Published in final edited form as:

J Child Psychol Psychiatry. 2009 January ; 50(1-2): 194–200. doi:10.1111/j.1469-7610.2008.01985.x.

Community intervention and public policy in the prevention of antisocial behavior

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Abstract

As academic clinical science moves to community intervention to achieve impact on population prevalence of antisocial behavior disorders, exciting potential is tempered by realistic caution. Three kinds of efforts are noted. First, individual evidence-based therapies are being implemented at scale. Difficulties in high-fidelity implementation are noted, and the unlikelihood of population impact is highlighted. Second, communities are receiving new resources to support individuals, although connecting community resources to highest-risk individuals is difficult. Third, community factors are being targeted for change through policy reform, with mixed results. As the field moves in this direction, the importance of adhering to principles of scientific rigor and empirical evidence is emphasized, to keep scientist-practitioners from overstepping their bounds.

Keywords

Antisocial behavior; community intervention; prevention; population prevalence

Academic psychology and psychiatry's move into clinical practice to prevent antisocial behavior disorders during the latter half of the twentieth century was a bumpy journey. Some academic scientists became enthralled with the power of changing an individual, abandoned the very principles of science that served them in research, and overstepped empirical evidence when treating individuals. But more generally, the scientist-practitioner model infused the professions of clinical practice with scientific thinking, rigor, the concept of falsifiability, and accountability. Although some well-intentioned therapies and preventive interventions have proven ineffective when subjected to scientific scrutiny, several clinical interventions to prevent or reduce antisocial behavior have proven efficacious when tested by randomized clinical trials, including, among others, nurse-practitioner home-visiting (Olds et al., 1998), multi-systemic therapy (Henggeler, Schoenwald, Bourduin, Rowland, & Cunningham, 1998) and multi-dimensional treatment-based foster care (Chamberlain, 2003). Meta-analyses (Yoshikawa, 1994) and reviews (e.g., Blueprints for Violence Prevention, Elliott & Mihalic, 1997) conclude that interventions in chronic violence prevention do indeed 'work'.

As we look forward to the first decades of the twenty-first century, the challenge to academic psychology and psychiatry will become magnified: society is now calling on psychology and psychiatry to change not only individual persons but also population rates of disorders, including conduct disorder and antisocial personality. The US Surgeon General's Conference on Children's Mental Health (2000) reported that, 'It is time that we as a Nation took seriously

the task of preventing mental health problems and treating mental illness in youth.' This report called for massive increases in funding for individual therapies in order to lower population prevalence of psychiatric disorders. The presumed task is to bring the theories, methods, and findings of clinical psychology and psychiatry to bear on changing communities and population prevalence for clinical problems.

The pressure for population change from community interventions comes from the growing recognition of the magnitude and cost of major clinical problems to society as a whole. Antisocial behaviors cost American society over one trillion dollars each year (Anderson, 1999). Child maltreatment alone costs over 94 billion dollars a year. Most of these costs are borne not by the afflicted individuals but accrue, instead, to society as a whole from costs to victims, treatment, civil management, policing, and lost wages and taxes (Cohen, 2005). Pressure also comes from the recognition that, in spite of the maturing of the clinical professions, no impact on population rates of these problems is yet apparent (Biglan, 1995; Tolan & Dodge, 2005). The population rates of conduct disorder, antisocial personality disorder, and violent crime have not been reduced by the past half-century of clinical practice (Dodge, Coie, & Lynam, 2006). Nonetheless, economic studies have shown that American taxpayers would be willing to pay large sums to prevent chronically violent outcomes (Cook & Ludwig, 2006), provided that the efforts actually reduce the community prevalence of violence (Cohen, Rust, Steen, & Tidd, 2004).

Although some interventions have been proven to help individuals, given the modest overall effect sizes of individual-level approaches to the prevention of antisocial behavior (Dodge et al., 2006; Lipsey, 2006), one might be skeptical about the likelihood that the clinical professions can have a discernible impact on the population prevalence of antisocial behavior. However, the theoretical rationale for community approaches comes from several sources. This article will review the rationale for clinical approaches to community change in antisocial behavior and will address both the promises and the pitfalls of clinical efforts to change populations.

