a pipeline randomisation approach, where participants were randomly assigned to the treatment group (who were trained in 2014) and the control group (who were trained in 2015). Importantly the outcomes of the control group (both survey-based outcomes as well as behavioural outcomes) were measured before the control group embarked on the programme (between January-July 2015).

2.2.3 Modeling fractional responses

In this section we turn our attention issues of estimation. Data that is generated through behavioural experiments often exhibit features that are not well proximated by the workhorse distribution used in applied statistical analysis: the normal distribution. In this section we discuss the issues at stake. What follows is a somewhat technical discussion but because a neglect of these technical details can have a dramatic affect on how we are to interpret our findings, we include this brief detour as a motivation behind the econometric model we employ in this section. Readers without an interest in these technicalities can skip this and the next subsection without losing much by way of the key findings.

Our data structure poses several challenges that requires a departure from the normal distribution. There are two issues that we clearly need to contend with. Firstly, the dependant variable (in levels) is clearly truncated from below (at zero) and from above (at 50); i.e., some participants give nothing in the trust experiment which other give everything. An obvious choice in this instance would the so-called two-limit Tobit model. However, in our case even for values of the dependant variable falling in (0,50), merely eyeballing the distribution suggests that the data is not normally distributed, so one would need to log-transform the data before applying the Tobit estimator. Of course this raises the issue of how to handle the zeros. A standard practice would be to make an arbitrary adjustment to the zeros thereby shifting the lower limit point to something slightly less than the smallest non-limit observation (see Cameron and Trivedi (2010) for an example) but this in effect compounds the problem.[2] An alternative approach would be to transform the dependant variable into a fraction and then run a Two-limit Tobit model. Since our dependent variable has a pile-up at both 0 and 1, a Two-limit Tobit model would seem logically consistent (since the limit points can be seen as corner solutions). However, even in this instance, as Wooldridge (2010) has pointed out, if one is interested in the effects on the conditional mean – as we are – the two-limit Tobit will generally produce inconsistent estimates of the conditional mean function.

Another approach, also suggested by Wooldridge (2010), would be to take a log-odds transform of the dependent variable. The idea here is that since a fraction is mathematically equivalent to a probability, one can view $y/(1 - y)$ as an odds-ratio. Thus log-transforming this pseudo odds ratio will map the result back

[2] For example, with a mass point at zero and 50, what would result is a further mass-point at $\ln(0.99) = -0.01005034$ and $\ln(50) = 3.912023$. One option if one were wedded to the idea of a Two-limit Tobit, would be to apply an inverse hyperbolic sine transformation to the data. We indeed experimented with both of these options, but both approaches generated a non-trivial number of predictions lying outside the support of the dependant variable.

into the real number line and then one can apply the standard linear regression approach. Again however, the problem lies with the fact that this quantity will be undefined for the limit observations.[3] A further concern is that even if $0 < y < 1$, interpretation is not straightforward.

Given the above considerations, there are two options. Firstly, one can employ what Wooldridge (2010) has called "fractional logit" (or probit). This approach is a straightforward extension of the binary logit or probit model but essentially the log-likehood function as applied to a Bernoulli distributed random variable will take on exactly the same structure for a fractional response and has the added benefit of mapping predictions into (0,1) (see Papke and Wooldridge (1996) for the details). The key limitation however is that the zeros and ones a not treated any differently. A further limitation is that one is forced to assume either the logistic or normal distribution for responses on $0 < y < 1$.

A further limitation is that even though the Bernoulli log-likelihood belongs to the linear exponential family, fractional logit or probit do not directly handle the fact that the conditional variance is a function of the mean. On the other hand, the beta distribution exhibits this attractive feature. In a series of papers Ferrari and Cribari-Neto (2004) and Simas, Barreto-Souza and Rocha (2010) developed regression models for beta distributed random variables using a parameterization of the beta law that is indexed by the mean and dispersion parameters. Ospina and Ferrari (2012) extended this original framework to handle fractional responses involving limit mass points. This new approach, which they dub zero-or-one inflated beta regression forms the basis of our approach, albeit with some slight differences.[4]

2.2.4 Zero-One Inflated Beta Regression

We first outline the basic framework for the case where there are no mass points at 0 or 1. Our dependent variable is the fraction of the endowment offered by player A in the trust game. For the moment, let us restrict this response to $0 < y < 1$. As is well known, the beta distribution can take on a variety of shapes. Let a and b define these two shape parameters, with increases in a pulling the density toward zero and b pulling the density towards 1. Under the assumption that y follows a beta distribution, its density will be

$$f(y; a, b) = \frac{1}{\mathcal{B}(a, b)} y^{a-1}(1-y)^{b-1}, \qquad 0 < y < 1$$

and zero otherwise, where the normalizing factor $\mathcal{B}(a, b)$ can be written is terms of the gamma function

$$\mathcal{B}(a, b) = \frac{\Gamma(a)\Gamma(b)}{\Gamma(a+b)}$$

which then implies

$$f(y; a, b) = \frac{\Gamma(a+b)}{\Gamma(a)\Gamma(b)} y^{a-1}(1-y)^{b-1}, \qquad 0 < y < 1$$

It can then be shown that

$$\begin{aligned} \mathrm{E}(y) &= \frac{a}{a+b} \\ \mathrm{Var}(y) &= \frac{ab}{(a+b)^2(a+b+1)} \end{aligned}$$

The goal now is to define a regression model for this beta-distributed random variable y. Following Ferrari and Cribari-Neto (2004), we sketch the framework using a different parameterisation of the beta density. Since interest in a regression context centres on modelling the conditional mean, it makes sense to set

$$\begin{aligned} \mathrm{E}(y) &= \frac{a}{a+b} = \mu \\ \mathrm{Var}(y) &= \frac{\mathrm{Var}(\mu)}{1+\phi} \end{aligned}$$

[3]Applying linear regression to the untransformed fractional data will also not do the trick as the conditional expectation function will be non-linear and the variance will decline as a function of the mean (as either limit point is approached).

[4]See also Kieschnick and McCullough (2003) for an early applications of this approach and more recently Cook, Kieschnick and McCullough (2008) for Beta regression in a self-selection framework.

where μ is a location parameter and $\phi = a + b$ is a "precision" parameter. Note that this definition now implies that $\mu\phi = a$, $(1-\mu)\phi = b$ and $\mathrm{Var}(\mu) = \mu(1-\mu)$. This therefore means that the density of y can now be written using this new parameterisation:

$$f(y;\mu,\phi) = \frac{\Gamma(\phi)}{\Gamma(\mu\phi)\Gamma((1-\mu)\phi)} y^{\mu\phi-1}(1-y)^{(1-\mu)\phi-1}, \qquad 0 < y < 1$$

where $0 < \mu < 1$ and $\phi > 0$

This density is very flexible: it accommodates bell-shaped, left or right skewed as well as the uniform distribution (as is the case when $\mu = 1/2$ and $\phi = 2$). To use these distributional features in a regression context, it is standard to employ a GLM framework. Let $y_1 \ldots, y_n$ be independent random variables such that $y_i \sim \mathcal{B}(\mu,\phi)$, $i, \ldots, n$. We can then define the beta regression model as

$$g(\mu_i) = \mathbf{x}_i\boldsymbol{\beta} = \eta_i$$

where η is a linear predictor. In this formulation $g(\cdot)$ is a link function that maps the conditional mean into the reals. In our specifications, we choose a logit link function.[5] Up to this point, estimation would proceed more or less in the same way as does fractional regression, with the exception that instead of using the binomial distribution in setting up the likelihood function, we use the beta distribution, and we model the zeros and ones as discrete choices. Since we in fact are dealing with a choice problem, where the outcome is observed after an experimentally induced treatment, a discrete choice approach for the limit points makes more sense, as opposed to assuming that the zeros and ones result purely because of sampling variability.

In this unbounded context, estimation of μ and ϕ would proceed by maximum likelihood. Taking the log of the above likelihood function (suppressing subscripts for notational convenience) we have:

$$l(\mu,\theta) = \log\Gamma(\theta) - \log\Gamma(\theta\mu) - \log\Gamma((1-\mu)\theta) + (\mu\theta - 1)y^* + (\theta - 2)y^\dagger$$

where, as already stated we are using the logit link function, so $y^* = \log[y/(1-y)]$ and $y^\dagger = \log(1-y)$ if $y \in (0,1)$ and $y^* = 0$ and $y^\dagger = 0$ otherwise.

Ospina and Ferrari (2012) showed that this model can be generalised to a context involving extreme values on the closed unit interval.[6] Their extension of the GLM framework to cover this case requires degenerate probability statements that produce a mixture density, which in turn effectively boils down to an additive term to the log-likehood function given above. Specifically, one can think of 3 possible cases: (a) the case of zero-inflation (which is the case covered by Ospina and Ferrari (2012)) where a new parameter is added to account for the probability of observing values at zero. This leads to the mixture density of the form:

$$f(y;p_0,\mu,\phi) = \begin{cases} p_0 & \text{if } y = 0 \\ (1-p_0)f(y;\mu,\phi) & \text{if } 0 < y < 1 \end{cases}$$

or, (b) the case of one-inflation (which is also a case covered by Ospina and Ferrari (2012)) where a new parameter is added to account for the probability of observing values at one. This leads to the mixture density of the form:

$$f(y;p_1,\mu,\phi) = \begin{cases} (1-p_1)f(y;\mu,\phi) & \text{if } 0 < y < 1 \\ p_1 & \text{if } y = 1 \end{cases}$$

or, (c) the case of zero-one inflated Beta Regression giving the following mixture density:

$$f(y;p_0,\mu,\phi) = \begin{cases} p_0 & \text{if } y = 0 \\ (1-p_0)(1-p_1 f(y;\mu,\phi) & \text{if } 0 < y < 1 \\ p_1 & \text{if } y = 1 \end{cases}$$

Although adding in both zero and one inflation complicates the likelihood function, this complication is merely additive (in the sense that two new terms are added to the likelihood function). As Ospina and Ferrari (2012) show, this estimator can be operationalised by separately fitting logit/probit regression for the binary outcomes p_0 and p_1 and then using the resulting predicted probabilities for $y = 0$ and $y = 1$ to construct the terms $(1-p_0)$ and $(1-p_1)$. The product of these terms is then used to "inflate" the Beta density.

[5] The link function is always necessary as we want to avoid predictions outside of the unit interval.
[6] See their paper for further details regarding inference and diagnostics.

2.3 Data Description

As is clear from the above discussion, in order to make any type of statement about the impact of the Activate programme, one needs a control group. Our control group for the this evaluation constituted the 2015 Activators and we conducted surveys and experiments with them before they started the first module of the programme. Both the treatment and control groups were randomised into Player As and Player Bs. Thus, we are able to look at both trust (proxied by the behaviour of the Player As) as well as trustworthiness (proxied by the behaviour of the player B's).

Table 2.1 shows the means and standard deviations of some of the demographic characteristics, trust game and perceptional trust questionnaire results of the treatment and control groups in the study, and table 2.2 reports whether these characteristics vary statistically between the treated and control samples. In terms of demographics, the sample is more or less equally balanced as there are no statistically significant differences in race and gender between the treatment and control groups. Interestingly, as shown in table 2.2, there appears to be a statistically significant difference between treatment and control on perceptional trust measures: after a year long training, individuals tend to be 11% more trusting of others. Similarly, treatment group individuals are less likely to agree with statements such as "Most people will take advantage of you" or "People are generally selfish", suggesting that the effect of the programme on trust behaviour is positive. Finally, compared to the control group, treatment group individuals also tend to be less risk averse as the risk measure is closer to 1 (we discuss the findings on risk in more detail in chapter 4).

Table 2.1: Summary Statistics: Trust Game Data

	Control	Treatment	Total
Race (1=African)	0.977	0.964	0.970
	(0.151)	(0.188)	(0.172)
Gender (1=Male)	0.560	0.603	0.583
	(0.497)	(0.490)	(0.494)
Amount sent (player A)	22.09	22.85	22.52
	(11.36)	(13.06)	(12.34)
Fraction of endowment offered (Player A)	0.442	0.457	0.450
	(0.227)	(0.261)	(0.247)
Amount sent after Doubling (player A)	44.36	46.18	45.39
	(22.91)	(27.35)	(25.49)
Subjective expectations (player A)	28.54	30.73	29.78
	(20.52)	(21.78)	(21.23)
Subjective Expectations as a fraction of B's total (player A)	0.287	0.300	0.294
	(0.163)	(0.172)	(0.168)
Amount returned by partner (player A)	14.65	17.07	15.97
	(12.61)	(17.13)	(15.27)
Total: Doubled Amount plus Endowment (player B)	82.79	85.13	83.86
	(24.74)	(27.89)	(26.21)
Amount returned (player B)	23.02	21.11	22.14
	(20.62)	(20.32)	(20.47)
Amount kept (player B)	59.97	65.02	62.29
	(20.74)	(20.60)	(20.79)
Most people can be trusted	0.464	0.574	0.524
	(0.500)	(0.495)	(0.500)
Friend/Neighbour unlikely to return wallet (1=yes)	0.488	0.450	0.467
	(0.501)	(0.498)	(0.499)
Stranger unlikely to return wallet (1=yes)	0.619	0.565	0.590
	(0.486)	(0.496)	(0.492)
Most people will take advantage of you (1 = strongly agree or agree)	0.680	0.605	0.639
	(0.467)	(0.490)	(0.481)
People are generally quite selfish (1 = strongly agree or agree)	0.505	0.377	0.436
	(0.501)	(0.485)	(0.496)
Your neighbours won't help you (1 = strongly agree or agree)	0.384	0.419	0.403
	(0.487)	(0.494)	(0.491)
Your colleagues won't help you (1 = strongly agree or agree)	0.306	0.356	0.333
	(0.462)	(0.479)	(0.472)
I have been disappointed in trusting others (1 = strongly agree or agree)	0.900	0.894	0.897
	(0.300)	(0.309)	(0.305)
Before I trust someone I have to know their intentions (1 = strongly agree or ag	0.900	0.839	0.867
	(0.300)	(0.368)	(0.340)
Holt-Laury Risk Measure	0.784	0.936	0.866
	(3.551)	(2.848)	(3.190)
Holt-Laury Risk Measure (no outliers)	0.812	0.951	0.886
	(0.454)	(0.558)	(0.516)
Observations	610		

