HUMAN CAPITAL

Summer jobs reduce violence among disadvantaged youth

Sara B. Heller[1,2]*

Every day, acts of violence injure more than 6000 people in the United States. Despite decades of social science arguing that joblessness among disadvantaged youth is a key cause of violent offending, programs to remedy youth unemployment do not consistently reduce delinquency. This study tests whether summer jobs, which shift focus from remediation to prevention, can reduce crime. In a randomized controlled trial among 1634 disadvantaged high school youth in Chicago, assignment to a summer jobs program decreases violence by 43% over 16 months (3.95 fewer violent-crime arrests per 100 youth). The decline occurs largely after the 8-week intervention ends. The results suggest the promise of using low-cost, well-targeted programs to generate meaningful behavioral change, even with a problem as complex as youth violence.

Every day in the United States, acts of violence kill almost 150 people and injure over 6000 more (*1*, *2*). This "public health crisis" (*3*) disproportionately involves youth, who are twice as likely as adults to be both victims and perpetrators of violence (*4*). The problem is most concentrated among disadvantaged minority youth; violent-crime arrest rates for African-American juveniles are five times as high as that of their white counterparts (*5*).

Poverty scholar W. J. Wilson identifies one major cause of these racial violence disparities among young people: joblessness (*6*). His argument adds to decades of social science investigating how poor job prospects cause crime, from weakening social bonds to generating psychic strain to reducing the perceived cost of punishment (*7*–*10*). Policy discussions often conclude that public employment and training programs are a solution to youth violence on the grounds that "nothing stops a bullet like a job" (*11*).

The empirical literature on youth employment programs, however, suggests that the practical applications of this idea are limited (*12*). Several major employment programs fail to reduce delinquency among youth (*13*, *14*). The two experimentally evaluated interventions that do lower crime, at least during the program, involve such intensity and expense that their benefits fail to outweigh their costs (*15*, *16*). Attempts to provide shorter, more scalable employment services have, if anything, increased adolescents' criminal behavior (*17*). The theoretical promise of using employment interventions to fight youth violence does not appear to translate easily into empirical reality; only very high levels of investment seem able to reduce crime (supplementary materials, section 2.1) (*18*).

But, one widespread type of youth employment program has not yet been rigorously evaluated: summer jobs (*19*, *20*). Despite some promising observational studies (*21*), there is little convincing causal evidence on the effects of these short-term, low-cost programs and, to my knowledge, no experimental evidence on whether summer jobs can reduce crime (supplementary materials, section 2.1).

This paper presents the results of a large-scale randomized controlled trial that tests a public summer jobs program in Chicago called One Summer Plus (OSP). OSP offers 8 weeks of part-time summer employment at Illinois' minimum wage ($8.25/hour). Local community organizations place youth in nonprofit and government jobs (for example, summer camp counselors, workers in a community garden, or office assistants for an alderman). Youth are assigned job mentors—adults who help them learn to be successful employees and to navigate barriers to employment—at a ratio of about 10 to 1. Half the treatment group also receives social-emotional learning (SEL) based on cognitive behavioral therapy principles, aimed at teaching youth to understand and manage the aspects of their thoughts, emotions, and behavior that might interfere with employment (supplementary materials, section 2.2).

The study randomly assigns 1634 8th- to 12th-grade applicants enrolled in 13 high-violence Chicago schools to program (jobs-only or jobs + SEL) or control conditions (*22*). Youth in the jobs-only group are offered 25 hours per week of paid employment; youth in the jobs + SEL group are paid for 15 hours of work and 10 hours of SEL weekly. Control youth are excluded from the program but free to pursue other opportunities.

The existing employment literature might suggest that this kind of low-dosage jobs program is unlikely to change criminal behavior. But, there is an important difference between summer jobs for in-school youth and the previous literature: The well-studied youth employment programs generally act as tertiary prevention, targeting youth already out of school and struggling in the labor market. Research in domains such as education and health, however, suggests that for many negative outcomes caused at least in part by prior behavior (for example, poor school performance and dropout, or certain types of cancer), primary and secondary prevention—intervening before onset rather than managing or trying to reverse a problem once it occurs—can improve outcomes more effectively, with less intensive treatment (*23*–*25*)

If this well-established idea that prevention can be more effective than remediation also applies to the employment domain, teaching adolescents how to be successful employees, facilitating connections to employer networks, and providing work experience before they drop out of school might reduce crime with less intensive intervention than is required for already disconnected youth. Offering summer employment at this key point in the life course could make crime a relatively less attractive option, strengthen social bonds, and develop "soft" skills such as self-efficacy and impulse control (*7*, *26*–*28*). Waiting until after dropout to intervene may make it more difficult to reduce violence in particular, given the apparently causal relationship between school dropout and criminal behavior, especially murder and assault (*29*). Limiting the intervention to summer avoids direct conflicts between work and school. Summer jobs also provide wages and structured activity during a high-crime season when youth might otherwise be idle, both of which may affect crime (*30*, *31*).