Challenges in community change

It cannot yet be argued that clinical practice has lowered population rates of important clinical problems such as conduct disorders (Biglan, 1995) or child maltreatment (Sanders et al., 2008). Three broad reasons for this shortcoming provide the rationales for the three major, complementary, approaches to community intervention in antisocial behavior.

Challenges in going to scale

The first shortcoming is that evidence-based preventive interventions with individuals have not yet been implemented with high fidelity at full scale in entire communities. Only about a third of all children with diagnosable disorder receive treatment (Burns et al., 1995), and much of this treatment does not follow from scientific evidence (Tolan & Dodge, 2005). If a community could muster the financial resources to bring known effective treatments to every needy case, so it is hypothesized, community rates would decrease because of the summed benefit of more treatment to more persons.

Although this concept seems obvious, closer scrutiny suggests that the summed benefits may not end up being as large as hoped. Consider the Fast Track preventive intervention (Conduct Problems Prevention Research Group, 2002, 2007), which has yielded an impressive 50% lifetime reduction in conduct disorder among the highest-risk sub-group that received the preventive intervention, as determined by the difference in rate of conduct disorder between those randomly assigned to preventive intervention and those assigned to control. If the program were brought to scale for all highest-risk children in a community, would a 50% reduction in population prevalence of conduct disorder result? A little arithmetic suggests

otherwise. First, although the stability of conduct problems approaches the stability coefficient of the intelligence quotient (Dodge et al., 2006), the majority of conduct disorder cases do not come from the highest-risk group. The screening instrument used in Fast Track revealed that the highest-risk group of just 3% of the kindergarten population eventually had a rate of conduct disorder of .41, compared with a rate in the remaining 97% of the population of only 6%. This 7-fold increase in risk and 'phenomenal' screening program still identified only 17.5% of all later cases of conduct disorder ($.41 \times .03 = .0123$ for the screened-in group, whereas $.06 \times .97 = .0582$ for the screened-out group; $.0123/.0705 = .175$). Second, Fast Track, like most research-based interventions, relied on volunteers, that is, consenting families. Non-consenting families might be expected to be at especially high risk and could represent a new group of treatment 'failures' during at-scale implementation. Third, even though research studies go to great lengths to retain participants, families, especially high-risk families, move in and out of communities, leading to more treatment 'failures' in community implementation. All told, perhaps only half of all eligible children would actually receive sustained intervention. So, when implemented at full scale, this intervention might be expected to be offered to only half of ultimately conduct-disordered youths and half of these youths might not receive full treatment. The impact of this intervention, which reduces conduct disorder by one-half, when implemented at community scale might well be to reduce the population prevalence of conduct disorder by only 2%.

According to the literature on implementation fidelity, the above example is a best-case scenario. Interventions that were developed for one segment of the population might or might not be optimal for other groups in other contexts. Modification of the intervention to fit new contexts and groups is often necessary and even encouraged. For example, when the Fast Track intervention was disseminated to Manchester, UK, as part of the Home Office's On Track Program, the reading tutoring component was intentionally omitted because it was determined that almost all five-year-old UK children had already achieved the reading readiness skills that the program emphasized. High fidelity was achieved through training and ongoing long-distance consultation, and outcomes, as evaluated by pre-post changes, were called a 'success' (Winn, Newall, Coie, & CPPRG, 2007). Thus, the modifications in this case were warranted and not harmful.

When Fast Track was disseminated to the Durham, North Carolina, community, however, local school officials would not endorse, on policy grounds, the concept of paying parents to attend parent-training sessions, which had been done in the randomized trial. So parents were encouraged but not incentivized to attend, and the result was a deep decline in attendance compared with the original implementation. The literature on dissemination indicates that most interventions lose fidelity when implemented in replications or in communities, and this loss of fidelity is associated with reduced impact (Dodge, 2001; Tolan & Dodge, 2005; Weisz & Jenson, 1999).

