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Stopped in the Name of the Law: Administrative Burden and its Implications for Cash Transfer Program Effectiveness

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Summary. — Cash transfer programs have achieved wide-ranging success in reducing poverty, yet there is little empirical research on how program rules and administrative capacity might limit program effectiveness. We examine administrative burden and quantify its implications for grant access and impacts in the South African Child Support Grant (CSG) program, as the age of eligibility and application requirements changed over time. We find that approximately 60% of the sampled children experienced an interruption or disconnection in cash transfer receipt, and that both timing and "dosage" loss are associated with adolescent engagement in risky behaviors, and for females, lower educational attainment.

Key words — cash transfers, program implementation, risky behaviors

1. INTRODUCTION

In the last two decades, cash transfer programs have emerged as a primary strategy for poverty reduction and social protection in Latin America and are rapidly expanding in Asia and Africa as well (Levy, 2007; Rawlings & Rubio, 2005; The Economist, 2015). Importantly, the implementation of cash transfer programs has often been accompanied by rigorous evaluation, generating valuable knowledge about how impacts vary across key program features, such as conditions on benefit receipt, targeting mechanisms, duration of benefits and institutional configurations for program monitoring and benefit distribution (Fiszbein & Schady, 2009; Handa, Devereux, & Webb, 2011). We find comparatively little empirical investigation in the literature, however, about how program rules, administrative capacity, and related factors that affect program access and the duration of benefit receipt may have moderated program impacts or limited the effectiveness of cash transfers in improving the lives of the poor. Qualitative research has identified administrative burdens and other barriers to cash transfer receipt, such as application and documentation requirements, lack of understanding or awareness of eligibility criteria, travel costs, wait times and more, and yet there has been little in the way of quantification of their costs and their implications for program impacts (Tabor, 2002).

In this paper, we undertake an intensive examination of access to cash transfer program benefits among households with children eligible for the South African Child Support Grant (CSG) program, an unconditional, means-tested cash transfer program that began in 1998 and underwent numerous changes in program rules and administration. At the time that it was first introduced, the CSG was limited to households with children younger than 7 years old (Agüero, Carter, & Woolard, 2007). Over the subsequent 14 years, the age limit for CSG eligibility increased multiple times (see Figure 1), until it was ultimately extended in 2012 to cover children up until their eighteenth birthday (Child Support Grant, 2012). These policy changes, along with other modifications to program application requirements and processes, contributed to variation in the age of first receipt and timing of grant receipt and to the duration or "dosage" of the cash transfer. As we show in our analysis, putatively avoidable interruptions to and disconnections from grant receipt associated with these changes also appreciably reduced total months of CSG receipt.

We construct measures of both *intended* cash transfer dosage and *actual* CSG receipt in months, the latter accounting for interruptions or prematurely permanent disconnections from grant receipt. We also investigate what factors contribute to the disconnections or disruptions, of which we find the large majority occur when the child is still legally eligible to receive benefits. Next we empirically examine the relationship between CSG receipt and program impacts on children's engagement in risky behaviors in adolescence using generalized propensity score modeling. We then use propensity score matching with exact matching (on intended CSG dose) to estimate how the programs' effects on children's educational attainment and engagement in risky behaviors are moderated when children receive less than the intended dose. Finally, we also use propensity score matching with exact matching (on actual CSG dose) to assess the implications of the timing of grant receipt for the magnitude of effects, specifically investigating the importance of receiving benefits during adolescence for youth participation in risky behaviors.

In the following section, we present a brief review of the literature on administrative burden and applicant costs in social welfare/protection programs, with attention to what we know about their implications for program effectiveness and to evidence of administrative burden in the South African CSG. We then present additional background on the South African CSG and describe the data and samples we use in this study. Next we describe in greater detail the methods employed in our analysis, followed by the presentation of

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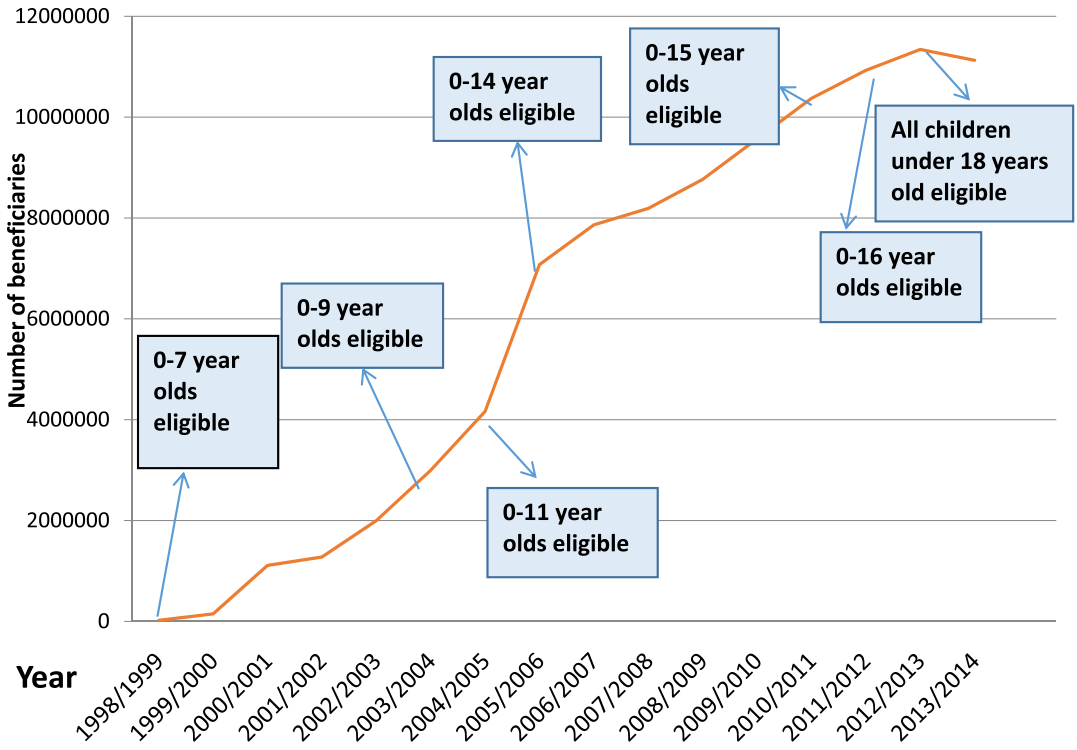

Figure 1. *Age of eligibility for children's grant receipt over time.*

our research findings. We conclude with a discussion of the implications of our results for improving cash transfer program effectiveness.

2. LITERATURE ON ADMINISTRATIVE BURDEN AND ITS IMPLICATIONS

In their study of a means-tested social protection program in the U.S. (Medicaid), Moynihan, Herd, and Rigby (2013) examined what they called "compliance administrative burden," or the onerous and sometimes problematic rules that are embodied in formal application requirements for accessing a public program. As they and other researchers have pointed out, means-tested transfer programs (like the CSG) that target poorer populations are, by design, more complex and demanding in their forms and procedures (i.e., than programs offering universal benefits), given the legal obligation to verify eligibility (Currie, 2006; Korpi & Palme, 1998). Some of the administrative burdens associated with means-tested cash transfer programs include learning costs, or the investment it takes an individual to find out about a program and its eligibility and application requirements; the procedural costs of applying for benefits, including the transaction costs of compiling required documents and other information, traveling to a designated location to apply, and completing forms, tests, or other requirements of the process; and also psychological costs associated with the intrusiveness of the application demands or rejection or stigma that might be experienced in the process (Bhargava & Manoli, 2011; Herd, DeLeire, Harvey, & Moynihan, 2013; Moynihan *et al.*, 2013). The Economist (2015, p. 54) recently described these as "ordeal mechanisms," implying that they are constructed to deter applications to cash transfer programs, while also acknowledging that "the tougher the ordeal, the greater the number of needy candidates who will fail to qualify."

One predictable and frequent finding of studies that have assessed these administrative burdens is that the resulting take up of benefits is constrained and/or low. In the U.S., take-up rates for a number of important social program interventions hover around 25%, including training and work supports, housing programs, and Medicaid (Currie, 2006; Shore-Sheppard, 2008; Wallace, 2002). In addition, some research has directly linked administrative burden to disconnections from or denial of benefits. In a study of welfare case closings, Bennett (1995) found that more than a quarter of welfare claimants had their benefits cut off while eligible because of problems with their documentation. It is more challenging, however, to find research that distinguishes effects or consequences associated with administrative actions originating on the "supply-side" from those linked to individuals' responses to administrative burden on the "demand side".[1] Brodkin and Majmundar (2010) differentiated program exclusion driven by formal or informal organizational practices *vs.* individual preferences and found that higher administrative or procedural burden established by the agency in the application process for Temporary Assistance for Needy Families (TANF) was associated with larger welfare caseload declines. That is, clients were not deterred from applying for benefits, but the rate of exit from the process or from welfare receipt due to procedural burdens was higher for more disadvantaged clients (i.e., those with less education and deeper poverty). This latter result was not unexpected, given that prior research has shown that more disadvantaged individuals have fewer personal resources to draw on in navigating administrative burden and procedural requirements (Bendick, Lavine, & Campbell, 1978; Cherlin, Bogen, Quane, & Burton, 2002; Super, 2004).

In one of the few studies that empirically examines these particular issues in a developing country context, Álvarez, Devoto, and Winters (2008) investigated the reasons behind dropouts from Mexico's Oportunidades program and the role of program requirements and changes in administrative rules in household dropouts. They also distinguished between household self-selection (e.g., failing to meet program conditions or to pick up checks) and the possibility that the grant conditions create high costs for very poor households that preclude their receipt of the cash transfers and work against the program's goal of reaching the most vulnerable. They found program administration had a significant influence on whether recipients stayed in the program, although only some of the ways in which it operated were problematic. They found particular cases—in the effort to aid indigenous populations and the extreme poor in low-marginality communities—where the operational guidelines for the program were increasing dropouts. Alternatively, they also found that a new "just-in-time monitoring system" led to corrections of inclusion errors that likely improved the program's efficiency. They also noted that the Oportunidades program administrators were actively working to create re-entry mechanisms for those who dropped out but were still very poor and vulnerable.

The literature on administrative burden also raises the issue as to what extent the imposition of procedural requirements is at the discretion of bureaucrats, whereas they may impose additional demands, or while invoking the law, misinterpret or misapply rules, require resubmission of documents, or slow the process of access to benefits with long waits for appointments or by requiring repeat visits (Bennett, 1995; Brodkin, 1997, 2007; Meyers, Glaser, & MacDonald, 1998; Sandfort, 2000; Soss, 2000). Moreover, although program rules for national or federal programs are typically formulated at the national level, their implementation is usually delegated to a lower level of government, allowing for the possibility that procedural discretion and the resulting administrative burdens will vary geographically or politically. In fact, there is a robust body of work that suggests bureaucratic discretion is sometimes used with the explicit intent to restrict access to benefits—also known as "bureaucratic disentitlement"—for purposes of social control, discrimination or otherwise rationing access to limited resources (Lipsky, 1984; Scott, 1997; Soss, Fording, & Schram, 2011). As Moynihan *et al.* (2013, p. 2) explain, "by constructing complex, confusing, and time-consuming application procedures, the state can effectively thwart an individual from accessing benefits, even if eligible by law." In their analysis of state Medicaid policies, Moynihan *et al.* found that in more socially liberal states in which Democrats exerted stronger political control, Medicaid claimants were less likely to face added administrative burdens.