However, the theory on summer jobs is not entirely clear-cut. Additional income could be spent on crime-inducing goods such as drugs and alcohol, and time spent traveling to and from work might increase exposure to criminal opportunities. Even the attempt to keep youth busy (the "incapacitation effect") may be poorly targeted because most jobs are during business hours, whereas most crime occurs during evenings and weekends. More broadly, a short-term subsidized job might not be enough to generate behavior changes beyond the summer itself. The effects of summer jobs are therefore an empirical question.

Data come from matching study youth to administrative data sources (supplementary materials, materials and methods). Program participation is from provider-tracked attendance records. Student-level administrative records from the Chicago Public Schools capture pre- and post-program academic outcomes from the 2011–2012 and 2012–2013 academic years, respectively. Demographic information on applicants' neighborhoods comes from matching the Census tract of youths' home addresses to the 2010 American Community Survey. The main outcome measures are from individual-level Chicago Police Department arrest records covering both juveniles and adults (*32*). The study uses data through 25 September 2013, which is 16 months after randomization (13 months after the end of the program).

The analysis categorizes arrests by offense type (violent, property, drug, and other). It is common for social interventions to have differential impacts on these different types of crime (*31*, *33*, *34*), likely because some of the underlying causes differ. Violent crime stems from conflicts between people, so problematic cognitive and emotional responses to social interactions—including hostile attribution bias, uncontrolled anger, and "hot" decision-making—are thought to be proximal causes of youth violence in particular (*35*, *36*). Nonviolent crimes, which involve property or

[1]Department of Criminology, University of Pennsylvania, Philadelphia, PA, USA. [2]University of Chicago Crime Lab, Chicago, IL, USA.
*Corresponding author. E-mail: hellersa@sas.upenn.edu

Downloaded from on February 8, 2016

drugs more often than interpersonal conflict, may be relatively more responsive to situational and economic factors (*37, 38*). Because OSP could affect how youth perceive and respond to social interactions differently from how it affects their economic situations, opportunities for crime, or drug use, the study estimates program effects separately by crime type.

Average applicant characteristics on a variety of preprogram measures are shown in Table 1. None of the differences across the columns is statistically significant, nor are they jointly significant ($F_{31,1517}$ = 0.37, P = 0.9995), confirming the success of randomization.

Applicants are on average just over age 16 years (minimum, 14 years; maximum, 21 years), and almost all are African-American. Over 90% are eligible for free or reduced price lunches (a proxy for family poverty), with average grade point averages (GPAs) around a C in the prior fall semester. Study youth missed 18% of the preprogram school year, or around 6 weeks. About 20% had been arrested at baseline, and just over 20% had been victimized. Applicants live in highly disadvantaged neighborhoods: Unemployment averages over 19%; a third of households are under the poverty line; and violent crime rates are extremely high (more than 2100 incidents per 100,000 people).

The main text presents intent-to-treat (ITT) estimates, which measure the average difference between those randomly assigned to treatment and control groups. Three quarters of treatment youth actually participate (supplementary materials, section 2.3, and table S1). Because not all youth who are offered the program enroll, the ITT understates the effect of the program on those who choose to participate. This treatment-on-the-treated effect is discussed and estimated in the materials and methods section and section 2.4 of the supplementary materials. Program impacts are shown both for the treatment group as a whole and each treatment arm.

During the 16-month follow-up period, ~17% of the control group (n = 155 youth) is arrested for any crime, with an average of 0.30 arrests per youth. The main ITT results are presented in Fig. 1. Violent-crime arrests among the treatment group decrease by 43% relative to the control group (0.0395 fewer arrests, or almost 4 fewer per 100 youth; P = 0.022). There are no significant changes in other types of arrests (*39*). The results are robust to accounting for the number of hypothesis tests conducted, allowing youth outcomes to be correlated within schools and using a nonlinear specification for count data (supplementary materials, section 2.5). Although the study was not powered to detect heterogeneous treatment effects across subgroups, there is suggestive evidence that arrests fall more among youth at higher risk of violence (supplementary materials, section 2.7).

An important question concerns what mechanisms drive this considerable behavioral change. One hypothesis for why prior youth employment programs require high intensity to succeed is that disadvantaged adolescents may lack the "soft skills"

Table 1. Mean preprogram characteristics for treatment and control groups. To test baseline equivalence, each characteristic was regressed on treatment indicator and blocking variables using heteroskedasticity-robust standard errors; the *P* value column reports statistical significance of treatment indicator's coefficient. Demographic and schooling data are from Chicago Public Schools' administrative records on pre-program year (2011–2012 school year). Free/reduced price lunch is proxy for family poverty. Days absent are reported as a percentage of days enrolled (average missed, 29 days). Arrest and victimization data are from Chicago Police Department administrative records. Neighborhood characteristics are from 2010 American Community Survey and Chicago Police Department's community-area crime rates. "Percent unemployed" is percent of civilian labor force over age 16 years looking for work but without a job. Table uses nonmissing data only (5 youth missing school attendance data, 60 missing fall GPA, 1 missing neighborhood violent-crime rate). Gender is not included in the table because it is captured by the blocking variables; 38% of the sample is male.