Table 2.2: T-tests: Trust Game Data

	Difference	
Race (1=African)	0.0133	(0.91)
Gender (1=Male)	-0.0428	(-1.02)
Amount sent (player A)	-0.759	(-0.52)
Fraction of endowment offered (Player A)	-0.0152	(-0.52)
Amount sent after Doubling (player A)	-1.824	(-0.60)
Subjective expectations (player A)	-2.186	(-0.87)
Subjective Expectations as a fraction of B's total (player A)	-0.0129	(-0.64)
Amount returned by partner (player A)	-2.423	(-1.28)
Total: Doubled Amount plus Endowment (player B)	-2.347	(-0.74)
Amount returned (player B)	1.910	(0.77)
Amount kept (player B)	-5.057**	(-2.03)
Most people can be trusted	-0.110*	(-2.73)
Friend/Neighbour unlikely to return wallet (1=yes)	0.0377	(0.93)
Stranger unlikely to return wallet (1=yes)	0.0539	(1.35)
Most people will take advantage of you (1 = strongly agree or agree)	0.0749***	(1.92)
People are generally quite selfish (1 = strongly agree or agree)	0.128*	(3.21)
Your neighbours won't help you (1 = strongly agree or agree)	-0.0351	(-0.88)
Your colleagues won't help you (1 = strongly agree or agree)	-0.0496	(-1.29)
I have been disappointed in trusting others (1 = strongly agree or agree)	0.00674	(0.27)
Before I trust someone I have to know their intentions (1 = strongly agree or ag	0.0615**	(2.23)
Holt-Laury Risk Measure	-0.151	(-0.58)
Holt-Laury Risk Measure (no outliers)	-0.139*	(-3.14)
Observations	610	

1. * $p < 0.01$, ** $p < 0.05$, *** $p < 0.10$.
2. Std. errors reported in parenthesis, robust to individual clustering.
3. Difference is treatment mean minus control mean

2.4 Estimates of Programme Impact on Trust

The basic take home message on trust is that measured impact of the programme seems to go in two different directions depending on whether an individuals perceptions of the trustworthiness of others can be altered by the programme. In the first set of regressions, the impact of the programme on trust behaviour is measured while controlling for gender, risk, perceptional trust, education, employment and marital status. The first line in the regression tables is the treatment effect on the treated (i.e., the programme impact) and we estimate the impact on trust. We report two different kinds of estimations: table 2.3 includes the full sample of Activators who participated in the trust game while table 2.4 excludes the outliers whose risk measures were at the extreme ends of the distribution.

Table 2.3 shows that there is a consistently strong gender effect while the other covariates do not seem to carry any statistical significance. The last four columns include an interaction term between treatment and perceptional trust; in other words the effect of the Activate! programme on the trusting behaviour of the participants. The significant and positive result suggests that conditional on the programme changing the attitudes on trust, Activators! exhibit a more trusting behaviour. More specifically, the finding from row 5 of Model 12 shows that the offers increase by 8% if the participants also believe that most people can be trusted. It also appears that married individuals exhibit a more trusting behaviour as they offer about 7% more than unmarried participants.

In table 2.4 where we exclude the outliers, we find that the treatment effect becomes statistically significant when we include the interaction term. In practical terms, this means that there is a divergence between how Activators! are responding to the programme. If the programme changes their beliefs about the trustworthiness of others, this is manifested in their behaviour as they make 5% higher offers in the trust game. However, if the programme has no impact on their perceptual trust degree, they exhibit less trusting behaviour. The other noteworthy point is that the level of education matters, especially for trust: Model 12 shows that completing matric translates into 11% and having some sort of a tertiary degree results in 8% higher offers.

Table 2.3: Impact of the Activate! Programme on Trust

	Model1	Model2	Model3	Model4	Model5	Model6	Model7	Model8	Model9	Model10	Model11	Model12
	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se
Treatment	-0.0028	-0.0011	-0.0025	-0.0059	-0.0008	-0.0043	-0.0056	-0.0039	-0.0446	-0.0448	-0.0446	-0.0450
	(0.022)	(0.023)	(0.022)	(0.023)	(0.023)	(0.024)	(0.023)	(0.024)	(0.035)	(0.035)	(0.035)	(0.035)
Gender (1=Male)		0.0539**			0.0538**	0.0512**		0.0510**	0.0505**	0.0490**	0.0487**	0.0491**
		(0.023)			(0.024)	(0.023)		(0.023)	(0.023)	(0.022)	(0.022)	(0.022)
Holt-Laury Risk Measure			-0.0018		-0.0015		-0.0021	-0.0020	-0.0010	-0.0012	-0.0012	-0.0010
			(0.003)		(0.003)		(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Most people can be trusted				0.0215		0.0264	0.0218	0.0268	-0.0170	-0.0166	-0.0166	-0.0185
				(0.023)		(0.023)	(0.023)	(0.023)	(0.034)	(0.034)	(0.034)	(0.034)
Treatment x Trust									0.0794***	0.0814***	0.0813***	0.0829***
									(0.047)	(0.048)	(0.048)	(0.049)
Completed matric										0.0157	0.0159	0.0266
										(0.063)	(0.064)	(0.067)
Completed tertiary training										-0.0049	-0.0047	0.0066
										(0.062)	(0.062)	(0.066)
Working for pay											-0.0038	-0.0041
											(0.024)	(0.024)
Married												0.0684***
												(0.039)
Observations	288	264	288	287	264	263	287	263	263	263	263	263

1. * $p < 0.01$, ** $p < 0.05$, *** $p < 0.10$.
2. Std. errors reported in parenthesis.
3. Dependant variable: the fraction of endowment offered by player A in the trust game.

Table 2.4: Impact of the Activate! Programme on Trust (no outliers)

	Model1	Model2	Model3	Model4	Model5	Model6	Model7	Model8	Model9	Model10	Model11	Model12
	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se
Treatment	-0.0028	-0.0011	-0.0104	-0.0059	-0.0073	-0.0043	-0.0127	-0.0097	-0.0604***	-0.0649***	-0.0647***	-0.0647***
	(0.022)	(0.023)	(0.024)	(0.023)	(0.025)	(0.024)	(0.024)	(0.025)	(0.036)	(0.036)	(0.036)	(0.036)
Gender (1=Male)		0.0539**			0.0515**	0.0512**		0.0497**	0.0479**	0.0449***	0.0445***	0.0439***
		(0.023)			(0.025)	(0.023)		(0.024)	(0.024)	(0.023)	(0.023)	(0.023)
Holt-Laury Risk Measure (no outliers)			0.0211		0.0196		0.0190	0.0169	0.0188	0.0258	0.0260	0.0265
			(0.026)		(0.026)		(0.026)	(0.026)	(0.025)	(0.026)	(0.026)	(0.026)
Most people can be trusted				0.0215		0.0264	0.0175	0.0230	-0.0305	-0.0342	-0.0341	-0.0356
				(0.023)		(0.023)	(0.025)	(0.025)	(0.034)	(0.035)	(0.035)	(0.035)
Treatment x Trust									0.1003**	0.1104**	0.1104**	0.1119**
									(0.050)	(0.050)	(0.050)	(0.051)
Completed matric										0.1127*	0.1135*	0.1134*
										(0.040)	(0.041)	(0.042)
Completed tertiary training										0.0795**	0.0803**	0.0806**
										(0.038)	(0.039)	(0.039)
Working for pay											-0.0051	-0.0060
											(0.025)	(0.025)
Married												0.0658***
												(0.037)
Observations	288	264	259	287	239	263	258	238	238	238	238	238

1. * $p < 0.01$, ** $p < 0.05$, *** $p < 0.10$.
2. Std. errors reported in parenthesis.
3. Dependant variable: the fraction of endowment offered by player A in the trust game.

Section 3

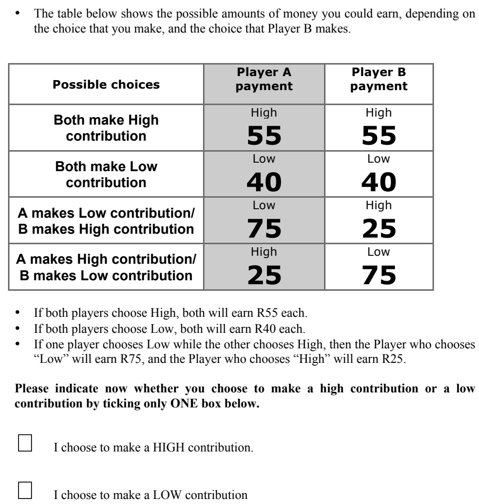
Co-operation

A key aim of the programme is to strengthen the proclivity of Activators towards innovation for the social good. One behavioural measure of such proclivity could be the propensity to co-operate with strangers to achieve a mutually desirable outcome, as opposed to simply acting in one's own best interest. To test this, we use a standard version of the classic prisoners dilemma game, but frame it in terms of a decision to make a High or Low contribution to a Common pot. Participants earn a return based on contributions to the common pot.

Participants were told that they were paired with another individual who was not at the venue where the experiments were being conducted. As with some of the other tasks, they were informed that their partner in this task had already been presented with the same set of instructions and choice, and had completed their decision sheets indicating their choice of whether to make a high or low contribution to the common pot. The participants were asked to make their choice in the absence of knowing what their partner had decided. This design allowed us to maintain the simultaneous move structure of the task, without having to have the partners present. Participants were asked to make their choice and record it on their decision sheet. At the end of the session, when payouts were implemented, each participant randomly picked a sheet out of the pile of potential partner sheets (which the UCT team had collected prior to coming to the Activate session). The Activator's choice was compared to that of their partner, and this set of choices determined the final payoffs.

If both players chose to make a high contribution to the common pot, they each earned R55. If both players chose to make a low contribution to the common pot, they each earned R40. If one player made a high contribution whilst the other chose low, then the player that opted to make a low contribution earned R75, whilst the player who opted to make a high contribution earned R25. The figure below presents part of the decision sheet that players saw when making their decisions.

Figure 3.1: Snippet of the decision sheet used in the prisoners dilemma task



- The table below shows the possible amounts of money you could earn, depending on the choice that you make, and the choice that Player B makes.

Possible choices	Player A payment	Player B payment
Both make High contribution	High 55	High 55
Both make Low contribution	Low 40	Low 40
A makes Low contribution/ B makes High contribution	Low 75	High 25
A makes High contribution/ B makes Low contribution	High 25	Low 75

- If both players choose High, both will earn R55 each.
- If both players choose Low, both will earn R40 each.
- If one player chooses Low while the other chooses High, then the Player who chooses "Low" will earn R75, and the Player who chooses "High" will earn R25.

Please indicate now whether you choose to make a high contribution or a low contribution by ticking only ONE box below.

☐ I choose to make a HIGH contribution.

☐ I choose to make a LOW contribution

Thus, the task maintains the standard prisoners dilemma structure in which defect (Contribute low) is the dominant strategy that results in a Pareto-inferior outcome. Typically, between 40-60% of subjects who complete these tasks choose to co-operate (in this case, contribute High) rather than defect, and this proclivity towards co-operation can be enhanced through allowing participants to communicate with one another, or by narrowing the differential between the payoff from defecting and the payoff from co-operating. Players who choose to co-operate in this setting are, in some respects, reciprocating expected co-operation from their partner.

Table 3.1 below presents the results for this task, recording the the mean number of participants who opted to make a "High" contribution to the common pot as opposed to a "Low" contribution. Estimates are presented based on actual treatment status, as well as the more stringent assigned treatment status. In both cases, the difference in the proportion of subjects choosing to co-operate with an unknown partner is negligible. Using assigned treatment status, 39% of control group subjects choose High compared to 37% of treatment group subjects. These differences in the unconditional mean are not statistically significant (t=0.018, p=0.42). A similar result is evident using actual treatment status (t=0,015, p=0.37).

Table 3.1: Proportion of subjects choosing "HIGH"

	Actual Treatment mean	Assigned treatment mean
Control	0.398	
Treatment	0.383	
Total	0.390	
Control		0.389
Group		0.372
Treatment		0.381
Observations	564	546

Table 3.2 below presents linear probability and instrumental variable (IV) regression estimates that model the impact of treatment status on the propensity to choose to make a High contribution in this game in a multivariate framework. In this multivariate context, the results suggest that the treatment effect, on average, is positive, albeit statistically insignificant. In other words, treated subjects are more likely to choose to make a high contribution once additional social and demographic factors are controlled for, although this effect is not statistically significant.

There is some heterogeneity in behaviour though. Individuals who are working for pay are significantly less likely to choose to co-operate, as are individuals who have completed some tertiary training.[1] Models 3 and 4 include additional interaction terms to check for heterogenous treatment effects by gender. While the co-efficients suggest that female subjects in the treatment group are less likely to choose High relative to their male counterparts, these differences are not statistically significant.

Participants in the Western Cape are significantly less likely to choose "High" in this task compared to their peers in other provinces (Models 1-4). However, there are no differences between treatment and control group subjects here. Once additional interaction terms are included to distinguish treated subjects from control group subjects (Models 5 and 6), it becomes clear that this Western Cape effect is the same for both groups. Similarly, in KZN and Inland, there are no significant differences in the behaviour of treated and control subjects.

Chapter 3: Result 1 *Whilst there is evidence that the propensity to co-operate is higher amongst the treatment group, this difference is not statistically significant, possibly due to small sample sizes and heterogeneity in behaviour.*

[1]Additional tests checked whether there was any treatment effect here, but none was found.