3. ADMINISTRATIVE BURDEN IN CASH TRANSFER PROGRAMS AND THE SOUTH AFRICAN CSG

The limitations of institutional capacity for program implementation are also well-documented in the literature on cash transfer programs in developing countries. The International Labor Organization (Tabor, 2002) compiled a long list of administrative problems in cash transfer programs, including: political interference and fragmentation in policymaking and administrative responsibilities; excessive administrative costs; poor staff remuneration; neglect of compliance and enforcement functions; difficulties in record-keeping; excessively complex procedures; delays in processing benefit claims; and inadequate attention to ensuring that applicants understand the rules and requirements of the program. Cash transfer programs with a means-test for establishing the eligibility of beneficiaries elevate the complexities and demands of program administration, and as the targeting criteria become more complicated, barriers to program take up increase (Bastagli, 2009). Furthermore, if eligibility criteria and application requirements are not widely understood, research suggests that coverage is more likely to be uneven, with the poorer among those eligible more likely to be excluded (Devereux, 2002; Hernanz, Malherbet, & Pellizzari, 2004).

Research specific to the CSG in South Africa describes a plethora of administrative and implementation challenges since the program's initiation. Although the CSG was (until recently) an unconditional cash transfer program, it still imposes a number of requirements that have to be met before a caregiver can begin to receive the CSG on behalf of an eligible child. For example, applicants are required to have a birth certificate for the child, an identity document for the mother or caregiver, a hospital card, a health record for the child known as the Road to Health Card, and documentation that the parental or caregiver income meets the means test limits (Zembe-Mkabile *et al.*, 2012). Studies have confirmed that these documentation requirements limit access to the CSG; in fact, at the time that the CSG first rolled out in 1998, only one-fourth of children in the eligible age range had a birth certificate (UNICEF, 2005). More recent studies confirm that document requirements continue to be an important barrier to grant access (Department of Social Development (DSD), South African Social Security Agency (SASSA) and UNICEF, 2011; Mirugi-Mukundi, 2009; National Income Dynamics Study (NIDS), 2009; Zembe-Mkabile *et al.*, 2012). Besides those described above, the DSD–SASSA–UNICEF study pointed to numerous other documents that might be requested in course of the application process ("in special circumstances"), such as: a police affidavit (if key documents are missing); a letter with the ward councilor's stamp (to establish proof of address); proof of (un)employment for the means test, and others. Comments from focus group participants in this study (pp. 27–28) are illuminating as to the types of *discretionary* administrative burden that CSG applicants have experienced, as well as to how these barriers affected the timing or age at first grant receipt:

> "*Sometimes they ask you to provide proof of residence, or electricity or water. If you are unemployed or staying in RDP houses you cannot have these things, because we do not pay for water and do not use metered electricity.*"
>
> "*When you get there they tell you to go and get an affidavit, and when you come back they tell you it is wrong.*"
>
> "*They wanted an affidavit proving that the father of the child has agreed that you apply for the CSG.*"
>
> "*She had a problem with her ID book, so she couldn't register. The child was only registered six years later because of no ID.*"
>
> "*You can lose your ID and go to Home Affairs to get another one, and you find that you do not get it for a long time and so you cannot register.*"

These administrative burdens have made it less likely for children whose mothers are deceased or absent to get access to the CSG, even though they are among the poorest and most vulnerable of eligible children (McEwen, Kannemeyer, & Woolard, 2009; Case, Hosegood, & Lund, 2005).

Other dimensions of administrative burden brought to light in research on the CSG, although likely not limited to this particular government program, include long queues and waiting

periods, limited service hours in application offices and language and communication barriers, which are likely correlated with documented infrastructure problems (including inadequately trained staff and a dearth of computers and privacy in processing applications) (Goldblatt, Rosa, & Hall, 2006; Mirugi-Mukundi, 2009). The lack of consistency in the application of program rules across government offices also exacerbated problems associated with lack of knowledge about how and where to apply and awareness of policy changes and their implications for accessing the CSG (Patel, 2011). In effect, misinformation about eligibility and application requirements added to the costs of applying, which were already high for many due to travel times, long waits at the social development offices and lost work time (Zembe-Mkabile *et al*., 2012). Quotes from participants of the DSD–SASSA–UNICEF (2011) study illustrate some of these challenges as well:

> "*If welfare officials are in a certain village and you go and try to apply, people from that village wouldn't allow you to apply.*"
>
> "*My wife used up a lot of my money during the application process; I ended up spending more for the application than what we were going to get!*"
>
> "*You find that they ask you too many questions which you cannot respond to and you end up giving up.*"
>
> "*We never tried because I was working piece jobs, and I just heard that if you work it does not matter how much you earn, you don't qualify.*"

At the same time, the DSD-SASSA-UNICEF (2011) study also documents improvements in the CSG application process over time, stemming from concerted efforts by the government to increase program take up. Some of the more recent changes include simplified documentation requirements, more clearly communicated program rules and procedures, and faster processing times. By 2012, the grant's coverage had expanded to nearly 11 million beneficiaries, making it one of the potentially most important policy initiatives for improving the well-being of South African youth (SASSA, 2012).

4. IMPLICATIONS OF ADMINISTRATIVE BURDEN FOR PROGRAM EFFECTIVENESS

As discussed above, studies of administrative burden have primarily assessed its implications for access to program benefits or services. For example, Herd *et al*. examined how reductions in administrative burden affect (increase) child enrollment in Medicaid in Wisconsin; Bhargava and Manoli (2011) demonstrated that lack of awareness and misinformation depress take up of the Earned Income Tax Credit, and Brodkin and Majmundar (2010) showed how inappropriate administrative exclusion in TANF programs drove down welfare caseloads. Studies of cash transfer programs in developing countries have likewise focused largely on the implications of administrative burden for take up of benefits or targeting effectiveness, i.e., avoiding unintended exclusions of those eligible or inclusion of ineligible individuals or households (Álvarez *et al*., 2008; Handa, Huang, Hypher, Veras, & Davis, 2012; Tabor, 2002; Zembe-Mkabile *et al*., 2012).

Because cash transfers can have immediate effects in increasing household consumption (Devereux, 2002; Samson *et al*., 2008), as well as in buffering against macroeconomic shocks experienced by the poor, improving access to the grant by reducing administrative burdens should be reflected in program impacts. We also know that the timing of benefit receipt—specifically early *vs*. late—may matter for outcomes such as children' enrollment in schooling and grade completion, as well as health outcomes such as micronutrient status and anthropometry, where earlier receipt is associated with larger impacts (de Janvry & Sadoulet, 2006; Leroy, Ruel, & Verhofstadt, 2009; Heinrich, Hoddinott, & Samson, 2013). These studies also show linkages between cash transfer "dosage" or the size of the cash transfer and program impacts. However, we have not uncovered studies that show how unintended diminution of benefits or disconnections from cash transfer programs reduce or limit impacts. For example, Rawlings and Rubio (2005) found (through PROGRESA payment records) that 27% of the eligible population (in the evaluation sample) had not received any benefits after almost 2 years of program operation, which they noted could lead "intent to treat" and "treatment on the treated" program impacts estimates to diverge. Yet to our knowledge, there has been no formal analysis comparing "intent to treat" *vs*. "treatment on the treated" program impacts in PROGRESA (or other program evaluations) to quantify to what extent problems in benefit administration may have limited program impacts. We turn now to our analysis that aims to fill this gap in the literature.

5. BACKGROUND AND DATA

Beginning in 1998, the primary caregiver of a child in South Africa could apply for the CSG and receive a 100 Rand cash transfer per month, provided that a means test based on household income was satisfied. The caregiver was required to offer proof of household income, and if not the child's parent, evidence that efforts to secure funds from the child's parents were unsuccessful. However, due to low take-up, the means test was changed in 1999 to determine CSG eligibility based on the caregiver's and spouse's income only, and in 2008, benefit payments were adjusted for cost of living. These changes, along with the increases in the child's age of eligibility and other purposeful efforts described above to improve administrative capacity and reduce applicant burdens, led to steady increases in CSG program participation (as shown in Figure 1).

In September 2008, the South African Department of Social Development (DSD) called for a rigorous evaluation of the CSG, in line with its monitoring and evaluation responsibilities and belief in supporting evidence-based policy and resource allocation. The terms of reference (TOR) reiterated the fundamental right of children to social protection (as enshrined in the Bill of Rights in South Africa's Constitution), which precluded a random assignment evaluation design. By this time, coverage of the CSG eligible population was very high, and any eligible child found outside of the program would be required to be immediately enrolled. The TOR requested a quantitative and qualitative evaluation strategy and the design of survey instruments for collecting data to measure program impacts. However, because a planned follow-up survey data collection was aborted (due to time and resource constraints), the evaluation strategy relied primarily on variation in the timing and length of benefit receipt to identify program effects (DSD–SASSA–UNICEF, 2012).

(a) Study data

We use rich data that were collected in surveys of households and children (designed specifically for the CSG impact evaluation) to measure the effects of the CSG on children's

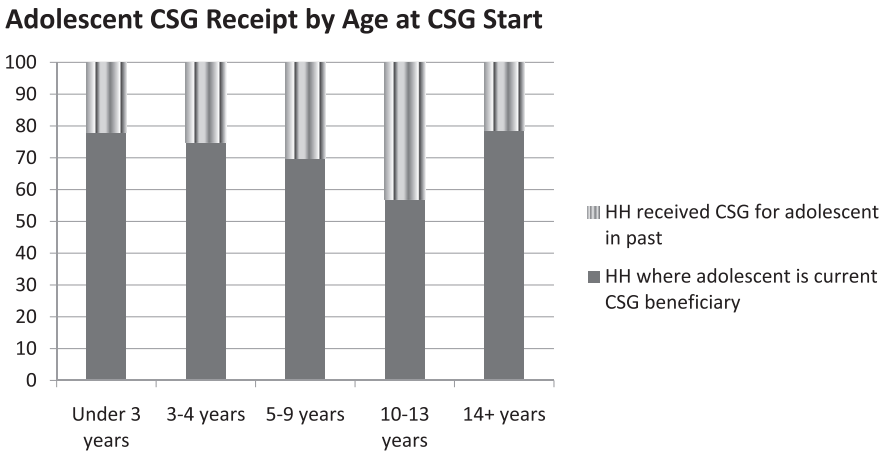

Figure 2. *Adolescents' age at first grant receipt.*

outcomes. These surveys were fielded in five South African provinces—Western Cape, Eastern Cape, KwaZulu-Natal, Gauteng, and Limpopo—between October 2010 and March 2011 using a two-stage process. The first stage consisted of selecting geographical areas (Primary Sampling Units or PSUs—the physical location where beneficiaries received their payments), with a probability of selection proportionate to the size of the CSG beneficiary population. Within the selected PSUs, households with three groups of children were chosen (using SASSA management information system data that verified the households' eligibility by the means test): (1) children born in 2000 who were enrolled in the CSG at birth or before they had reached 18 months; (2) children born in 2000 who enrolled between age 5 and 9 years, and (3) children 15–17 years old who were CSG beneficiaries (sampled from the SASSA data). In addition, a group of 15–17-year-old children who were not currently receiving the CSG were randomly selected in the PSUs to complete the surveys, although those cases are not included in this study.[2] One questionnaire gathered data from the entire household and another focused in depth on the sampled child. In addition, a confidential, self-administered survey was completed by adolescents[3] (in a private room) to gather sensitive information, after which the survey was sealed and handed to the field researcher. Together, these survey instruments collected detailed information on household wealth, demographic structure, caregiver and other household characteristics; CSG application, enrollment and access; children's schooling, labor, time allocation and participation in risky behaviors, and many more variables.