	Control mean	Treatment mean	P value
	(n = 904)	(n = 730)	
Demographics			
Age	16.79	16.78	0.46
Grade	10.15	10.12	0.55
Percent Black	96%	94%	0.84
Percent Hispanic	2.9%	3.8%	0.74
Percent free/reduced price lunch	92%	92%	0.98
Schooling			
Percent days absent	18%	18%	0.99
GPA in fall 2011 (4 point scale)	2.37	2.32	0.93
Percent enrolled in summer school 2011	8.8%	9.0%	1.00
Crime and victimization			
Percent ever arrested	19%	22%	0.37
No. of violent-crime arrests	0.13	0.18	0.20
No. of property-crime arrests	0.09	0.09	0.71
No. of drug arrests	0.05	0.08	0.55
No. of other arrests	0.15	0.19	0.55
Percent ever victimized	21%	24%	0.36
No. of victimizations	0.29	0.33	0.30
Neighborhood characteristics			
Percent unemployed	19%	19%	0.94
Percent below poverty line	33%	35%	0.71
Median household income (US$)	35,665	34,321	0.58
Violent crime rate (per 100,000)	2,128	2,136	0.95

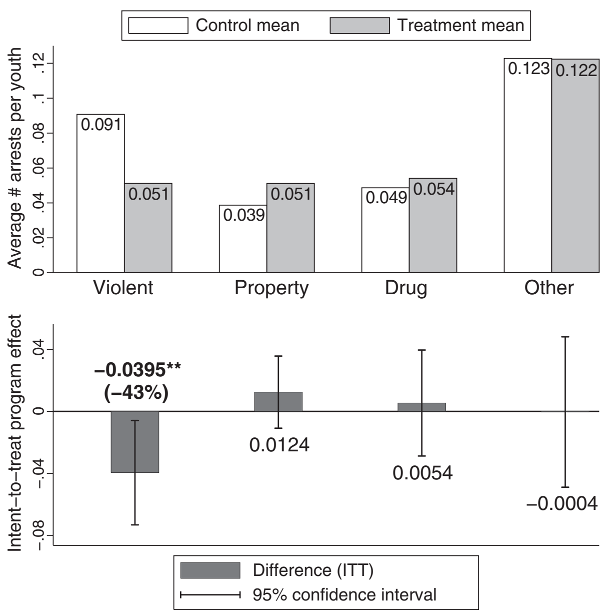

Fig. 1. ITT program effect on crime by arrest type. Shown are the control group mean, regression-adjusted treatment mean, difference between the two (ITT), and 95% confidence interval for the difference by arrest type. Data are from Chicago Police Department administrative arrest records through 16 months after randomization. ITT was calculated by using ordinary least squares regression with heteroskedasticity-robust SEs, controlling for baseline covariates and blocking variables. Percent change is relative to the control mean. ***P < 0.01, **P < 0.05, *P < 0.10

to engage with less intensive pro-social programming (40). If so, OSP's SEL curriculum—which focuses on emotion and conflict management, social information processing, and goal setting—could be a key driver of the violence decrease. Because SEL was randomly assigned, the study can separately identify any additional role of SEL.

ITT violence effects by treatment group are shown in Table 2. Not only is the difference between groups statistically insignificant, but the magnitude is also tiny (1% of the control mean). The nearly identical point estimates suggest that the statistical similarity across groups is not just the result of limited power to detect subgroup differences. Instead, it appears that both groups of youth experienced very similar drops in violence. The same is true for other crime types (supplementary materials, section 2.6, and table S4).

Although the experimental design can only directly isolate the role of SEL, another potentially important mechanism is testable indirectly: whether the decrease in violence is a mechanical result of youth having less time to engage in crime while working over the summer (an "incapacitation" effect). If so, and youth returned to their prior behavioral patterns immediately after the program ended, one would expect to see a large violence drop during the program period, after which there would be no treatment-control difference. This is not the case. The ITT violence decrease during the 3 months between random assignment and the end of the program is proportionally large but statistically insignificant (–0.005 violent-crime arrests, $P = 0.39$, control mean = 0.01). During the following 13 months (excluding the program period), treatment reduces the number of violent-crime arrests by –0.035 ($P = 0.03$, control mean = 0.08).

Table 2. ITT effect on violent-crime arrests by treatment group. Results are from regression of number of violent-crime arrests on separate treatment group indicators, baseline covariates, and block fixed effects. Bottom row shows the magnitude and SE of the difference in effect across treatment arms. Heteroskedasticity-robust standard errors are in parentheses. $^{***}P < 0.01$, $^{**}P < 0.05$, $^{*}P < 0.10$

Jobs-only	–0.0391* (0.0205)
Jobs + SEL	–0.0399** (0.0203)
Difference between groups	0.0008 (0.0220)

The time path of the violence decrease is shown in Fig. 2 in greater detail. It graphs the cumulative treatment effect over time, with each point adding an additional month of data to the prior effect. The drop in arrests becomes statistically different from zero around month 6, 3 months after the end of the program, and continues to grow through month 11, after which it flattens out. The downward slope of the effect makes it clear that the bulk of the drop in violence accrues between months 5 and 11, well after the end of the program at month 3.