On the other hand, it is plausible that simultaneous intervention with large numbers at full scale could have a positive synergistic effect. Plenty of examples from public health highlight the futility of immunization if only one person opts for the intervention. Clinical considerations argue for the benefits of population-level implementation because of the mutual influence that high-risk youth have on each other over the course of development. Bronfenbrenner's (1975) ecological model frames the individual as living in a series of concentric circles of influence that include the microsystem of the individual; the mesosystem of the individual's family; the exosystem of the community, society, and culture; and the macrosystem of the world. Factors at all of these levels influence the individual. The single clinician working only in the microsystem trying to protect an individual from an exosystem that includes deviant peers and a culture that does not value nonaggressive behavior faces an uphill task. In contrast, the

clinician working alongside other clinicians will be afforded support, and youths working alongside other youths will be encouraged.

Economic considerations might also argue for the benefits of mass production of clinical interventions. Given the high fixed costs of bringing an evidence-based program to a single case (training of interventionist, supervision, manuals, monitoring, evaluation), a critical mass of interventionists could be trained and supervised at lower per-case costs. Thus, it can be argued that individual approaches to preventing antisocial behavior fail precisely because they have been implemented with a small number of individuals rather than at the population level.

Of course, it is no small matter to ramp up the capacity to meet the needs of an entire community. Although some economists might argue for an economy of scale that would lower the per-case cost, another perspective is that the 'easiest' cases are the ones currently being treated and at-scale implementation would face failure with hard-to-treat individuals. Furthermore, going to scale would undoubtedly strain existing capacity. Already, most families, even those who are able to pay for services, find it difficult to identify and access qualified clinicians (US Public Health Service, 2000). Current clinician capacity is able to serve only about 10% of all deserving cases (Jenkins, 1998). Also, clinicians are focused on children with the most profound symptoms (President's New Freedom Commission on Mental Health, 2003), with ethnic biases rampant. Services are least accessible in communities that have the highest rates of mental health needs (Leaf et al., 1996). Because of limited capacity to identify qualified clinicians, the quality of delivery of an intervention at full implementation is likely to be less than when delivered in an efficacy study, and the result is likely to be less effectiveness (Weisz & Jenson, 1999).

Insight can be gained from analogy to the largest known attempt to bring an evidence-based education reform to scale. The Tennessee Student/Teacher Achievement Ratio (STAR) Project was a randomized experiment with 11,600 young children who were assigned to either regular-size (25 students) classrooms or smaller-size (17 students) classrooms. Children assigned to smaller classes performed better, beginning with academic scores in elementary school (Folger, 1989, Fall) and continuing through high school with test scores, graduation rates, and college enrollment rates (Pate-Bain, Boyd-Zaharias, Cain, Word, & Binkley, 1997). The findings were impressive, and it seemed clear that smaller class size would benefit children because of increased teacher contact time. As a result, in 1996, the State of California began to invest over one billion dollars per year in a statewide at-scale implementation of smaller class size. What happened? Bohrnstedt and Stecher (1999) reported that the first effect was that the credentials (certification, experience) of the average teacher in California schools decreased markedly. That is, there were not enough qualified teachers to go around and so less-qualified teachers were hired to fill smaller classrooms. Furthermore, the problem with poorly credentialed teachers was exacerbated in low-income areas and in schools with disadvantaged children where it is difficult to hire high-quality teachers. As a result, the net benefits of smaller class size were largely offset by negative effects of being taught by a less-credentialed teacher, and achievement gaps across ethnic and socioeconomic groups widened rather than shrank. A major lesson learned is that bringing efficacious programs to community scale may bring unexpected outcomes because of false assumptions that all conditions will remain the same.

Perhaps the boldest attempt to bring an evidence-based individual clinical intervention to full scale is the effort by several communities to implement the nurse-practitioner home-visiting program by Olds et al. (1998). This program, which is directed toward high-risk first-time mothers, has been shown in a randomized trial with small numbers of mothers in Elmira, New York, to prevent child abuse, at least among the subgroup that is low-income, unmarried, teenage, and not in a domestically violent relationship. Replication trials in Memphis and Denver did not yield significant impact on child abuse but did reveal positive impacts on other

measures. The program is now being rolled out with large sums of money in communities across the United States and the world. The Robert Wood Johnson Foundation has supported an effort to bring the program to 10,000 families in 100 communities. The State of Colorado allocated 75 million dollars over 10 years for programs in 30 communities across Colorado. Twenty million dollars per year has supported the program in 20 communities in Pennsylvania. By 2006, the program was being implemented in 20 states serving 20,000 families. The marketing plans for this program call for expansion to 34,000 families by 2010, with implementation in multiple countries (Goodman, 2006). To his credit, Olds has tried to maintain adequate training of all clinicians, highest fidelity of implementation, ongoing supervision, and sufficient ongoing funding when this program has been implemented across entire communities.