In this study, we use only the data collected from households with adolescents. The total sample consisted of 1,726 adolescents, and among these, 1,531 (89%) completed the self-administered, confidential survey.[4] As indicated above, our analysis sample excludes youth who were never CSG beneficiaries ($n = 439$); analyses of the household characteristics of these omitted cases showed that they were more advantaged than adolescents from households that had satisfied the means test (i.e., higher parental education and more income from work/wages). The household and adolescent survey data on adolescents receiving the CSG at the time of the survey or who had received it sometime in the past (and with completed confidential surveys) were combined with SASSA administrative data to develop detailed measures of grant receipt—both intended and actual—over the course of their childhood.

We also examined the age at which adolescents first started receiving the CSG, regardless of whether the household was currently receiving the CSG for the adolescent. Figure 2 shows the age at which adolescents first began receiving the CSG, stratified by whether the household is currently receiving the CSG for the adolescent ($n = 1{,}113$). This graph suggests that youth who first received the CSG at an early age (4 years or younger), or more recently at age 14 years or older, are significantly more likely to be in households that were *currently* receiving the CSG for the adolescent at the time of the questionnaire. Also notable is the comparatively low proportion of adolescents who first began receiving the CSG at age 10–13 years and that were in households currently receiving the CSG for them (i.e., 57% *vs*. 78% for those starting at the youngest or the oldest ages). Not surprisingly, the age at which adolescents first began receiving the CSG is correlated with the length of CSG receipt, but this exception creates a nonlinearity in the measure of CSG receipt.

(b) Measures of treatment receipt, interruptions and disconnections, and outcomes

Because our analysis is focused on adolescents who had a caregiver that received the CSG at some time in their lives (even if not at the time of the survey), our measures of treatment are constructed to capture "dose," or the number of months the adolescent was exposed to or could benefit from cash transfer receipt. The crude measure of CSG dose that was calculated and employed in the DSD–SASSA–UNICEF (2012) impact evaluation—using the age at which children first began to receive the grant as the start date and the survey date as the end date—was first replicated. However, using data from the household survey for adolescents that asked if receipt of the CSG had ever been interrupted, we found that 60% of all adolescents in the study sample had experienced an interruption in CSG receipt at some point. Furthermore, among those whose cash transfer receipt was interrupted, more than half did not restart CSG receipt again during the period that we observed them. We therefore sought to construct a more accurate measure of CSG dose that accounted for these interruptions and disconnections in CSG receipt.

To develop this improved measure of CSG receipt, we first generated variables based on responses to survey questions that recorded: (i) the first date that the adolescent received the CSG; (ii) the date of disconnection from CSG receipt; (iii) the date of CSG restart, if benefit receipt was restored;

(iv) the date of the survey, which represented the last date that we could possibly observe CSG receipt for the adolescent, and (v) the length of the interruption, or time (in months) between the interruption and restart dates. We then used these variables to measure an individual beneficiary's CSG dose (in months) in one of three ways: (1) from start date to stop date (for those who were disconnected), (2) from start date to survey date minus time interrupted (for those interrupted), and (3) from start date to the survey date (for those never interrupted). Individuals whose answers to these questions were contradictory or chronologically improbable were removed from the analysis sample—including those who reported starting the CSG before the program's initiation (the large majority, or 134 cases) or who reported a dose greater than the adolescent's age or an interruption longer than total grant receipt feasible or a problematic birthdate—leaving us with measures of "actual" dose for 459 male adolescents and 468 females in this study. Table 1 presents descriptive statistics for all measures used in this study and compares adolescent and household characteristics, treatment measures and adolescent outcomes for all adolescents with CSG receipt *vs*. the subsample of those with dosage measures (separately for males and females). As is evident in looking at Table 1, there were no statistically significant differences between the adolescents with and without the improved measures of actual CSG receipt.

To generate values of "intended" CSG dose for all adolescents in the sample, we calculated the number of months in which the adolescents were age-eligible for the program, starting from the month in which they first started receiving the CSG and through to the age when they would no longer be eligible to receive the grant (or when they were surveyed). This measure takes into account the changes in eligibility that occurred over the course of the program's rollout, i.e., in which the age of eligibility increased up to 9 years in 2003, 11 years in 2004, 14 years in 2005, 15 years in 2008, and up to age 18 in 2010. In effect, this measure is the dosage the adolescents would receive if there were no interruptions or disconnections from cash transfer receipt from the time they first became eligible to the time they were no longer eligible to receive the grant or were observed in the survey.

Additionally, we determine if any of these interruptions or stops occurred while an adolescent was still eligible to receive the CSG. We identify those whose grant receipt was interrupted or stopped while age-eligible as "bad stops" in the sample. We find that these "bad stops" account for 81.5% of interruptions and disconnections. This number corresponds closely to household self-reports of why the grant was stopped; of 399 households that responded, 85.7% believed that the grant was stopped because the child did not quality by the age eligibility rules. Only three of the 399 indicated that their income was too high, which is consistent with SASSA administrative data that identified these as households that satisfied the means test.[5] We also observe significantly lower rates of grant interruptions and stops for children enrolling in the CSG in 2007 and afterward (one-half to one-third those of prior years), which coincides with government efforts to simplify documentation requirements, process applications faster, and more effectively communicate changes in program rules (e.g., age of eligibility). We are cautious in attributing these changes to administrative reforms, however, as we also recognize that a longer time receiving the grant could make it more likely that we would we observe a stop or interruption.

Still, the cost of these stoppages in terms of lost grant receipt is high. We calculate the average "dose loss" as intended dose minus actual dose. The average dose loss among all adolescents in the sample is 19.7 months of cash transfer receipt; for those with "bad stops," the average dose loss is considerably higher at over 30 months (see Figure 3).

(c) *Outcomes*

Prior studies of the CSG have found that the cash transfers improve children's school attendance and nutrition and reduce child hunger, child labor, and risky behaviors among adolescents (DSD–SASSA–UNICEF, 2012; Samson, Heinrich, & Regalia, 2011). Given that we are focusing on adolescents in this study, we likewise model the impacts of the CSG on adolescent risky behaviors, as well as on their educational attainment. Researchers have described an "enormous risk associated with adolescence in South Africa," due to the high prevalence of HIV and alcohol use among young people that is consistently associated with sexual risk taking and sexual coercion (Kalichman & Kaufman, 2007; Morojele *et al*., 2004). In addition, a growing body of evidence suggests that risky behaviors, particularly sexual activity, vary with household consumption expenditures and income shocks, which implies a role for cash transfer programs in helping to mitigate risky behavior, as well as poverty and hunger (Yeh, 2006).

The administration of the confidential survey to adolescents yielded new data to quantitatively analyze the scope and implications of these risks to adolescents and the potential of the CSG to ameliorate them. We analyze the effects of the timing and dosage of cash transfers with improved measures of CSG receipt on the following risky behaviors (as confidentially reported by the adolescents at the time of the survey): sexual activity and number of sex partners; pregnancy; alcohol use and age at first alcohol use; drug use and criminal activity. Sexual activity is measured as an indicator that the adolescent "never had sex" (i.e., sexual intercourse); the number of sex partners is an interval measure, and pregnancy is an indicator of "ever pregnant." Approximately 17.5% of the adolescents in this sample reported having sexual intercourse, and 17% of these had more than one sex partner. Similarly, alcohol use is measured as "never drank alcohol," and drug use as "never used drugs." About one-third of these adolescents had started drinking alcohol and another quarter had initiated drug use. Age at first alcohol use is recorded as zero if the adolescent never drank alcohol, and lower values represent later starting ages (while higher values indicate an earlier start in drinking). Finally, criminal activity is a binary measure indicating "no criminal activity"; the youth were asked if they had ever participated in any of the following criminal activities: stealing, "housebreaking," rape or sexual assault, selling drugs, assault or "none." Approximately 27% of adolescents in our sample had engaged in at least one of these criminal activities. Educational attainment is measured as the highest grade completed, where the average is grade 8.8 for males and grade 9.2 for females (see Table 1).

6. METHODS AND MODEL SPECIFICATIONS

(a) *Estimating the impacts of CSG dosages*

We first follow the strategy of the DSD–SASSA–UNICEF (2012) impact evaluation in estimating CSG impacts by taking advantage of the variation in timing and length of CSG receipt among beneficiaries and using the same crude measure of CSG dosages. Although we do not model selection into the CSG because our estimation sample consists only of adolescents who were CSG beneficiaries at some point in their lives, we