One way the program might have changed behavior during these later months is by increasing time spent in school, either by developing pro-social attitudes or providing information on how the labor market values education. However, schooling data suggest that this is unlikely to be the key mechanism. There is no treatment effect on days present (or other academic outcomes) during the following school year, with confidence intervals small enough to make changes in attendance an implausible cause of the drop in violence (supplementary materials, section 2.10, and table S7).

Prior research on youth employment suggests that only costly and intensive employment programs can reduce crime. The current study demonstrates that when offered to youth still in school, an intervention need not be lengthy to change behavior; an 8-week summer jobs program reduces violence among an adolescent population living in some of the most violent neighborhoods in the country.

The results are consistent with the idea that employment programs can have a larger impact at lower cost when offered to disadvantaged youth before school exit. There are, however, other possible reasons why this study finds larger effects than previous work. For example, OSP involved an adult job mentor making regular visits to the workplace, whereas most of the evaluated U.S. youth programs do not. The measurement of crime may also matter. If reaching youth before school exit is the key difference, one might expect multiple-year interventions for high school youth that aim to improve both education and employment outcomes to generate even larger impacts than seen here. In fact, they generally find no effects on crime (41, 42). But, the evaluations of those two programs use self-reported overall crime rates, separating only drug and alcohol use. Given that effects are heterogeneous by crime type, it is possible that these programs also reduced violence, but that the studies aggregated crime outcomes to a level that masked the effect.

The estimates in this paper rely on administrative arrest records, which only capture offenses known to the police. Nationally, about half of violent incidents are reported to police, about half of which result in an arrest (43, 44). As such, the estimates here may considerably understate the number of violent crimes prevented (supplementary materials, section 2.9).

The fact that both the jobs-only and jobs + SEL versions of the program were equally effective means that SEL cannot be the only operative mechanism. A similar SEL program has reduced violence on its own (45), but the jobs-related programming was enough to generate the steep drop in violence independently. Because replacing 2 hours of work with an SEL curriculum neither improved nor reduced the intervention's impact, the relevant mechanism is likely to be something common across the two types of programming.

One possibility is that the substance of the SEL curriculum—teaching youth to process social information, manage thoughts and emotions, and set and achieve goals more successfully—was taught equally well on the job. Youth could have learned these skills from interactions with the job mentors, who were part of both treatment arms. Reports from the program providers suggest that the mentors play a large part in helping youth learn to manage conflict in the workplace, in addition to teaching basic job skills. The process of working itself might also be enough to improve self-control, develop self-efficacy, and reduce frustration (all potential determinants of how youth perceive and respond to conflict). One study, which randomly assigned adults to an earnings subsidy program to increase employment, found that treated individuals report a greater sense of control over their lives and less anger about their lack of opportunities (46).

Another possibility is that the relevant mechanism was something else that both groups received: income, a caring adult, job skills, and/or employer connections. The experimental design cannot tease out which program element or elements generate the decrease in violence. Nonetheless, because schooling outcomes show little movement, it is unlikely that changes in

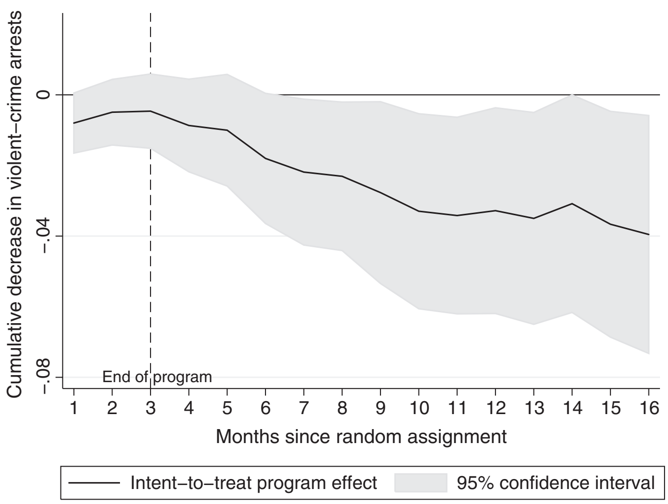

Fig. 2. Time path of violence decrease. Shown is the regression-adjusted cumulative difference in the number of violent-crime arrests between treatment and control youth by month. Random assignment lottery occurred at month 0; program ended at month 3. Regressions include baseline covariates and blocking variables. Confidence intervals were calculated by using heteroskedasticity-robust SEs.

perceptions about the value of schooling, or simply the amount of time in school, are the main drivers of the violence drop. The program provides a relatively large income shock, averaging $1400 in neighborhoods where one third of households are below the poverty line and median income is about $35,000. Additional income could change criminal behavior directly or increase parental supervision by reducing how much parents need to work away from home. It is also possible that improved post-program employment plays a role, although the direction of that effect is theoretically ambiguous. Working more could reduce all types of crime through incapacitation or by making it more costly to be incarcerated. But, the increased income could also have mixed effects, for example by reducing economically motivated crimes such as theft while increasing the ability to purchase drugs.

