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# **Reading fiction and economic preferences of rural youth in Burkina Faso**

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**Abstract:** This paper presents results from a reading program for youth living in villages in southwestern Burkina Faso. Standard experimental games were used to measure the effects of increased reading of fiction on several attitudes and preferences important for economic development. After six months of access and encouragement to read appropriate young adult fiction, there were few differences in any of four measured outcomes (trust, contribution to public goods, risk, and patience) between those participating in the reading program and the control group. Since the rise of mass-distributed novels in the 1800s, many have hypothesized that fiction would have significant effects on the values and preferences of readers. Advocates of fiction have argued that readers develop better intuitions about the interior lives of themselves and of others. These enhanced intuitions might change social behavior and actions that influence future selves. The null results presented here suggest the relevance of more research on this question, as countries in sub-Saharan Africa devote public resources to fund reading promotion programs in and out of school.

**Keywords:** Reading, Fiction, Africa, Burkina Faso, Trust, Public Goods, Social Preferences

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### *Potential conflict of interest*

One potential conflict of interest should be addressed. The author since 2001 has been an unpaid director of Friends of African Village Libraries ([www.favl.org](http://www.favl.org)), a small non-profit that encourages the establishment and support for rural libraries. Given that experience and commitment, the non-significant findings in this paper presumably add to the credibility of the findings.

### *Pre-registration of analysis and IRB approval*

The research plan was not pre-registered. The research was funded by a Fulbright Scholar research grant, and the Fulbright application submitted in August 2011 constitutes a "quasi pre-analysis plan." Human Subjects approval was granted by the IRB at Santa Clara University on March 15, 2013. The research followed the Fulbright and IRB proposal, although the more ambitious proposal in terms of sample size and reading programs implemented was scaled down due to budget limitations. More details are in the body of the paper.

It seems plausible that if something as simple as a book can leave the impression that one's life has been changed, then perhaps it is powerful enough to cause changes in brain function and structure.

*Berns, Blaine, Prietula, and Pye (2013)*

If reading literature does not allow us to more clearly, or more economically, or more thoroughly, or more compassionately, think through and take action in our own life, what good is it?

*Wells Jopling (2014)*

## **Introduction**

The cover of the 2011 *Handbook of Research on Children's and Young Adult Literature* features an illustration by Raina Telgemeier. A girl on a camping trip has just finished reading a powerful graphic novel about the atomic bomb's effects on Hiroshima. She is disconsolate, sitting on a dark hillside. As her mother approaches, the girl mutters, "I think that book ruined my life." Her mother replies, "Maybe it actually made your life better. You just haven't realized it yet."

Teachers, librarians and parents all over the world promote reading of literature because they think it will have positive, life-changing effects for young readers. The magnitude of these supposed effects, however, remains unmeasured. Is there reason to believe that *Literature will save the planet*, the title of a recent book by Ammons (2010)? Literature will save the planet, presumably, by positively changing attitudes, preferences, ideologies, culture, identity, modes of

thinking, and a host of other mental habits and processes. But perhaps fiction will play only a small role in changing people, let alone saving the planet. Perhaps fiction does little more than provide readers with entertainment and respite. Neither Ammons nor the chapters in the *Handbook of Research on Children's and Young Adult Literature* offer evidence that reading literature has a measurable impact. This is a noteworthy oversight, given that the effects of fiction are a major preoccupation of both books.

Only recently have social psychologists and other social scientists begun to investigate this question of the effects of reading fiction. A number of papers have reported significant effects on a variety of outcomes such as empathy, self-authorization or self-identity awareness, and “theory of mind.” Many of the findings, however, refer to short term effects (Bal & Veltkamp, 2013; Kidd & Castano, 2013). There is, moreover, a likelihood of positive publication bias inherent in this novel area of enquiry (Gerber, Green, & Nickerson, 2001; Simonsohn, Nelson, & Simmons, 2014).

If literature has the power to improve people's lives, the marginal effect might be high in poor, developing countries in sub-Saharan Africa. Adult literacy is especially low in rural villages, and so youth of the 21<sup>st</sup> century are the first generation to have extensive access to schooling. The increased schooling has not led to much reading, because access to books is limited. Rural areas in most of Africa are not covered by library services. There are few bookstores outside of capital cities. Prices of books are prohibitive for rural residents. Rural per capita incomes in most of rural Africa, outside of South Africa, are about \$1 per day, and a typical African novel costs approximately \$6. A typical novel, thus, costs about one week of work at prevailing rural income levels. Although few comprehensive surveys of leisure reading habits exist, small scale surveys suggest that levels of leisure reading are low (Arua, 2003; Dent, Goodman, & Kevane, 2014; Issak, 2000). In an environment of very low levels of reading fiction, the marginal effects of extra reading might be high. Of course, it might also be that limited exposure to fiction via books and other media such as film and the Internet, as opposed to locally produced traditional storytelling and chatting, means

that fiction will have limited effects. The question is open about whether there is a certain level of “fiction familiarity” that needs to be attained before fiction has its supposed positive effects.

This paper presents measures of the effects of increased reading of fiction on attitudes and preferences relevant for economic development, for youth (aged 15-24) living in villages in southwestern Burkina Faso. These attitudes and preferences would presumably change as readers developed more nuanced sentiments of empathy or understanding of the minds of others. Youth who had completed primary school and who expressed interest in participating in a reading program were randomly assigned into a treatment or control group. The reading program delivered to the participants a selection of short young adult fiction and graphic novels (*bandes dessinées*) each week (later every two weeks). The sample sizes for treatment and control group were fairly large: 295 in the treatment group and 262 in the control group. The research used standard experimental games and choice frameworks to measure four outcomes: inclinations to be cooperative, to trust others, to be patient, and to take risks. Increasingly, these and other mental predispositions (or attitudes or preferences) have been seen as important in the process of economic development (Algan & Cahuc, 2007; Castillo, Ferraro, Jordan, & Petrie, 2011; Laurent, 2009; P. J. Zak & Knack, 2001).

The results were quite clear: after six months of reading (from June to November), which involved delivery of about 20 different books to each participant, there were no differences on any of the four measured outcomes between those participating in the reading program and the control group. Preferences were measured in August 2013 and then again in May 2014. More research with larger and more diverse samples, with other outcomes measures, with larger payoffs, and with longer periods of reading, and other program modalities, is certainly warranted given the decisions that polities and donors in Africa and elsewhere may take to increase the access of literature for the general public. But for now, one should perhaps be more skeptical of statements about the transformative power of reading fiction.

One caveat is in order. The focus of the research was on how reading fiction may have changed underlying economic preferences. There is evidence from developing countries that reading fiction has significant effects on reading capabilities, especially at younger ages (Elley, 1996). Programs to directly improve reading capabilities, however, sometimes have fairly low impact (Abeberese, Kumler, & Linden, 2014; Lucas, McEwan, Ngware, & Oketch, 2014). Summer reading programs in the United States also seem to have quite variable effects on reading capabilities and schooling performance (Guryan et al., 2015; Kim, 2006; Kim & Quinn, 2013; White, Kim, Kingston, & Foster, 2014). There is little research in rural Africa on whether leisure reading in adolescence improves reading capabilities or changes reading habits or has other effects on schooling or job outcomes. The reading program described here did not have any significant effects on a simple measure of reading capability. But the program was not intended or designed to improve reading capabilities.

### **Theory of effects of reading on attitudes and preferences**

Waples, Berelson and Bradshaw (1940) were among the first to theorize the effects of reading by classifying and delimiting the large set of possible relevant outcomes. They drew inspiration from an extensive record of individual accounts of the effects of reading, left by memoirists and biographers. They suggested five categories of effects, which were fairly self-explanatory: (1) instrumental effects from acquiring information; (2) self-esteem or prestige effects from validating or generating an identity; (3) reinforcement effects from having an ideology be validated, or generating a conversion; (4) enrichment of aesthetic appreciation; and (5) relaxation and distraction from anxieties.

Many literary-oriented researchers emphasize broad identity and self-esteem transformations that result from reading, extending Waples et al.'s second category. A few examples are representative of this area of research, which often uses ethnographic and

participatory methods, but has concentrated, hitherto, on theorizing the experiences of self-selected avid readers in developed countries. For Appleyard (1990, p. 96), adolescent reading informed the process of discovery of the subjective self, a self that becomes conscious of an external self that is presented to others, and reflects on inconsistencies and contradictions between internal thoughts and external behavior. Reading helped adolescents become observers of others. Reading complemented and perhaps hastened the normal transition away from the self-centered worldview of childhood. Sicherman's (2010) study of women readers growing up just before and during the Gilded Age of the late 1800s in the United States also focused on identity questions. Many women were vexed by the contradiction between their desires to act in the public sphere in a manner that was socially engaged and the discourses of the times that overwhelmingly reinforced an identity of domesticity for women. Sicherman uncovered considerable autobiographical testimony suggesting that adolescent reading was a key factor in enabling some successful women to resolve the contradiction and develop and realize their aspirations. She hypothesized that reading affected self-awareness, determination, self-control and self-esteem. She remarked (2010:2) more generally that the "scarcity of models for nontraditional womanhood has prompted women more often than men to turn to literature for self authorization." Sicherman supposed that reading prompted the feeling of emotions, and these emotions had enduring consequences for a person's sense of identity.

Another strand of theorizing in understanding the effects of reading has examined the psychological or neurological mechanisms through which reading affects identity, self-esteem and empathy. Reading fiction, in this view, facilitates the construction or modification, in the mind of a person, of a set of understandings or meanings about the social world they live in. The understandings are affected through the experience of a virtual reality consisting of the social and physical interactions evoked by the book (Schubert & Crusius, 2002). Miall and Kuiken (2002), for example, suggested that narrative fiction generates a progression of feelings, culminating in what they call "self-modifying feeling." Kaufman and Libby (2012) argued that narrative fictions result in



a phenomenon they called “experience-taking” where a person’s identity is partially subsumed with that of a literary character. Zunshine (2006) and Oatley (1999), among others, emphasized how reading fiction makes use of and cultivates, in a semi-conscious way, the reader’s ability to have and meta-represent a “theory of mind,” where a person has certitude about his or her ability to apprehend and evaluate the thoughts of another person. Mar et al. (2008) proposed that reading fiction enables the reader to become much more adept at engaging in complex interpersonal relationships. In their view, reading enhances sympathy for others, that is, reading enhances emotional intelligence. Reading fiction is like being a participant in a complex simulated social setting, except that time is compressed, and the entire social situation can be experienced in a matter of hours.

“Transportation” is a key term for describing these psychological processes, as researchers have tried to unpack the experience of getting “lost in a book.” Many authors emphasize also the importance of “genre understanding” in facilitating these mental processes. Liao (2011) explored the importance of genre in enabling readers to become transported into the fictional world, and then evaluate the story and theme in the context of the fictional world. Genre familiarity and expectations presumably shape the reference frame of readers. This is important, because it suggests that some of the effects of fiction are path-dependent or cumulative; only until readers have read widely and have been exposed to a number of genres are they likely to be significantly influenced by fiction.

Fiction genre is but one of a panoply of characteristics of reading that make up a “reading technology” and “reading culture” of material and social practices that presumably mediate effects of reading on the individual. Local reading culture practices influence the provision of texts by authors, publishers and other distributors of novels and stories. Social institutions may or may not encourage the teaching of literature in schools, libraries that promote reading, and social approval of reading as a leisure activity. The effects of reading, then, might vary as the technology of reading

changes. For schoolchildren, reading literature is closely tied to interaction with teachers. As pedagogies around evaluation and appreciation of literature change, so too might the effects of reading. Long (2003), for example, showed how for many American women reading has been intertwined with membership in social book clubs. Intentional reading with the goal of discussing with others in a durable social group presumably has quite different effects on people than random, solitary reading with little discussion. Other new “technologies” of reading might include the growing use of hyperlinks in online and e-reader environments and e-readers, the availability of websites such as Goodreads and amazon.com reviews that provide ready forums to sample and contribute opinions and reactions to texts, and the availability of online videos that enable readers to see and hear authors discussing their work.