Although one can be in awe of the scope of this implementation, the numbers reveal that no community is likely to receive full penetration to all qualifying mothers, and no report has yet been issued regarding the effect on population prevalence of child abuse or conduct disorder in any community. Furthermore, even though the empirical support for this program may be stronger than almost any other program, the findings of the three trials on which the rollout is based are inconsistent with each other in that specific positive outcomes replicate in only two of the three trials. The outcomes that show positive evidence vary across trials in unexplained ways. Also, the subgroups that show positive effects vary across trials. Given the modest evidence and the challenges in community implementation, it is not clear that population-level impact will occur even for this best-evidence and best-funded program.

Challenges in resourcing communities

An implication of the problem of limited capacity is that preventing the population incidence of antisocial behavior problems will require community-level improvements in the resources, context, financing, and access to individual-level preventive interventions. The problem is akin to community approaches in other sectors, such as education, health care, and economic development, in which policy reform variously addresses work force development, training, standards of care, regulations regarding professional providers of service, and financing. These issues have been well researched and thoroughly tackled in other domains, and even in children's mental health care they have been given great attention by service researchers. Calls for increasing the professional work force in children's mental health are not new (Knitzer, 1982); however, *how* the work force needs to change is much less clear.

Relatively few community resource development efforts have been guided by a comprehensive clinical-developmental understanding of how antisocial behavior problems grow and can be prevented (CPPRG, 1992; Dodge & Pettit, 2003). This understanding suggests the need for indigenous adults to relate effectively to the child at various points in development. In very early life, attachment theory provides a model for how the adult scaffolds the infant's development, but soon the functions of adults grow to include modeling, reinforcing, relating, educating, supervising, and monitoring (Dodge, 2001). Optimally, this network of adults begins with both mother and father, extends to broader family members, and comes to include neighborhood and community members. When the network is insufficient or the child's needs cross into the clinically significant range, mental health professionals take on corrective roles. Building on this model, community resources to prevent antisocial behavior problems should include not only increasing professional resources but also paraprofessional and volunteer resources that involve specific adults as well as settings that support the functions of adults (such as mentors and supervised after-school programs that provide monitoring and supervision).

Empirical support for the role of caring adults as a measure of social capital comes from Werner and Smith's (1992) seminal longitudinal study, which found that resilient children relied on a

network of informal relationships that included ministers, trusted teachers, and other adults. Empirical evidence that community resources affect children's antisocial behavior problem rates comes from a variety of sources, including correlational and experimental treatment studies (Tolan & Gorman-Smith, 1997). The largest experiment in community mentoring was conducted by Big Brothers/Big Sisters of America (Tierney, Grossman, & Resch, 1995), which randomly assigned 959 youth in eight American cities to a mentor (or not). After 18 months, mentored youth were half as likely to initiate drug use, 27% less likely to initiate alcohol use, and one-third less likely to use violence. The national Big Brothers/Big Sisters Program is attempting at-scale dissemination of this model, although it is nowhere near fully implemented yet. Numerous challenges must be surmounted before at-scale implementation is achieved, let alone population prevalence impact, including community capacity to identify qualified mentors, infrastructure to match mentors and children, and penetration to highest-risk youths. Although this program targets disadvantaged youth, it may not reach the most antisocial youth. The Willie M. Program in North Carolina attempted to bring community mentoring to very high-risk antisocial youth, with mixed results (Vance, Bowen, Fernandez, & Thompson, 2002).