Table 1. *Descriptive statistics for study measures (by gender and subsample)*

	Males with grant receipt			Males with dosage measure		
	# Obs.	Mean	Std. dev.	# Obs.	Mean	Std. dev.
Adolescent and household characteristics						
Age-1st grant receipt	556	8.126	4.088	459	8.155	3.531
Age-time of survey	556	15.871	0.711	459	15.856	0.700
HH education: K-5	556	0.234	0.424	459	0.237	0.426
HH education: 6–8	556	0.257	0.437	459	0.242	0.429
HH education: 9–11	556	0.218	0.413	459	0.216	0.412
HH education: 12+	556	0.095	0.294	459	0.100	0.301
HH disabled	556	0.079	0.270	459	0.081	0.273
HH chronically ill	556	0.399	0.490	459	0.403	0.491
HH age	552	50.922	12.273	456	51.009	12.531
HH female	556	0.649	0.478	459	0.660	0.474
HH not African	556	0.092	0.289	459	0.092	0.289
All HH income from CSG	556	0.164	0.370	459	0.161	0.368
Rural	556	0.394	0.489	459	0.403	0.491
Periurban	556	0.275	0.447	459	0.272	0.446
Informal setting	556	0.077	0.267	459	0.074	0.262
Gauteng	556	0.248	0.432	459	0.231	0.422
Eastern Cape	556	0.133	0.340	459	0.139	0.347
Western Cape	556	0.121	0.326	459	0.111	0.315
Limpopo	556	0.099	0.299	459	0.092	0.289
Adol. not aware of eligibility	556	0.106	0.308	459	0.109	0.312
Adol. encouraged HH to apply	556	0.261	0.439	459	0.270	0.445
Adol. knowledge-formal sources	556	0.182	0.386	459	0.192	0.394
Adol. knowledge-informal sources	556	0.507	0.500	459	0.510	0.500
Adol. knows eligible age	556	0.113	0.317	459	0.115	0.320
Mother applied	556	0.613	0.487	459	0.621	0.486
Re-applied due to change in eligibility	556	0.378	0.485	459	0.362	0.481
# times re-applied	556	0.777	1.333	459	0.719	1.160
Document problems	556	0.040	0.195	459	0.046	0.209
Hours waited reapplying	555	1.205	3.059	458	1.225	3.202
HH distance to social welfare office	555	6.697	11.980	458	6.428	11.607
HH knows eligible age	556	0.043	0.203	459	0.044	0.204
HH knowledge-formal sources	556	0.401	0.491	459	0.405	0.491
Treatment measures						
Never interrupted	453	0.393	0.489	459	0.390	0.488
Bad stop	477	0.509	0.500	455	0.495	0.501
Received CSG in adolescence	556	0.692	0.462	459	0.684	0.465
Intended CSG dose	556	119.354	24.310	459	119.200	24.584
Actual CSG dose	453	71.923	42.840	459	71.804	42.816
Adolescent outcomes						
Never had sex	393	0.814	0.389	332	0.822	0.383
Never used drugs	476	0.689	0.463	398	0.696	0.461
Never drank alcohol	509	0.631	0.483	421	0.644	0.479
Age at first alcohol use	429	0.914	1.837	360	0.906	1.832
No criminal activity	452	0.688	0.464	371	0.693	0.462
No gang activity	409	0.929	0.257	344	0.936	0.245
Number of sex partners	389	0.440	1.093	326	0.396	0.967
Ever pregnant						
Highest grade attained	553	8.859	1.449	456	8.840	1.443
	Females with Grant Receipt			Females with Dosage Measure		
	# Obs.	Mean	Std. Dev.	# Obs.	Mean	Std. Dev.
Adolescent and household characteristics						
Age-1st grant receipt	553	8.009	3.945	467	7.968	3.434
Age-time of survey	553	15.904	0.710	468	15.891	0.713
HH education: K-5	553	0.251	0.434	468	0.250	0.433
HH education: 6–8	553	0.268	0.443	468	0.265	0.442
HH education: 9–11	553	0.260	0.439	468	0.278	0.448
HH education: 12+	553	0.087	0.282	468	0.079	0.270
HH disabled	553	0.099	0.300	468	0.096	0.295
HH chronically ill	553	0.439	0.497	468	0.444	0.497

Table 1. (*continued*)

	Females with Grant Receipt			Females with Dosage Measure		
	# Obs.	Mean	Std. Dev.	# Obs.	Mean	Std. Dev.
HH age	552	50.444	12.193	467	50.452	12.364
HH female	553	0.637	0.481	468	0.635	0.482
HH not African	553	0.078	0.268	468	0.081	0.273
All HH income from CSG	553	0.146	0.354	468	0.150	0.357
Rural	553	0.398	0.490	468	0.404	0.491
Periurban	553	0.248	0.432	468	0.250	0.433
Informal setting	553	0.069	0.253	468	0.068	0.253
Gauteng	551	0.265	0.442	466	0.242	0.429
Eastern Cape	551	0.160	0.367	466	0.157	0.364
Western Cape	551	0.156	0.363	466	0.159	0.366
Limpopo	551	0.078	0.268	466	0.086	0.280
Adol. not aware of eligibility	553	0.101	0.302	468	0.109	0.312
Adol. encouraged HH to apply	553	0.291	0.455	468	0.278	0.448
Adol. knowledge-formal sources	553	0.197	0.398	468	0.182	0.386
Adol. knowledge-informal sources	553	0.568	0.496	468	0.577	0.495
Adol. knows eligible age	553	0.081	0.274	468	0.085	0.280
Mother applied	553	0.718	0.450	468	0.718	0.450
Re-applied due to change in eligibility	553	0.351	0.478	468	0.348	0.477
# times re-applied	553	0.817	1.958	468	0.801	1.877
Document problems	553	0.051	0.219	468	0.053	0.225
Hours waited reapplying	553	1.186	2.354	468	1.205	2.392
HH distance to social welfare office	550	6.913	11.866	465	6.626	11.617
HH knows eligible age	553	0.036	0.187	468	0.038	0.193
HH knowledge-formal sources	553	0.421	0.494	468	0.404	0.491
Treatment measures						
Never interrupted	553	0.345	0.476	468	0.408	0.492
Bad stop	485	0.497	0.501	464	0.481	0.500
Received CSG in adolescence	553	0.678	0.468	468	0.679	0.467
Intended CSG dose	553	120.143	24.354	468	120.259	24.723
Actual CSG dose	466	74.107	41.478	468	73.953	41.530
Adolescent outcomes						
Never had sex	440	0.895	0.306	371	0.906	0.293
Never used drugs	500	0.798	0.402	422	0.791	0.407
Never drank alcohol	508	0.717	0.451	432	0.704	0.457
Age at first alcohol use	455	0.734	1.704	386	0.777	1.748
No criminal activity	468	0.767	0.423	394	0.766	0.424
No gang activity	458	0.963	0.189	390	0.964	0.186
Number of sex partners	413	0.240	0.852	346	0.234	0.872
Ever pregnant	340	0.065	0.246	282	0.067	0.251
Highest grade attained	549	9.257	1.252	465	9.241	1.253

still need to be concerned with the potential for selection bias in the timing and length of CSG receipt. The qualitative research that preceded the survey data collection provided the opportunity to probe for information about individuals' experiences with the CSG, how they became aware of program changes, the application process, any grant disruptions and household efforts to reapply, their interactions with the social welfare offices and related issues that might affect when the grant was first accessed and how long the grant was received. The qualitative research findings, discussed above and in the next section, informed the design of the household and adolescent surveys with the explicit intention of empirically measuring factors that influenced selection into different levels (months) of CSG receipt and the timing (earlier or later in the child's life). We accordingly use generalized propensity score (GPS) matching methods in this analysis to adjust for selection into *levels* (number of months) of treatment. GPS is an extension of propensity score matching (PSM) methods to cases in which treatment is a continuous rather than binary measure (Hirano & Imbens, 2004). After performing these GPS matching analyses with the crude dosage measures, we then estimate them with our actual dosage measures that account for interruptions and disconnections from the CSG.

Matching methods measure program impact as the average difference in outcomes for treated units minus a weighted average of outcomes for comparison units, where the weights are a function of observables X,

$$\Delta^{ATT} = \frac{1}{n}\sum_{i\in T}\left\{Y_i^1 - \sum_{j\in C} w(X,i,j)Y_j^0\right\}$$

The difference between alternative matching methods hinges primarily on to their approach to estimating the weights, $w(X, I, j)$.

In this context, PSM constructs a statistical comparison group by matching treated units to comparison units with similar values of the propensity to receive the treatment. In other words, if CSG recipients in the treatment and

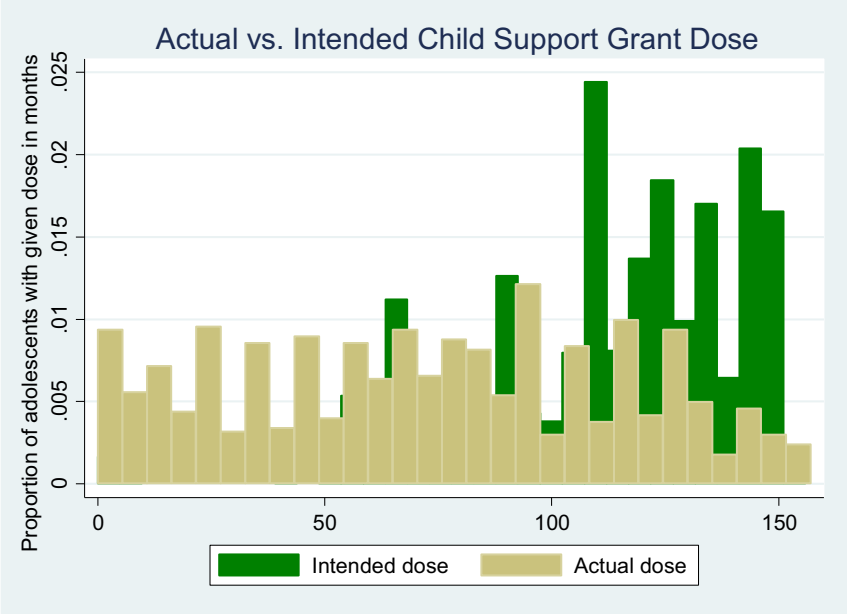

Figure 3. *Actual vs. intended child support grant doses in months.*

comparison group have the same propensity scores, the distribution of X across these groups will be the same:

$$Y_0 \perp D|X \Rightarrow Y_0 \perp D|P(X)$$

and they can be compared on the basis of their propensity scores alone, where D is a measure of program treatment. The difference between their mean outcomes is then calculated to yield an estimate of the average impact of the treatment, and after-matching balancing tests are used to assess the quality of the matches. The validity of this approach relies on two assumptions: (i) conditional mean independence—that is, conditional on their observed characteristics, comparison group members have the same mean outcomes as the treatment group would have in the absence of treatment—and (ii) sufficient common support (or overlap in the distribution of propensity scores for treatment) to produce valid matches (Rosenbaum & Rubin 1983).

In the GPS extension of PSM, we estimate a "dose–response function," where the dose is initially measured by the age at first receipt (or a crude approximation of duration of CSG receipt), and the response is the impact of that level of transfers on an outcome of interest. Because the duration of CSG receipt is not a random variable, failing to control for factors that affect both the dosage of transfers and the outcomes of interest would contribute to bias in this estimated relationship. We calculate the average dose–response function, $\mu(t) = E[Y(t)]$, where T is a specific treatment level, while controlling for X and assuming unconfoundedness (i.e., that mean outcomes for comparison cases are identical to outcomes of adolescents who received T years of the grant after conditioning on X). The generalized propensity score, R, is defined as $R = r(T, X)$, so that under this assumption and within strata with the same value of $r(T, X)$, the probability that $T = t$ does not depend on the value of X (Hirano & Imbens 2004). We estimate values of the GPS using maximum likelihood, assuming the treatment variable is normally distributed, conditional on the covariates X (see additional details in the Appendix A).