This paper reports on measures of four preferences or dispositions-- trust, cooperation for public goods, patience and risk aversion. There is little explicit theoretical work about how these preferences may correlate with empathy or self-authorization, the mental states discussed most prominently in the literature on the effects of reading fiction. There is, then, much room for exploratory empirical analysis. Increased empathy and self-identity, one might argue, could likely lead to increased trust and willingness to contribute to the public good. They might also lead people to be more patient: as they come to know themselves better, through fiction reading, they can take a more expansive perspective on their life, and be more willing to value their future self and hence invest in the well-being of that future self. For choices about risk, young adult fiction probably has a more mixed message. Young adult heroes and heroines often take extraordinary risks to help friends or to do what is right. At the same time, most young adult novels feature the negative consequences of reckless decisions. So the net effects of these different immersions in the consequences of taking risks are not obvious *a priori*.

## **Evidence on effects of reading fiction**

Waples et al. (1940) offered a succinct presentation of early evidence about the effects of reading. He distinguished comparative, broad historical analysis of effects of reading from small-scale analysis of particular reading and particular individuals. Waples was hard-pressed, apparently, to find much more than assumptions, assertions, or fine-grained historical writing that attempted to establish correlation. That is, a couple dozen or so monographs reviewed by Waples were structured in the following way: “during this historical era, these books were read, so the books must have had an effect on the era.” Some authors went further, according to Waples: Spengler (1932) arguing that the decline of the west was due to reading newspapers and abandoning “the bookworld, with its profusion of standpoints that compelled thought to select and criticize”; the French Revolution was due to reading of pamphlets, according to Collins (1928); and arguments that social reforms in early America were influenced by fiction reading (L. Wright, 1939; T. G. Wright, 1920). On the whole, no convincing case was made that reading actually had any of the supposed effects. Waples et al. further noted that there had been numerous studies of how reading could change attitudes for college students. The studies overwhelmingly examined short-term effects: “The studies have repeatedly shown that reading can change attitudes.” But the studies, for Waples et al., lacked external validity. Were college students representative of the general population, they wondered?

Correlational studies suggest that reading is associated with many indicators of personal preferences. Fong, Mullin, and Mar (2013), for example, used a lifetime print exposure measure to classify reader habits into different genres and found that romance and suspense/thriller genre readers scored higher on measures of interpersonal sensitivity, after controlling for other personal characteristics. These studies, however, are unable to establish causality: persons with particular personality traits and preferences may choose to read more or less or different sorts of fiction genres. Waples et al. (1940, p. 118) seem to have been the first to note an important omitted

variable bias for observational studies of reading: "Because the prestige effect [reading a book validates identity or attitude] must be inferred from the readers' predispositions and from the content, we need to understand the readers' traits and the publications read before we can safely conclude that the reading increases prestige." In current jargon, most reading, even at a group level, is self-selected. If anguished adolescents read stories of adolescent angst, causal effects of reading are hard to identify.

Estimates of the effects of reading are more likely to be valid if reading conditions are manipulated in experimental settings. Recent experimental research suggests that the virtual experiences or simulations of the social situations encountered in books may change, at least in the short term, attitudes and preferences. Reading fiction seems to prime readers and this can affect their performance on subsequent activities. A few studies give a flavor of the field of research. Hakemulder (2000) conducted a meta-analysis that confirmed a strong correlation between reading and attitudes of empathy towards others. In subsequent experimental work, he exposed subjects to stories in narrative and non-narrative forms, and found that narratives generated stronger feelings of empathy. Mar and Oatley and co-authors (R. A. Mar et al., 2008; Raymond A. Mar, Oatley, Hirsch, Paz, & Peterson, 2006) found that subjects responded differently to narratives, and also responded differently when they were explicitly asked to "get lost in the book," compared with subjects who were asked to take notes on structure while reading (and thus unable to "lose themselves"). Bal and Veltkamp (2013) likewise found that the effect of reading on empathy was mediated by transportation. Wu and Guillen (2012) showed how small framing variations shaped reference groups for readers, and changed their stated aspirations, and consequently their sense of well-being. The effects were short-lived, however, and dissipated quickly when attention was drawn to other tasks before bringing attention back to aspirations and well-being.

Social psychologists have asked what might be the proximate correlates of transportation (Green & Carpenter, 2011 ; Green, Chatham, & Sestir, 2012 ; Odağ, 2011 ; Thompson & Haddock,

2012 ). Berns et al. (2013) used neuroimaging of 19 college students who read a novel (*Pompeii*, a page-turning thriller and love story set in the shadow of the volcano). They found there was strong evidence that reading the novel changed certain neural networks and this “suggest[s] a potential mechanism by which reading stories not only strengthen language processing regions but also affect the individual through embodied semantics in sensorimotor regions.”

A general feature of these studies of the effects of reading has been that they have measured outcomes over a short time-frame, ranging from immediately after the reading to several weeks later.

As with much research in the sciences and social sciences, the publication and attention to these positive findings should be tempered in the light of findings of significant bias amongst journals and researchers to publish and disseminate positive findings, and “shelve” negative or non-significant findings (Gelman & Loken, 2014).

### **Economic preferences: Measurement and significance**

Economists and psychologists have identified a wide variety of complex predispositions, attitudes or preferences that people have regarding economic choices. These include trust, patience, risk-taking and risk-avoidance, altruism, cooperation, competitiveness, equality, fairness, tolerance, self-control, innovation, and respect for property. For example, many people exhibit “social preferences” for inequality aversion and fairness, and weigh the benefits from various choices in terms of how they affect both their own well-being, the well-being of others, and the relative changes of own and other well-being (Fehr & Fischbacher, 2002). Other preferences, such as risk attitudes and patience, involve preferences of the present self with regard to a future self (Golsteyn, Grönqvist, & Lindahl, 2014). There is an extensive literature on measurement and correlates of these preferences, and research has increasingly used experimental games to measure preferences (Humphreys & M.Weinstein, 2009; Levitt & List, 2007).

Economic preferences may be correlated with economic outcomes across social groups, and economic preferences in social groups may vary over time in predictable ways with events and interventions. The literature is sizable, and a few examples in African settings illustrate the breadth of research. Voors et al. (2012) used experimental games and found that people who experienced violence during the civil war in Burundi were more altruistic, were less risk-averse, and had higher discount rates. A number of randomized control trials have shown that education interventions have changed economic and social preferences. Friedman, Kremer, Miguel and Thornton (2011) explored the five-year effects of increased access to schooling on civic attitudes, knowledge and practices. Jakiela, Miguel and te Veld (2012) found that Kenya secondary school students who had received vouchers in a randomized experiment, thus enabling them to further their education, were substantially more willing to respect experimentally-induced earned property rights (i.e. by giving legitimate owners more in dictator games), compared with the control group.

The remainder of this section briefly reviews four economically-relevant preferences that were measured in Burkina Faso (trust, willingness to contribute to public goods, risk attitudes, and patience). The reader is referred to the various review articles and other citations herein for more discussion.

Generalized trust, usually thought of as the willingness to trust others in the ordinary interactions of economic life, has been studied extensively as an economic preference of considerable importance for economic development (Dearmon & Grier, 2009; Glaeser, Laibson, Scheinkman, & Soutter, 2000; Stolle, 1998). Trust has been measured through surveys and experimental games (Capra, Lanier, & Meer, 2008; Chuang & Schechter, 2015; Glaeser et al., 2000; Lonnqvist, Verkasalo, Walkowitz, & Wichardta, 2011; Naef & Schupp, 2009). The World Values Survey and Afrobarometer, for example, were used by Nunn and Wantchekon (2011) to link contemporary mistrust in African ethnic groups to exposure to the slave trade. Using survey data, Francois, Fujiwara and van Ypersele (2011) found that states that experienced banking

deregulation and hence subsequently higher levels of inter-firm competition, had greater increases in trust as measured by responses to survey questionnaires.

Trust has also been measured through experimental games, where the standard trust game has become a benchmark for the experimental literature (Croson & Buchan, 1999; Johnson & Mislin, 2011). In the trust game, a person has the choice to keep an endowment, typically provided by the experimenter, or send it to an anonymous person whose social identity may be revealed by the experimenter (person in the same group, person of same ethnicity or gender in the locality, etc.). If the first person sends part of the endowment, it is then multiplied by a factor (2 or 3 are typical factors for the multiplication) and “received” by the other person, who then will be offered the option to return part of the value received to the sender. The greater the expectation a person has that receivers will return more than is sent, the more should be sent. The more is sent on average, the more one might say that people trust persons of that social identity, relative to another group of persons or relative to another social identity of receivers.

Many studies have shown differences in trust across social groups (Castro, 2008; Cesarini et al., 2008; Henrich, 2000; Henrich et al., 2005). Randomized control trials have increasingly examined impacts of interventions on levels of trust. Some experiments have shown that exposure to oxytocin generated large changes in trust levels (Kosfeld, Heinrichs, Zak, Fischbacher, & Fehr, 2005; Paul J Zak, Kurzban, & Matzner, 2004). There appear to be significant “framing” effects that generate different average levels of trust. Framing leads people to change their economic preferences (relative to the control group) for at least the duration of the experimental session (Cassar, d’Adda, & Grosjean, 2012; Cronk, 2007; Jakiela, 2009).

A disposition or preference for providing the community with public goods may be mutually-beneficial even if individually against strict self-interest. Unlike trust, contributions to public goods and the propensity of people to favor public goods is often observed directly. There appear to be large differences in average behavior in willingness to contribute to public goods.

Fearon, Humphreys and Weinstein (2009) found that even limited exposure to participatory development projects had sizable short term effects on people's willingness to contribute to local public goods. Willingness to contribute to public goods can also be measured in experiments. The typical experiment endows a group of individuals, and enables them to contribute part or all of their endowment to a common pot that will then be shared equally among members of the group. The public good aspect is that the contributions are multiplied by a factor smaller than the number of members of the group. Thus each individual's contribution implies a "return" less than one, so each member of the group individually would prefer to free-ride and not contribute. Obviously, if few contribute then there is little benefit from the public good. The more people contribute, the more the group is collectively better off. The public goods game has been played in many settings around the world (Angeon, Magdalou, & Célimène, 2013; Ensminger, 2004).

Binswanger (1981) was among the first to measure risk preferences using experimental methods (real-payout lottery games). Schechter (2007) and Barr (2003) measured risk preferences with very simple measures analogous to the trust game: the player decided how much of the initial endowment to risk, and received some multiple (from zero to three, typically) of the amount bet according to a random outcome (throwing a die). Dave, Eckel, Johnson, and Rojas (2010) found that for low math ability the simple Binswanger choice generated risk aversion preferences that were relatively stable over time. Lonnqvist, Verkasalo, Walkowitz and Wichardta (2011) argued that a questionnaire of hypothetical risky choices that were relevant for developed country contexts (driving, careers, sports) was more stable than lottery choice elicitation (even if for real stakes), and was also correlated with sending behavior in the trust game. They concluded that "nonincentivised questionnaires based on so called Likert statements in which subjects specify their level of agreement to a certain statement" may be more stable and valid measures of risk preferences. Observational data offers some confirmation that aggregate dispositions to take risk vary across social groups and over time (Malmendier & Nagel, 2011).