Table 3.2: Predicting co-operation (HIGH) in a multivariate framework

	Model1	Model2	Model3	Model4	Model5	Model6
	b/se	*b/se*	*b/se*	*b/se*	*b/se*	*b/se*
Actual treatment status based on date of interview	0.4238	1.7358	0.4730	1.8798	0.4238	1.9018
	(0.375)	(2.486)	(0.377)	(2.525)	(0.376)	(2.538)
Female	−0.0626	−0.0463	−0.0133	−0.0216	−0.0628	−0.0468
	(0.047)	(0.047)	(0.067)	(0.070)	(0.047)	(0.048)
Western Cape	−0.1416*	−0.1512*	−0.1425*	−0.1521*	−0.1355	−0.0528
	(0.064)	(0.062)	(0.064)	(0.062)	(0.095)	(0.103)
KwaZulu-Natal	0.0179	0.0070	0.0171	0.0062	0.0482	0.0329
	(0.063)	(0.060)	(0.063)	(0.060)	(0.087)	(0.090)
Completed matric (1=yes)	−0.1412	−0.1630	−0.1353	−0.1601	−0.1451	−0.1663
	(0.111)	(0.103)	(0.113)	(0.104)	(0.112)	(0.104)
Completed some type of tertiary training after matric (1=yes)	−0.1596	−0.2040*	−0.1571	−0.2034*	−0.1641	−0.2071*
	(0.114)	(0.106)	(0.115)	(0.107)	(0.115)	(0.107)
Single	0.0668	0.2537	0.0689	0.2577*	0.0673	0.2632
	(0.154)	(0.157)	(0.151)	(0.156)	(0.154)	(0.162)
Working for pay (1=yes)	−0.1523*	−0.1419*	−0.1591	−0.1451*	−0.1506*	−0.1420*
	(0.077)	(0.077)	(0.078)	(0.078)	(0.078)	(0.078)
Unemployed and actively searching for work (1=yes)	−0.1371	−0.1161	−0.1383	−0.1164	−0.1386	−0.1173
	(0.091)	(0.090)	(0.091)	(0.091)	(0.091)	(0.092)
Full time student (1=yes)	−0.1125	−0.1169	−0.1181	−0.1194	−0.1129	−0.1130
	(0.082)	(0.082)	(0.083)	(0.083)	(0.082)	(0.083)
Unpaid volunteer (1=yes)	−0.0663	−0.0515	−0.0696	−0.0524	−0.0682	−0.0453
	(0.098)	(0.098)	(0.099)	(0.098)	(0.099)	(0.099)
Treatment x female			−0.0971	−0.0482		
			(0.093)	(0.097)		
Treatment x Western Cape					−0.0114	−0.1919
					(0.129)	(0.144)
Treatment x KZN					−0.0644	−0.0501
					(0.125)	(0.139)
Constant	0.2495	−0.7229	0.1978	−0.8303	0.2536	−0.8392
	(0.194)	(1.788)	(0.199)	(1.832)	(0.195)	(1.847)
Observations	515	503	515	503	515	503

1. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.
2. ‘Robust Std. errors reported in parenthesis
3. Models 1, 3 & 5 are linear probability estimates that rely on actual treatment status
4. Models 2, 4 & 6 are two stage least squares regression that use assigned treatment status as an instrument for observed treatment status
5. Additional controls for date of interview/experimental session are included but not reported.

Section 4

Bargaining

A further hypothesised area of behavioural change that the Activate! programme might impact is bargaining behaviour. If part of what the programme does is further empower young people and raise their self-esteem, this could translate into how they respond to situations that are perceived as being unfair. To test this aspect of behaviour, we use the ultimatum game. The ultimatum game is a strategic interaction involving two individuals randomly assigned to take the role of either Player A or Player B. In this task, Player A is given an initial endowment and asked if they would like to give any to Player B. Player B is then given the option to accept or reject the offer. If Player B accepts the offer, the split is implemented as dictated by A's offer. In the event that Player B rejects the offer made by Player A, both players receive zero and the task ends. The game theoretic prediction in this task is that Player B should accept any offer made by Player A, since receiving a small share of money is better than remaining with none. By backward induction, the prediction is that Player A should make the smallest possible offer, and this should be accepted by Player B.

Typically, this is not what is observed in these interactions. Instead, player B's, on average, exhibit a willingness to reject low offers, usually anything below 25-30% of the initial endowment. In turn, it appears that Player A's in these tasks anticipate this kind of response, and tend to offer 25% or more in the first instance. Taken together, these results suggest that participants have clear ideas about what constitutes an unfair offer, and that social norms about what is fair may affect choices, even if it comes at personal cost.

In this task, subjects were randomly assigned to be Player A or Player B. In both instances, they were paired with a stranger in their choice decision. Those assigned to Player A status were given an initial endowment of R50, and asked to make a decision regarding an offer to Player B. Offers were made in increments of R5, and to simplify the task, players were presented with a set of choices, and they simply had to tick the relevant box to indicate their choice. Prior to this session, we recruited UCT students and assigned them to the role of Player B for this interaction. These players received the same information about the task, and were asked to indicate, for every possible offer that Player A might make, whether they would accept or reject the offer. This strategy method allowed us to collect a complete set of responses from the Player B partners prior to running the task with the Activators. Thus, on the day of the experiments with the Activators, those assigned to the role of Player A simply indicated how much they would offer to Player B. At the end of the session, when payoffs were being implemented, each player drew one response sheet (from a Player B) at random. The 2 sheets were read together, namely, we checked what Player B's response was for the actual offer made by player A. If B indicated that they would accept such an offer, the split was implemented. If player B rejected the offer, then both players received zero.

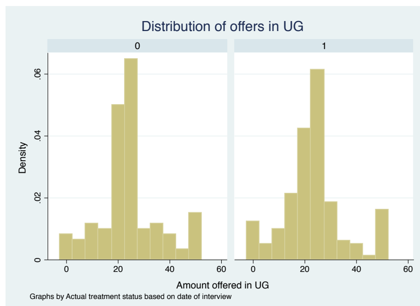
For those players assigned to Player B status, a similar procedure was followed. Prior to the session, we recruited UCT students to participate in this task as a Player A. These subjects were given an endowment of R50 and asked to indicate their offer to Player B. On the day of the experiments with the Activators, the task was explained to the Player B's, who then each randomly picked one of these sheets. They were thus able to observe the actual offer being made, and simply had to then decide whether to accept or reject the offer. This choice would determine the outcome of the game.

Utilising this task allows us to explore a number of questions, namely, is it the case that subjects in the treatment group make higher offers than those in the control. If yes, this could indicate greater awareness of social norms around fairness, and a commitment to treat others well even though the nature of the interaction would allow for for some exploitation. Conversely, in terms of the behaviour of Player B's, is it the case that subjects in the treatment group exhibit a greater propensity to reject unfair offers than those in the control. If this were the case, it could indicate that one of the effects of the programme is to empower young people to reject outcomes that are perceived to be unfair, even at some material cost to themselves.

4.1 Player A

We begin by examining the behaviour of Player As in this task. The histogram plots below present the distribution of offers made by Player A's in the treatment and control groups respectively. It is apparent that the distributions are quite similar (chi-square=0.396, p=0,52), and consistent with previous evidence, the mass of the offers are 25% or higher.

Figure 4.1: Distribution of offers made by Player As



The table below presents the unconditional mean offer by treatment status, using both observed treatment status and assigned treatment status. On average , subjects offered 48% of their endowment. Offers by treatment group subjects are slightly lower than the control group (47% vs 49%), but these differences are negligible, and none are statistically significant. As a second measure of bargaining behaviour, we look at the number of subjects who made very low offers, that is, offers of less than R20. On average, a quarter of subjects assigned to the treatment group made very low offers, compared to 17% of control group subjects. Again, this difference is insignificant in these simple unconditional mean estimates.

Table 4.1: Average rand offers in ultimatum game by Player A

	Actual treatment % offered	Assigned treatment % offered	Actual treatment Low offer	Assigned treatment Low offer
Control	0.487		0.183	
Treatment	0.468		0.244	
Total	0.476		0.218	
Control		0.490		0.171
Treatment		0.467		0.252
Total		0.477		0.215
Observations	280	274	280	274

1. Mann-Whitney test of difference in fraction offered: Actual: z=0.63; p=0.53. Assigned: z=0.60; p=0.55.

2. Mann-Whitney test of difference in fraction of subjects making low offers Actual: z=-1.21; p=0.23. Assigned: z=-1.62; p=0.11.

Table 4.2 below presents regression estimates of the effect of treatment status on offers made by Player A's in this task. Once additional controls are included, the results suggest positive treatment effects, albeit statistically insignificant.

Chapter 4: Result 1 *Treated subjects make higher offers in the ultimatum game than control group subjects although the differences are not statistically significant.*

In models 3 and 4, we include additional interaction terms to check for heterogeneous treatment effects, and find weak evidence that treated females subjects may be more prone to making more generous offers in this situation compared to their male counterparts. This result holds qualitatively in the IV specification, but loses its statistical significance[1] (Model 4). Similarly, treated females make higher offers than control group females, although the results lose significance in the IV specification.

Chapter 4: Result 2 *There is weak evidence that treated females make higher offers than treated males, and control group female subjects.*

Again, there is evidence of heterogeneity by location. Note that offers by subjects in KZN are significantly lower in this task compared to subjects in other provinces. In models 5 and 6, we include additional interaction terms to check whether there is any differential effect by treatment status *within* province. We do not find any significant differences here.

[1] The interaction term on Treatment x female is significant at the 12% level.

Table 4.2: Regression estimates of treatment effect on offers made by Player A in ultimatum game

	Model1	Model2	Model3	Model4	Model5	Model6
	b/se	b/se	b/se	b/se	b/se	b/se
Actual treatment status based on date of interview	0.0883	0.5410	0.0692	0.4194	0.0835	0.5197
	(0.084)	(0.494)	(0.086)	(0.459)	(0.088)	(0.498)
Female	−0.0204	−0.0176	−0.0750*	−0.0652	−0.0227	−0.0193
	(0.029)	(0.027)	(0.043)	(0.042)	(0.029)	(0.027)
Western Cape	−0.0325	−0.0230	−0.0387	−0.0294	−0.0885	−0.0915
	(0.048)	(0.045)	(0.049)	(0.045)	(0.093)	(0.089)
KwaZulu-Natal	−0.1579***	−0.1491***	−0.1574***	−0.1505***	−0.1600**	−0.1768**
	(0.043)	(0.039)	(0.042)	(0.039)	(0.059)	(0.060)
Completed matric (1=yes)	0.0348	0.0366	0.0362	0.0410	0.0287	0.0340
	(0.107)	(0.100)	(0.103)	(0.097)	(0.107)	(0.101)
Completed some type of tertiary training after matric (1=yes)	0.0812	0.0783	0.0911	0.0904	0.0749	0.0761
	(0.108)	(0.102)	(0.104)	(0.099)	(0.108)	(0.103)
Single	0.0601	0.1112	0.0607	0.0927	0.0620	0.1085
	(0.070)	(0.128)	(0.073)	(0.121)	(0.070)	(0.127)
Working for pay (1=yes)	−0.0044	0.0128	0.0063	0.0186	−0.0055	0.0107
	(0.064)	(0.064)	(0.063)	(0.062)	(0.064)	(0.064)
Unemployed and actively searching for work (1=yes)	−0.0703	−0.0490	−0.0612	−0.0453	−0.0731	−0.0495
	(0.073)	(0.073)	(0.072)	(0.071)	(0.074)	(0.074)
Full time student (1=yes)	0.0325	0.0449	0.0454	0.0543	0.0312	0.0447
	(0.072)	(0.071)	(0.070)	(0.069)	(0.072)	(0.071)
Unpaid volunteer (1=yes)	0.0089	0.0539	0.0229	0.0609	0.0069	0.0537
	(0.077)	(0.075)	(0.075)	(0.072)	(0.077)	(0.076)
Treatment x female			0.0973*	0.0826		
			(0.059)	(0.058)		
Treatment x Western Cape					0.0766	0.0998
					(0.107)	(0.105)
Treatment x KZN					0.0012	0.0572
					(0.083)	(0.094)
Constant	0.3573*	0.2383	0.3641**	0.2742	0.3680**	0.2614
	(0.138)	(0.192)	(0.138)	(0.185)	(0.141)	(0.191)
Observations	253	251	253	251	253	251

1. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.
2. Robust Std. errors reported in parenthesis
3. Models 1,3 & 5 are OLS estimates that rely on actual treatment status
4. Models 2, 4 & 6 are two stage least squares regression that uses assigned treatment status as an instrument for observed treatment status
5. Additional controls for interview date/experimental session are included but not reported.

Table 4.3 revisits this result by examining the likelihood that Player A makes a low offer, that is an offer of less than R20. These results confirm those already reported above. Treated subjects are less likely to make very low offers in this game. However, subjects in KZN are significantly more likely to bargain low, although this tendency is reduced in the treatment group relative to the control group. In addition, the results show that subjects who have completed matric or tertiary training are significantly less likely to make a low offer. In additional specifications (not reported), we include additional interaction terms to check for treatment effects in this domain, but no significant effects were found.

Table 4.3: Linear probability regression estimates of treatment effect on the likelihood of a low offer being made by Player A in ultimatum game

	Model1	Model2	Model3	Model4	Model5	Model6
	b/se	b/se	b/se	b/se	b/se	b/se
Actual treatment status based on date of interview	−0.2597*	−0.1046	−0.2407	−0.0956	−0.2480	−0.0714
	(0.151)	(0.898)	(0.152)	(0.883)	(0.159)	(0.939)
Female	0.0172	0.0220	0.0712	0.0255	0.0179	0.0209
	(0.057)	(0.053)	(0.083)	(0.079)	(0.058)	(0.054)
Western Cape	0.0024	0.0026	0.0086	0.0031	0.0400	0.0786
	(0.082)	(0.077)	(0.083)	(0.077)	(0.158)	(0.147)
KwaZulu-Natal	0.2457**	0.2467***	0.2452**	0.2468***	0.2648**	0.3400***
	(0.074)	(0.070)	(0.074)	(0.070)	(0.099)	(0.094)
Completed matric (1=yes)	−0.3250*	−0.3445*	−0.3264*	−0.3448*	−0.3241*	−0.3518*
	(0.155)	(0.144)	(0.151)	(0.144)	(0.157)	(0.147)
Completed some type of tertiary training after matric (1=yes)	−0.2832	−0.2969*	−0.2930	−0.2978*	−0.2826	−0.3059*
	(0.155)	(0.148)	(0.152)	(0.148)	(0.159)	(0.151)
Single	0.0721	0.0922	0.0714	0.0935	0.0730	0.0969
	(0.090)	(0.196)	(0.091)	(0.194)	(0.092)	(0.203)
Working for pay (1=yes)	−0.0469	−0.0437	−0.0576	−0.0441	−0.0457	−0.0403
	(0.115)	(0.107)	(0.115)	(0.107)	(0.116)	(0.108)
Unemployed and actively searching for work (1=yes)	0.0698	0.0734	0.0608	0.0731	0.0696	0.0656
	(0.129)	(0.119)	(0.129)	(0.119)	(0.130)	(0.120)
Full time student (1=yes)	−0.0779	−0.0754	−0.0906	−0.0761	−0.0778	−0.0781
	(0.118)	(0.108)	(0.117)	(0.108)	(0.118)	(0.108)
Unpaid volunteer (1=yes)	−0.0395	−0.0713	−0.0533	−0.0718	−0.0398	−0.0779
	(0.140)	(0.132)	(0.139)	(0.132)	(0.141)	(0.134)
Treatment x female			−0.0964	−0.0061		
			(0.112)	(0.112)		
Treatment x Western Cape					−0.0557	−0.1259
					(0.180)	(0.173)
Treatment x KZN					−0.0403	−0.2034
					(0.153)	(0.168)
Constant	0.5667*	0.5447	0.5600*	0.5420	0.5538*	0.5060
	(0.222)	(0.317)	(0.221)	(0.313)	(0.229)	(0.324)
Observations	253	251	253	251	253	251

1. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.
2. Robust Std. errors reported in parenthesis
3. Models 1, 3 & 5 are OLS estimates that rely on actual treatment status
4. Models 2, 4 & 6 are two stage least squares regression that uses assigned treatment status as an instrument for observed treatment status
5. Additional controls for interview date/experimental session are included but not reported.