(b) *Estimating the implications of administrative burden*

The primary question of interest in this research, however, is to understand how administrative burden—which we show reduces total transfers (or dosage) and can also affect the timing of grant receipt—influences adolescent outcomes (i.e., adolescent engagement in risky behaviors). We employ an exact matching strategy with PSM to estimate the effects of grant interruptions and disconnections. This approach allows us to specify or require exact (or hard) matches for a specific covariate—in this case, the intended dose of CSG (i.e., total months of CSG the adolescent was eligible to receive)—among the many factors that we control for (or match on) in predicting interruptions and disconnections and estimating their effects on adolescent outcomes. In estimating the effects of grant interruptions and disconnections on educational attainment, we additionally exact match on the adolescents' age at the time of the survey, so that we are comparing the highest grade completed for youth of the same age. We also exact match on actual dosages of CSG receipt (and separately, the age the adolescent first received the CSG) to estimate the effects of receiving the grant during adolescence on risky behaviors. We use the treatment effects estimator (in Stata) with nearest neighbor matching and set the tolerance to use in determining exact matches (for each outcome estimated), as well as the distance metric (Euclidean). We also specify Abadie-Imbens robust standard errors.

Using PSM with exact matching as described above, we hypothesize that among adolescents with the same *intended* dose of CSG receipt, those who receive the grant for less time due to interruptions or disconnections will have diminished or smaller (positive) impacts. In addition, we hypothesize that among adolescents who received the CSG for the same number of months during the course of their childhood, those who are receiving the grant during adolescence (i.e., when surveyed) will be less likely to engage in risky behaviors. And similarly, we hypothesize that among adolescents who start receiving the CSG at the same age, those who received the

grant as an adolescent (i.e., currently benefitting at the time of the survey), will be less likely to engage in risky behaviors.

In the next section that presents our study findings, we first begin with a discussion of our first-stage model estimation for the GPS and PSM analyses, in which we predict CSG dosage, interruptions or disconnections, and the timing of grant receipt. In addition to their implications for program impacts, it is also of interest to understand what factors drive administrative burden and its associated problems with access to or continuation of grant receipt.

7. FINDINGS OF ANALYSES OF CASH TRANSFER PROGRAM IMPACTS AND THEIR MODERATION THROUGH ADMINISTRATIVE BURDEN

(a) *Predicting cash transfer receipt*

The variables that we use to predict the level of cash transfers received, the timing of grant receipt, and interruptions and disconnections from the CSG are critical to adjusting for possible selection bias in estimating adolescent program outcomes and to understanding the potential role of administrative burden in moderating these outcomes. Guidance for specifying these models came from both qualitative and quantitative data on the rules and requirements for CSG receipt, as well as documentation of some of the challenges in implementing the CSG program (as discussed above).

One set of variables included in these first stage models was intended to adjust for any differences in adolescent and household demographic and geographic characteristics (age, gender, and race and education level of the head of household, whether the household head was disabled or chronically ill, and geographic location, including the province in which the household resided and whether the residential setting was rural, periurban, or informal (*vs.* urban). As discussed previously, the households included in the study sample met the means test for CSG eligibility, but it is possible that there were still varying levels of poverty among those receiving the grant. To further explore this, we used a measure reporting the household's main source of income—including the response categories "work for salaries and/or wages," "remittances", "child support grant", "old age pension and other grants", "retirement pension", "self-employment", "rent" and "no income"—along with a corresponding measure of what proportion of income came from the household's main source of income, to create a variable indicating if all of the household's income came from the CSG, as well as indicators for more than three-fourths of household income from the CSG, one-half to three-fourths, and less than one half of household income from the CSG. Although we found that households that reported receiving all of their income from the CSG (53% of those with CSG income) were significantly more likely to reside in the poorest province (Limpopo), these measures of income/poverty were not statistically significant predictors of grant dosage, interruptions, or disconnections or "bad stops." We also tested alternative proxy measures of household wealth, such as whether the household had made home improvements, had access to electricity and whether it possessed various assets, but these were also not statistically significant predictors in our first-stage models. Thus, we adhered to the set of household demographic and geographic measures (as shown in the results tables that we discuss below).

The second set of variables included in the first-stage models predicting levels of CSG receipt (and timing and interruptions/disconnections) were intended to account for caregivers' and adolescents' experiences in accessing and maintaining access to the CSG (and in turn, the total months of grant receipt for the adolescent). These include measures of their awareness and knowledge of CSG availability, such as whether the adolescent was aware of the CSG and knew the eligible age for the CSG; the formal and informal sources from which the adolescent learned of the CSG (e.g., public agencies, school teachers, social workers, hospital, churches, NGOs, radio, friends, neighbors, etc.), and whether the adolescent encouraged someone in the household to apply for the CSG. In addition, we measured if the mother applied for the CSG, whether the household respondent knew the current eligible age for the CSG, and if the household respondent learned of the CSG from formal sources. Our inclusion of these variables was also motivated by our understanding (from the qualitative and quantitative data) that the frequent changes in the age of eligibility for the CSG contributed to confusion among those potentially eligible for the grant as well as among those administering the program in social welfare offices. However, we are not able to distinguish problems in accessing and maintaining access to the CSG due to administrator error in understanding eligibility rules *vs.* confusion about the rules among those eligible. We also included measures of barriers to application, such as problems with documentation requirements, time spent waiting and distance/time from the application office, as well as measures of households' persistence in applying, such as whether they re-applied after changes in rules governing program eligibility and the number of times they re-applied.

In predicting CSG dosage (separately for female and male adolescents) in the GPS estimation, we found several factors to be consistently predictive of months of cash transfer receipt. First, for both males and females, getting access to the grant at a later age and being older at the time of the 2011 survey are the strongest (negative) predictors of total months of grant receipt; as expected, those who started receiving the grant later in their childhood have significantly lower "doses" of grant receipt. In addition, adolescents from two of the poorest provinces, Eastern Cape and Limpopo, had significantly lower dosages, as did female adolescents living in informal settings. For males, we also saw that having a more educated head of household predicted higher cash transfer dosages, while the number of hours the applicant waited to apply for the CSG was negatively (and statistically significantly) related to months of cash transfer receipt. For brevity, we do not present the full set of these results for males and females and turn now to focus on what predicts interruptions in or disconnections from CSG receipt, which also play an important role in determining cash transfer "dosages."

In analyzing interruptions and disconnections from the CSG, we find that nearly half of those stoppages occur when the youth are age 13 or 14 years old (and two-thirds occur between ages 12 and 15), and that the large majority of these youth with interruptions of disconnections did not begin receiving the grant until they were school-aged. In examining the factors that contribute to stoppages in grant receipt, we distinguish between those who were interrupted but then restarted CSG receipt and those who stopped grant receipt and never restarted. We also further examine interruptions or disconnections that were in error—that is, looking at what predicts "bad stops" in CSG receipt (i.e., those that occurred while the youth was still eligible to receive the grant).

Table 2 presents the results of a multinomial regression that predicts the determinants of grant disconnections (i.e., stopped and never restarted) and interruptions (stopped and then

restarted), compared to the reference group of youth who did not experience any stoppage during their time of grant eligibility and receipt. Older youth were significantly more likely to be either disconnected or interrupted. Not surprisingly (given the repeatedly changing rules for age of eligibility), the odds of being disconnected are over 4,100% higher as age increases by 1 year. Given that the lion's share of stoppages were "bad stops," it is possible that many of these youth were disconnected in error, perhaps as the policy changes were working down to implementation at the social welfare office level (although we cannot confirm this). On the other hand, the odds of disconnection and interruption are slightly lower for those who start the grant later, possibly reflecting the declining rates of stoppage for those 15 and 16 years old, who presumably had only recently accessed the CSG at the time of survey (when the age of eligibility had just been increased through 17 years).

The next most influential predictor of disconnections appears to be directly related to administrative burden, that is, problems in producing documents required for access to the CSG. Individuals that have problems producing the required documentation have approximately 1,000% higher odds of being disconnected (with no grant restart); they also have about 340% greater odds of being interrupted. Interestingly, re-applying for the CSG due to changes in eligibility is negatively related to grant disconnections and significantly positively related to interruptions. We believe this suggests that those who re-apply are more likely to be interrupted (*vs.* permanently disconnected) from grant receipt; the odds of being disconnected are 66% lower if someone in the household re-applies for the grant after a change in eligibility, while the odds of interruption are 550% higher in these cases (reflecting in part that those with no disconnection or interruption do not have to re-apply). Other factors associated with a higher odds of disconnection are residence in the Limpopo province (one of the poorest in South Africa); not being aware of eligibility for the grant or hearing of it from informal sources, and cases in which the adolescent encourages the household head to apply (possibly suggesting that the caregiver is not taking initiative to apply).

In Table 3, we present the results of a logistic regression model that was estimated to specifically assess what factors

Table 2. *Predicting disconnections and interruptions from the CSG*

($n = 928$)	Disconnected			Interrupted		
Predictor	Odds ratio	Z	p-Value	Odds ratio	Z	p-Value
Age-time of survey	**42.939**	**13.29**	**0.000**	**2.678**	**5.06**	**0.000**
Age-1st grant receipt	**0.901**	**−3.12**	**0.002**	**0.809**	**−6.73**	**0.000**
Adolescent not aware of eligibility	**2.561**	**2.56**	**0.011**	0.827	−0.48	0.634
HH encouraged to apply by adolescent	**1.805**	**2.25**	**0.025**	1.308	1.18	0.240
Adol. knowledge-formal sources	1.035	0.09	0.928	0.782	−0.75	0.452
Adol. knowledge-informal sources	**2.122**	**2.36**	**0.018**	0.725	−1.14	0.256
Adolescent knows eligible age	1.590	1.24	0.214	1.059	0.17	0.867
Mother applied	0.634	−1.60	0.110	1.287	0.97	0.333
HH no school (omitted)						
HH education: K-5	0.695	−0.96	0.336	0.742	−0.90	0.366
HH education: 6–8	0.496	−1.73	0.083	0.607	−1.46	0.144
HH education: 9–11	0.463	−1.78	0.074	0.683	−1.02	0.307
HH education: 12+	0.482	−1.44	0.149	0.498	−1.51	0.130
HH disabled	0.919	−0.20	0.839	0.879	−0.37	0.713
HH chronically ill	0.939	−0.24	0.807	1.069	0.30	0.763
Re-applied due to change in eligibility	**0.435**	**−2.39**	**0.017**	**6.481**	**6.40**	**0.000**
# times re-applied	1.003	0.03	0.976	1.049	0.52	0.601
Document problems	**11.103**	**3.25**	**0.001**	**4.447**	**2.27**	**0.023**
Hours waited reapplying	1.006	0.11	0.910	0.969	−0.68	0.499
HH distance to social welfare office	0.995	−0.50	0.618	0.999	−0.07	0.946
HH knows eligible age	2.384	1.46	0.144	0.702	−0.58	0.563
HH knowledge-formal sources	0.791	−0.99	0.321	1.109	0.51	0.613
Adolescent-male	1.045	0.19	0.848	1.171	0.79	0.428
HH age	0.993	−0.62	0.533	0.994	−0.67	0.503
HH female	1.163	0.63	0.531	1.223	0.94	0.345
HH not African	0.401	−1.77	0.077	0.819	−0.51	0.613
Urban (omitted)						
Rural	0.552	−1.61	0.107	0.932	−0.23	0.819
Periurban	1.290	0.73	0.463	1.167	0.53	0.598
Informal setting	2.409	1.81	0.071	1.695	1.19	0.232
KwaZulu-Natal (omitted)						
Gauteng	0.618	−1.39	0.163	0.588	−1.75	0.081
Eastern Cape	1.467	1.08	0.282	1.087	0.25	0.801
Western Cape	1.098	0.20	0.840	1.676	1.38	0.166
Limpopo	**3.855**	**3.08**	**0.002**	1.143	0.34	0.734
Constant	0.000	−12.82	0.000	0.000	−4.83	0.000
Pseudo R^2			38.40%			
Log likelihood			−610.05			