Impatience, sometimes modelled as hyperbolic discounting, where a person underestimates the ability of future selves to be patient, has long been thought to be an important preference for development (Becker & Mulligan, 1997; Clark, 2008). Greater patience means a greater willingness to undertake investments. Godoy, et al. (2004) conducted experiments in patience for monetary and candy rewards among Amerindian society in the Amazon basin and found very high discount rates. Patience and risk can both be interpreted in some sense as aspects of a relationship between a person's present and future self. Will one's future self regret one's earlier self for taking a risk, or congratulate an earlier self for being patient?

### **Reading program: Youth of Tuy Read**

In rural Tuy province in southwestern Burkina Faso in 2013, the setting and time of this study, potential readers and their social technology for mediating what they read were as rudimentary as the technologies of agricultural production. The country had a per capita income level of approximately \$1,000 per person measured using PPP exchange rates, although income was considerably lower for the 70% of the population living in rural areas. Farming in these rural areas was still largely done by hand and with oxen. Tractor, pesticide and herbicide use has been growing, but remains limited. Villages were not electrified, nor connected to sources of potable water. The Ministry of Education in Burkina Faso had embarked in the early 2000s on an ambitious effort to attain the Millennium Development Goal of universal primary schooling. In 2013, school enrolment rates were close to 80%. Enrolment rates were higher in urban areas and lower in rural areas. Secondary school enrolment was considerably lower, at approximately 35%. Electronic media were just beginning to be adopted, and at the time of the program virtually no youth had access to the Internet via smartphones. SMS messaging was, however, increasingly common. Adult literacy was on the order of 20% or lower in Tuy province, so there was no "culture of reading" and certainly no book groups or reading promotion activities, with the exception of the project.

Students typically only read two or three novels in secondary school, and often the novels were quite unappealing (Jean-Jacques Rousseau's *Confessions*) and students simply memorized notes to prepare for examinations.

In April 2013, Friends of African Village Libraries (FAVL) initiated a project to promote reading among youth (aged 15-24) of Tuy province in southwestern Burkina Faso. FAVL had been operating seven village libraries in the area since 2001. Each library had a part-time librarian, and a regional coordinator was based in the area. The libraries are simple one-room buildings (often a refurbished village building) with collections varying from 500-1000 books. They typically serve about 200 members of the village community and rarely serve residents of other villages (typically villages are about 10-20 km apart in the area).

The reading promotion program was called *Jeunes du Tuy Lisent* (JTL) and was intended to bring access to books to youth in villages without libraries. Flyers were distributed in 40 villages in the area inviting applications from youth who had completed the CM1 level (5th grade) and who were interested in participating in the program. 696 applicants submitted completed applications by the deadline for consideration, 295 were randomly selected to participate in the program, and 262 were randomly selected to be in a control group.

The JTL reading program delivered novels and reading material to program assistants who lived in each village. Assistants set up one day each week for collection and distribution of books. In addition, if a person did not come to the distribution day, assistants would deliver books to their homes. Most youth in the area had bicycles, and so time to exchange books was only about 15-30 minutes per week. As mentioned, the program lasted from late May 2013 to the end of November 2013, and then from mid-March 2014-mid-May 2014. In total youth in the reading program were delivered approximately 20 books in the first session, and 8 in the second session. (In October the program switched to meeting every two weeks instead of every week.)

The books were short young adult novels and graphic novels. For example, *Mademoiselle* by Aminata Mbaye Sow, from Senegal, is about a young teacher in 1960s Senegal, just beginning her career in a remote small town. The short memoir-style novel is suffused with a sense of nostalgia and appreciation of basic values of hospitality and generosity. More importantly, the young heroine consistently chooses to "do the right thing" in a series of situations where a person of less integrity might choose self-interest over principle. *Mademoiselle* is civically engaged, generous, honest, and goes out of her way to help strangers. She cares about the misfortunes of others, and shares what she can, without going overboard. Reading her story compels the reader to want to be like her; the novel inspires sentiments of empathy for others and desire to be more principled and more altruistic.

Another book read by youth was *Le Devoir de Classe* by the team of Missa Hebie, Noraogo Sawadogo and Sophie Heidi Kam. *Le Devoir de Classe* is based on a television series produced for Burkinabè television. (Televisions were quite rare in villages in 2013, most viewers were in the two large cities.) The novel is about a young boy in upper primary school, Ismael, who is the son of a simple gardener. His father's employer helps pay for Ismael to be in the elite school. Ismael is studious and honest. He is tricked by the selfish son of a government minister, and is expelled from school. His friends stand by him, and in the end he is vindicated. The message of the book is that honesty and effort are important virtues, but perhaps more importantly that friends help each other in times of need. Ismael inspires trust amongst his friends. That trust is what enables them to outsmart the devious son of the minister. The minister's son, on the other hand, has no trust in others, and no one trusts him. That is his downfall.

Numerous interviews with program participants during May, June, July and August suggest the program operated smoothly. There were few reports of significant delays in obtaining books. Occasionally heavy rains made roads impassable, or made it difficult from participants to gather on the normal day for the book exchange. Rural residents are well-used to these delays, and they

appear not to have negatively impacted program participation. Here is one participant's commentary on the program (translated from the French):

My name is Rabo Ladifatou. I am from Bala village. Currently I am in secondary specifically the fourth year. My book that I have taken this week is interesting. The novel is a comic book by Mady Kafando. Mady tells the story of Samori and Tiefo Amoro. Thanks to this comic I could add some knowledge about the resistance of Tiefo Amoro. The JTL showed me what is a novel and a comic book. Since my primary school, it is only this year I knew books other than my textbooks. Since I started reading I felt I already improved the way I speak French with my friends. My vocabulary has improved little by little. My favorite novel is *The Secret of Mr. Clodomir*. He's my hero at the moment. At home my father has sent only girls in school. Boys, none go to school. I do not know why, but often he says men can take care of himself yet the woman needs help. So I can say that I have a chance that I should use to succeed as much my father wants. Forgive us and continue to give us books or help make a library in Bala. This library will help my other friends who do not have access to books. My father gives me enough time to read books and to share my games. When he sees me reading aloud he hides himself and smiles. I want to be a teacher when I grow up.

### **Methods for implementing program and measuring economic preferences**

At the time of initial conceptualization in 2011, registering pre-analysis plans was just beginning to become an important issue in the social sciences (Miguel et al., 2014). The research reported on here was not pre-registered. The research was funded by a Fulbright Scholar research grant, and the Fulbright application submitted in August 2011 constitutes a "quasi pre-analysis plan." Human

subjects approval was granted by the IRB at Santa Clara University on March 15, 2013. The research followed the Fulbright and IRB proposal, although the more ambitious proposal in terms of sample size and reading programs implemented was scaled down due to budget limitations. The intent was to measure the average treatment effect of a reading program on economic preferences, and that is what is reported here. The research was designed to reduce some important biases in experimental fieldwork and impact evaluation studies. The survey and experimental games team did not know who in the room was in the program when they administered the games, and so questions and recording of answers were not influenced by knowledge of whether the participants were in treatment or control. Likewise, responses were recorded all at the same time, in the same setting, for persons in the treatment and control groups. Participants themselves recorded their answers and choices in a reasonably private setting. Given the nature of the research, about the effects of participation in a reading program, it was not possible for the research to be double-blind since there was no placebo treatment available for the control group.

The random selection of the 696 program applicants into 295 program participants and 262 control group participants arose from the following sampling strategy. The applicants were stratified by nine clusters of villages, in order to ensure an even distribution of workload for program assistants. The clusters are contiguous to each other and were selected for reasons of logistics; the clusters were not a random sample of village clusters. In each of five clusters that did not have village libraries, 40 participants and 40 control group persons were randomly selected from among the applicants from that cluster. In each of four clusters in villages with village libraries, 20 participants and 20 control group persons were selected from among the applicants from that cluster. The sample is thus a clustered random sample of self-selected applicants, and so is not representative of the broader population of youth in the region, but rather is representative of the target population of youth who are likely interested in reading. In some clusters, there were many more applicants than spots available. In other clusters the number of applicants was very

close to the number of spots. In one cluster, the number of applicants was smaller than an equal number of treatment and control, so the control group was smaller (the intended treatment group was not reduced in order to have a larger control group). In most of the analyses below, the inverse of selection probabilities within each cluster is used to weight the observations.

The sample size of 557 persons, with 295 program participants and 262 control group participants, has sufficient power to detect reasonable likely treatment effects. As seen below in Table 5, the standard deviation of the amount contributed in the trust game was about 140 FCFA (the amount available to participants was 500 FCFA, and the mean contribution was around 230 FCFA). A sample of 250 in each group would be sufficiently large to detect an effect of .25 standard deviations at the conventional .80 power level. The precision of the estimate of the effect may be increased when covariates are taken into account. In particular, as seen below, a first round of experimental games was conducted before the program began. So the effect of the treatment (the reading program) is the difference in changes in the way that participants played the experimental game, rather than the difference in levels. Since the levels of contributions in the trust game, and other choices in other games, had idiosyncratic variation, controlling for that variation leads to somewhat more precise estimates of the effect, and consequently the power of the test is increased. As seen below in Table 7, for a regression estimating the effect of selection into the reading group, the standard error on the treatment coefficient is 11.87. This implies that at the .80 power level an effect of 30 is detectable, or about .21 standard deviations. Another issue when thinking about the power of the tests conducted in the paper is that there are five outcome variables considered, rather than one outcome variable. If the null hypothesis is that the reading program has no effects on any of the outcomes, and would be rejected if any of the coefficients in regressions (or comparisons of means) were significant, then the power would be very high with the given sample size and quite reasonable effect sizes for the different outcomes. Of course, by the same token, if the hypothesis were that the reading program would affect all five outcomes with reasonably sized

effects, that hypothesis might be underpowered. One contribution of this paper is to generate estimates of standard deviations of outcomes that may inform subsequent analyses of reading programs and their effects on preferences.

Those in the program group began the reading program in late-May 2013. Each week they received a short novel for reading. The novels were selected by FAVL staff based on experiences with young readers in the community libraries in the region. Virtually all novels were by African authors, written for youth or young adults, and available in the main bookstores of Ouagadougou. (There are almost no bookstores outside of Ouagadougou, and no bookstores in Tuy province.) Few of the books were available in the existing village libraries.

Prior to the beginning of the program, all treatment and control group participants received a free copy of a short graphic novel, *Kouka: Le Rancon de la Corruption*, about corruption in Burkina Faso. The graphic novel was produced by an NGO called RENLAC that promotes awareness of corruption and investigates specific instances of alleged corruption. The graphic novel series *Kouka* in 2013 comprised ten different short stories, written and illustrated each year by a local Burkinabè illustrator selected through an open competition. Two other issues of *Kouka* were included in the program for the treatment group.

One week after the distribution of *Kouka*, program and control group participants were invited to participate in experimental game sessions held in their villages. Participants were given several days advance notice of the games, and a brochure describing how the games would be played, with examples, was distributed to each applicant. Multiple sessions were held in each of the nine village clusters. Participants with mobile phones received SMS messages notifying them of the games. Participants were told they would receive 500 FCFA (approximately \$1 dollar at current exchange rates) for sure, and have a possibility of earning between 500-1500 FCFA more. The prevailing daily wage for agricultural labor at the time was about 500 FCFA. (Average per capita GDP at market exchange rates was also about \$1 per day in 2013.)

The first round of games was held in the last week of May 2013. Game sessions lasted approximately two hours, and up to 40 participants could play at a time. Games were held in a large room with individual chairs for each applicant, provided either by the local mayor or by the school director. Only research assistants and participants were allowed in the room during the time of the games. Participants were asked to turn off their mobile phones during the games (a few did not, and would occasionally speak in low voices on their phones). A research assistant carefully read through instructions for how to play the games, and gave several examples of each game and how payoffs would be calculated. Examples featured people earning a lot and people earning a little. The vast majority of players nodded attentively as the games were explained. Games were played one at a time, and the next game explained again before playing. Participants were informed at the beginning of the session that only one game would be selected for payout. The game was selected randomly by a participant drawing a number out of a bag with cards numbered for each of the four games. Realizations of chance outcomes (for risk and patience games) were also determined through public random selection. While payoffs were calculated, participants completed a short questionnaire and reading comprehension test with multiple choice questions.