4.2 Player B

In terms of the behaviour of Player B's in this task, the question that arises is whether or not there are differences in the rejection rates by treatment status. Table 4.4 below presents the mean rejection rates by treatment status. The first thing to note is that there are very few rejections at all. Only 25 subjects rejected an offer, and of these, they were almost evenly split between treatment and control groups, with 8% of subjects rejecting an offer in both treatment and control groups. Thus, it is not surprising that there are no significant differences in rejection rates by treatment status. The average value of rejected offers is just over R8.

Chapter 4: Result 3 *Rejection rates are low and do not vary by treatment status*

Table 4.4: Rejection rates for Player B by treatment status

	Actual %	Assigned %	Actual Mean rejected offer (R)	Assigned Mean rejected offer (R)
Control	0.0811		8.333	
Treatment	0.0800		10.50	
Control		0.0828		8.846
Treatment		0.0600		8.333
Observations	273	257	22	19

The multivariate regression results confirm the absence on any strong treatment effect (see table 4.5). The co-efficient on the treatment variable is effectively zero and lacks significance. Again, however, we see some difference in behaviour in the KZN group. In model 6, the results suggest that treated subjects in KZN are significantly less likely to reject an offer than compared to the control group as a whole.

Table 4.5: Linear probability regression estimates of treatment effect on the likelihood of an offer being rejected in ultimatum game

	Model1	Model2	Model3	Model4	Model5	Model6
	b/se	b/se	b/se	b/se	b/se	b/se
Treatment	−0.0090	−0.0168	−0.0141	−0.0058	0.0166	0.0044
	(0.035)	(0.035)	(0.047)	(0.051)	(0.040)	(0.039)
African	0.0891***	0.0700**	0.0901***	0.0692**	0.1019**	0.0953*
	(0.023)	(0.024)	(0.024)	(0.024)	(0.033)	(0.038)
Female	−0.0074	−0.0185	−0.0128	−0.0075	−0.0086	−0.0192
	(0.036)	(0.034)	(0.050)	(0.049)	(0.035)	(0.033)
WC	0.0523	0.0629	0.0528	0.0616	0.0731	0.0604
	(0.050)	(0.051)	(0.050)	(0.049)	(0.068)	(0.068)
KZN	0.0365	0.0244	0.0367	0.0238	0.0861	0.0900
	(0.053)	(0.047)	(0.053)	(0.047)	(0.081)	(0.080)
Treatment*female			0.0115	−0.0242		
			(0.069)	(0.068)		
Treatment*WC					−0.0457	0.0097
					(0.099)	(0.121)
Treatment*KZN					−0.1065	−0.1468*
					(0.103)	(0.085)
Constant	−0.0127	0.0044	−0.0114	0.0007	−0.0373	−0.0301
	(0.030)	(0.023)	(0.032)	(0.027)	(0.041)	(0.044)
Observations	253	244	253	244	253	244

1. * $p < 0.01$, ** $p < 0.05$, *** $p < 0.10$.
2. Robust Std. errors reported in parenthesis
3. Models 1, 3 & 5 are OLS estimates that rely on actual treatment status
4. Models 2, 4 & 6 are two stage least squares regression that uses assigned treatment status as an instrument for observed treatment status

Section 5

Risk preferences

A further key question of interest to the programme implementers was to examine the risk and time preferences of programme participants, and to examine whether these preferences change over the duration of the Activate! programme. The value of measuring risk and time preferences is self-evident. Attitudes towards risk affect a whole host of choices that an individual might make, from investment choices, to willingness to extend trust to strangers, to choices about sexual behaviour and the decision to engage in illegal activities, or consume addictive goods. In turn, each of these choices holds consequences for both the individual and social good. Similarly, measuring individual attitudes towards time is crucial for understanding how people view choices in the present relative to those in the future, where such choices involve smaller rewards in the present, but larger rewards if gratification can be delayed. An unwillingness to delay gratification (or impatience as it is sometimes termed) may explain a host of common social problems, ranging from low savings rates, poor nutritional and educational choices and risky sexual behaviour to name a few.

Behavioural economists typically measure risk preferences by offering subjects a series of incentivised lottery choices, and use the outcomes of these decision tasks to estimate risk parameters. The obvious caveat here is that in this context, risk relates to choices over uncertain **monetary** payoffs. It is an open question in the literature to what extent risk preferences exhibited towards monetary gambles reflect risk attitudes and behaviours in other domains of an individual's life.

Following the international literature in the field of risk and time experiments, we offer participants a series of lottery choices. Each participant was presented with 4 tables. Each table consisted of 10 lottery choices, so that by the end of the task, each participant had made 40 separate lottery choices. An example from one of the tables used in the task is presented below for ease of exposition.

Figure 5.1: Snippet of the multiple price list used to elicit risk preferences

Decision	Option A	Option B	Your Choice (Circle A or B)
1	R90 if dice is 1 R60 if dice is 2 3 4 5 6 7 8 9 0	R160 if dice is 1 R20 if dice is 2 3 4 5 6 7 8 9 0	A B
2	R90 if dice is 1 2 R60 if dice is 3 4 5 6 7 8 9 0	R160 if dice is 1 2 R20 if dice is 3 4 5 6 7 8 9 0	A B
3	R90 if dice is 1 2 3 R60 if dice is 4 5 6 7 8 9 0	R160 if dice is 1 2 3 R20 if dice is 4 5 6 7 8 9 0	A B
4	R90 if dice is 1 2 3 4 R60 if dice is 5 6 7 8 9 0	R160 if dice is 1 2 3 4 R20 if dice is 5 6 7 8 9 0	A B
5	R90 if dice is 1 2 3 4 5 R60 if dice is 6 7 8 9 0	R160 if dice is 1 2 3 4 5 R20 if dice is 6 7 8 9 0	A B

On each of the ten rows in a given table, participants were asked to indicate whether they preferred Option A or Option B by circling their preferred choice. In any given table, the monetary payments associated with the lottery choices remained constant in order to simplify the task. For example, in the example above, every lottery A choice involved a weighted probability of winning R90 or R60, whilst lottery B involved a weighted probability of winning R160 or R20. The only factor that changed in a table was the probability associated with winning a particular outcome.

The notion of probabilities was explained in simple language using a 10-sided die. Participants were told that the chance of winning a particular monetary payment would be determined by the roll of the 10-sided die. As per the table above, in Row 1, if the participant had chosen lottery A, and this was the choice selected for payment [1], they would roll the die. If the die returned the number 1, then they would win R90. If the die returned any other number, then they would win R60. Alternatively, if on Row 1, the participant had circled Option B as their preferred option and this choice was selected for final payment, then if the die returned a value of 1, they would win R160, and if the die returned any other value, they would win R20. It should be evident that as one moves down the rows in the table, the probability of winning the larger value in any lottery choice increases, whilst the probability of winning the smaller amount decreases.

In explaining this task to participants, the researchers used posters in conjunction with practice tables in the game packs that participants were given. At the start of the task, participants were asked to come to the front of the room, where a large poster with a practice table (such as the one presented above) was used to explain the task. Participants were afforded the opportunity to roll the 10-sided die, make choices and talk through numerous examples until the researcher was convinced that they understood what was required.

5.1 Modelling risk preferences

The theoretical model underlying this approach assumes a constant relative risk aversion (CRRA) utility function. The functional form is given by $U(x) = x^r$ where x is the lottery prize and r is the risk parameter to be estimated from the data. In this framework, expected utility is simply the probability weighted utility of each outcome in each lottery choice. The expected utility for each of the 40 lottery pairs is calculated by observing whether the individual chooses Option A or Option B. If the individual chooses Option A, it is assumed they do so because they prefer Option A (i.e. it gives them more expected utility) over option B. By doing this comparison for each lottery choice, one is able to generate the latent index $\triangle EU = EU_B - EU_A$. This latent index is linked to observed choices using a standard cumulative normal distribution function. Utilising these techniques, the goal is to estimate r, the risk parameter. A value of $r = 1$ indicates risk neutrality; a value of $r < 1$ indicates risk aversion, and a value of $r > 1$ indicates risk loving behaviour on the part of participants.

Table 5.1 below presents regression estimates of the risk parameter r. (Figure 4.2 presents distributional plots of estimated risk parameters by treatment status). In these specifications, r is given by the constant term. Model 1 presents the results when no additional controls are included. The r-coefficient is 0,67 (compared to a coefficient of 0,55 in for the 2012 cohort, and 0.58 for the 2013 cohort at endline) indicating that all subjects are relatively risk averse.

[1] In order to generate sufficient data to estimate risk co-efficients, one needs participants to make multiple choices. However, budgetary constraints mean that it is impossible to pay participants for every single choice they make. Hence, whilst participants made 40 choices, only one choice was finally selected for payment. The decision of which choice to select for final payment was made as follows: at the end of the session, participants would individually approach the table where the researcher was seated. They were asked to pick a card from a pile of 8 cards. The cards in the pile were numbered from 1 to 4 (with a duplicate of each) and these numbers corresponded to the Table number. Recall, participants were asked to completed 4 tables, labelled Table 1, Table 2 etc. Once participants had selected a numbered card, this would indicate which table had been selected for payment. Next, the participant would roll the 10 sided die. This would determine which row on the table had been selected for payment. For example, if the die returned a value of 4, this would mean that row 4 (and the lottery choice made on that row by the participant) was the one chosen for final payment. The participant and researcher would then check that row in the table to see which lottery the participant had chosen, A or B. Lastly, the participant would be asked to roll the die one last time. The number returned by the die would then determine which monetary amount the individual would receive based on the lottery choice they had made.

Table 5.1: Risk estimates by observed treatment status

	Model1	Model2	Model3	Model4	Model5
	b/se	b/se	b/se	b/se	b/se
Actual treatment status based on date of interview		−0.0152	0.9465***	0.9640***	0.9468***
		(0.049)	(0.156)	(0.163)	(0.155)
Race (1=African)			−0.1422	−0.1451	−0.1311
			(0.183)	(0.183)	(0.185)
Female			−0.0932*	−0.0760	−0.0913*
			(0.052)	(0.072)	(0.052)
Western Cape			−0.0721	−0.0730	−0.1418
			(0.086)	(0.085)	(0.110)
KwaZulu-Natal			−0.0440	−0.0440	−0.0414
			(0.070)	(0.070)	(0.094)
Completed matric (1=yes)			0.2080*	0.2111*	0.2076*
			(0.120)	(0.120)	(0.119)
Completed some type of tertiary training after matric (1=yes)			0.2425*	0.2442**	0.2414**
			(0.126)	(0.126)	(0.125)
Single			−0.2217	−0.2215	−0.2253
			(0.142)	(0.141)	(0.141)
Working for pay (1=yes)			−0.0612	−0.0637	−0.0594
			(0.113)	(0.112)	(0.112)
Unemployed and actively searching for work (1=yes)			−0.1073	−0.1081	−0.1075
			(0.132)	(0.132)	(0.133)
Full time student (1=yes)			0.0067	0.0047	0.0044
			(0.120)	(0.120)	(0.120)
Unpaid volunteer (1=yes)			−0.0347	−0.0361	−0.0386
			(0.127)	(0.127)	(0.128)
Smoke cigarettes (1=yes)			−0.1346	−0.1325	−0.1344
			(0.090)	(0.091)	(0.090)
Have had unprotected sex			0.0592	0.0583	0.0577
			(0.052)	(0.051)	(0.052)
Have had HIV test			−0.0298	−0.0301	−0.0304
			(0.065)	(0.066)	(0.065)
No. alcoholic units consumed			0.0228**	0.0227**	0.0233**
			(0.009)	(0.009)	(0.009)
Treatment x female				−0.0349	
				(0.103)	
Treatment x Western Cape					0.1360
					(0.165)
Treatment x KZN					−0.0124
					(0.136)
Constant	0.6722***	0.6663***	−0.4592*	−0.4747*	−0.4657*
	(0.048)	(0.032)	(0.277)	(0.281)	(0.278)
mu	0.2696***	0.2696***	0.2661***	0.2661***	0.2660***
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Observations	22613	22613	20336	20336	20336

1. * $p < 0.01$, ** $p < 0.05$, *** $p < 0.10$.
2. Robust Std. errors reported in parenthesis
3. Treatment = Observed treatment status

Models 2-5 presents the risk co-efficient once additional controls are included. It is clear that treatment and control subjects differ significantly in their risk preferences, with control group subjects being relatively more risk averse than treated subjects on average. Importantly, the co-efficient for both treated and control subjects is less than one, indicating that all subjects are risk averse. However, treated subjects are less risk averse than their control group counterparts. This is demonstrated in the figure below which shows the density plots of the risk distributions for treated and control group subjects respectively.