Coefficients in boldface are statistically significant at $\alpha \leqslant 0.05$.

predict "bad stops," or disconnections or interruptions that occurred while the youth was apparently still eligible for the grant. As expected, given that more than four-fifths of disconnections and interruptions appeared to be in error, the factors predicting "bad stops" are similar to those shown in Table 2. We see again that having problems with documents required for grant access is one of the most influential factors, with the odds of a bad stop more than 300% higher for those with document problems. Having to re-apply due to changes in eligibility (likely associated with changes in the eligible age for youth) is also significantly associated with bad stops, although it appears that if one spends a longer time waiting to re-apply, an unintended stop is less likely to occur. These results are suggestive of a role for administrative burden in driving disconnections and interruptions that should not have occurred, although again, we cannot determine if the difficulties on the supply- *vs*. demand-side of the process dominate. It is also notable that geography appears to be important as well; those living in periurban and informal settings have 64% and 103% higher odds (respectively) of experiencing a bad stop, while those residing in Gauteng (a less disadvantaged province) were significantly less likely to be disconnected or interrupted in error. (The relationship between age and bad stops is similar to that for any stoppage in cash transfer receipt). We now examine how cash transfer program impacts may be moderated when youth receive fewer months of cash transfers than intended due to interruptions and disconnections linked to administrative burden.

(b) *Cash transfer dosage and adolescent outcomes*

As indicated earlier, we first estimated GPS models to explore the relationship between cash transfer dosage and adolescent outcomes using the crude measure of dosage—approximated by the youth's age at first receipt of the CSG—as was done in the DSD–SASSA–UNICEF (2012) impact evaluation. Here we present just a sampling of these results, which replicate those in the original report for female sexual activity (abstinence from sexual intercourse) and the number of sex partners that adolescents reported in the confidential survey. [6] The results of these dose–response analyses are summarized graphically in Figures 4 and 5. Figure 4 shows that the

Table 3. *Predicting bad stops from the CSG*

($n = 929$)	Bad stop		
Predictor	Odds ratio	Z	p-Value
Age-time of survey	**2.965**	**9.05**	**0.000**
Age-1st grant receipt	**0.895**	**−4.91**	**0.000**
Adolescent not aware of eligibility	1.268	0.94	0.346
HH encouraged to apply by adolescent	1.339	1.68	0.093
Adol. knowledge-formal sources	0.924	−0.32	0.746
Adol. knowledge-informal sources	1.235	1.03	0.303
Adolescent knows eligible age	1.296	1.03	0.304
Mother applied	1.047	0.25	0.806
HH no school (omitted)			
HH education: K-5	0.938	−0.26	0.795
HH education: 6–8	0.656	−1.67	0.095
HH education: 9–11	0.674	−1.42	0.155
HH education: 12+	0.560	−1.71	0.087
HH disabled	0.882	−0.47	0.637
HH chronically ill	1.024	0.14	0.888
Re-applied due to change in eligibility	**2.427**	**3.96**	**0.000**
# times re-applied	1.062	0.76	0.449
Document problems	**4.029**	**2.85**	**0.004**
Hours waited reapplying	**0.932**	**−2.03**	**0.042**
HH distance to social welfare office	1.006	0.83	0.406
HH knows eligible age	0.715	−0.89	0.376
HH knowledge-formal sources	1.098	0.60	0.547
Adolescent-male	1.092	0.58	0.559
HH age	1.006	0.76	0.450
HH female	1.257	1.40	0.161
HH not African	0.763	−0.89	0.374
Urban (omitted)			
Rural	0.805	−0.89	0.371
Periurban	**1.639**	**2.16**	**0.031**
Informal setting	**2.034**	**2.14**	**0.033**
KwaZulu-Natal (omitted)			
Gauteng	**0.564**	**−2.47**	**0.013**
Eastern Cape	1.342	1.24	0.214
Western Cape	1.547	1.50	0.133
Limpopo	1.497	1.35	0.176
Constant	0.000	−8.81	0.000
Pseudo R^2		16.07%	
Log likelihood		−540.410	

Coefficients in boldface are statistically significant at $\alpha \leqslant 0.05$.

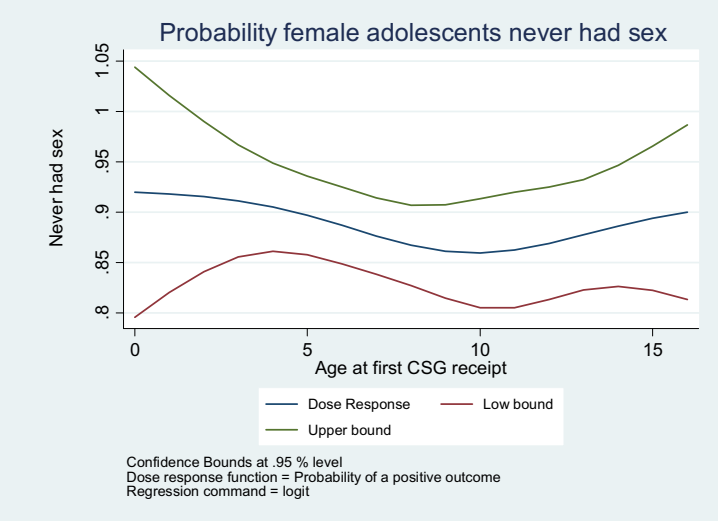

Figure 4. *Impacts of CSG dosage on female adolescent sexual activity: dose approximated by age at first grant receipt.*

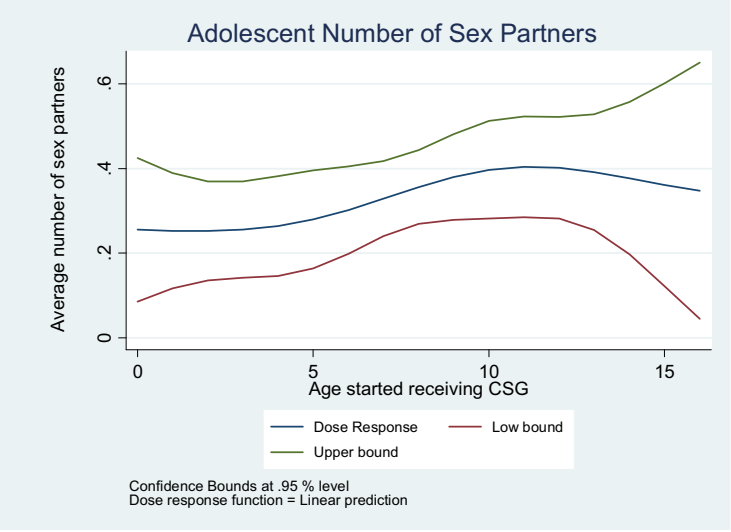

Figure 5. *Impacts of CSG Dosage on Adolescent Number of Sex Partners: Dose Approximated by Age at First Grant Receipt.*

probability that females report *never* having had sex is higher when they began receiving the CSG at a younger age (and accordingly receive a higher "dose" of the CSG). However, we also see a "trough" in the predicted probabilities of "never had sex" around age 10 years, before the predicted probability of never having had sex increases again for those who began receiving the CSG at age 11 years or older. One explanation advanced for this result is that CSG receipt at the time of adolescence may provide important protection against adolescent engagement in sexual activity. We know from the analysis shown in Figure 2 that youth who first began receiving the CSG at age 10–13 years were less likely to continue receiving the grant through their teenage years (and that they were also more likely to reside in the poorest provinces). Looking to Figure 5, we see the same general pattern in results for the GPS estimation of the impact of CSG dosage on adolescents' number of sexual partners. As the age at first receipt of CSG (or approximate dosage) increases, the number of sexual partners likewise increases, but then there is a downturn in the number of sexual partners for adolescents who start receiving the CSG in adolescence (around age 12 or older).

The crude measures of CSG dosage do not account for the substantial months of "dose loss" that we described in the preceding section or the timing of the disconnections and interruptions in CSG receipt, which we showed were most likely to occur between the ages of 12 and 15 years. We estimated these same GPS analyses again using the improved measures that we developed of actual dose in months—that is, CSG dosages that account for cash transfer loss due to disconnections and interruptions. [7] Figures 6 and 7 present the estimated dose–response effects when we use the actual dose measure for the same two outcomes, female adolescent sexual activity and the number of sex partners adolescents reported (respectively). These two graphs show that once we correct or adjust the CSG dosage measures for interruptions and disconnections, the relationship between cash transfer dosage and these outcomes is relatively clear-cut. As the number of months of CSG receipt increases, the probability that female adolescents

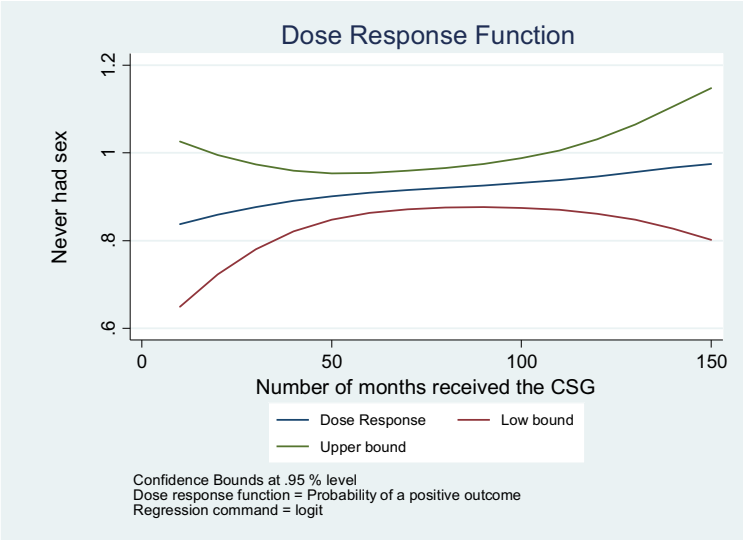

Figure 6. *Impacts of age at grant receipt on female adolescent sexual activity: actual (adjusted) dose measure.*

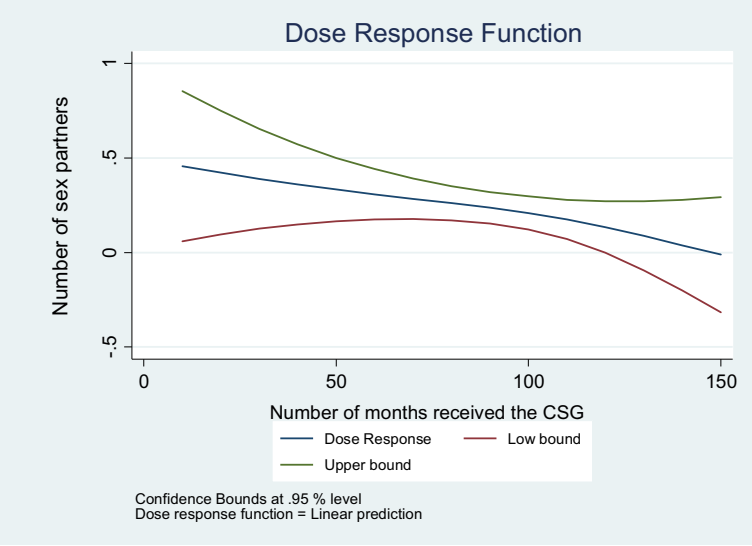

Figure 7. *Impacts of age at grant receipt on adolescent number of sex partners: actual (adjusted) dose measure.*

abstain from sexual activity increases (see Figure 6). And similarly, as the number of months of CSG receipt increases, the number of sexual partners reported in adolescence decreases (see Figure 7). Although the error bounds are wider at the tail ends of the distribution of CSG dosage (where frequencies are lower), the estimates are statistically significant.