The games are described below. The sense of the team that administered the games was that the trust, public goods and risk games were understood by almost all participants. The patience game, however, was more difficult to explain, and the team realized early on that many participants probably did not understand how to play, and indeed responses are often inconsistent.

The rains came early to the region, and just as the games were scheduled heavy rains started falling. Local farmers understand there is a small window for planting, and many parents required their households to work planting and clearing fields. Nevertheless, of the program and treatment participants, 440 participated in the May 2013 games, out of the 557 program and control group participants, for a participation rate of 79%.



In late July, youth in the treatment and control groups were again invited to participate in a round of experimental games. Only small modifications were made to the game protocols. The game choices were copied in a booklet form, for example, rather than loose sheets of paper in a folder. The games were held in the last days of July and early August. Attendance was similar to the May games, with 426 participants attending, for a participation rate of 76%. The reading program continued until the end of November, when funds for the program ran out. At the end of March 2014 the program was restarted for two months, including both participants and control group. Members of the control group had been promised that the project would attempt to secure funds to enable them to have a chance to also read the books. In early May 2014, youth in the treatment and control groups were once again invited to participate in a round of experimental games. At that point they had been distributed five books.

So the first round of games in May 2013 may include a Hawthorne effect (did the mere knowledge of being in the treatment group generate effects?), the second round in August 2013 may be thought as measuring differences arising from reading for a couple of months (about eight short novels), while the third round in May 2014 may be thought of as measuring longer-term effects of a reading program (most in the treatment group had read 20 more books than the control group, over the course of the preceding year).

### **Balance across treatment and control and determinants of participation in game sessions**

Table 1 presents summary statistics of characteristics of people in the treatment and control groups. The characteristics are taken from the application form, completed before the program began. The randomization produced appropriate balance between treatment and control groups. There were no statistically significant differences between the groups. About 40% were female, and the average age was about 17.5 years (there were many more secondary students applicants than applicants from older youth no longer in school). Participants had on average almost eight

years of schooling (six years of primary and two of junior secondary). The minimum for applicants was five years of schooling. Participants had been asked to note on their applications how many books they had read in the previous month, and to list the titles of up to three books they had read in the previous month. The average number of books read varied from .51-.66, and only about 30% had read at least one book. Respondents listed on average about .40 titles, and only about 20% wrote down at least one title. About 20% of participants indicated they were subscribers to the community library (located in four of the village clusters and basically inaccessible to most of applicants in other five village clusters). About 45% of the participants indicated they had their own mobile phones, and most others indicated they knew a relative, friend or neighbor with a mobile phone and could provide the number. Close to 60% of participants were from the Bwa ethnic group indigenous to the region. The other major ethnic group consists of Mossi who have migrated from northern Burkina Faso. About one quarter of participants misspelled the French word for the occupation of their mother or the occupation name of their father. Typically, they spelled the occupations phonetically. Missing such standard words (the equivalent of writing housewife as “howswiv” and farmer as “farmore”) suggests the lack of leisure reading. Leisure reading at the very least would bring about greater familiarity with spelling of common words.

The short questionnaire completed by applicants also asked about trust. The question was the French version of the standard question asked on the World Values Survey (“Generally speaking, would you say that most people can be trusted, or that you can’t be too careful in dealing with people?”). Less than one-third of participants agreed that people could be trusted. Importantly, there was no difference for this question between the treatment and control.

Table 2 presents data on participation in the experimental game sessions in May 2013, August 2013, and May 2014. The table compares those who did participate with those who did not, using characteristics from the initial application. For the games in May 2013, participants were younger (17 versus 19), were more likely to be subscribers to a library (22% versus 12%), were

more able to list titles of books they had read (.42 versus .18), and were more likely to be female (43% versus 31%). They were also much more likely to participate in the other two game sessions (89% and 74% for August 2013 and May 2014, compared with 33% and 37% for those who missed the first set of games). There was no difference in program status (treatment or control). For the games in August 2013, participants were again younger, more likely to have listed more titles, more likely to be Bwaba, more likely to be subscribers to a library, and more likely to have participated in the earlier games and to have participated in the May 2014 games. Those in the program group were somewhat more likely to have participated (56% versus 45%). For the games in May 2014, again participants were more likely to have listed more titles, be female, be Bwaba, be subscribers, and to have participated in the earlier game sessions. There was no difference in program status (treatment or control).

Since there are multiple comparisons, it is pertinent to note that if a Bonferroni correction were applied there would be fewer significant differences between participants and non-participants.

The 301 persons who participated in all three experimental sessions were younger, less likely to have their own mobile phones, more likely to be subscribers to a library (24%) and more likely to be Bwaba (not reported in the table). The 58 young persons (10% of the sample) who did not attend any of the three game sessions, were, on average, a full year older than those who participated at least once (not reported in the table). They were less likely to be a subscriber to a library (because they were more likely to be in the village without libraries), they listed fewer titles of books read, and they were less likely to be Bwaba ethnicity.

All in all, the participation results are not surprising: the significant variables are likely the same determinants of geographic mobility. Rainfall in southwestern Burkina Faso is sharply seasonal, and periods of intense agricultural operations vary significantly from month to month. When agricultural activities are slack, older youth and males move to towns or to artisanal gold

mines. Mossi people move more often than Bwaba because they have historic migration networks throughout the country. Youth in villages with fewer economic opportunities are more likely to move seasonally (two of the villages with libraries are the district centers and somewhat larger than other villages, with more diverse and less seasonal economic activities).

The finding of some systematic causes of absence in the August 2013 and May 2014 game sessions suggests that estimates of the program effects need to correct for the possibility of bias due to selective non-participation in the game sessions. The patterns of attrition across the three game sessions appeared to be similar and not to vary much with assignment status. For the May 2013 games, 79% of the program group participated and 77% of the control group participated. For the August 2013 sessions, 79% of the program group participated and 73% of the control group participated, with p-value of the difference in proportions equal to .073. For the May 2014 sessions, 66% of each group participated.

### **Impact of program on reading outcomes**

After the program began, participants were asked about their reading habits, in the experimental game sessions in August 2013 and then in May 2014, and in a questionnaire in March 2014 when the program restarted after a three month hiatus. Participants were asked to write the titles of books they had read in the previous two months, to name a character in each book, and to briefly identify what the character did in the book. The variable counting the number of titles listed is constructed in two ways, one with all the titles, and the other including just the titles that were actually in the program (JTL books).

Table 3 presents several different ways of estimating the difference between mean reading frequencies for the treatment and control groups at different times in the program, for these measures of reading. The first set of columns presents the means for the two groups where outcomes are weighted according to the survey design. Individuals in clusters that were

overrepresented in the sample relative to the share of total applicants in the cluster (who represent the population from which the sample was drawn) have weights lower than one, so they are discounted. Individuals from clusters that were underrepresented in the sample relative to the share of total applicants in the cluster have weights higher than one, so they are inflated. The second set of columns reports means that are adjusted to take into account the differential sample attrition noted above (non-attendance at game sessions, where reading questionnaires were completed). It is possible that the absences were systematically related to the program or selection into the program.

The second set of columns of Table 3 present the difference in means, the standard errors and p-values, as well as confidence interval for the differences in means, for each game outcome when the treatment or control samples are trimmed to have equal representation. The method implemented is that of Lee (2009) using the Stata package *leebounds* (Tauchmann, 2013). The Lee bounds method has become a standard way of assessing the robustness of statistical findings to differential attrition. An earlier method, proposed by Manski, involved assuming the outcomes for the missing cases that would be “worst” for the hypothesis being tested. In this case, that would involve assuming that the missing control group choices in, say, the trust game were the maximum contribution while the missing cases in the program group would be assumed to have made zero contribution. (The hypothesis is that reading fiction would positively increase the amount contributed.) The Lee bounds procedure generates a much smaller width for the bounds on the possible average treatment effect. The assumption is that treatment status only affects attrition in one direction (i.e. makes it more or less likely), and is not dependent on other covariates. Then bounds on the average treatment effect can be calculated by trimming the sample so that the share of observed respondents is the same for treatment and control. That is, if more of the control group is missing, then treatment group respondents are ordered by outcome and the top and bottom tranches are excluded, respectively, to calculate upper and lower bounds on the effect. A covariate

may be used to tighten the bounds further, by calculating bounds for different covariate outcomes and then aggregating them.

The third set of columns of Table 3 presents the p-value for testing differences in means between treatment and control computed by the method of randomization inference (Gadbury, 2001; Gerber & Green, 2012; Ho & Imai, 2006). The method is implemented using the *tsrtest* package in Stata (Kaiser & Lacy, 2009). In this method, a distribution of possible differences in outcomes is generated from 10,000 samples drawn from the observed data, where observations are randomly assigned to be in the treatment or control group. That is, the maintained null hypothesis is that there is zero treatment effect for every observation. The actual difference between the means is then compared with the likelihood of observing such a difference, under the null hypothesis. The likelihood that the average of differences between treatment and control would have been at least as high as the average difference actually observed is the p-value. Randomization inference methods usually do not differ much from asymptotic t-tests unless sample sizes are small. So the real value of the randomization inference method is to examine differences at the cluster level, where treatment and control groups are between 20 and 40 individuals. The third set of columns presents the differences between treatment and control for each reading outcome measure for each of the nine village clusters. Asterisks are used to denote the randomization inference statistical significance of the difference in the means.

The various methods for estimating the difference in means suggest that the treatment group did indeed read more than the control group. There was no difference evident in May 2013, before the reading program began. The difference was large and statistically significant for responses from the August 2013 session. Overall, program participants listed 1.55 titles on average, while the control group only listed .36 titles. The difference was just as large when restricted to JTL titles: 1.19 versus .17. Differences were also large for naming characters and describing the actions of characters. Participants were more likely to list characters and say what

those characters had done, providing confirmation that many participants were reading at least part of the various books. Likewise there were statistically significant differences in March 2014, in a survey administered before the program restarted for both treatment and control. The program participants noted twice as many JTL titles, on average.

The averages for the number of book titles listed are consistent with the absence of anecdotal evidence from the program team that there was spillover from treatment to control group. Control group members listed very few JTL titles in August 2013 and March 2014. If they had been borrowing the books of the treatment group, there might have been many more JTL titles listed.

Once the program restarted, when participants were surveyed in May 2014, there was no difference between treatment and control. Those who had been in the control group increased the number of titles they could list to match the mean of the treatment group. Recall that the version of the program that started in March 2014 also offered books to the control group participants.

The randomization inference results for the village clusters are very consistent with the whole sample analysis. When there were significant differences in the whole sample, there were also likely to be statistically significant differences in many of the village clusters. The cluster analysis suggests that the reading program may have had more effects in clusters such as Bekuy, Bouahoun and Sara. The cluster analysis also suggests that in some clusters when the program was restarted, by May 2014 the control group members, who now were included in the program, listed more titles than the old program group. Again, this result seems to confirm the effectiveness of the program.

Table 4 shows the results for linear regressions with the number of titles listed by the respondents as the outcome variables. For each session, one regression includes only the treatment status (equal to one if the participant was in the reading program), one includes a set of controls such as the gender, age, schooling, and ethnicity (whether Bwaba, with other participants being

Mossi) of the person, and the final regression in each set includes the March 2013 baseline measure of reading frequency as a pre-program covariate to estimate more precisely the effect of the program on reading frequency. The explanatory variable of interest in the regressions is the dummy variable for whether the person was selected to participate in the reading program. Observations are weighted by the survey weights, as described earlier.