The fact that OSP only reduces violent crime is perhaps most consistent with a role for improvements in self-control, social information processing, and decision-making, which as discussed above are more central to violent behavior than to other types of crime. Other interventions that target these skills through either explicit curricula or mentorship have also reduced youth delinquency with very few contact hours (*47*, *48*).

One key insight from the data is that incapacitation alone—the mechanical reduction in free time during the summer—cannot explain the program's success. Violence reductions are large and statistically significant during the 13 months after the program is over, and the point estimate is 7 times larger at the end of the follow-up period than at the end of the summer. This pattern is not consistent with the idea that behavior only changed during the initial 8 weeks of the program; something about youths' summer experiences also changed their future behavior.

As in any social experiment, external validity is not guaranteed. The study sample is fairly representative of low-income, African-American youth living in urban neighborhoods who are still enrolled in school. Many other cities have successfully implemented summer jobs programs, suggesting that the basic approach is one that may not be difficult to scale (although displacement effects that reduce the number of jobs available to nonprogram youth become a bigger concern in larger-scale programs). A better understanding of how broadly these results will generalize requires replications in other cities with different service environments and youth populations. Policymakers should proceed with caution until future studies can establish exactly what works for whom, how long effects persist, and whether the program's benefits outweigh its costs (supplementary materials, section 2.11).

In the meantime, this study provides causal evidence on the effects of a widespread but understudied intervention. The results echo a common conclusion in education and health research: that public programs might do more with less by shifting from remediation to prevention. The findings make clear that such programs need not be hugely costly to improve outcomes for disadvantaged youth; well-targeted, low-cost employment policies can make a substantial difference, even for a problem as destructive and complex as youth violence.