Chapter 5: Result 1 *Whilst all subjects display risk aversion, treatment group subjects are significantly less risk averse than control group subjects.*

Moreover, there are other interesting differences in behaviour. Model 3 suggests that female subjects are significantly more risk averse than their male counterparts on average. We explore this gender dimension further in Model 4, by including an interaction term to test whether the behaviour of treated females is different than females in the control group. The results suggest no significant differences in this dimension.

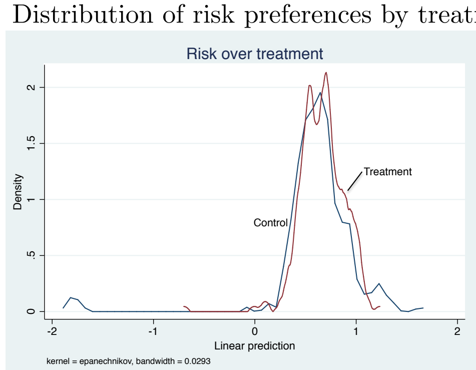
Chapter 5: Result 2 *Females subjects are significantly more risk averse than males, but this propensity does not differ by treatment status.*

Model 5 explores the geographic dimension by including interaction terms with the provincial dummies. However, there are no significant differences between treatment and control subjects within province. It is interesting to note that the co-efficients sometimes move in opposite directions here, though, indicating some differences in behaviour, albeit statistically insignificant.

Chapter 5: Result 3 *There are no significant differences in estimated levels of risk aversion between or within province*

Finally, it is worth noting that the estimated risk coefficient is significantly higher for individuals who have completed matric or gone on to complete some form of tertiary education. In other words, education is associated with lower levels of risk aversion. The risk co-efficient is also increasing in the reported level of alcohol consumption, and this effect is significant. In other words, those individuals who report drinking larger quantities of alcohol on average also display significantly lower levels of risk aversion. We conduct additional regression specifications to check whether these effects are different between treatment and control subjects, and find no evidence to support this. We do similar checks for smoking, unprotected sex and smoking status and again, confirm that there are no differences between treatment and control group subjects in these domains insofar as they relate to their risk preferences.

Figure 5.2: Distribution of risk preferences by treatment status



Section 6

Time preferences

A second set of experiments was run in order to characterise the time preferences of Activators. Time preferences indicate how individuals view choices in the present versus choices in the future, and the estimated discount rate provides some measure of delayed gratification. Individual time preferences and discount rates are important for understanding individual planning processes, consumption decisions, lapses in willpower, savings decisions, health choices and a myriad of other decisions. In standard discount tasks, an individual is presented with the following choice: Do you prefer R100 today or R100 + x tomorrow? If the individual chooses R100 today, we infer their discount rate is higher than x percent per day. For example, If the annual discount rate is 10%, the individual would be indifferent between R100 today and R110 in a year.

6.1 Experimental Design

In these tasks, individuals were again presented with 4 tables. Each table contained 10 separate choices, where the individual had to indicate whether they preferred Option A, which typically involved a smaller reward sooner, over Option B, which had a larger reward but would be received further in the future. An excerpt from one of the tables used is presented below. In this example, Option A always involved receipt of R175 that same day. The amounts under Option B were larger but would only be received in a month. In addition, the amounts associated with Option B increased as one moved down the table. This reflected increased interest being added.

Figure 6.1: Snippet of the multiple price list used to elicit risk preferences

Decision	Option A (Pays amount below today)	Option B (Pays amount below in 1 month)	Your Choice (Circle A or B)
1	**R175.00** *+ 10% interest =* *Today*	**R176.46** *1 month*	A B
2	**R175.00** *+ 20% interest =* *Today*	**R177.94** *1 month*	A B
3	**R175.00** *+ 30% interest =* *Today*	**R179.43** *1 month*	A B
4	**R175.00** *+ 40% interest =* *Today*	**R180.93** *1 month*	A B

As with the risk task, participants had to indicate whether they preferred Option A or Option B for every row in each of the 4 tables. Again, given budgetary constraints, individuals were only paid for one of the 40 choices they made. A similar decision rule was used to decide which choice to implement as in the risk task. [1].

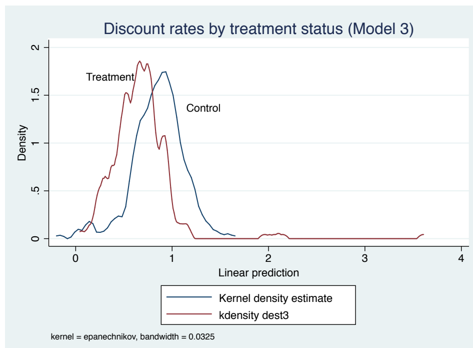
[1]At the end of the session, participants would individually approach the table where the researcher was seated. They were

6.2 Simple discount rate estimates

Chapter 6: Result 1 *Treated subjects exhibit significantly less impatience than control group subjects on average.*

Table 6.1 below presents simple estimates of discount rates generated through the choices made by treatment and control group subjects. The constant term returns the estimate of a discount rate on average. The results in model 1 suggest an annualized discount rate of 75% (compared to 78% for the 2013 cohort). However, there are significant differences between treatment and control group subjects, with model 2 results suggesting that control subjects are significantly more likely to exhibit extreme impatience. This result remains robust in Models 3-6 as additional controls are included. This is evident in the figure below.

Figure 6.2: Distribution of discount rates by treatment



Chapter 6: Result 2 *Treated subjects in WC are especially willing to delay gratification.*

Whilst female subjects are significantly more patient (willing to delay gratification) than their male counterparts, there is no treatment effect in this domain (Model 4). There is some geographic variation though with treated subjects from the Western Cape exhibiting significantly lower rates of impatience compared to their treated colleagues in KZN and the Inland node (Model 5). It is also the case that individuals who have completed some tertiary training are more willing to delay gratification.

Chapter 6: Result 3 *Treated subjects who have had an HIV test display even greater willingness to delay gratification, whilst the converse holds true for control group subjects.*

Finally, in model 6, we include an interaction to examine the interplay between having had an HIV test and an individual's willingness to delay gratification. We interact the variable "HIV test" with treatment status. Interestingly, amongst treated subjects, those who report having had an HIV test exhibit even greater willingness to delay gratification than treated subjects who have not had such a test. However, amongst control group subjects, the opposite holds true. Those who have had an HIV test exhibit higher level of impatience in this task compared to those who have not. In other words, amongst all subjects who had taken a protective measure such as an HIV test, treated subjects appear far more willing to delay gratification than control group subjects.

asked to pick a card from a pile of 8 cards. The cards in the pile were numbered from 1 to 4 (with a duplicate of each) and these numbers corresponded to the Table number. Recall, participants were asked to completed 4 tables, labelled Table 1, Table 2 etc. Once participants had selected a numbered card, this would indicate which table had been selected for payment. Next, the participant would roll the 10 sided die. This would determine which row on the table had been selected for payment. for example, if they die returned a value of 4, this would mean that row 4 (and the option choice made on that row by the participant) was the one chosen for final payment. The participant and researcher would then check that row in the table to see which option the participant had chosen, A or B. Lastly, the participant would be asked to roll the die one last time. In order to win and be paid for their choice, the die had to return a value of "0"

Table 6.1: Discount rate estimates by observed treatment status

	Model1 b/se	Model2 b/se	Model3 b/se	Model4 b/se	Model5 b/se	Model6 b/se
Actual treatment status based on date of interview		-2.8761***	-3.1476***	-3.0862***	-3.1471***	-2.7291***
		(0.362)	(0.300)	(0.314)	(0.298)	(0.342)
Race (1=African)			0.1571	0.1438	0.1112	0.1404
			(0.251)	(0.251)	(0.249)	(0.249)
Female			-0.1606**	-0.0955	-0.1672**	-0.1618**
			(0.071)	(0.096)	(0.071)	(0.071)
Western Cape			0.0470	0.0437	0.2533	0.0496
			(0.110)	(0.108)	(0.163)	(0.108)
KwaZulu-Natal			0.2096*	0.2139*	0.2128*	0.1945*
			(0.111)	(0.112)	(0.130)	(0.108)
Completed matric (1=yes)			-0.1621	-0.1542	-0.1662	-0.1672
			(0.141)	(0.142)	(0.139)	(0.139)
Completed some type of tertiary training after matric (1=yes)			-0.2394*	-0.2365*	-0.2476*	-0.2527*
			(0.144)	(0.144)	(0.142)	(0.142)
Single			0.0516	0.0489	0.0457	0.0726
			(0.180)	(0.183)	(0.182)	(0.178)
Working for pay (1=yes)			0.0936	0.0799	0.1016	0.1015
			(0.148)	(0.149)	(0.145)	(0.142)
Unemployed and actively searching for work (1=yes)			0.0363	0.0227	0.0423	0.0520
			(0.171)	(0.172)	(0.168)	(0.165)
Full time student (1=yes)			0.1212	0.1069	0.1503	0.1610
			(0.154)	(0.156)	(0.155)	(0.150)
Unpaid volunteer (1=yes)			0.0788	0.0640	0.0855	0.1044
			(0.174)	(0.176)	(0.169)	(0.170)
Have had HIV test			0.1436	0.1440	0.1502	0.3273***
			(0.095)	(0.095)	(0.095)	(0.102)
Smoke cigarettes (1=yes)			-0.1576	-0.1489	-0.1503	-0.1366
			(0.115)	(0.115)	(0.115)	(0.112)
No. alcoholic units consumed			-0.0077	-0.0074	-0.0107	-0.0092
			(0.013)	(0.013)	(0.013)	(0.013)
Have had unprotected sex			0.0879	0.0775	0.1021	0.0955
			(0.077)	(0.078)	(0.076)	(0.075)
Treatment x female				-0.1509		
				(0.143)		
Treatment x Western Cape					-0.4618**	
					(0.227)	
Treatment x KZN					0.0096	
					(0.245)	
Treatment x HIV test						-0.5427**
						(0.221)
Constant	0.7539***	3.5499***	3.6010***	3.5601***	3.6276***	3.5277***
	(0.034)	(0.171)	(0.417)	(0.425)	(0.415)	(0.408)
noiseDR	31.6190***	29.8899***	30.3391***	30.3527***	30.2873***	30.0660***
	(1.365)	(1.329)	(1.458)	(1.463)	(1.472)	(1.439)
Observations	22584	22584	20306	20306	20306	20306

1. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.
2. Robust Std. errors reported in parenthesis
3. Treatment=Observed status

Section 7

Loss aversion

We go on to test whether there are any differences in loss aversion between treatment and control subjects. Loss aversion is the tendency for individuals to respond more strongly to losses than equal sized gains relative to a reference point. Loss aversion can be present in risky and riskless choices (Kahneman and Tversky, 1979; Tversky and Kahneman, 1991). An example of loss aversion in risky choices is when individuals reject small gambles that have a positive expected value but may involve losses (Rabin, 2000; Fehr and Goette, 2007). Since we explored the risk preferences of treatment and control group subjects, we chose to supplement this work with a simple loss aversion experiment involving a risky choice[1]. We adopt the experimental procedure used by Gachter et al (2007) because of it's simplicity, and simply change the monetary payoffs accordingly. In this task, participants are offered a series of 20 coin flips which each involve a 50-50 chance of a fixed gain of R100, and losses that vary from R20 to R115. Below we include an excerpt from the decision table. Thus, for the first 16 rows of the table, the expected value of the coin flip is positive, it is zero on row 17, and on rows 18-20, the coin flip has a negative expected value.

Figure 7.1: Coin flip task

ROW	COIN FLIP OFFERED TO YOU	Do you want to flip the coin? (circle YES or NO)	
1.	Heads you win R100; Tails you lose R20.	YES	NO
2.	Heads you win R100; Tails you lose R25.	YES	NO
3.	Heads you win R100; Tails you lose R30.	YES	NO
4.	Heads you win R100; Tails you lose R35.	YES	NO
5.	Heads you win R100; Tails you lose R40.	YES	NO
6.	Heads you win R100; Tails you lose R45.	YES	NO
7.	Heads you win R100; Tails you lose R50.	YES	NO
8.	Heads you win R100; Tails you lose R55.	YES	NO

For each of the twenty choices, participants had to simply indicate if they wanted to flip the coin or not. Participants circled their choices for each of the 20 possibilities. At the end of the session, subjects then rolled 2 ten-sided die, and the sum of the two faces was used to determine which row of the table was eligible for payment. If on that particular row, the participant had circled "No", then no further action was taken since the participant had refused the coin flip, and they could not earn any money from the task. In the event that the participant had circled "Yes" to indicate their willingness to flip the coin, the coin was then flipped and the outcome was implemented.

Rabin (2000) argues that risk aversion cannot plausibly explain choice behaviour in small stake risky prospects such as these. Thus the prediction is that subjects should accept the coin flips that have positive expected values if they behave in accordance with expected utility theory. Gachter et al (2007) argue that any rejection of such small stakes gambles make indicate loss aversion rather than risk aversion. The measure of loss aversion that derives from this task is based on cumulative prospect theory (Tversky and Kahneman,

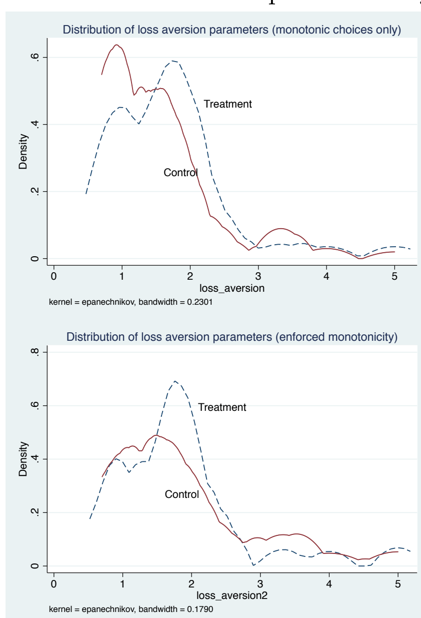
[1] Gachter et al (2007) demonstrate that subjects who exhibit loss aversion in a riskless choice are also more likely to exhibit loss aversion in a risky setting, with a correlation of 0.635.