Challenges in changing communities

The third possible reason that clinical interventions delivered to individuals do not lower population prevalence is that clinical problems themselves are partially caused by community-level factors which can be changed only by community-level intervention. Comprehensive theoretical models of conduct disorder (CPPRG, 1992; Dodge et al., 2006) point toward distal factors such as neighborhood-level violence, cultural endorsement of aggression, culture of peer deviance, discrimination against specific groups, and socioeconomic class divides between high-risk families and mainstream institutions of education and juvenile justice. Individual-level interventions may take into account these factors but cannot easily alter these factors. Clinicians 'work around' or 'work with' community risk factors; they almost never work to change these factors. Going to scale with individual-level interventions may ignore cultural and community causes, leading to the perpetual replication of new cases with little net impact on community rates of problems. Removing one drug trafficker from the street corner may only lead a new trafficker to emerge; removing the class deviant from the middle school classroom may only grow a new student to fill this role. This perspective suggests the need for interventions, mostly through public policy, that address community factors directly.

This community perspective runs counter to prevailing theoretical perspectives and empirical evidence that community factors such as neighborhood poverty operate on a child through mediation by parenting practices. That is, community poverty makes it difficult for parents to relate effectively to their children, and parenting is more proximal in its impact on a child (McLoyd, 1990). Nonetheless, it may be that clinical interventions to alter parenting are impeded by community factors that bred poor parenting initially.

In political science, Putnam (2000) has heralded the importance of community social capital in supporting all kinds of beneficial outcomes, including individual mental health and lower violence. The evidence that he has mounted is substantial and convincing. Other community factors have been cited as contributing to antisocial behavior as well, including collective efficacy (Sampson, Raudenbush, & Earls, 1997), a culture of honor (Nisbett & Cohen, 1996), and norms regarding 'broken windows' and community reinforcements and punishments for crime (Wilson, 1983). Numerous efforts have been undertaken to alter these community-level factors, including community policing, public policies of deterrence, and funding of neighborhood watch programs. Unfortunately, evidence is still lacking that community factors can be changed and that community change can lower population prevalence of antisocial behavior problems.

Conceptualizing community intervention and individual impact

This article has reviewed the possibility that individual-level clinical interventions might have impact on community prevalence of disorders and that community-level interventions might have impact on individual outcomes. What is suggested by this analysis is a simple two-by-two factorial representation of interventions depicted in Figure 1. One factor is the target of intervention, as individually-targeted or community-targeted. The second factor is the measurement of outcome, as the individual or community score. Interventions that are targeted toward individuals and measured at the individual level are traditional psychotherapies. Interventions that are targeted toward community factors for individual impact are very expensive and probably not cost-beneficial. Interventions that are targeted toward individual for community impact are at-scale implementations of individual therapies. Interventions targeted toward communities for community impact are public policies.

The importance of staying scientific

As the academic clinical field embarks on community impact, the temptation will be great to reach beyond the state of scientific evidence to extrapolate, conjecture, and let one's political values override science. The credibility of the field is at stake, however, and adherence to empirical rigor remains as important as ever. Unfortunately, experiments, the gold standard of scientific evidence, cannot be conducted very readily when the community is the unit of analysis. Random assignment of communities is unwieldy and expensive, although they should be promoted as much as possible. The fact of non-random implementation and small-*n* studies of huge community programs may scare off some clinical scientists altogether, on the grounds that rigorous analysis is not possible.

Although experiments are desirable, the field must also rely on other methods. Natural experiments, such as when funding for a community program is distributed randomly across communities or is cut off suddenly, present opportunities for analysis. Fortunately, or unfortunately, much public policy remains random.

Key points

- Clinical scientists are being asked to bring their theories and therapies to bear on achieving population change in the prevalence of psychiatric disorders.
- At-scale implementation of evidence-based therapies is being attempted but is unlikely to achieve population change.
- Community social capital development and related resourcing of communities holds promise but has not been rigorously evaluated.
- Addressing community factors, based in clinical and developmental science, through public policy and community development, also holds promise.
- Adherence to rigorous empirical analysis, with the community as the unit of measurement, must be followed to maintain credibility of the field.

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Figure 1.

A two-dimensional representation of community interventions