REFERENCES AND NOTES

1. Center for Disease Control and Prevention, Web-based Injury Statistics Query and Reporting System (WISQARS) (2014).
2. Numbers are daily averages from the 5 most recent years of violence- related fatalities (from death certificates) and nonfatal injuries (from emergency room reports).
3. Center for Disease Control and Prevention, STRYVE to Prevent Youth Violence Website, 2014; available at www.cdc.gov/features/youthviolence/index.html.
4. Authors' calculations for ages 10 to 24 years versus over 24 are from 2012 Federal Bureau of Investigation (FBI) Uniform Crime Reports, Census data, and (*1*).
5. Office of Juvenile Justice and Delinquency Prevention, OJJDP Statistical Briefing Book, U.S. Department of Justice, Washington DC, 2014; available at www.ojjdp.gov/ojstatbb/crime/JAR_Display.asp?ID=qa05261.
6. W. J. Wilson, *When Work Disappears: The World of the Urban Poor* (Alfred Knopf, New York, 1996).
7. G. S. Becker, *J. Polit. Econ.* **76**, 169–217 (1968).
8. T. P. Thornberry, R. L. Christenson, *Am. Sociol. Rev.* **49**, 398–411 (1984).
9. R. J. Sampson, J. H. Laub, *Am. Sociol. Rev.* **55**, 609–627 (1990).
10. R. K. Merton, *Am. Sociol. Rev.* **3**, 672–682 (1938).
11. P. J. Cook, J. Ludwig, in *Controlling Crime: Strategies and Tradeoffs*, P. J. Cook, J. Ludwig, J. McCrary, Eds. (University of Chicago Press, Chicago, 2011), pp. 1–39.
12. The focus here is on youth programs because disadvantaged youth are at such disproportionate risk of violent offending. There have been many public efforts to provide vocational or on-the-job training, job search assistance, and remedial education to adults as well. Those programs' crime effects are also mixed (*49*), perhaps in part because the largest employment impacts tend to be among adult women—a group with very low levels of criminal involvement (supplementary materials, section 2.1).
13. M. Millenky, D. Bloom, S. Muller-Ravett, J. Broadus, *Staying on Course: Three-Year Results of the National Guard Youth ChalleNGe Evaluation* [Manpower Demonstration Research Corportation (MDRC), New York, 2011].
14. C. Uggen, *Am. Sociol. Rev.* **65**, 529–546 (2000).
15. P. Z. Schochet, J. Burghardt, S. McConnell, *Am. Econ. Rev.* **98**, 1864–1886 (2008).
16. G. Cave, H. Bos, F. Doolittle, C. Toussaint, *JOBSTART. Final Report on a Program for School Dropouts* (MDRC, New York, 1993).
17. H. S. Bloom *et al*., *J. Hum. Resour.* **32**, 549–576 (1997).
18. Although there is a consensus that generating measurable changes in youth outcomes requires comprehensive and expensive intervention, conclusions about how much investment is desirable differ by author (*19*, *40*, *49*, *50*).
19. R. J. LaLonde, in *Means-Tested Transfer Programs in the United States*, R. A. Moffitt, Ed. (Univ. Chicago Press, Chicago, IL, 2003), pp. 517–586.
20. A. L. Fernandes-Alcantara, *Vulnerable Youth: Federal Funding for Summer Job Training and Employment* (Congressional Research Service, Washington, DC, 2011).
21. A. E. Schwartz, J. Leos-Urbel, *Expanding Summer Employment Opportunities for Low-Income Youth* (Brookings Institution, The Hamilton Project, Washington, DC, 2014).
22. Youth were individually randomized within a blocked design, in which blocks were defined by school and gender. All analyses control for the blocking variable. Materials and methods are available as supplementary materials on *Science* Online.
23. E. I. Knudsen, J. J. Heckman, J. L. Cameron, J. P. Shonkoff, *Proc. Natl. Acad. Sci. U.S.A.* **103**, 10155–10162 (2006).
24. D. M. Cutler, *J. Econ. Perspect.* **22**, 3–26 (2008).
25. The emphasis on those already exhibiting symptoms of the underlying problem is true of most major programs focused on youth employment. The youth elements of the National Supported Work Demonstration, JOBSTART, the National Guard ChalleNGe, and the Job Training Partnership Act all focus on high school dropouts (*13*, *16*, *17*, *51*). Job Corps requires applicants to need additional education, training, or vocational skills—all signs that they are already experiencing difficulties in the labor force (*15*). Year Up serves youth no longer in school (*52*). There are well-evaluated programs for high school youth that integrate employment programming with long-term educational interventions [such as (*41*, *42*)]. However, because they are also lengthy and intensive—and aim to improve educational attainment directly, which may have an independent impact on crime (*29*)—they do not provide a test of the hypothesis proposed here: that low-dose, low-cost employment interventions could have a larger effect than suggested by the existing employment literature when used as primary and secondary prevention.
26. R. J. Sampson, J. H. Laub, *Crime in the Making: Pathways and Turning Points Through Life* (Harvard Univ. Press, Cambridge, MA, 1995).
27. R. Agnew, *Criminology* **30**, 47–88 (1992).
28. P. Moss, C. Tilly, *Work Occup.* **23**, 252–276 (1996).
29. L. Lochner, E. Moretti, *Am. Econ. Rev.* **94**, 155–189 (2004).
30. J. Grogger, *J. Labor Econ.* **16**, 756–791 (1998).
31. B. A. Jacob, L. Lefgren, *Am. Econ. Rev.* **93**, 1560–1577 (2003).
32. Additional results on violent victimizations are provided in supplementary materials, section 2.7.
33. D. Deming, *Q. J. Econ.* **126**, 2063–2115 (2011).
34. J. R. Kling, J. Ludwig, L. F. Katz, *Q. J. Econ.* **120**, 87–130 (2005).
35. K. A. Dodge, D. Schwartz, in *Handbook of Antisocial Behavior*, D. M. Stoff, J. Breiling, J. D. Maser, Eds. (John Wiley & Sons, Hoboken, NJ, 1997), pp. 171–180.
36. J. Ludwig, A. Shah, "Think Before You Act: A New Approach to Preventing Youth Violence and Dropout," Discussion Paper 2014-02 (The Hamilton Project, Brookings Institution, Washington, DC, 2014).
37. A. M. Piehl, in *The Handbook of Crime and Punishment*, M. Tonry, Ed. (Oxford Univ. Press, Oxford, 1998), pp. 302–319.
38. S. Raphael, R. Winter-Ebmer, *J. Law Econ.* **44**, 259–283 (2001).
39. The reliance on official arrest data means that violent crime is a better-measured outcome; nationally, violent crimes are 2.5 times more likely to result in an arrest than are property crimes (*43*). Differential arrest probabilities also help explain why the control means for property and drug arrests are around half as large as for violent-crime arrests, despite the fact that nonviolent crimes are more common (*43*). The supplementary materials, section 2.9, discusses how underreporting might affect the results.
40. C. J. Heinrich, H. J. Holzer, *Ann. Am. Acad. Pol. Soc. Sci.* **635**, 163–191 (2011).
41. J. J. Kemple, C. J. Willner, *Career Academies: Long-Term Impacts on Labor Market Outcomes, Educational Attainment, and Transitions to Adulthood* (MDRC, New York, 2008).
42. A. Schirm, E. Stuart, A. McKie, *The Quantum Opportunity Program Demonstration: Final Impacts* (Mathematica Policy Research, Washington, DC, 2006).
43. Federal Bureau of Investigation, "Crime in the United States 2012," Uniform Crime Report (U.S. Department of Justice, Washington DC, 2012).
44. L. Langton, M. Berzofsky, C. Krebs, H. Smiley-McDonald, "Victimizations Not Reported to the Police, 2006–2010" (Bureau of Justice Statistics, U.S. Department of Justice, Washington, DC, 2012).
45. S. Heller, H. A. Pollack, R. Ander, J. Ludwig, "Preventing Youth Violence and Dropout: A Randomized Field Experiment," NBER Working Paper no. 19014 (National Bureau of Economic Research, Cambridge, MA, 2013).
46. P. Gottschalk, *J. Policy Anal. Manage.* **24**, 485–498 (2005).
47. J. B. Grossman, J. P. Tierney, *Eval. Rev.* **22**, 403–426 (1998).
48. R. Barnoski, S. Aos, "Outcome evaluation of Washington State's research-based programs for juvenile offenders" (Washington State Institute for Public Policy, Olympia, WA, 2004).
49. S. Raphael, in *Controlling Crime: Strategies and Tradeoffs*, P. J. Cook, J. Ludwig, J. McCrary, Eds. (Univ. Chicago Press, Chicago, 2011), pp. 521–565.
50. J. J. Heckman, A. B. Krueger, *Inequality in America: What Role for Human Capital Policies?* (MIT press, Cambridge, MA, 2003).
51. Manpower Demonstration Research Corporation, *Summary and Findings of the National Supported Work Demonstration* (Ballinger Publishing Company, Cambridge, MA, 1980).
52. A. Roder, M. Elliott, *A Promising Start: Year Up's Initial Impacts on Low-Income Young Adults' Careers* (Economic Mobility Corporation, New York, 2011).