1992) and reduces very simply to $\alpha = \frac{Gain}{Loss}$

On of the first problems we encounter in this task is the non-monotonicity of choices exhibited by subjects. In others words, subjects make multiple switches between wanting to flip the coin as opposed to not, and this does not proceed linearly with the expected value of the gamble. Just under 30% of subjects exhibit this non-monotonicity of choices, meaning that 71% of subjects make a single switch only. This leaves us with a decision concerning how to calculate the loss aversion co-efficient. We compute 2 measures; the first includes only those individuals that switch once only (exhibit monotonic choices). This reduces the sample by just under a third. As an alternative measure, we include all subjects, and treat their first switch in choice as their only switch. In other words, if an individual chose to flip the coin on Rows 1-2, No flip on Row 3, Flip on Row 4, and then No Flip on Row 5, we treat Row 3 as the point at which they choose not to flip. The advantage of this is that it allows us to include the entire sample, but the downside is that there is considerable error in the data. For completeness, we include both measures.

The figure below presents the distribution of choices by treatment status across the 20 choices, first for only those cases where subjects exhibited monotonic choices, and then for the full sample where we impose monotonicity. The distributions look very similar, except that once monotonicity is imposed, it reduces the distance between the treatment and control group at the lower end of the distribution. This is confirmed by the mean estimates of loss aversion by treatment status under these two measures as well.

Figure 7.2: Distribution of loss aversion parameters by treatment status



Relying only on the subsample who exhibited monotonicity in their choices, the control group has a mean loss aversion parameter of 1.6 compared to 1.9 for the treatment group. In other words, the treatment group exhibits greater loss aversion over these gambles than the control group. These differences are significant at the 1% level (z=-2.98; p=0.00). When we relax monotonicity and increase the sample size, it remains the case that treated subjects exhibit greater loss aversion than control group subjects, but the differences are no longer statistically significant.

Table 7.1: Mean loss aversion estimates by treatment status

	Actual Treatment mean	Assigned treatment mean	Actual treatment mean	Assigned treatment mean
Control	1.641		1.999	
Treatment	1.851		1.983	
Total	1.759		1.990	
Control		1.634		1.984
Treatment		1.869		2.011
Total		1.753		1.997
Observations	366	343	515	486

Chapter 7: Result 1 *Treated subjects are more loss averse than control subjects, although these differences lack significance once selection effects are controlled for.*

Tables 7.2 and 7.3 present multivariate regression estimates of loss aversion using our two different measures of loss aversion. In table 7.2, we present estimates for subjects exhibiting monotonic choices, that is, only a single switch in their choice. Models 1,3 and 5 present OLS estimates using observed treatment status. In all cases, the results suggest that treated subjects are significantly more loss averse than control group subjects. However, these results lose statistical significance in the IV specifications, suggesting that there may be important selection effects into actual treatment status that explain these results. However, it is also the case that females are significantly more loss averse than their male counterparts (Models 1,2, 5 and 6). In Models 3 and 4, we include interaction effects to test whether there is any differential effect for treated females in this regard, but do not find any statistically significant differences. Finally, in Models 5 and 6, we explore geographic variation in loss aversion. We do not find any significant differences in loss aversion by node, nor within node.

Chapter 7: Result 2 *Females are significantly more loss averse than males, but there is no differential treatment effect in this domain.*

Table 7.3 reproduces these results using the measure of loss aversion that does not enforce monotonicity in choices. The qualitative results are no different than those reported in table 7.2. There is a strong gender dimension in loss aversion, and these results suggest that loss aversion is higher amongst unemployed individuals and unpaid volunteers. Given the contrast in the significance of these variables to those in table 7.2, this may indicate that non-monotonic switching was higher amongst unemployed and volunteer individuals in the sample.

Table 7.2: Regression estimates of loss aversion (monotonic choices) by treatment status

	Model1	Model2	Model3	Model4	Model5	Model6
	b/se	b/se	b/se	b/se	b/se	b/se
Actual treatment status based on date of interview	1.0210*	3.3848	1.0215*	3.0149	0.9888*	3.3265
	(0.485)	(4.778)	(0.486)	(4.656)	(0.490)	(4.784)
Female	0.2734**	0.3027***	0.2002	0.1520	0.2844**	0.3139***
	(0.097)	(0.092)	(0.145)	(0.141)	(0.098)	(0.093)
Western Cape	0.1838	0.2341*	0.1853	0.2403*	0.1861	0.2202
	(0.154)	(0.144)	(0.154)	(0.144)	(0.227)	(0.208)
KwaZulu-Natal	0.1547	0.1838	0.1618	0.2018*	0.0173	0.0556
	(0.130)	(0.120)	(0.132)	(0.121)	(0.161)	(0.159)
Completed matric (1=yes)	-0.0199	-0.1294	-0.0226	-0.1265	-0.0043	-0.1132
	(0.334)	(0.343)	(0.333)	(0.341)	(0.336)	(0.342)
Completed some type of tertiary training after matric (1=yes)	-0.1242	-0.2457	-0.1234	-0.2319	-0.0983	-0.2207
	(0.330)	(0.357)	(0.329)	(0.354)	(0.333)	(0.356)
Single	-0.0179	0.5503	-0.0023	0.5357	-0.0270	0.5410
	(0.313)	(0.945)	(0.315)	(0.941)	(0.318)	(0.948)
Working for pay (1=yes)	-0.0063	0.1212	-0.0003	0.1318	-0.0200	0.1075
	(0.195)	(0.198)	(0.196)	(0.199)	(0.199)	(0.202)
Unemployed and actively searching for work (1=yes)	0.1527	0.2685	0.1520	0.2634	0.1439	0.2596
	(0.245)	(0.227)	(0.245)	(0.227)	(0.244)	(0.226)
Full time student (1=yes)	0.0363	0.1729	0.0437	0.1843	0.0379	0.1729
	(0.195)	(0.197)	(0.195)	(0.199)	(0.199)	(0.200)
Unpaid volunteer (1=yes)	0.1352	0.1821	0.1380	0.1800	0.1308	0.1730
	(0.261)	(0.218)	(0.261)	(0.219)	(0.264)	(0.221)
Treatment x female			0.1292	0.2740		
			(0.202)	(0.205)		
Treatment x Western Cape					-0.0057	0.0248
					(0.306)	(0.321)
Treatment x KZN					0.2487	0.2366
					(0.259)	(0.258)
Constant	1.9604***	1.2083	1.9590***	1.2814	1.9667***	1.2175
	(0.520)	(1.294)	(0.518)	(1.280)	(0.528)	(1.299)
Observations	332	322	332	322	332	322

1. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.
2. Robust Std. errors reported in parenthesis
3. Models 1, 3 & 5 are OLS estimates that rely on actual treatment status
4. Models 2, 4 & 6 are two stage least squares regression that uses assigned treatment status as an instrument for observed treatment status

Table 7.3: Regression estimates of loss aversion (relaxed monotonicity) by treatment status

	Model1	Model2	Model3	Model4	Model5	Model6
	b/se	b/se	b/se	b/se	b/se	b/se
Actual treatment status based on date of interview	0.4677	1.6494	0.4978	2.0138	0.4715	1.6413
	(0.495)	(5.693)	(0.508)	(5.788)	(0.500)	(5.712)
Female	0.3065**	0.3330***	0.3377*	0.4119**	0.3050**	0.3321***
	(0.095)	(0.095)	(0.149)	(0.153)	(0.096)	(0.095)
Western Cape	−0.0261	−0.0038	−0.0263	−0.0060	0.0649	0.0290
	(0.140)	(0.134)	(0.140)	(0.135)	(0.192)	(0.197)
KwaZulu-Natal	0.0920	0.1070	0.0916	0.1046	0.1878	0.1365
	(0.129)	(0.122)	(0.129)	(0.123)	(0.184)	(0.181)
Completed matric (1=yes)	−0.0673	−0.0713	−0.0629	−0.0603	−0.0809	−0.0757
	(0.286)	(0.270)	(0.287)	(0.271)	(0.287)	(0.271)
Completed some type of tertiary training after matric (1=yes)	−0.1313	−0.1496	−0.1291	−0.1457	−0.1449	−0.1538
	(0.290)	(0.277)	(0.291)	(0.277)	(0.292)	(0.278)
Single	−0.1519	−0.1230	−0.1526	−0.1178	−0.1457	−0.1216
	(0.226)	(0.309)	(0.225)	(0.308)	(0.228)	(0.309)
Working for pay (1=yes)	0.0958	0.1549	0.0923	0.1461	0.1006	0.1562
	(0.188)	(0.182)	(0.187)	(0.180)	(0.188)	(0.182)
Unemployed and actively searching for work (1=yes)	0.3894*	0.4537*	0.3885*	0.4518*	0.3836*	0.4516*
	(0.217)	(0.206)	(0.217)	(0.205)	(0.218)	(0.207)
Full time student (1=yes)	0.0145	0.0903	0.0109	0.0818	0.0191	0.0916
	(0.192)	(0.191)	(0.192)	(0.190)	(0.193)	(0.192)
Unpaid volunteer (1=yes)	0.4968**	0.5532*	0.4944**	0.5486*	0.4947*	0.5529*
	(0.245)	(0.231)	(0.245)	(0.230)	(0.245)	(0.232)
Treatment x female			−0.0597	−0.1506		
			(0.195)	(0.203)		
Treatment x Western Cape					−0.1696	−0.0623
					(0.280)	(0.307)
Treatment x KZN					−0.1962	−0.0614
					(0.257)	(0.279)
Constant	1.8981***	1.0405	1.8669***	0.7648	1.9025***	1.0487
	(0.353)	(3.975)	(0.373)	(4.066)	(0.358)	(3.994)
Observations	471	459	471	459	471	459

Section 8

Civic engagement

8.1 Project activities (past year)

In addition to the experiments, participants were interviewed at baseline and endline, during which time additional attitudinal data as well as data on civic participation, trust, and social connectedness were elicited.

Chapter 8: Result 1 *Project involvement by treated subjects after a year of exposure to the programme increases relative to the control group (with no exposure)*

Table 8.1 below presents summary statistics for project activities engaged in by treatment and control group subjects in the preceding year. These questions were asked at baseline and endline for both groups. However, it is important to note that the endline measures for the Treatment group reflect their answers at the completion of the first year of Activate, whilst for the control group, this reflects their answers just prior to their entry into the first year of Activate!. Thus, a comparison of the difference in means at endline is useful in highlighting whether a treatment effect exists or not. However, it may also be instructive to compare the change in outcomes over time relative to the baseline interviews.

The data from the simple unmatched tests of significance suggest that at baseline, treated subjects were significantly less likely to have initiated a community project in the year preceding the baseline interview. At endline, there are no significant differences between treatment and control subjects in this domain, suggesting that a year of exposure to the programme has assisted treatment subjects to close this gap. Indeed, it is worth noting that community engagement amongst control group participants declines over time, whilst for treated subjects, it increases.

Table 8.1: Project engagement by Treatment

	Baseline		Endline	
	Control mean	Treatment mean	Control mean	Treatment mean
Project involvement in last year	0.822	0.780*	0.788	0.825
Borrowed money to finance project	0.215	0.151	0.167	0.174
Amt. borrowed	1893.6	1581.7	3635.5	2593.8
Written project proposal in last year	0.532	0.478	0.535	0.538
Observations	549		610	

1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$.

8.2 Civic participation (past 12 months)

Chapter 8: Result 2 *Treated subjects report significantly higher levels of civic participation in some domains compared to control group subjects.*

Table 8.2 below presents summary statistics relating to civic participation by treatment and control group participants at baseline and endline. Civic participation is measured through the reported propensity to attend a community meeting, engage a local politician or alert the media to a local problem and other such activities. With the exception of media engagement, at baseline, there are no significant differences in the propensity of treatment and control group subjects to engage in these kinds of civic participation activities. Moreover, it is notable that at baseline, subjects in the treatment group are significantly less likely to report that they had engaged the media during the preceding year. In contrast, at endline, treated subjects are significantly more likely to report that they had attended a community meeting in the preceding 12 months as well as having engaged a politician, and having organised a community meeting or initiative.

Table 8.2: Civic participation by Treatment

	Baseline		Endline	
	Control mean	Treatment mean	Control mean	Treatment mean
Attend community mtg in past year	0.760	0.806	0.674	0.826***
Engaged politician in past year	0.429	0.469	0.491	0.599***
Participated in protest in past year	0.288	0.253	0.241	0.272
Alerted media to issue in past year	0.349	0.245***	0.301	0.300
Notified police of issue in past year	0.321	0.377	0.301	0.281
Organised community meeting/initative in past year	0.498	0.513	0.511	0.615***
Observations	548		609	

1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$.

8.3 Organisational involvement and leadership

Chapter 8: Result 3 *There are no strong treatment effects in terms of organisational involvement and leadership.*

Participants were also asked to report their levels of organisational involvement, and whether or not they played a leadership role in the organisation (Table 8.3). The extent of organisational involvement varies by organisation type and appears to be mostly stable over time, with few significant differences between treatment and control group subjects. However, in a few instances, apparent treatment effects are evident. For example, at baseline, significantly fewer treated subjects reported being involved in volunteer activities compared to control group subjects. By endline, this difference had disappeared, and indeed, control group volunteering had declined whilst it had increased amongst the treated group. In contrast, however, control group subjects were significantly more likely to report playing a leadership role in such volunteer groups compared to treated subjects at endline.

Interestingly, at endline, we observe a significant difference in leadership roles in stokvels for treated subjects compared to control group subjects. Conversely, we see a significant decline in political leadership roles being assumed by treated subjects at endline compared to baseline.

Table 8.3: Organisational Associations by Treatment

	Baseline		Endline	
	Control mean	Treatment mean	Control mean	Treatment mean
Church/Mosque	0.833	0.846	0.788	0.777
Mucis grp.	0.359	0.390	0.336	0.368
Sports grp	0.366	0.359	0.360	0.346
Volunteer grp	0.819	0.744**	0.788	0.832
Stokvel	0.102	0.132	0.177	0.138
Political party	0.341	0.385	0.314	0.373
Youth grp.	0.731	0.722	0.749	0.786
Church leader	0.511	0.528	0.495	0.492
Music grp. leader	0.606	0.660	0.649	0.600
Sports grp. leader	0.730	0.571**	0.598	0.602
Volunteer grp. leader	0.733	0.724	0.758	0.686*
Stokvel leader	0.393	0.333	0.300	0.467*
Political leader	0.266	0.408**	0.337	0.369
Youth grp. leader	0.729	0.689	0.652	0.646
Observations	549		610	

1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$.