As with the table of means, there are no differences in reading habits for the treatment and control for the May 2013 session. For the August 2013 and March 2014 sessions, the dummy variable for treatment is positive and significant in specifications with covariates. Program participants on average listed 1.3 more titles than non-participants in the control group in August 2013, and .30 more books in March 2014. Participants who had more schooling also listed more titles. Each additional year of schooling was associated with listing .30 more titles. Age seemed to be somewhat negatively associated with the number of titles listed, though mostly the coefficient was not statistically significant. In May 2014 there is again no difference between treatment and control as now the control group has also started reading. Notice that the constant term is substantially higher; treatment and control were both reading more.

Figure 1 presents a scatterplot that graphically represents the regression results for August 2013. The x-axis measures the residual from a regression explaining, with basic demographic variables (gender, ethnicity, age and schooling), the number of titles at the time of the initial round of experimental games in March 2013. So the x-axis values are the component of the respondent's outcome in March 2013 that is not explained, or not predicted, by their gender, ethnicity age and schooling. It is a measure of their "unexplained" reading choices. On the y-axis is the residual of the regression of the August 2013 number of titles listed on the explanatory variables (gender, ethnicity, age and schooling). The y-axis measures the unexplained portion of the choices in August. The scatter plot shows the two residuals for each student. Each point on the scatter plot represents a student, and their residual choices in March 2013 are measured on the x-axis and their



residual choices in August 2013 are measured on the y-axis. A regression of the two residuals using the whole sample yields the same estimated coefficient as the March 2013 explanatory variable on the August 2013 outcomes as in the regression in Table 4. In the figure, the residuals for the reading program participants have been represented by the hollow squares, and the residuals for the control group youth are represented by the gray diamonds. The two fitted lines are the regressions of the March 2013 residuals on the August 2013 residuals for each of these two groups. The fitted line for the reading program youth (the short dashed line) is everywhere above the fitted line for the control group (the long dashed line) in Figure 1, indicating that the program led to greater reading (as measured by titles listed) for every level of March 2013 reading frequency residual.

Overall, the data suggests that program participants were much more likely to list titles and characters and the actions of characters. In August 2013, about 55% of program participants listed two or three titles, 10% listed only one title, and only 35% listed no titles. By contrast, 82% of youth in the control group listed no titles, and only 13% listed two or three titles. The percentages were similar for naming characters in the books; program participants could name more characters, while control group youth named fewer. The program was effective, then, at exposing individuals to the young adult novels.

### **Impact of program on economic preferences**

Table 5 presents summary means and standard deviations for the various game outcomes. For the trust game, each participant was told they had an endowment of 500 FCFA (about \$1, equivalent to a full daily wage), and could send to their anonymous partner any part of the endowment (in units of 100 FCFA). The mean amounts sent, as seen in the table, declined from 248 in the first session to 221 in the May 2014 session. The modal amount sent was 200 FCFA. About 10% of youth sent the full 500 FCFA. Participants in the trust game were told that the amount sent would be multiplied by

three, and that recipients could then remit some of that amount back to the sender. Each player was asked to complete a short form indicating how much they would remit for each of the possible amounts sent by the sender. The fraction of the amounts sent that would be returned is sometimes called an indicator of trustworthiness. Trustworthiness here was measured by the weighted average of the fraction returned. For each participant, the amount that they returned, as a fraction of the amount sent (before it was multiplied by three), was calculated for each amount that the sender could have sent (100, 200, 300, 400 and 500). These were then averaged, weighted by the ratio of the amount sent to the sum of the amounts that could have been sent (that is, 1500). There were five possibilities and the most weight is placed on the 500 FCFA case. If the person returned a lot in that case their overall average fraction returned would be higher, since it was given more weight. By and large youth in Tuy province proved themselves quite trustworthy. The fraction was greater than one for more than two-thirds of the players, and the weighted mean fraction of about 1.2 did not vary much across the three sessions. If participants had known the distribution of the average fractions returned, they might have been even more trusting.

For the public goods game, each participant was told they had an endowment of 500 FCFA, and could contribute (in units of 100 FCFA) to the group. The other members of the group were anonymous. Contributions would be added up and multiplied by three, and then divided equally among the six members of the group, regardless of whether or how much they had contributed. (When the number of participants was not divisible by six, a “fictitious” partner choosing 300 CFA was included in the group for purposes of determining payouts.) The distributions of contributions suggest that there was a significant change in strategy for many of the participants. In May 2013 about 25% of participants contributed the full 500 FCFA to the group, by August 2013 and May 2014 this percentage had dropped to fewer than 10%. The modal contribution went from 500 FCFA to 100 FCFA. The average contribution declined from 276 FCFA to 238 FCFA. Evidently

participants learned that many of their co-villagers were free-riding in the public good game, and they decided to contribute less.

The amounts sent and returned in the trust and public goods games were comparable to those found from other game sessions in Burkina Faso. Hadnes and Schumacher (2012) found that in a trust game where participants had to decide whether to send the entire endowment to the partner, or not, 69% of control group participants and 87% of treatment group participants sent the full amount. The treatment was a prime for salience of “ancestors” who are regarded by many in Burkina Faso as possibly influencing the current world and punishing violations of social norms. Sawada et al. (2015) found that mean contributions in a public goods game in Burkina Faso were about 300 FCFA, with 500 FCFA as the endowment.

In the risk game, participants were asked to choose one of six different gambles, each with two outcomes (a sure thing, with 1000 FCFA payout in either case, and gambles with outcomes 1300-900, 1600-800, 1900-600, 2200-500 and 2500-100). In each gamble, the two outcomes had 50% probability of being selected (blue and white tokens were picked out of a sack). If participants chose gamble 1, they received 1,000 FCFA for sure. If they chose gamble 6, then at the end of the session, if the risk game were the selected payoff game, a token was chosen from the sack. The tokens were chosen individually by each player who had selected a risky choice. If the token were blue, the player would receive only 100 FCFA. If the token were white, the person received 2500 FCFA. The modal choice was gamble number 1 (the no-risk choice) in all three sessions. In the August and later May 2014 sessions, some people who had previously chosen the intermediate risk choices decided to switch to the safe choice, and some switched to riskier choices. So the mean choice stayed at about 3.25, while the extreme choices were somewhat more likely.

The patience game was more complicated compared with the other games. There were 28 choices in the game, seven choices in each of four groups. The groups had the same choices over amounts of money to be paid sooner rather than later, but differed in the timing of the payments.

Participants decided to be patient or not for each of the 28 choice pairs. For example, they had to choose between 1000 FCFA to be paid at the end of the game session, or 1200 FCFA to be paid the following day. Another choice involved receiving 900 FCFA in two days or 1200 FCFA in 30 days. If a person chose the “pay early” option, they would receive the FCFA immediately (if that version of the patience game were selected for payout), or in two days (if that were their choice and that option were randomly selected). All of the implied “per day” discount rates were quite high; respondents ought to have been patient for every single choice they confronted. The first choice in each group of choices involved receiving 1100 FCFA soon or 1200 FCFA later. The discount factor was .92. This factor could be turned into an implicit annual interest rate depending on the amount of time that would elapse between the sooner choice and the later choice. Choosing 1100 FCFA today rather than 1200 FCFA in five days would imply foregoing a large implied interest rate return of about 9% for just five days, or an annual rate of 635%. The May 2014 patience game was simplified to only 10 choices. At the end of the session, if the patience game were chosen, then one of the 28 (or 10 in May 2014) choices would be chosen for payout, with payouts for that choice according to what participants had chosen, whether to be patient or impatient. If the outcome involved delay, participants were given a certificate that was redeemed with their local program assistant after the delay had elapsed.

The data permit calculation of the implied discount rate for when the person “switched” to being patient. For the May 2013 and August 2013 sessions, about 50% of participants chose to be patient for the very first choice (1100 FCFA soon versus 1200 FCFA later) and for all subsequent choices. The other 50% were impatient for the very first choice in each block, but when the amount of the “soon” payment declined a bit they switched to being patient. About 80-90% revealed discount factors higher than .75. Only about 10% of participants chose to be very impatient and receive money immediately rather than waiting, even when waiting was enormously profitable. This percentage roughly doubled when the choice was between receiving money in two days or else

waiting 30 days. That is, the further off the future payment, the more participants chose to receive payment quickly, even though the implied discount rate was still very high. About one third of the participants were inconsistent in their choices (alternating impatient and patient within groups, indicating they either did not understand the basic logic of the question, or did not care about the outcome). The outcome measure used for this paper is a simple count of how many times a participant chose to be patient. Inconsistent players are thus also included in this count. Table 6 suggests that participants were considerably more impatient in May 2014 compared with the earlier sessions. In May and August 2013, participants on average chose to be patient in about 18 of the 28 choices (that is, about 64% of choices). In May 2014, participants on average chose to be patient in 4.8 of the 10 choices, or about 48% of choices.

Table 6 presents several different ways of estimating the difference between mean outcomes for the treatment and control groups for the different experimental games and sessions. The first set of columns presents the means for the two groups where outcomes are weighted according to the clustered survey design. The second set of columns reports means that correct for sample attrition (non-attendance at game sessions) using the method of Lee (2009) as discussed above. The third set of columns presents the p-value for testing differences in means between treatment and control computed by the method of randomization inference, as discussed above, including differences between treatment and control for each game and session for each of the nine village clusters. Asterisks are used to denote statistical significance.

For the survey weighted, Lee bounds, and RI method for computing differences in means, there is limited evidence that the reading program had any significant effect on the economic preferences of participants. Program participants contributed on average about 40 FCFA more when playing the trust game in May 2013, before the program had actually started. This difference was possibly a Hawthorne effect, where participants who had been designated to be in different groups were perhaps “primed” to play differently in the very first game of the game sessions. Using

the Lee bounds procedure, there was also a difference of about 33 FCFA in August 2013 in the trust game. But using the randomization inference method the difference is not significantly different for the overall sample, and appears to be accounted for by only one of the nine village clusters (Béréba outskirts). The other difference is in the average of the weighted fraction of the amount that respondents said would be returned to their partner in the trust game. The difference was small and significant in the August 2013 game session; the program group had an average fraction of 1.36 compared with 1.21 for the control group, a difference of about 12%. The randomization inference columns indicate that the difference was due to a large difference in the Bekuy cluster.

There are 90 comparisons for the nine village clusters and the five measured outcomes in each of the two post-program game-sessions (August 2013 and May 2014). Of these, 19 exhibit p-values less than .10 using the randomization inference method, about 20%. But within many of the game outcomes there are some positive and some negative differences. So the overall evidence is quite mixed.

Table 7 shows the results for linear regressions with outcome measures of the trust games played in May 2013, August 2013 and May 2014. There are two outcome variables: how much was sent, and how much would be returned (a weighted average of the amounts indicated for the five possibilities). For each of the outcome variables, one regression includes only the assignment status (equal to one if the participant was in the reading program), a second includes socio-economic controls including the gender, age, years of schooling and ethnicity (whether Bwaba, with other participants being Mossi) of the person, and the third adds the game choice of the individual in May 2013 before the reading program began. The main explanatory variable of interest in the regressions is the dummy variable for whether the person was selected to participate in the reading program. Standard errors are adjusted for the clustering into zones and the survey design.

Mostly, the coefficients on the assignment into the reading program variable (the treatment assignment) do not support a hypothesis that reading induced people to be more trusting. The

coefficients are significant and positive in the May 2013 game session, before the program had started. As noted earlier, this is likely a Hawthorne effect, where youth who had been notified they had been selected for the program perhaps made extra effort to appear to be more trusting. In the August 2013 game specifications, the treatment indicators are not significant at the conventional level. In the May 2014 games, longer-term program participants were more likely to have contributed less. The magnitude of the coefficient is not very large, about 10% of the mean contribution level. In the trust game of August 2013, program participants were somewhat more likely to return a greater fraction of the amounts sent to them by their anonymous partner. The magnitude was about a 10% increase. The treatment indicator is not significant once the baseline May 2013 amount returned is included. The two somewhat significant results then go in opposite directions (contributing less, but then returning more).