ACKNOWLEDGMENTS

This project was supported by award B139634411 from the U.S. Department of Labor and grant 2012-MU-FX-0002 from the Office of Juvenile Justice and Delinquency Prevention, Office of Justice Programs, U.S. Department of Justice. The study received approval from the University of Chicago's IRB (protocol 12-1112) with secondary approval from the University of Pennsylvania's IRB (protocol 818707). I gratefully acknowledge the City of Chicago and Mayor R. Emanuel, the work of the Chicago Department of Family Support Services, especially E. Diaz, J. Axelrod, A. Fernandez, and J. Welch, and the organizations that implemented the program: Phalanx Family Services, Sinai Community Institute, Saint Sabina Employment Resource Center, SGA Youth and Family Services, and Youth Guidance. I thank Chicago Public Schools partners J. Foreman, K. Klein, J. Loudon, and S. Norris; Chief R. Tracy, T. Lavery, and the Chicago Police Department; R. Goerge and Chapin Hall; R. Ander, S. Coussens, J. Davis, G. Cusick, M. Egan, H. Pollack (Chicago PI), R. Harris, N. Hess, A. Métivier, and J. Rountree for project support; and K. Charles, J. Guryan, C. Loeffler, J. Ludwig, J. MacDonald, E. Owens, S. Raudenbush, S. Sotelo, and D. Tannenbaum for helpful comments. Replication data are posted at the University of Michigan's ICPSR data depository (http://doi.org/10.3886/E18627V1); see supplementary materials section 1.5 for details. All content is the responsibility of the author and does not represent the official position or policies of the Chicago Police Department, Chicago Public Schools, Chicago Department of Family and Support Services, U.S. Department of Justice, or U.S. Department of Labor.

SUPPLEMENTARY MATERIALS

www.sciencemag.org/content/346/6214/1219/suppl/DC1
Materials and Methods
Supplementary Text
Tables S1 to S8
References (*53–96*)

23 June 2014; accepted 7 November 2014
10.1126/science.1257809

PALEOCLIMATE

Coherent changes of southeastern equatorial and northern African rainfall during the last deglaciation

Bette L. Otto-Bliesner,[1*] James M. Russell,[2] Peter U. Clark,[3] Zhengyu Liu,[4,5] Jonathan T. Overpeck,[6] Bronwen Konecky,[3,7] Peter deMenocal,[8] Sharon E. Nicholson,[9] Feng He,[4] Zhengyao Lu[5]

During the last deglaciation, wetter conditions developed abruptly ~14,700 years ago in southeastern equatorial and northern Africa and continued into the Holocene. Explaining the abrupt onset and hemispheric coherence of this early African Humid Period is challenging due to opposing seasonal insolation patterns. In this work, we use a transient simulation with a climate model that provides a mechanistic understanding of deglacial tropical African precipitation changes. Our results show that meltwater-induced reduction in the Atlantic meridional overturning circulation (AMOC) during the early deglaciation suppressed precipitation in both regions. Once the AMOC reestablished, wetter conditions developed north of the equator in response to high summer insolation and increasing greenhouse gas (GHG) concentrations, whereas wetter conditions south of the equator were a response primarily to the GHG increase.

The future response of African rainfall to increasing greenhouse gas (GHG) concentrations is a critical socio-economic issue, with implications for water resources, agriculture, and potential conflict (*1*), but uncertainties among model projections remain (*2–4*). African hydroclimate changed substantially during the last deglaciation, the most recent time period during which natural global warming was associated with increases in GHG concentrations. Numerous proxy records from Africa indicate that dry conditions during the Last Glacial Maximum (LGM) (~21,000 years ago, or 21 ka) were rapidly replaced by a much wetter interval, referred to as the African Humid Period (AHP), starting ~14.7 ka over much of Africa. Over North Africa (NA), the start of the AHP has been widely recorded in lake-level records (*5*, *6*) and proxies of aeolian and fluvial processes preserved in marine sediments from the eastern Atlantic Ocean (*7–10*). At the same time, a near-contemporaneous precipitation increase is also recorded in southeastern equatorial Africa (SEA) (to 9°S) by lake-level records (*11–14*), as well as in pollen and geochemical records from lake sediments (*14–16*).