8.4 Resource Generator

One area of innovation in the 2013 study was the inclusion of elements of the Resource Generator in the post-task questionnaire as a means of trying to quantify the social capital of young people. The social capital of an individual is a measure of the "collection of resources owned by members of an individual's personal social network, which may become available to the individual..." (van der Gaag and Snijders, 2005). The simple idea is that the possibility that an individual is able attain their goals may depend on the kinds of resources, skills and help they are able to access through their personal social networks. To this end, the Resource Generator asks about access to a fixed list of resources, which each represent different aspects of social capital, namely human, cultural, financial, political and physical capital.

Chapter 8: Result 4 *There are no treatment effects evident on the diversity of network resources, although treatment group subjects report improved access to financial contacts at endline relative to baseline.*

We use three sets of edited tables from the resource generator. We discuss each of these in turn. The first table asked subjects about the occupations of people in their social networks, as a way to gauge the diversity, quality and social prestige of the skill sets that each individual might be able to access. For each occupation type, subjects had to record whether the person they knew in this category was a family member, friend, or acquaintance.[1]

We present 5 tables below that analyse the results from this data in different ways. Table 8.4 presents the average number of contact mentions across the three categories reported by subjects. To be clear, if an individual indicated that they knew a family member who was a doctor, this would count as one mention. If they indicated that they knew a family member and a friend who were doctors, this would count as two mentions, and so on. Hence, this variable is bound between 0 (for no mentions at all) and 3 (where the subject said they knew at least 3 people in that category, spanning a family member, friend ands acquaintance).

With the exception of a member of provincial/national government, there are no significant differences in the number of contacts reported by treatment and control group subjects at baseline. Moreover, whilst all subjects report greater numbers of contacts at endline, there are no significant differences in contact availability at endline either.

Table 8.4: Average number of contact types who are.....

	Baseline		Endline	
	Control	Treatment	Control	Treatment
Member of provinicial/national government	0.529	0.618*	0.788	0.798
Teacher	1.179	1.185	1.528	1.557
Accountant	0.515	0.550	0.773	0.798
Musician/Artist/Writer	0.912	0.956	1.180	1.270
Councillor in municipality	0.729	0.757	0.834	0.898
School principal	0.839	0.868	0.972	1.021
Librarian	0.526	0.478	0.680	0.645
Journalist/Reporter/Works in media	0.628	0.636	0.848	0.948
Bank manager	0.300	0.277	0.387	0.419
Business Owner	1.187	1.129	1.574	1.526

1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$.

Table 8.5 presents an alternative, and perhaps more intuitive, measure of this data, coding the data to reflect whether or not the subject knew at least one person in a given category. For example, at baseline, 47% of control group subjects reported knowing at least one person in provincial or national government, compared to 55% of the treatment group. This difference is significant at baseline but disappears by endline. Conversely, control group subjects were significantly more likely to report knowing a business owner compared to the treatment group at baseline. This difference disappears by endline with treatment group subjects showing a larger increase in this regard. Finally, at endline, treatment group subjects are significantly more likely to report knowing at least one teacher compared to control group subjects. On all other measures, there are no significant differences. As was the case in 2013, this suggests that the programme itself is not playing any significant role in shaping or changing the kinds of occupational resources (with the exception of teachers perhaps) that Activators are accessing having been through the programme.

[1] Following Van der Gaag and Snijders, it was made clear that an acquaintance was someone that the subject knew by name and could initiate a conversation with upon meeting them in the street.

Table 8.5: Person knows at least one person who is.....

	Baseline		Endline	
	Control mean	Treatment mean	Control mean	Treatment mean
Member of provincial/national govt.	0.467	0.548*	0.615	0.654
Teacher	0.967	0.956	0.968	0.991**
Accountant	0.493	0.498	0.631	0.661
Musician/Artist/Writer	0.759	0.750	0.852	0.862
Councillor in municipality	0.689	0.710	0.777	0.794
School principal	0.785	0.801	0.879	0.875
Librarian	0.500	0.452	0.601	0.587
Journalist/Reporter/Works in media	0.562	0.574	0.702	0.758
Bank manager	0.282	0.262	0.351	0.367
Business owner	0.890	0.838*	0.940	0.942

1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$.

Table 8.6 and Table 8.7 present the same data, but make a distinction between whether or not the contact mentioned constitutes a strong tie (family member or friend) or a weak tie (acquaintance). A rich sociological literature argues that weak ties may be more useful in promoting access to new opportunities which is why we explore this dimension.

There are a few differences here. With respect to strong ties, (Table 8.6) at baseline, for the most part, there are no significant differences between treatment and control subjects, with the exception of access to national/provincial politicians and school principals. By endline, these differences disappear, due to the control group reporting improved access in these domains relative to baseline. However, at endline, treated subjects are significantly more likely to report having at least one strong tie who is an accountant or a musician/artist/writer than their control group counterparts. On all other fronts, whilst treated subjects report slightly better access on average, none of the differences are significant.

Table 8.6: Person has at least one strong tie who is a....

	Baseline		Endline	
	Control mean	Treatment mean	Control mean	Treatment mean
Member of provinicial/national government	0.237	0.324**	0.318	0.324
Teacher	0.736	0.734	0.830	0.829
Accountant	0.347	0.384	0.436	0.505*
Musician/Artist/Writer	0.584	0.581	0.643	0.718**
Councillor in municipality	0.267	0.239	0.254	0.265
School principal	0.288	0.357*	0.351	0.358
Librarian	0.259	0.283	0.270	0.318
Journalist/Reporter/Works in media	0.405	0.382	0.496	0.528
Bank manager	0.132	0.151	0.177	0.193
Business Owner	0.670	0.658	0.748	0.752

1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$.

With respect to weak ties, (Table 8.7), at baseline, treated subjects are significantly less likely to report having access to librarians or a bank manager compared to control group subjects. Differential access to librarians remains at endline, but the difference in access to bank managers disappears, mainly due to improved access being reported by treated subjects. In addition, at endline, treated subjects are significantly more likely to report having at least one weak tie who is a councillor in a municipality. This is consistent with the earlier results on civic participation.

Table 8.7: Person has at least one weak tie who is a....

	Baseline		Endline	
	Control mean	Treatment mean	Control mean	Treatment mean
Member of provinicial/national government	0.263	0.261	0.396	0.419
Teacher	0.337	0.328	0.426	0.471
Accountant	0.153	0.140	0.291	0.239
Musician/Artist/Writer	0.266	0.261	0.378	0.371
Councillor in municipality	0.447	0.489	0.551	0.618*
School principal	0.529	0.489	0.582	0.618
Librarian	0.255	0.188*	0.388	0.315*
Journalist/Reporter/Works in media	0.201	0.232	0.326	0.371
Bank manager	0.165	0.114*	0.188	0.214
Business Owner	0.363	0.309	0.496	0.492

1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$.

Table 8.8 presents a summary measure that aggregates across the 15 occupational categories. For example, at baseline, all subjects made mention of just over 7 contacts in total out of a potential of 45, and there was no significant difference by treatment status. In any given category, both treatment and control subjects report fewer than one contact, and there are no treatment effects in any dimension.

Table 8.8: Averages

	Baseline		Endline	
	Control mean	Treatment mean	Control mean	Treatment mean
Total no. of contact types	7.367	7.409	9.556	9.877
Avge no. of contact types	0.737	0.741	0.956	0.988
Total weighted contacts	0.588	0.601	0.755	0.781
Avge. no. strong ties	0.394	0.407	0.453	0.477
Avge no. of weak ties	0.298	0.280	0.401	0.414

1. *** $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

In Table 8.9, we present summary measures by contact occupational type, disaggregated by category to distinguish different possible forms of social capital, namely, financial, service, cultural, educational and political contacts. In making these distinctions, we group the 15 categories from the resource generator list as follows:

1. Service: includes Doctor, Engineer, IT worker/consultant, Hairdresser
2. Political: includes Member of prov. or national govt; Councillor in municipality; NGO worker
3. Educational: includes Teacher; School principal; Librarian
4. Financial: includes Accountant; Business Owner; Bank manager
5. Cultural: includes Musician/Artist/Writer; Journalist/Reporter/Works in media

We analyse the results from this data in different ways. Below, we define what each of the different measures means:

1. Total number of contact types reports the total number of contact mentions reported by subjects across the occupational categories. For example, in the service occupational categories, if an individual indicated that they knew a family member who was a doctor, this would count as one mention. If they indicated that they knew a family member and a friend who were doctors, this would count as two mentions, and so on. Hence, for any particular occupational category (e.g. doctor, teacher etc) this variable is bound between 0 (for no mentions at all) and 3 (where the subject said they knew at least 3 people in that category, spanning a family member, friend and an acquaintance). To construct this total variable, we add up the number of mentions across the occupational categories in that group.

For example, at baseline, both treatment and control subjects report between 0 and 1 contacts on the questions relating to individuals in cultural occupations.

2. An alternative, and perhaps more intuitive, measure of this data, is given by the measure "At least one contact". Here we code the data to reflect whether or not the subject knew at least one person in a given category. For example, at baseline, 79% of control group subjects reported knowing at least one contact in a cultural occupation, compared to 80% of treated subjects.

3. A further measure, the average number of contact types, reports the average number of mentions within a category grouping. For example, in the service category, the total number of possible mentions that any individual could give would be 12 (one family member, one friend and one acquaintance in each occupation). If an individual reported that they knew a friend who was a doctor, a family member who was an engineer, and an acquaintance who was a hairdresser, their average for this category would be $0,25$.

4. Finally, we also make a distinction between whether or not the contact mentioned constitutes a strong tie (family member or friend) or a weak tie (acquaintance).

Once again, the results confirm that at baseline, there are no significant differences in the kinds of occupational resources that subjects allocated to treatment are able to access compared to the control group (with the exception being strong cultural ties). This remains the case at endline, with the exception that treated subjects report significantly more weak financial ties that they are able to access.

Table 8.9: Averages by occupational contact type

	Baseline		Endline	
	Control mean	Treatment mean	Control mean	Treatment mean
Total no. of cultural contact types	0.839	0.868	0.972	1.021
Avge no. of cultural contact types	0.839	0.868	0.972	1.021
Avge. no. cultural strong ties	0.288	0.357*	0.351	0.358
Avge no. of cultural weak ties	0.529	0.489	0.582	0.618
At least one cultural contact (avge)	0.785	0.801	0.879	0.875
Total no. of service contact types	2.864	2.923	3.846	3.948
Avge no. of service contact types	0.716	0.731	0.962	0.987
Avge. no. service strong ties	0.410	0.432	0.479	0.498
Avge no. of service weak ties	0.264	0.253	0.384	0.395
At least one service contact (avge)	0.625	0.634	0.723	0.747
Total no. of political contact types	0.813	0.822	1.153	1.217
Avge no. of political contact types	0.407	0.411	0.577	0.609
Avge. no. political strong ties	0.240	0.267	0.306	0.349
Avge no. of political weak ties	0.158	0.126	0.238	0.226
At least one political contact (avge)	0.386	0.380	0.491	0.514
Total no. of educational contact types	2.099	2.085	2.752	2.798
Avge no. of educational contact types	1.049	1.042	1.376	1.399
Avge. no. educational strong ties	0.626	0.619	0.695	0.735
Avge no. of educational weak ties	0.315	0.285	0.436	0.433
At least one educational contact (avge)	0.824	0.794	0.895	0.902
Total no. of financial contact types	0.729	0.757	0.834	0.898
Avge no. of financial contact types	0.729	0.757	0.834	0.898
Avge. no. financial strong ties	0.267	0.239	0.254	0.265
Avge no. of financial weak ties	0.447	0.489	0.551	0.618*
At least one financial contact (avge)	0.689	0.710	0.777	0.794

1. *** $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The final set of tables (Tables 8.10 through 8.13) ask a series of questions about contacts who could provide help (in the form of advice or support). In Table 8.10, at baseline, subjects assigned to treatment are significantly more likely to report having contacts who could provide advice about work conflict or give legal advice. However, these differences cease to be significant at endline. However, at endline, treated subjects are significantly more likely to report having contacts with whom they could discuss politics.

Table 8.11 presents the same data but refines the definition to be that the individual knew at least one person who could help in a specific area. Whilst it remains the case that the baseline advantages that subjects assigned to treatment report disappear by endline, it is also the case that treated subjects are significantly more likely to report having at least one contact with whom they could discuss politics or who would provide help with a new project.

Table 8.10: Average no. of contact types who can help to...

	Baseline		Endline	
	Control mean	Treatment mean	Control mean	Treatment mean
Find holiday job for you/friend	1.233	1.243	1.613	1.613
Give advice about work conflict	1.344	1.473*	1.670	1.709
Lend large sum of money	0.600	0.781	0.925	0.911
Give advice about family conflict	1.189	1.243	1.441	1.411
Discuss politics	0.900	1.054	1.071	1.251**
Give advice re law	1.044	1.324***	1.367	1.402
Give you a good job reference	1.389	1.419	1.689	1.678
Help with new project	1.400	1.554	1.785	1.759
Give access to computer/internet	1.533	1.689	1.860	1.880

1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$. %

Table 8.11: Person knows at least one person who can help to...

	Baseline		Endline	
	Control mean	Treatment mean	Control mean	Treatment mean
Find holiday job for you/friend	0.778	0.797	0.848	0.859
Give advice about work conflict	0.944	0.946	0.961	0.963
Lend large sum of money	0.533	0.671*	0.654	0.657
Give advice about family conflict	0.944	0.973	0.975	0.960
Discuss politics	0.667	0.770	0.711	0.801***
Give advice re law	0.811	0.932*	0.915	0.939
Give you a good job reference	0.956	0.932	0.971	0.972
Help with new project	0.911	0.932	0.932	0.972**
Give access to computer/internet	0.967	0.959	0.971	0.972

1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$. %

Tables 8.12 and 8.13 break the data down to distinguish between weak and strong ties. At baseline, subjects allocated to treatment are significantly more likely to have at least one strong tie who is able to lend them a large sum of money (this accords with the earlier results regarding project financing) or offer legal advice. In contrast, they are significantly less likely to have a strong tie who is able to provide them with a good job reference. This difference remains in place at endline albeit the magnitude of the difference is considerably smaller than at baseline. Differences in the other two domains disappear by endline. However, at endline, treated subjects are significantly more likely to have at least one strong tie with whom they can discuss politics or gain access to a computer or internet facilities.