(c) *How disconnections and interruptions in cash transfer receipt moderate program impacts*

We now turn to the core analysis of this paper, where we estimate the extent to which administrative burden and errors (that result in disconnections and interruptions in cash transfer receipt) moderate CSG program impacts. We begin with the analyses that *exact* match adolescents on *intended* dose of CSG receipt (and also match on other variables that influence CSG receipt), and then estimate the effects of interruptions in or disconnections from grant receipt on adolescent outcomes. We use two measures of "treatment" in this analysis: (i) an indicator for "never interrupted" (i.e., no interruptions or disconnections from grant receipt), and (ii) an indicator for "bad stops," which takes a value of one if CSG receipt was interrupted in error (while the youth was still eligible to receive the grant). The other conditioning variables that are used to predict "never interrupted" or "bad stops" in the first-stage estimation of the propensity scores are those shown in Tables 2 and 3. We estimate these models separately for males and females and adjust the tolerance band (or caliper) in the second stage matching procedure to ensure that we have exact matches on *intended* dose per month for all cases in the estimation sample, as required by the matching technique we use.[8] Again, the hypotheses we test are: (1) for adolescents with the same *intended* dose of CSG receipt, those whose grant receipt was never interrupted will have better outcomes in

Table 4. *Effects of interruptions and problematic disconnections from CSG on adolescent outcomes*

Adolescents exact matched on intended dose	Never interrupted			Bad stop		
	Difference (ATE)	Standard error	*p*-Value	Difference (ATE)	Standard error	*p*-Value
Outcome (females)						
Never had sex	**0.055**	**0.030**	**0.071**	−0.010	0.032	0.749
Number of sex partners	**−0.235**	**0.090**	**0.009**	**0.193**	**0.113**	**0.088**
Ever pregnant	−0.046	0.030	0.129	0.027	0.034	0.429
Never drank alcohol	0.017	0.048	0.720	−0.021	0.048	0.653
Age at first alcohol use	0.099	0.195	0.612	−0.051	0.195	0.791
Never used drugs	0.009	0.044	0.830	0.004	0.042	0.926
No criminal activity	**0.134**	**0.045**	**0.003**	**−0.074**	**0.045**	**0.095**
Highest grade completed	**0.197**	**0.102**	**0.054**	**−0.233**	**0.120**	**0.053**
Outcome (males)						
Never had sex	0.047	0.044	0.293	**−0.077**	**0.042**	**0.068**
Number of sex partners	−0.144	0.108	0.182	0.044	0.102	0.668
Never drank alcohol	0.054	0.051	0.288	**−0.087**	**0.048**	**0.072**
Age at first alcohol use	−0.270	0.193	0.162	**0.425**	**0.185**	**0.022**
Never used drugs	−0.072	0.055	0.193	0.006	0.050	0.907
No criminal activity	**−0.127**	**0.055**	**0.021**	0.039	0.050	0.438
Highest grade completed	0.025	0.056	0.647	−0.017	0.053	0.743

Notes: Table 1 shows the number of observations available for each of the outcome measures (for females and males with dosage measures) in the above treatment effects estimations.
Coefficients in boldface are statistically significant at $\alpha \leqslant 0.05$.

adolescence, and (2) for adolescents with the same *intended* dose of CSG receipt, those whose grant receipt was incorrectly stopped or disconnected will have worse outcomes.

Table 4 summarizes the results of these matching analyses for the two treatment variables (separately for males and females) and eight different adolescent outcomes (described earlier): (i) never had sex, (ii) number of sex partners, (iii) ever pregnant (females only), (iv) never used alcohol, (v) age at first alcohol use, (vi) never used drugs, (vii) never engaged in criminal activity and (viii) highest grade completed. We report the difference in outcomes (average treatment effects) for the matched samples, the Abadie-Imbens robust standard errors (which account for the fact that the propensity scores are estimated), and the *p*-values (for ease in assessing statistical significance). With one exception, where statistically significant (at $\alpha < 0.10$), the estimated effects are in the direction expected. Female adolescents are significantly more likely to have abstained from sex (predicted probability is 5.5 percentage points higher); to have had fewer sex partners (about one-fourth fewer) and to have refrained from criminal activity (by 13.4 percentage points) if their receipt of cash transfers was not interrupted. In addition, the highest grade completed for females increases by about one-fifth of a grade (0.197) if their receipt of the CSG was not interrupted. Correspondingly, when benefits are stopped in error for females, they are predicted to have more sex partners (about one-fifth more) and are less likely to have refrained from criminal activity (predicted probability is 7.4 percentage points lower), and their educational attainment decreases by 0.233 grades. Male adolescents also have a lower predicted probability of abstaining from sex (by 7.7 percentage points) and refraining from alcohol use (by 8.7 percentage points) if their benefits were stopped in error; they are also significantly more likely to start alcohol at an earlier age if they experience a bad stop. (With the exception of criminal activity, the other estimated effects for males are in the expected direction, albeit not statistically significant).

In the final set of analyses, we explore the implications of the timing of CSG receipt, i.e., of receiving the CSG during adolescence, on adolescent engagement in these risky behaviors. We hypothesize that adolescents who receive the grant during adolescence (i.e., when surveyed) will be less likely to engage in risky behaviors. We test this hypothesis with two different estimation approaches, exact matching on: (1) the age that youth first started receiving the grant, and (2) on their actual dosages of grant receipt, and then estimate the impact of receiving the grant as an adolescent (i.e., the treatment measure) on outcomes associated with risky behaviors. In other words, we expect that among adolescents who start receiving the grant at the same age, those who are receiving it in adolescence will have better outcomes. Similarly, among adolescents with the same total dosages of the CSG, those who receive the grant in adolescence will have more favorable outcomes. We use the same matching approach as described above for these analyses.

Table 5 presents the results of these analyses (estimated separately for males and females), again showing the differences in outcomes (average treatment effects) for the matched samples (exacting matching on either the age at first start of the CSG or total CSG dose), the standard errors and p-values. For females, receiving the grant in adolescence has notable, statistically significant effects on risky sexual behaviors—increasing the likelihood of abstinence (by 10–11 percentage points) and reducing the number of sexual partners (by one-fourth) and the likelihood of pregnancy (by 9–12 percentage points). For males, receiving the grant in adolescence also appears to reduce the number of sexual partners (by nearly one half) and increases the likelihood that they refrain from alcohol use (by about 12 percentage points). These results provide some support for the tentative conclusion in the DSD–SASSA–UNICEF (2012) impact evaluation that receipt of the CSG in adolescence provides some additional protection for adolescents against the initiation of (or toward reduction in their engagement in) risky behaviors during the teenage years.

The other main take-away from the analysis of adolescent outcomes in the DSD–SASSA–UNICEF impact evaluation was that there should be more concerted efforts to ensure continuous access to the CSG for eligible children through adolescence, so that they could fully realize the potential benefits of the cash transfers. Our analysis of the implications of

Table 5. *Effects of receiving the CSG in adolescence on outcomes in adolescence*

Treatment: received CSG in adolescence	Exact matched on age at first grant receipt			Exact matched on actual CSG dose		
	Difference (ATE)	Standard error	*p*-Value	Difference (ATE)	Standard error	*p*-Value
Outcome (females)						
Never had sex	**0.096**	**0.040**	**0.018**	**0.111**	**0.043**	**0.010**
Number of sex partners	**−0.243**	**0.120**	**0.043**	**−0.337**	**0.136**	**0.013**
Ever pregnant	**−0.090**	**0.045**	**0.046**	**−0.119**	**0.047**	**0.011**
Never drank alcohol	0.030	0.058	0.604	0.049	0.054	0.366
Age at first alcohol use	0.150	0.182	0.409	−0.013	0.209	0.951
Never used drugs	−0.019	0.044	0.670	0.039	0.050	0.447
No criminal activity	0.035	0.056	0.528	0.080	0.057	0.157
Outcome (males)						
Never had sex	0.030	0.051	0.560	0.033	0.052	0.525
Number of sex partners	**−0.456**	**0.212**	**0.031**	−0.175	0.111	0.117
Never drank alcohol	−0.002	0.056	0.956	**0.124**	**0.058**	**0.034**
Age at first alcohol use	0.061	0.214	0.774	−0.163	0.216	0.450
Never used drugs	−0.018	0.061	0.768	0.024	0.067	0.724
No criminal activity	−0.034	0.061	0.576	−0.048	0.055	0.377

Note: Table 1 shows the number of observations available for each of the outcome measures (for females and males with dosage measures) in the above treatment effects estimations.
Coefficients in boldface are statistically significant at $\alpha \leqslant 0.05$.

interruptions in and disconnections from CSG receipt confirms that adolescents whose grant receipt is stopped at some point in their childhood (either permanently or until re-application is successful) may be less well protected from negative outcomes than those who stay connected with the grant. Furthermore, we also found that "dose loss" varied geographically, with adolescents in Limpopo (one of the poorest provinces) losing significantly more months of cash transfers due to administrative burden and errors. Thus, these problems could be disproportionately affecting the youth who most need the protection that the cash transfers provide.