Models and data establish that the initial increase of NA summer monsoonal rainfall occurred in response to increasing local insolation associated with orbital variations (*17*), amplified through feedbacks with the ocean and possibly vegetation (*18–20*), but the cause of the abrupt start of the AHP remains unclear. Proposed triggers include a nonlinear threshold response to gradually changing summer insolation (*8*) and/or the recovery of deep convection in the North Atlantic following cessation of a Northern Hemisphere meltwater event (*21*). Similarly, the cause for the synchronous onset of the AHP in the SEA region has remained enigmatic, as models and theory suggest that orbital forcing of local summer insolation at these latitudes should have reduced precipitation (*22*).

Here, we analyze transient simulations of the climate evolution from the LGM to the early Holocene (11 ka) with a global coupled atmosphere–ocean–sea ice–land general circulation model (CCSM3) to assess possible mechanisms for the abrupt, synchronous onset of the AHP in NA and SEA. The model has a latitude-longitude resolution of ~3.75° in the atmosphere and ~3° in the ocean and includes a dynamic global vegetation module (supplementary text). The model successfully captures the large-scale observed modern features of African climate, including seasonal shifts of winds, the intertropical convergence zone (ITCZ), and precipitation to the summer hemispheres (figs. S2 and S3). To characterize the regional precipitation responses during the deglaciation, we examine model changes in the NA region defined by 11.1° to 18.6°N and 5.6° to 20.6°E and in the SEA region defined by 0° to 7.4°S and 24.4° to 43.1°E (see supplementary text and fig. S7 for sensitivity of model results to the definitions of the regions).

The prescribed forcings and boundary conditions for the full-forcing simulation (TraCE) include orbitally forced insolation changes, increasing atmospheric concentrations of the long-lived GHGs, and retreating ice sheets and associated meltwater release to the oceans (*23*, *24*) (fig. S1). We also explore the individual contributions of orbital forcing and GHGs during the deglaciation with two sensitivity experiments: (i) TraCE orbital-only, where only the orbital forcing is allowed to vary, with all other forcings kept at their values for 17 ka, and (ii) TraCE GHG-only, where only the concentrations of the GHGs change, increasing from low concentrations at 17 ka to close to their pre-industrial concentrations by 10 ka. In both sensitivity experiments, the ice sheets and meltwater release are held constant at 17-ka conditions, and this meltwater maintains a strongly reduced Atlantic meridional overturning circulation (AMOC) afterward.

The temporal evolution of the simulated deglacial precipitation shows good agreement with individual proxy records. TraCE and a proxy record of humidity (*9*) both show dry conditions in the central Sahel at the LGM, a decrease in precipitation at ~17 ka, an abrupt increase at the onset of the Bølling-Allerød warm interval, an episode of drying during the Younger Dryas (YD) (12.9 to 11.7 ka), and an increase during the early Holocene (Fig. 1B). The total leaf-area index of simulated vegetation over the Sahel

[1]Climate and Global Dynamics Division, National Center for Atmospheric Research, Boulder, CO 80307-3000, USA. [2]Department of Earth, Environmental, and Planetary Sciences, Brown University, Providence, RI 02912, USA. [3]College of Earth, Ocean, and Atmospheric Sciences, Oregon State University, Corvallis, OR 97331, USA. [4]Center for Climatic Research and Department of Atmospheric and Oceanic Sciences, University of Wisconsin-Madison, Madison, WI 53706, USA. [5]Laboratory for Climate, Ocean and Atmosphere Studies, School of Physics, Peking University, Beijing 100871, P. R. China. [6]Department of Geosciences and Institute of the Environment, University of Arizona, Tucson, AZ 85721, USA. [7]Cooperative Institute for Research in Environmental Sciences, University of Colorado Boulder, Boulder, CO 80309, USA. [8]Department of Earth and Environmental Sciences, Columbia University, New York, NY 10027, USA. [9]Department of Earth, Ocean, and Atmospheric Science, Florida State University, Tallahassee, FL 32306, USA.
***Corresponding author. E-mail: ottobli@ucar.edu**

Science AAAS

Summer jobs reduce violence among disadvantaged youth
Sara B. Heller
Science **346**, 1219 (2014);
DOI: 10.1126/science.1257809

This copy is for your personal, non-commercial use only.

If you wish to distribute this article to others, you can order high-quality copies for your colleagues, clients, or customers by clicking here.

Permission to republish or repurpose articles or portions of articles can be obtained by following the guidelines here.

The following resources related to this article are available online at www.sciencemag.org (this information is current as of February 8, 2016):

Updated information and services, including high-resolution figures, can be found in the online version of this article at:
/content/346/6214/1219.full.html

Supporting Online Material can be found at:
/content/suppl/2014/12/03/346.6214.1219.DC1.html

This article **cites 40 articles**, 9 of which can be accessed free:
/content/346/6214/1219.full.html#ref-list-1

This article has been **cited by** 1 articles hosted by HighWire Press; see:
/content/346/6214/1219.full.html#related-urls

This article appears in the following **subject collections:**
Economics
/cgi/collection/economics

Downloaded from on February 8, 2016

Science (print ISSN 0036-8075; online ISSN 1095-9203) is published weekly, except the last week in December, by the American Association for the Advancement of Science, 1200 New York Avenue NW, Washington, DC 20005. Copyright 2014 by the American Association for the Advancement of Science; all rights reserved. The title *Science* is a registered trademark of AAAS.