Table 8 presents regression results where the contribution in the public goods game is the outcome variable. There are three game sessions, May 2013, August 2013 and May 2014, and again there are multiple specifications for each session. The indicator variable of assignment into the reading program is not significant (and has a very small coefficient) in all of the specifications.

Overall, the impression from the regressions in Tables 7 and 8 confirms the comparison of means: the reading program did not have a large, significant, and consistent effect on how youth approached playing the trust and public goods games.

The absence of a strong result for program assignment status contrasts with three robust results for the covariates seen in the regressions. Females contributed more in both the trust and public goods games, and returned more in the trust game (they were more trustworthy). Youth with more years of education contributed less and returned less. The magnitudes of these effects are generally greater than the magnitudes of the program selection dummy variable coefficient. Finally, the most important determinant of how youth played the trust and public goods games in August 2013 and May 2014 was how they had played the game in May 2013, before the reading

program had begun. The coefficients on prior game play are quite significant and large in magnitude, explaining much more of the variation in game play compared with the program variable or the other socio-economic variables.

Table 9 presents results from regressions explaining the choices of youth in the risk games. The risk game had outcomes varying from 1 to 6, with 6 indicating the riskiest choice. Selection for participation in the program did not affect the choices that youth made when playing the risk games. None of the specifications had a significant coefficient, except for the final column with the risk game of May 2014 where program participants were more likely to choose more risky choices, significant at the 10% level. The magnitude of the coefficient is comparable to those on the female indicator variable and the ethnic indicator variable (is person Bwaba). Schooling and age did not seem to matter for risk choices. Respondents were consistent, and the choices in the May 2013 game sessions were correlated with choices in the later sessions.

Table 10 displays results from regressions explaining the choices of youth in the patience games. The outcome variable is the simple count of the times that a person selected to be patient, for the scenarios that were presented. Again, selection for participation in the program did not affect the choices that youth made when playing these games. None of the specifications had a significant coefficient. There was no consistent pattern here for socio-economic variables affecting choices. Choices in the May 2013 games played before the program began were, however, correlated with choices in August 2013 and May 2014.

In March 2014 and again in May 2014, participants were asked to respond to a small set of Likert-scale questions about attitudes towards risk and trust. As seen in Table 11, an index of responses to these questions showed no differences for those selected to be reading program participants. Here the earlier answers to survey questions from May 2013 are mostly not correlated with responses a year later, suggesting that survey questions might be less reliable indicators of some underlying preferences than game play.



One other check of robustness of the results to the attrition of respondents was conducted. In results not reported here, the Stata procedure (mi impute) for imputing values to missing values and then estimating the same regressions as above was conducted. The coefficients on the treatment variable were generally not statistically significant except for the regression explaining the number of titles listed.

The results then are fairly clear. For the patience, risk and public goods games, there was no significant effect of assignment into the program. For the trust game, there is some evidence of effects, but the results are not robust across methods, seem to be concentrated in just some of the village clusters, and are somewhat contradictory (less trusting, more trustworthy). The tests of the significance of the coefficients discussed here do not take into account that multiple tests are being conducted; if they did there would be even fewer significant coefficients. All in all, the basic conclusion is that the reading program had no effects on the economic preferences measured during the game sessions.

## **Conclusion**

Readers have always believed that reading has important effects on their preferences (i.e., what choices they will make in various situations). The belief is most likely generated by introspection and is reinforced by salient examples (especially of authors declaring how much other books they have read have mattered in their lives). There are few studies, however, measuring how much influence reading fiction has on preferences and attitudes. The results presented in this paper suggest some skepticism about how much reading fiction affects people's economic preferences. If one's prior belief was that these effects were large, then that belief should perhaps be revised downwards. The reading program implemented in villages in southwestern Burkina Faso had the desired effect of increasing the availability of young adult reading material, and there was much qualitative evidence that readers enjoyed reading the novels. But the increased reading seemed to

not have any significant effects, either in the statistically significant sense or in terms of magnitudes of effects. The effects estimated were generally very small, and thus not statistically significantly different from zero.

External validity of these results is definitely an important issue. The results in this paper are for a very particular group of youth living in rural villages in southwestern Burkina Faso. These youth have completed primary school, but have had little access to fiction. The way that reading fiction affects preferences may be quite different for youth with limited exposure to reading, compared with youth who have been reading fiction since childhood and have progressed through an extensive set of “tropes” that enable them to better appreciate nuance and meaning in young adult fiction. Some might further argue that the fiction authored and published in francophone West Africa may have quite different qualities, for the reader, compared with fiction in other world regions and languages.

Internal validity may also be an issue. Attrition and non-compliance are significant, and may lead to findings of no effects. The estimates presented in Table 6, where Lee bounds were calculated, suggest that attrition was not a major concern for the estimates of the selection into treatment effect. Likewise, there was no evidence during the implementation of the program to believe that there were significant spillover effects. The control group did not appear to increase their reading, relative to baseline, by borrowing books offered to those in the treatment group. The titles listed by the control group only increased in May 2014, after they had been included in the program. But many program participants readers did not read. It is possible that a reading program with significant incentives to complete reading (such as the AR testing done in many middle schools in the United States) would generate larger average effects because it would induce more compliance. The intent of the reading program in Burkina Faso was to model a feasible and replicable reading program in the current budgetary and infrastructure landscape of rural Burkina Faso. No real reading program in the near future is likely to do more to promote reading than the

JTL program did. So, the results here should be interpreted as being that a realistic reading program generated no sizable change in economic preferences. An extraordinary reading program certainly might have large effects.

Another concern is that there may have been heterogeneity in the effects of reading fiction. Perhaps only “good readers” are affected by reading fiction. Perhaps only adults are affected by reading fiction. Studies specifically designed to focus on some of the possible heterogeneity in treatment effects would be desirable, for the future. This paper has focused on the main treatment effect, in order not to present potential findings of interaction terms as statistically significant. It was not the protocol of the research to test any specific hypotheses about the importance of any particular pattern of heterogeneity.

Similarly, another concern may be that the wrong outcomes have been measured. The research should have measured empathy, emotional intelligence, theory of mind, creativity, or imagination. One of the virtues of the focus on economic preferences is that experimental techniques have become quite standardized, and so the results here are comparable to other trust, public goods, risk, and patience game results. The patterns of variation in choices with education, age, gender and ethnicity suggest these games have considerable validity in capturing differences in some underlying constructs of economic preferences. Using similar standardized methods for measuring other hypothesized outcomes of reading should be a priority. The data and analysis from the research reported here might be useful in guiding such efforts. For example, they permit simulation of how statistical power will be affected by studies designed to test multiple outcomes, multiple interactions, and varying sampling designs.

Reading *Barefoot Gen*, the Japanese manga series by Keiji Nakazawa that Raina Telgemeier alludes to in her cover illustration 2011 *Handbook of Research on Children's and Young Adult Literature*, cannot help but have a profound effect on the reader. Gen's family and home are destroyed by the atomic bomb, and his survival and recovery are harrowing and inspiring.

Educators and parents share that common intuition of the profound impression felt by most readers. The research reported here does not contradict that intuition, but rather agrees with the ambiguity of Telgemeier's illustration: the girl thinks her life is ruined, the mother assures her life will be better for it. The effects of fiction reading may be complex and contingent. Further research in the area will help us understand more of the possibly powerful impacts of reading.

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## Tables and Figures

	Control group		Program group		diff?
	mean	st. dev.	mean	st. dev.	
Age in May 2013	17.42	3.82	17.47	5.73	
Years in school	7.82	2.04	7.64	2.70	
How many books read past month	0.66	1.16	0.51	0.98	
How many titles listed?	0.41	0.92	0.33	0.78	
Is person female?	0.42	0.52	0.39	0.52	
Is person Bwaba?	0.59	0.50	0.59	0.55	
Does person have mobile phone?	0.45	0.51	0.42	0.54	
Subscriber to library?	0.20	0.42	0.20	0.43	
From village with library?	0.46	0.53	0.45	0.55	
Did misspell mother job?	0.25	0.45	0.23	0.45	
Did misspell father job?	0.15	0.38	0.18	0.41	
Agrees that can trust people?	0.25	0.45	0.29	0.49	
Observations	262		295		

Note: The final column "diff?" indicates whether significant statistical difference between control group and reading program group, respectively with statistical significance level indicated by \*  $p < 0.10$  \*\*  $p < 0.05$  \*\*\*  $p < 0.01$ . Means and standard deviations and differences between means are calculated for each variable with maximum data available for that variable; some variables, such as books read in previous 30 days, had missing observations. Means and differences using only the complete sample are qualitatively very similar. All variables are weighted by probability of inclusion in the sample, based on cluster and applicant pool. Means and differences without sampling weights are qualitatively similar.

Table 2: Means of socio-economic variables for participants and non-participants in experimental game sessions

	May 2013 session					August 2013 session					May 2014 session				
	non-participants			participants		non-participants			participants		non-participants			participants	
	mean	st dev		mean	st dev	mean	st dev		mean	st dev	mean	st dev		mean	st dev
Age in May 2013	18.86	4.75	***	17.05	4.9	18.28	4.71	**	17.19	4.96	17.83	4.86		17.25	4.94
Years in school	7.62	2.05		7.76	2.5	7.56	2.24		7.78	2.46	7.60	2.33		7.79	2.45
How many books read past month	0.56	1.03		0.59	1.09	0.55	1.05		0.59	1.08	0.49	0.97		0.63	1.12
How many titles listed?	0.18	0.64	***	0.42	0.9	0.25	0.74	*	0.40	0.88	0.24	0.69	***	0.43	0.92
Is person female?	0.31	0.5	**	0.43	0.52	0.39	0.51		0.41	0.52	0.34	0.49	**	0.44	0.53
Is person Bwaba?	0.56	0.52		0.60	0.53	0.48	0.52	***	0.63	0.52	0.50	0.53	***	0.64	0.52
Has access to phone?	0.51	0.53	*	0.42	0.52	0.44	0.52		0.44	0.53	0.48	0.52		0.42	0.53
Subscriber to library?	0.12	0.36	**	0.22	0.44	0.14	0.38	*	0.22	0.44	0.15	0.38	**	0.23	0.45
Live in library village?	0.28	0.49	***	0.50	0.55	0.41	0.52		0.47	0.55	0.41	0.52		0.48	0.55
Did misspell mother job?	0.29	0.47		0.22	0.44	0.26	0.46		0.23	0.45	0.21	0.44		0.25	0.46
Did misspell father job?	0.15	0.37		0.17	0.4	0.14	0.36		0.18	0.40	0.15	0.38		0.18	0.40
Agrees that can trust people?	0.32	0.49		0.26	0.46	0.28	0.46		0.27	0.47	0.31	0.47		0.25	0.46
In program group?	0.52	0.55		0.53	0.53	0.45	0.52	**	0.56	0.54	0.51	0.52		0.54	0.54
Attended May 2013 session						0.38	0.49	***	0.91	0.29	0.59	0.49	***	0.88	0.33
Attended August 2013 session	0.33	0.47	***	0.89	0.32						0.52	0.5	***	0.89	0.31
Attended May 2014 session	0.37	0.48	***	0.74	0.44	0.31	0.46	***	0.77	0.42					
n=	122			435		132			425		188			369	

Notes: All variables are weighted by probability of inclusion in the sample, based on cluster and applicant pool. Results are qualitatively similar using unweighted data. Middle column for each game session indicates whether difference between non-participants and participants was statistically significantly different. \* p<0.10 \*\* p<0.05 \*\*\* p<0.01. Adjustment for multiple comparisons not made in the table.