8. CONCLUDING DISCUSSION

A 1985 article in *World Development* that examined political and bureaucratic dimensions of the allocation of foreign aid to developing countries (Cohen, Grindle, & Walker, 1985, p. 1217) criticized the lack of attention in research to "how and why bureaucratic organizations operate as they do," noting the need for more conceptual and empirical studies about the constraints on bureaucratic actors in program and policy administration and "opportunities to change bureaucratic action that impedes more effective development initiatives." This study has focused on the role of one particular "bureaucratic dimension"—administrative burden—that manifests in various ways to impede program effectiveness. Administrative burden and related implementation challenges are ubiquitous in government programs, and as described earlier, they tend to be more burdensome in means-tested transfer programs that target poorer populations. The South African CSG was in no way unique in its relatively complex and demanding requirements for application and corresponding costs for those applying, including significant documentation (birth certificate, identity document, hospital card, child health record, and proof of caregiver income), long lines and waits at social welfare offices with limited service hours, and other infrastructure and capacity problems that may have contributed to misapplication of program rules and uneven coverage as the program was expanded. To its credit, the South African government recognized these problems and sought to improve the CSG application process over time by simplifying documentation requirements, better communicating program rules and procedures, and speeding up processing times. While repeated changes in the age of eligibility for the CSG appear to have exacerbated administrative problems and errors, they also facilitated a substantial expansion in program coverage to approximately 11 million beneficiaries by 2012. In addition, the rate of disconnections and interruptions of grant receipt declined over time.

Our empirical examination of administrative burden in South Africa's CSG confirmed the potential for high costs associated with it for children whose program benefits were interrupted or disconnected, frequently in error. For both male and female adolescents, disconnections or interruptions in cash transfer receipt are associated with higher rates of engagement in risky behaviors in adolescence, including a lower likelihood that they abstain from sexual activity and a higher number of sexual partners. For females, they are also associated with lower educational attainment and a higher likelihood of criminal activity. Males also appear less likely to refrain from alcohol use and are more likely to start drinking at a younger age if their cash transfer receipt is stopped in error. Because young people living in South Africa are at significantly higher risk of HIV infection (with prevalence rates over 20% for females 18–24 years, Pettifor, Levandowski, & Mcphail, 2011), engagement in these behaviors could have irrevocable negative impacts. Thus, the fact that some CSG-eligible adolescents (and the poorer among them) appear to receive less protection from these risks—possibly due to administrative burden and error (something that can be alleviated with better public policies and administration of cash transfer programs)—is a noteworthy and troubling finding.

As discussed earlier, some prior research has confirmed that administrative or procedural discretion is sometimes used intentionally to restrict access to benefits. There has also been work specific to the CSG suggesting that not only were some initial rules (particularly related to documentation requirements) applied in ways that did not support equal access to the grant, but also that there was deliberate discouragement of applicants by some corrupt officials (Mirugi-Mukundi, 2009). Our research suggests that this type of manipulation of program rules by those implementing social welfare programs could have negative consequences that are too serious

to ignore. Going forward, governments implementing cash transfer and other social welfare programs should take early and aggressive steps to ensure that application requirements and processes are as simple and transparent as possible; that front-line staff administering the programs understand and are committed to applying program rules fairly; that updates to program requirements get communicated quickly and clearly, and that local (formal) infrastructure is used to communicate with and support the eligible population's efforts to successfully complete the application process.

Finally, the findings of this study also affirm the decision by the government of South Africa to extend the age of eligibility of the CSG to children 17 years old and under. There was considerable advocacy within South Africa for this expansion of benefits to children of all ages, and our findings suggest that providing cash transfers for children in their teenage years may have substantially diminished their engagement in risky behaviors, particularly those associated with sexual activity. These effects appeared to be particularly strong for female adolescents, who are arguably at greatest risk of the negative consequences associated with early and risky sexual activity. They were significantly more likely to abstain from sex, significantly less likely to become pregnant and had fewer sexual partners if they benefitted from the CSG in adolescence (holding constant the total number of months of benefit receipt or the age at which benefit receipt began). There also appeared to be important benefits for male adolescents (related to their number of sex partners and alcohol consumption) of receiving the grant in adolescence. This is a policy change—providing access to cash transfers for children of all ages—that could and should be considered for wider implementation in cash transfer programs in developing countries, to the extent that resources allow. The experience of the CSG also suggests that it might be better not to implement this policy piecemeal (or via gradual extensions), as this appeared to add to administrative burden and unintended disconnections from the program.

Our study is subject to limitations similar to those of the lion's share of nonexperimental evaluations. Our estimates of the effects of grant receipt (CSG doses), interruptions and disconnections from the grant, and the timing of the grant rely on the assumption that we have satisfied the requirement of unconfoundedness, in particular, that we have appropriately adjusted for selection into *levels* of treatment and into treatment as otherwise defined in our study. Although we have an unusually rich set of data for predicting cash transfer receipt, the timing of receipt and interruptions or disconnections, we are always subject to the possibility that some relevant, omitted variable could bias our results. We are encouraged by the fact that our models for the most part satisfy the balancing tests, as well as by the robustness of our findings across specifications and their consistency with prior work on the impacts of the South African CSG.

NOTES

1. We thank an anonymous reviewer for raising this point.

2. The sampling of adolescents not receiving the CSG was intended to facilitate a regression-discontinuity analysis that would estimate program effects by comparing adolescents currently receiving the CSG with those who had just missed the age-eligibility cutoff (when the program expanded in 2010 to include children up to the age of 16 years). However, the rapid changes in the age of eligibility (including during the data collection period) made it difficult to find a large enough sample of adolescents within a sufficiently narrow bandwidth of the age eligibility cutoff to enable good matches. This strategy was therefore abandoned. Additional details on the sampling strategies, surveys, and survey administration can be found in DSD, SASSA and UNICEF (2012).

3. We define adolescents as teenagers, or any focal (sampled) child aged 13–18 years.

4. We assessed the validity and representativeness of the study sample using two nationally representative studies. Sub-samples from the 2008 National Income Dynamics Study (NIDS) and the 2010 General Household Survey (GHS) data were compared to the CSG samples, with results showing that the CSG, NIDS, and GHS surveys produced highly comparable if not nearly identical results across a range of key measures, including household size and composition, household living conditions, income and assets, and others (see DSD, SASSA & UNICEF, 2012). We also compared the characteristics of adolescents who completed the confidential surveys with those who did not to check for significant differences. In addition, we looked at the adult household respondent reports of several youth "negative" behaviors and compared them to those of the adolescents in the confidential surveys. We found that the adolescents reported significantly higher rates of work outside the home and absences from school than the household respondents. In general, although we are unable to determine if some adolescents may have provided "socially desirable" responses on their confidential surveys, our analysis did not suggest problematic patterns or reasons for concern about this issue.

5. Another possible concern is that migration could contribute to disconnections if the caregiver has to re-apply to receive the CSG at a different location. However, only seven (of the 399) respondents who reported that the CSG was stopped indicated it was because they had moved, suggesting this was likely not a serious problem.

6. Balance among the covariates after matching (within quartiles of the age at start distribution) was checked, and balance was obtained across the covariates and intervals with one exception (a province indicator within one interval).

7. We again checked for covariate balance (after matching) within quartiles of the distribution of CSG dose in months, and balance was attained for the covariates with the exception of "age at first CSG start" within two intervals. When we exclude "age at first CSG start" and only include the adolescent's age at the time of the survey in the first-stage model predicting dosage, the pattern in the results (dose response) is similar. We further address imbalance on this covariate in our subsequent analyses of the effects of the timing of CSG receipt by exact matching on the "age at first CSG start."

8. We estimate these models using the *teffects* command in Stata with nearest neighbor matching. The *teffects* command has an advantage over *psmatch2* in that it takes into account the fact that propensity scores are estimated rather than known in calculating the standard errors. It also has a disadvantage in that there is no post-estimation command for checking after-matching balance of the covariates. We handle this by estimating the same model with *psmatch2*, although we only match (rather than exact match) on the intended dose, and we obtain balance after matching for all covariates. Exact matches on intended dose in the *teffects* estimation were typically obtained for all cases using a tolerance band specification of about 6 months (of intended dose), although this varied to some extent by the outcome analyzed.

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APPENDIX

As described above, we draw on Rosenbaum and Rubin's (1983) contribution which shows that matching can be made on the basis of the probability (or propensity) to receive treatment, given a set of characteristics X. Let $P(X)$ be the probability, for example, of receiving the CSG at a given age. Using this notation, $P(X) = \Pr(D = 1|X)$, propensity score matching constructs a statistical comparison group by matching "treated" observations to observations not treated with similar values of $P(X)$. If outcomes (Y) are independent of treatment after conditioning on X, then outcomes are independent of treatment after conditioning only on $P(X)$, and propensity score matching provides a valid method for obtaining unbiased estimates of the average impact of the treatment on the treated, i.e., $E(Y^1|X, D = 1) - E(Y^0|X, D = 1)$.

Hirano and Imbens (2004) have extended propensity score methods to cases where, as with the CSG, treatment is continuous. Define T as the set of all treatment levels (such as the number of months a child has received the CSG) and T as a specific treatment level. Define the treatment interval $[t_0, t_1]$, so that $T \in [t_0, t_1]$. We are interested in calculating the average dose–response function, $\mu(t) = E[Y(t)]$. Hirano and Imbens note that the unconfoundedness assumption in the binary case can be generalized to the case where T is continuous. They define the Generalized Propensity Score, R, as $R = r(T, X)$ and explain that the GPS has a balancing property similar to that of the standard propensity score. Within strata with the same value of $r(T, X)$, the probability that $T = t$ does not depend on the value of X. Hirano and Imbens prove that assignment to treatment is unconfounded, given the generalized propensity score.

To implement their approach, we first estimate the values of the GPS. We assume that the treatment variable is normally distributed, conditional on the covariates X:

$$g(T)|X \sim N\{h(\gamma, X), \sigma^2\}.$$

and use maximum likelihood to calculate the GPS as:

$$\check{R}_i = [2\pi\sigma^2]^{(-0.5)} \exp\left[(-(2\sigma^2)^{-1})[g(T_i) - h(\gamma, X)]\right].$$

As with the case of a binary outcome, it is important to assess the balancing properties. As described in Kluve, Schneider, Uhlendorff, and Zhao (2007), we divide the sample into four equalizing sized groups based on the distribution of the treatment variable, cutting the sample at its quartiles. We then divide each group into five blocks by the quintiles of the GPS using only the GPS distribution of adolescents in that group. Within each block, we calculate differences in means of each element of X for adolescents in a given block compared to adolescents in the same group but in different blocks. As Kluve *et al.* note, this procedure tests if, within each group, covariate means of adolescent characteristics belonging to the particular treatment-level group are significantly different from those with a different treatment level (but similar GPS). A weighted average over the five blocks in each treatment-level group is then used to calculate a t-statistic of the differences-in-means between the particular treatment-level group and all other groups. This procedure is repeated for each treatment-level group and each covariate. If adjustment for the GPS properly balances the covariates, differences in means should not be statistically different from zero.

After checking for balance, we next estimate the conditional expectation of Y, given T and R. Ex ante, we do not know the functional form this takes, so we initially use a linear specification that only includes the treatment level, the GPS, and the interaction (treatment level × GPS) of these two terms. We use the results of this estimation to calculate a dose–response function at specified levels of transfers and use bootstrap methods to calculate the confidence intervals for these.