With respect to weak ties (Table 8.13), there are very few differences between treatment and control subjects. However, subjects allocated to treatment are significantly more likely to report having at least one weak tie who could give them a good job reference at baseline, and whilst this advantage remains at endline, the magnitude of the difference is reduced. In addition, at endline, treated subjects are significantly more likely to report having a weak tie who is able to assist them with a new project.

Table 8.12: Person knows at least one strong tie who can help to...

	Baseline		Endline	
	Control mean	Treatment mean	Control mean	Treatment mean
Find holiday job for you/friend	0.644	0.689	0.741	0.745
Give advice about work conflict	0.867	0.851	0.894	0.887
Lend large sum of money	0.411	0.616***	0.568	0.560
Give advice about family conflict	0.867	0.919	0.872	0.899
Discuss politics	0.589	0.622	0.596	0.700***
Give advice re law	0.589	0.743**	0.769	0.791
Give you a good job reference	0.722	0.568**	0.796	0.733*
Help with new project	0.756	0.757	0.842	0.823
Give access to computer/internet	0.889	0.892	0.892	0.934*

1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$. %

Table 8.13: Person knows at least one weak tie who can help to...

	Baseline		Endline	
	Control mean	Treatment mean	Control mean	Treatment mean
Find holiday job for you/friend	0.333	0.324	0.436	0.488
Give advice about work conflict	0.200	0.230	0.333	0.380
Lend large sum of money	0.122	0.110	0.175	0.193
Give advice about family conflict	0.189	0.135	0.256	0.255
Discuss politics	0.178	0.257	0.250	0.309
Give advice re law	0.333	0.311	0.356	0.371
Give you a good job reference	0.422	0.581**	0.546	0.620*
Help with new project	0.478	0.486	0.538	0.608*
Give access to computer/internet	0.344	0.338	0.475	0.440

1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$. %

Section 9

Labour Market Outcomes

The term "active labour market programme" is used specifically to describe programmes that are designed to move marginalised populations from a strong reliance on welfare recipiency and unemployment benefits, to a situation where they take active steps to find employment. Often these programmes involve some type of basic training programme aimed at improving the (soft) skills of the unemployed, thus bolstering their employability. While the scale and scope of these types of programme vary from country to country, an intermediate outcome that programmes of this sort seek to affect, is in the attitudes of the participants towards wanting to engage with the labour market more actively; say through more active search.

At first glance, a programme like Activate! is not obviously an "active labour market" programme: i.e., it is not first and foremost a programme about moving people in the direction of participating more actively in the labour market. However, it is possible that this might occur because the Activate! programme is broadly about empowering participants to be "change drivers", in their own lives to start with, and then society more broadly. While it is without doubt that the programme seeks to do this primarily through the medium of encouraging a feeling of optimism for change, it is possible that this optimism translates into activism (both in the personal lives of the participants themselves) as well as the impact they have on their communities and the lives of others.

In section we examine the personal dimension of this potential type of impact of the programme; namely the labour market effects. We present impacts on several labour market outcomes. Both at baseline and endline, we asked detailed questions on employment, probing a variety of possible types of employment an individual could conceivable hold simultaneously. We probed 4 specific types of employment that (predominantly urban) young people are likely to have exposure to: regular employment (both as a first job and as a second job), where the employment is predictable and a salary is received (whether full time or part-time); self-employment and casual employment. in some cases, only one of these categories of employment will have been applicable to any given individual. However in most cases, individuals are observed to engage in a multitude of activities.

We start by presenting the descriptive statistics for all the wage and employment variables in table 9.1. Table 9.2 reports the t-tests of differences in means. The general picture is that there are no statistically significant differences between treatment and control at baseline at either baseline or endline. The finding that there are no significant differences by treatment status at baseline indicates that the randomisaiton successfully balanced out these effects. The lack of statistically significant differences at endline however, indicates that the programme had no labour market impacts. However, to properly model the treatment effect, we should account for the selective compliance in treatment status. In table 9.3 (Models 1-6) we present estimates of the programme's impact on these 6 different labour force participation variables. These results are based on a bivariate probit framework where we first model the probability of being treated, using the randomly assigned treatment status as an instrumental variable for treatment status. These results are reported in the bottom half of the table. As is clearly indicated, the randomised treatment assignment is a strong predicted of actual treatment status: being randomised into the treatment group increases the probability of realised treatment by about 90%. This is not that surprising: for those in the study sample that took up their offers of a place in the programme (in either 2014 or 2015) and (in the case of the treatment group) had successfully completed the programme, compliance with assigned treatment status was about

95%.

Table 9.1: Summary Statistics: Labour Market Participation

	Baseline			Endline		
	0	1	Total	0	1	Total
Regular employment: 1st job (1=yes)	0.408	0.402	0.405	0.433	0.381	0.405
	(0.492)	(0.491)	(0.491)	(0.496)	(0.486)	(0.491)
Regular employment: 2nd job (1=yes)	0.0667	0.0417	0.0533	0.0738	0.113	0.0935
	(0.251)	(0.201)	(0.225)	(0.262)	(0.318)	(0.292)
Self-employment (1=yes)	0.246	0.263	0.255	0.294	0.320	0.308
	(0.432)	(0.441)	(0.436)	(0.457)	(0.467)	(0.462)
Casual employment (1=yes)	0.238	0.249	0.244	0.238	0.250	0.244
	(0.427)	(0.433)	(0.430)	(0.426)	(0.434)	(0.430)
empstatus	4.550	4.781	4.674	3.819	4.183	4.015
	(3.575)	(3.747)	(3.667)	(3.637)	(3.615)	(3.627)
Subjective expectations of fair wages	6000	6559.1	6315.4	8478.3	8368.1	8422.0
	(4703.7)	(4603.9)	(4594.4)	(6397.5)	(11960.9)	(9614.4)
Monthly hours worked	78.15	74.58	76.23	94.51	93.06	93.73
	(75.31)	(73.55)	(74.33)	(71.58)	(69.69)	(70.51)
Monthly gross earnings	3081.4	3246.7	3168.6	4654.5	3795.0	4186.5
	(3985.5)	(5139.4)	(4625.4)	(5205.0)	(4969.6)	(5091.1)
Monthly hours worked (IHST)	3.397	3.304	3.347	4.143	4.219	4.184
	(2.573)	(2.600)	(2.586)	(2.250)	(2.149)	(2.194)
Monthly gross earnings (IHST)	7.701	7.661	7.680	7.513	7.014	7.241
	(2.179)	(2.394)	(2.292)	(3.281)	(3.476)	(3.395)

Standard deviations reported in parenthesis.
IHST = inverse hyperbolic sine transformation

Table 9.2: T-tests: Labour Market Participation

	Baseline		Endline	
Regular employment: 1st job (1=yes)	0.00570	(0.14)	0.0515	(1.29)
Regular employment: 2nd job (1=yes)	0.0250	(0.83)	-0.0391	(-1.05)
Self-employment (1=yes)	-0.0172	(-0.46)	-0.0258	(-0.69)
Casual employment (1=yes)	-0.0107	(-0.29)	-0.0124	(-0.36)
empstatus	-0.231	(-0.74)	-0.364	(-1.24)
Subjective expectations of fair wages	-559.1	(-0.37)	110.3	(0.07)
Monthly hours worked	3.573	(0.59)	1.449	(0.25)
Monthly gross earnings	-165.3	(-0.36)	859.5	(1.87)
Monthly hours worked (IHST)	0.0933	(0.44)	-0.0760	(-0.43)
Monthly gross earnings (IHST)	0.0401	(0.17)	0.499	(1.63)
Observations	610		610	

1. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
2. Std. errors in parenthesis

However we find no statistically significant effects on labour force participation. On the other hand, there is some evidence that the programme did have an effect on labour supply. Table 9.4 shows the estimated impacts on wages and hours worked. While there is no evidence to suggest that earnings are responsive to treatment, there is evidence of a labour supply. Model 4 shows that the programme causes women to increase their hours worked by about 59% ($\exp(0.4615) - 1 = 0.586$). This is a very large effect. We expect that this could be attributed to elements of the programme that might be having a strong empowering effect differentially by gender.

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Table 9.3: Impact of the Activate! Programme on Employment (Bivariate Probit)

	Model1 b/se	Model2 b/se	Model3 b/se	Model4 b/se	Model5 b/se	Model6 b/se
treatment	-0.1670	0.4324	-0.0521	-0.0828	-0.2320*	0.0640
	(0.123)	(0.285)	(0.127)	(0.131)	(0.123)	(0.149)
Completed matric (1=yes)	0.1513	-0.8758	-0.2051	-0.0256	-0.3354	-0.1064
	(0.283)	(0.544)	(0.276)	(0.290)	(0.274)	(0.322)
Completed some type of tertiary training after matric (1=yes)	0.6320**	-0.8899	-0.0007	0.0575	0.2512	-0.0662
	(0.287)	(0.545)	(0.280)	(0.294)	(0.280)	(0.327)
Excellent health (1=yes)	0.1393	0.1948	-0.0254	-0.1185	-0.0901	0.1569
	(0.110)	(0.238)	(0.114)	(0.118)	(0.111)	(0.133)
Are you currently enrolled in any school, classes or correspondence courses of a	-0.0102	0.1921	0.1579	0.0581	-0.4573***	-0.4493***
	(0.110)	(0.238)	(0.114)	(0.117)	(0.110)	(0.138)
Risk						0.0080
						(0.020)
Constant	-0.5473*	-0.8783	-0.4706*	-0.6157**	0.5721**	-0.8877***
	(0.287)	(0.555)	(0.280)	(0.293)	(0.280)	(0.323)
Completed matric (1=yes)	-0.0137	0.0846	-0.0137	-0.0137	-0.0137	-0.0141
	(0.048)	(0.107)	(0.048)	(0.048)	(0.048)	(0.048)
Completed some type of tertiary training after matric (1=yes)	0.0215	0.1142	0.0215	0.0215	0.0215	0.0211
	(0.048)	(0.106)	(0.048)	(0.048)	(0.048)	(0.048)
Excellent health (1=yes)	0.0060	0.0338	0.0060	0.0060	0.0060	0.0056
	(0.019)	(0.036)	(0.019)	(0.019)	(0.019)	(0.019)
Are you currently enrolled in any school, classes or correspondence courses of a	0.0380**	0.0202	0.0380**	0.0380**	0.0380**	0.0383**
	(0.019)	(0.037)	(0.019)	(0.019)	(0.019)	(0.019)
Assigned treatment status	0.8926***	0.8435***	0.8926***	0.8926***	0.8926***	0.8926***
	(0.019)	(0.037)	(0.019)	(0.019)	(0.019)	(0.019)
Risk						0.0009
						(0.003)
Constant	0.0618	-0.0133	0.0618	0.0618	0.0618	0.0614
	(0.048)	(0.108)	(0.048)	(0.048)	(0.048)	(0.048)
athrho2_1	0.0356	-0.1818	0.0437	0.0815	0.1085*	-0.0932
	(0.060)	(0.171)	(0.062)	(0.065)	(0.064)	(0.084)
lnsigma2	-1.4957***	-1.3105***	-1.4957***	-1.4957***	-1.4957***	-1.4957***
	(0.030)	(0.047)	(0.030)	(0.030)	(0.030)	(0.030)
Observations	549	222	549	549	549	549

1. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Std. errors reported in parenthesis.
In all models, realised treatment status is instrumented with randomly assigned treatment status
The first block of coefficients are for the outcome equation (i.e., conditional probability of employment in [. . .]. The second block of coefficients are for the selection equation (i.e., conditional probability of treatment where randomly assigned treatment serves as the key exclusion restriction).
Model 1 dependant variable: regular employment: first job (1=yes)
Model 2 dependant variable: regular employment: second job (1=yes)
Model 3 dependant variable: self employment (1=yes)
Model 4 dependant variable: casual employment (1=yes)
Model 5 dependant variable: any type of employment (1=yes)
Model 5 dependant variable: unemployed but searching (1=yes)

Table 9.4: Impact of the Activate! Programme on Earnings and Labour Supply (Bivariate Probit)

	Model1 b/se	Model2 b/se	Model3 b/se	Model4 b/se
treatment	-1.05e+03**	-0.6201*	-6.9604	-0.1358
	(526.043)	(0.366)	(6.617)	(0.207)
_ws_gender	862.0972	0.0088	10.3506	0.5423**
	(696.604)	(0.485)	(8.692)	(0.271)
Completed matric (1=yes)	469.4869	0.1499	-4.1332	-0.1522
	(1165.628)	(0.811)	(14.638)	(0.457)
Completed some type of tertiary training after matric (1=yes)	2576.3523**	1.1443	25.0667*	0.6322
	(1178.468)	(0.820)	(14.883)	(0.465)
Excellent health (1=yes)	219.1894	-0.1749	3.5553	0.0558
	(468.071)	(0.326)	(5.941)	(0.186)
Are you currently enrolled in any school, classes or correspondence courses of a	934.4216**	0.4491	-11.8958**	-0.3827**
	(467.188)	(0.325)	(5.931)	(0.185)
Constant	2754.5058**	6.8178***	92.8021***	4.2283***
	(1183.563)	(0.823)	(14.856)	(0.464)
Observations	445	445	549	549

*** $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Std. errors reported in parenthesis.
The first block of coefficients are for the outcome equation (i.e., the conditional mean of earnings or labour supply).
The second block of coefficients are for the selection equation (i.e., conditional probability of treatment where randomly assigned treatment serves as the key exclusion restriction).
In all models, realised treatment status is instrumented with randomly assigned treatment status
Model 1 dependant variable: monthly gross earnings (rands)
Model 2 dependant variable: monthly gross earnings (rands) (inverse hyperbolic sine transform)
Model 3 dependant variable: monthly work hours
Model 4 dependant variable: monthly work hours (inverse hyperbolic sine transform)

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