Table 3: Differences in means of indicators of reading, according to whether in program group or control group											
		(1)			(2)						
		Survey weighted means			Lee bounds test of difference in means						
		Control	Program	pval	Difference	St. error	p-value		CI lower	CI upper	
Number of titles	May 2013	0.453	0.389	0.386	-0.055	0.072	0.443		-0.265	0.074	
	August 2013	0.357	1.551	0.000 ***	1.365	0.148	0.000 ***		0.809	1.608	
	March 2014	0.612	0.805	0.15	0.194	0.133	0.147		-0.152	0.454	
	May 2014	1.912	1.959	0.711	0.095	0.18	0.599		-0.243	0.418	
Number of titles JTL program only	May 2013	0.059	0.063	0.891	0.005	0.03	0.869		-0.097	0.057	
	August 2013	0.174	1.195	0.000 ***	1.152	0.122	0.000 ***		0.609	1.352	
	March 2014	0.196	0.402	0.007 ***	0.207	0.08	0.010 ***		-0.020	0.364	
	May 2014	1.228	1.205	0.849	0.006	0.15	0.969		-0.326	0.272	
Write down a character?	August 2013	0.477	1.605	0.000 ***	1.306	0.145	0.000 ***		0.738	1.544	
	March 2014	0.988	1.627	0.000 ***	0.641	0.158	0.000 ***		0.331	0.949	
	May 2014	2.053	2.183	0.266	0.173	0.14	0.216		-0.125	0.422	
Write what character did?	August 2013	0.419	1.405	0.000 ***	1.14	0.143	0.000 ***		0.566	1.375	
	May 2014	2.014	2.044	0.803	0.079	0.152	0.6		-0.227	0.349	
(3)											
Randomization inference											
Mean of program group minus mean of control, and asterisks indicating statistical significance											
		All sample p-value	Bekuy	Bouahoun	Béréba	Béréba alentours	Dohoun	Karaba	Kiere	Ouakuy	Sara
Number of titles	May 2013	0.588									
	August 2013	0.000 ***	1.48***	1.5***		-.46**	.54*	1.38***	.58**	1.64***	1.56***
	March 2014	0.138		.42*		.86***	.9**				
	May 2014	0.780	-.82**	.74**							
Number of titles JTL program only	May 2013	0.739									
	August 2013	0.000 ***	.92**	1.54***		.56**	.82***	1.66***	.3*	1.28***	1.48***
	March 2014	0.012 **		.44**							.48**
	May 2014	0.678	-.6*					-.74**			
Write down a character?	August 2013	0.000 ***	.88**	1.52***		1.18***		1.16***		1.64***	1.56***
	March 2014	0.000 ***		.8**					.8**		1.3**
	May 2014	0.286	-.44*	.7**							
Write what character did?	August 2013	0.000 ***	1.26***	1.48***		.8***		.88*	.6**	1.2***	1.34***
	May 2014	0.941									

Notes: \* p<0.10 \*\* p<0.05 \*\*\* p<0.01 for difference between treatment and control. Survey weights used in column (1) are described in text. Lee bounds test is described in text, and so-called "tightening variable" was indicator variable of Bwaba ethnicity. The tightening variable does not always work well for the titles, while it does for the game outcomes; for consistency the results are reported here.

Table 4: Effects of assignment into reading program (treatment) on reading outcomes

	Titles listed in May 2013			Titles listed in August 2013			Titles listed in March 2014			Titles listed in May 2014		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
In program group?	-0.0631 (0.0728)	-0.0472 (0.0692)	-0.0428 (0.0689)	1.195*** (0.111)	1.288*** (0.100)	1.299*** (0.0987)	0.194 (0.134)	0.284** (0.118)	0.311*** (0.115)	0.0473 (0.128)	0.0829 (0.119)	0.0836 (0.119)
Years schooling (May 2013)		0.0916*** (0.0263)	0.0762*** (0.0265)		0.280*** (0.0418)	0.256*** (0.0419)		0.296*** (0.0479)	0.259*** (0.0475)		0.230*** (0.0436)	0.228*** (0.0464)
Is person female?		-0.0392 (0.0710)	-0.0475 (0.0709)		0.0407 (0.108)	0.0249 (0.108)		-0.114 (0.132)	-0.149 (0.128)		-0.155 (0.128)	-0.156 (0.128)
Is person Bwaba?		0.0327 (0.0823)	0.00814 (0.0836)		0.207 (0.127)	0.174 (0.125)		0.221 (0.147)	0.159 (0.139)		-0.198 (0.151)	-0.201 (0.151)
Age in May 2013		-0.0147 (0.0145)	-0.0156 (0.0143)		-0.0299 (0.0217)	-0.0327 (0.0216)		-0.0438** (0.0219)	-0.0470** (0.0208)		-0.00875 (0.0252)	-0.00902 (0.0252)
Does village have library?		0.145 (0.303)	0.161 (0.293)		-0.0182 (0.454)	0.0232 (0.450)		0.668 (0.523)	0.665 (0.521)		-0.860*** (0.311)	-0.852*** (0.315)
How many titles read in March 2013?			0.104** (0.0457)			0.159** (0.0748)			0.262*** (0.0902)			0.0140 (0.0805)
Constant	0.453*** (0.0562)	-0.0864 (0.291)	-0.00382 (0.286)	0.357*** (0.0660)	-1.048** (0.442)	-0.899** (0.444)	0.612*** (0.0880)	-1.359*** (0.487)	-1.159** (0.488)	1.912*** (0.0904)	0.869* (0.490)	0.880* (0.500)
R-squared	435	430	430	425	424	424	402	402	402	369	368	368
Observations	0.002	0.103	0.114	0.207	0.379	0.388	0.005	0.246	0.269	0.000	0.148	0.148

Notes: Estimated using Stata svy regress command. \* p<0.05 \*\* p<0.01 \*\*\* p<0.001. Dummy variables for village clusters included but not reported.

Figure 1: Youth reading program participants increased # of titles listed as read in previous 30 days August 2013

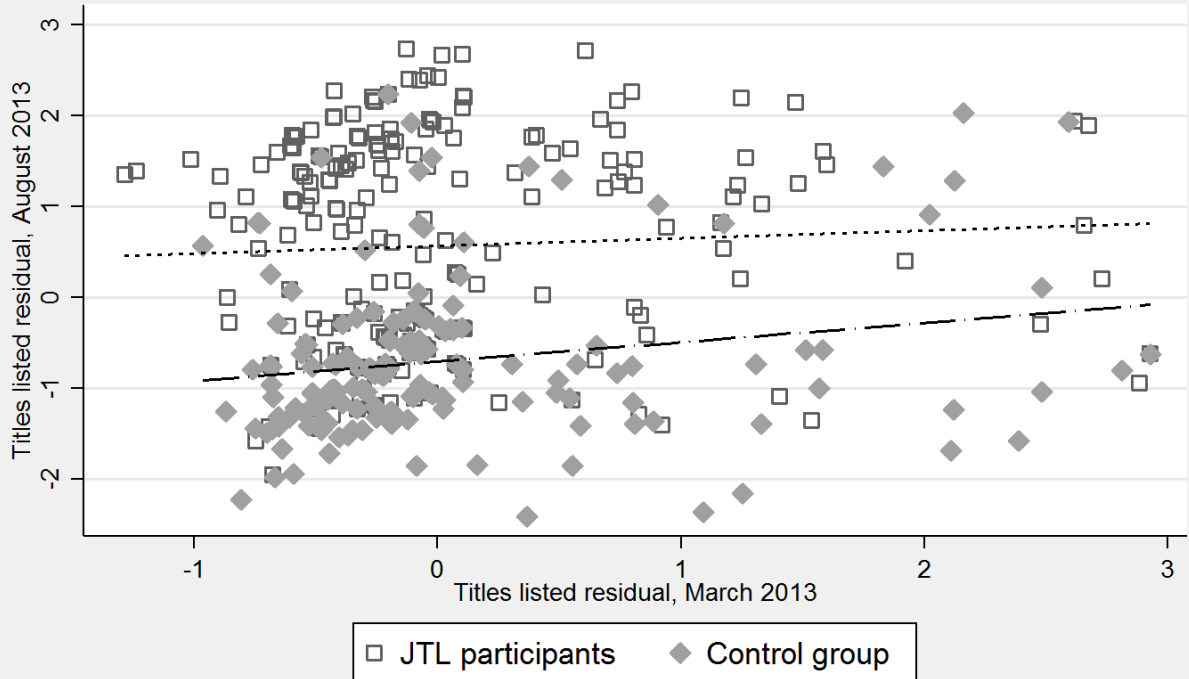


Table 5: Means and standard deviation of game outcomes by game session

	May 2013			August 2013			May 2014	
	mean	st dev		mean	st dev		mean	st dev
Contribution in trust game	247.64	135.77	***	225.43	140.06		220.79	134.74
Weighted average of fraction returned in trust game	1.25	0.73	*	1.30	0.84	**	1.16	0.73
Contribution in public goods game	275.68	166.93	***	224.12	151.17		237.98	166.43
Count of times chose to be patient	18.9	8.71		17.88	8.91		4.80	3.34
Was consistent in patience game?	0.62	0.51	***	0.65	0.50	*	0.71	0.50
Choice in risk game	3.26	1.81		3.18	2.00		3.26	2.10
Sample size	435			425			369	

Notes: \*  $p < 0.10$  \*\*  $p < 0.05$  \*\*\*  $p < 0.01$  for difference between outcome in game session and outcome in game session of May 2014, with exception of count of times chose to be patience, where May 2014 game had 10 choices and other sessions had 28 choices. Game outcomes are weighted by probability of inclusion in sample.

Table 6: Differences in means of outcomes of experimental games measuring economic preferences, according to whether in program group or control group

		(1)			(2)								
		Survey weighted means			Lee bounds test of difference in means								
		Control	Program	pval	Difference	St. error	p-value	CI lower	CI upper				
Contribution in trust game	May 2013	225.99	267.57	0.001 ***	47.12	16.17	0.004 ***	8.09	75.24				
	August 2013	218.34	231.60	0.29	32.75	15.62	0.036 **	-43.41	58.45				
	May 2014	231.19	209.68	0.121	-14.09	16.67	0.398	-66.39	13.97				
Weighted average of fraction returned in trust game	May 2013	1.19	1.31	0.073 *	0.14	0.08	0.094 *	-0.04	0.29				
	August 2013	1.21	1.36	0.041 **	0.29	0.10	0.005 ***	-0.18	0.45				
	May 2014	1.12	1.17	0.451	0.13	0.09	0.158	-0.23	0.29				
Contribution in public goods game	May 2013	268.90	282.32	0.4	20.75	17.07	0.224	-22.13	50.03				
	August 2013	220.86	226.89	0.665	26.43	16.52	0.11	-52.14	53.61				
	May 2014	237.88	236.32	0.923	11.33	20.06	0.572	-54.84	44.70				
Choice in risk game	May 2013	3.20	3.34	0.443	0.22	0.20	0.285	-0.29	0.56				
	August 2013	3.22	3.15	0.717	0.18	0.24	0.453	-0.81	0.58				
	May 2014	3.12	3.34	0.285	0.30	0.26	0.251	-0.32	0.75				
Count of times chose to be patient	May 2013	18.95	19.04	0.895	0.50	0.85	0.556	-1.58	1.97				
	August 2013	18.51	17.41	0.15	0.67	1.12	0.547	-4.01	2.51				
	May 2014	4.50	5.01	0.116	0.63	0.41	0.122 *	-0.29	1.35				
(3)													
Randomization inference													
Mean of program group minus mean of control, and asterisks indicating statistical significance													
		All sample p-value			Bekuy	Bouahoun	Béréba	Béréba alentours	Dohoun	Karaba	Kiere	Ouakuy	Sara
Contribution in trust game	May 2013	0.001 ***				51*	79*	61**		86*			
	August 2013	0.224						57*					
	May 2014	0.303		91**	-54*						-80*		
Weighted average of fraction returned in trust game	May 2013	0.111											.56***
	August 2013	0.020 **		.46**									
	May 2014	0.397											
Contribution in public goods game	May 2013	0.241		111***									
	August 2013	0.366											
	May 2014	0.881		101**							-129**	-71*	
Choice in risk game	May 2013	0.404											
	August 2013	0.833											
	May 2014	0.417			.84*					1.12*			
Count of times chose to be patient	May 2013	0.575											-3.84*
	August 2013	0.184		-4.88**		6.6*	-3.74**			-4.58**			-4.4*
	May 2014	0.261				2.64**					1.5**		

Notes: \* p<0.10 \*\* p<0.05 \*\*\* p<0.01 for difference between treatment and control. Survey weights used in column (1) are described in text. Lee bounds test is described in text, and so-called "tightening variable" was indicator variable of Bwaba ethnicity.



Table 7: Effects of selection into reading program (treatment) on amounts contributed and amounts returned in trust game

	Contributed									Returned								
	Trust game May 2013		Trust game August 2013			Trust game May 2014			Trust game May 2013		Trust game August 2013			Trust game May 2014				
	(1)	(2)	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(1)	(2)	(3)	(1)	(2)	(3)		
In program group?	41.58*** (12.62)	40.27*** (12.07)	13.26 (12.52)	10.16 (12.25)	2.957 (11.87)	-21.52 (13.85)	-26.14* (13.50)	-35.11** (13.99)	0.117* (0.0653)	0.0858 (0.0631)	0.150** (0.0731)	0.132* (0.0693)	0.0635 (0.0631)	0.0556 (0.0736)	0.0231 (0.0685)	0.00747 (0.0668)		
Is person female?		8.842 (12.14)		31.66** (12.58)	26.50** (12.25)		16.47 (13.68)	16.58 (13.85)		-0.0767 (0.0629)		0.153** (0.0710)	0.134** (0.0624)		0.193*** (0.0719)	0.208*** (0.0669)		
Is person Bwaba?		10.29 (14.66)		10.29 (14.67)	9.255 (14.52)		5.018 (15.04)	-3.267 (15.49)		0.0371 (0.0770)		-0.0480 (0.0850)	-0.0448 (0.0687)		0.0513 (0.0834)	0.0241 (0.0765)		
Years schooling (May 2013)		-13.71** (5.441)		-15.90*** (4.997)	-11.92** (4.605)		-15.17*** (5.822)	-12.10** (5.641)		-0.106*** (0.0242)		-0.113*** (0.0262)	-0.0707*** (0.0224)		-0.118*** (0.0270)	-0.0761*** (0.0255)		
Age in May 2013		6.730** (2.946)		0.803 (2.832)	-2.098 (2.659)		6.100* (3.127)	5.738* (3.191)		0.0296* (0.0155)		-0.00670 (0.0153)	-0.0231 (0.0142)		0.0181 (0.0170)	0.0104 (0.0161)		
How much FCFA contributed in trust game May 2013 (or returned in final cols.)					0.281*** (0.0536)			0.307*** (0.0624)					0.584*** (0.0484)			0.461*** (0.0568)		
Constant	226.0*** (9.213)	253.2*** (54.61)	218.3*** (8.758)	303.3*** (54.24)	241.2*** (54.41)	231.2*** (10.47)	275.9*** (61.71)	162.9** (65.85)	1.193*** (0.0488)	1.647*** (0.306)	1.214*** (0.0491)	2.177*** (0.322)	1.327*** (0.285)	1.115*** (0.0521)	1.821*** (0.327)	0.993*** (0.318)		
Observations	426	421	422	421	374	354	353	307	421	416	422	421	368	353	352	305		
R-squared	0.025	0.093	0.003	0.064	0.161	0.007	0.085	0.177	0.008	0.096	0.011	0.124	0.384	0.002	0.133	0.299		

Notes: Estimated using Stata svy regress command. \* p<0.05 \*\* p<0.01 \*\*\* p<0.001.

Table 8: Effects of assignment into reading program (treatment) on choices in public goods games

	Public goods game May 2013		Public goods game August 2013			Public goods game May 2014		
	(1)	(2)	(1)	(2)	(3)	(1)	(2)	(3)
In program group?	13.42 (15.93)	7.099 (14.99)	6.034 (13.92)	2.523 (13.48)	2.670 (13.16)	-1.569 (16.15)	-8.671 (14.72)	-7.176 (15.34)
Is person female?		13.28 (15.70)		39.09*** (13.82)	31.59** (13.59)		58.51*** (16.16)	39.81** (15.96)
Is person Bwaba?		42.70** (17.69)		20.61 (16.64)	4.276 (16.96)		-27.91 (19.49)	-20.64 (19.28)
Years schooling (May 2013)		-25.12*** (5.821)		-19.24*** (4.784)	-10.33** (4.569)		-24.72*** (6.653)	-14.60** (6.587)
Age in May 2013		1.359 (3.315)		1.524 (2.843)	1.119 (2.810)		2.484 (3.673)	3.703 (3.729)
Choice in public good game May 2013					0.310*** (0.0427)			0.284*** (0.0508)
Constant	268.9*** (11.68)	424.0*** (63.24)	220.9*** (10.67)	350.9*** (51.41)	197.1*** (51.37)	237.9*** (11.73)	398.3*** (66.07)	213.1*** (70.68)
Observations	422	417	424	423	373	364	363	314
R-squared	0.002	0.120	0.000	0.087	0.212	0.000	0.149	0.220

Notes: Estimated using Stata svy regress command. \* p<0.05 \*\* p<0.01 \*\*\* p<0.001.

Table 9: Effects of assignment into reading program (treatment) on choices in risk games

	Risk game May 2013		Risk game August 2013			Risk game May 2014		
	(1)	(2)	(1)	(2)	(3)	(1)	(2)	(3)
In program group?	0.132 (0.172)	0.0664 (0.162)	-0.0670 (0.185)	-0.0793 (0.182)	-0.0531 (0.186)	0.219 (0.204)	0.261 (0.193)	0.370* (0.201)
Is person female?		-0.715*** (0.165)		-0.369* (0.198)	-0.224 (0.199)		-0.0769 (0.210)	0.0125 (0.228)
Is person Bwaba?		0.219 (0.200)		0.0256 (0.234)	-0.0500 (0.227)		0.501** (0.235)	0.520** (0.249)
Years schooling (May 2013)		-0.0217 (0.0615)		0.0121 (0.0647)	0.0595 (0.0670)		0.0479 (0.0748)	0.0428 (0.0799)
Age in May 2013		0.0473 (0.0365)		0.0128 (0.0400)	-0.0266 (0.0415)		0.00476 (0.0416)	-0.0178 (0.0442)
Choice in risk game May 2013					0.317*** (0.0589)			0.175** (0.0675)
Constant	3.203*** (0.125)	2.585*** (0.717)	3.221*** (0.133)	3.446*** (0.823)	2.651*** (0.907)	3.122*** (0.142)	1.995** (0.855)	1.572 (0.966)
Observations	432	427	424	423	381	362	361	314
R-squared	0.001	0.120	0.000	0.035	0.115	0.003	0.103	0.149

Notes: Estimated using Stata svy regress command. \* p<0.05 \*\* p<0.01 \*\*\* p<0.001.

Table 10: Effects of assignment into reading program (treatment) on choices in patience games

	Patience game May 2013		Patience game August 2013			Patience game May 2014		
	(1)	(2)	(1)	(2)	(3)	(1)	(2)	(3)
In program group?	0.0976 (0.740)	0.162 (0.698)	-1.102 (0.764)	-0.822 (0.715)	-0.764 (0.677)	0.510 (0.324)	0.439 (0.303)	0.363 (0.313)
Is person female?		0.680 (0.682)		1.099 (0.757)	0.449 (0.693)		0.00267 (0.341)	0.00743 (0.345)
Is person Bwaba?		0.805 (0.835)		2.848*** (0.914)	2.541*** (0.835)		0.00139 (0.370)	0.196 (0.376)
Years schooling (May 2013)		0.168 (0.262)		0.168 (0.272)	0.314 (0.265)		0.00327 (0.123)	0.0177 (0.119)
Age in May 2013		-0.248 (0.179)		-0.514*** (0.161)	-0.437** (0.181)		-0.124* (0.0734)	-0.0404 (0.0765)
Choice in risk game May 2013					0.341*** (0.0633)			0.0875*** (0.0244)
Constant	18.95*** (0.518)	19.50*** (2.985)	18.51*** (0.532)	22.30*** (3.158)	14.16*** (3.474)	4.503*** (0.231)	5.326*** (1.416)	2.137 (1.564)
Observations	435	430	425	424	384	369	368	323
R-squared	0.000	0.140	0.005	0.161	0.269	0.007	0.137	0.185

Notes: Estimated using Stata svy regress command. \* p<0.05 \*\* p<0.01 \*\*\* p<0.001.

Table 11: Effects of assignment into reading program (treatment) on responses to survey questions												
	Trust questions March 2014			Trust questions May 2014			Patience questions March 2014			Patience questions May 2014		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
In program group?	0.0305 (0.0905)	0.0313 (0.0901)	0.0332 (0.0908)	0.0709 (0.0570)	0.0599 (0.0548)	0.0594 (0.0547)	-0.188 (0.159)	-0.146 (0.153)	-0.240 (0.165)	0.0253 (0.0542)	0.0455 (0.0522)	0.0490 (0.0570)
Is person female?		0.0549 (0.0955)	0.0551 (0.0957)		-0.0557 (0.0570)	-0.0553 (0.0568)		-0.214 (0.164)	-0.103 (0.166)		-0.0533 (0.0546)	-0.0321 (0.0560)
Is person Bwaba?		0.214* (0.116)	0.216* (0.116)		-0.0222 (0.0641)	-0.0242 (0.0641)		0.0825 (0.196)	0.108 (0.208)		0.0199 (0.0634)	0.0587 (0.0698)
Years schooling (May 2013)		0.0164 (0.0384)	0.0164 (0.0383)		-0.0851*** (0.0247)	-0.0849*** (0.0246)		0.0522 (0.0628)	0.0556 (0.0662)		0.0715*** (0.0198)	0.0717*** (0.0212)
Age in May 2013		0.0124 (0.0198)	0.0126 (0.0199)		-0.00937 (0.0142)	-0.00897 (0.0142)		0.0645* (0.0365)	0.0560 (0.0412)		-0.000147 (0.0114)	-0.00466 (0.0128)
Index of responses trust or risk, May 2013			-0.0475 (0.105)			0.0549 (0.0701)			-0.137 (0.0904)			-0.0866*** (0.0326)
Constant	1.847*** (0.0654)	1.223*** (0.381)	1.234*** (0.378)	2.089*** (0.0400)	2.887*** (0.256)	2.859*** (0.262)	4.138*** (0.115)	2.472*** (0.694)	2.873*** (0.816)	2.587*** (0.0409)	2.172*** (0.215)	2.355*** (0.243)
Observations	363	363	363	365	364	364	392	392	336	363	362	319
R-squared	0.000	0.042	0.042	0.005	0.076	0.078	0.004	0.099	0.109	0.001	0.092	0.118

Notes: Estimated using Stata svy regress command. \* p<0.05 \*\* p<0.01 \*\*\* p<0.001.