

Are we going to close social gaps in HIV? Likely effects of behavioral HIV-prevention interventions on health disparities

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Although experimental behavioral interventions to prevent HIV are generally designed to correct undesirable epidemiological trends, it is presently unknown whether the resulting body of behavioral interventions is adequate to correct the social disparities in HIV-prevalence and incidence present in the United States. Two large, diverse-population meta-analytic databases were reanalyzed to estimate potential perpetuation and change in demographic and behavioral gaps as a result of introducing the available behavioral interventions advocating condom use. This review suggested that, if uniformly applied across populations, the analyzed set of experimental (i.e. under testing) interventions is well poised to correct the higher prevalence and incidence among males (vs. females) and African-Americans and Latinos (vs. other groups), but ill poised to correct the higher prevalence and incidence among younger (vs. older) people, as well as men who have sex with men, injection-drug users, and multiple partner heterosexuals (vs. other behavioral groups). Importantly, when the characteristics of the interventions most efficacious for each population were included in the analyses of behavior change, results replicated with three exceptions. Specifically, after accounting for interactions of intervention and facilitator features with characteristics of the recipient population (e.g. gender), there was no behavior change bias for men who have sex with men, younger individuals changed their behavior more than older individuals, and African-Americans changed their behavior less than other groups.

Keywords: health disparities; HIV; communication; persuasion

Introduction

It is both common and justified to link HIV disparities to unequal structural forces, including differential access to HIV-prevention resources, uneven decision-making power, and unfair distribution of poverty (Sumartojo, 2000; Sweat & Denison, 1995). The epidemic has clearly distributed HIV in a way that further burdens already oppressed groups such as ethnic and sexual-orientation minorities. It is perhaps less common to scrutinize health interventions as potential contributors to these health disparities. Of course, early in an epidemic, intervention practices are unlikely to say much about disparities in HIV prevalence and incidence, but how

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about after 25 years of HIV-prevention-intervention research? Is a lack of analysis of the impact of our HIV-prevention interventions justified? On the basis of currently available data, are our experimental interventions likely to correct current social disparities in HIV? Are experimental interventions more or less acceptable and efficacious for the groups that bear the burden of the disease than other groups?

Consider US social disparities in HIV prevalence. With respect to gender, 74% of HIV cases are male (Centers for Disease Control, 2007) even when the general population has comparable representations of men and women (United States Census, 2007). With respect to race/ethnicity, 60% of the HIV cases are African-Americans (Centers for Disease Control, 2007) even though only 13% of the general population is African-American (United States Census, 2007). Likewise, 16% of the HIV cases are Latino (Centers for Disease Control, 2007) even though only 15% of the general population is Latino (United States Census, 2007). With respect to age, 50% of the HIV cases are between 16 and 34 years old (Centers for Disease Control, 2007) even though only 40% of the general population falls in this age range (United States Census, 2007). With respect to behavior, 53% of people living with HIV contracted the virus through male-to-male sexual contact, 32% through high-risk heterosexual contact, 12% through injection-drug use, and 3% through either male-to-male sexual contact or injection-drug use (Centers for Disease Control, 2007). And here again, these sex- and drug-related behaviors are sometimes higher in people living with HIV than the US population as a whole (Brady et al., 2008; National Opinion Research Center, 1998). In this context, are the experimentally tested HIV-prevention interventions more likely to reach and reduce the behavioral risk of men than women, Black/African-Americans and Latinos than whites, or men who have sex with men, injection-drug users (IDUs), and multiple-partner heterosexuals (MPHs) than other groups?

A number of meta-analyses have summarized the results of interventions as they are being tested in the trials designed to establish intervention efficacy, hereafter termed "experimental interventions" (Noar, 2008). Unfortunately, most of these meta-analyses have considered a limited number of studies and/or selected a single population, thus failing to systematically analyze efficacy associations with gender, race/ethnicity, age, or behavioral risk let alone compare these results with present social disparities. Other meta-analyses have failed to consider demographic variables altogether. Understanding health disparities necessitates estimating not just if an intervention has a significant impact in a specific group, but also whether it has *differential* impact across groups. Fortunately, however, two comprehensive meta-analytic databases with demographic and behavioral-risk information are available, which we reanalyzed to estimate associations of demographic and behavioral-risk information with intervention completion and behavior change in the US. The goal was to inspect differences in intervention completion and efficacy and then judge whether the analyzed intervention set is likely to close social gaps in HIV prevalence.

The meta-analysis that provided data on completion of HIV-prevention interventions, which shared 33 primary studies with Noguchi et al.'s meta-analysis, included 28,429 participants from studies that reported either or both enrollment or attrition rates (Noguchi, Durantini, Albarracín, & Glasman, 2007). The interventions included in this meta-analysis required the presence of a condom-use component. As condom use is recommended in all HIV-prevention interventions, this set also represents the broad HIV-prevention-intervention domain even though it sometimes targets other preventive behaviors such as cleaning injection equipment.

The current analyses are based on the included US studies, which comprised 14,889 participants from 71 statistically-independent conditions. To calculate completion rates, we divided the number of participants who completed the study by the number who commenced the study, obtaining odds of completion. Odds of 1 represent 50/50 chances of completing versus not returning to the last session of the program and corresponding follow-up in control groups. Enrollment rates were also calculated but, due to lack of frequent report of targeted and enrolled participants, we had insufficient data for meaningful analyses. These effects were calculated in both intervention groups and non-intervention control groups (e.g. wait-list controls).

The other meta-analysis, which provided data on condom-use change, included 138,850 participants enrolled in studies of the outcomes of condom-use-promoting interventions (Albarracín et al., 2005). Again, condom use is recommended even when other behaviors such as cleaning injection equipment are advocated, providing the broadest possible HIV-prevention-intervention set. The current analyses are based on the US studies, which included 100,759 participants from 353 statistically-independent conditions. In this meta-analysis, a *g* statistic representing condom-use change over time was calculated by subtracting the mean level of condom use at the baseline from the mean level of condom use at the most immediate posttest for which condom use was measured, and dividing the result by the standard deviation of the pretest. For both meta-analyses, we were able to regress effect sizes on each of the following demographic and behavioral variables: (a) proportion of males in the group, (b) proportion of European-Americans in the group, (c) proportion of Latinos in the group, (d) proportion of Asian-Americans or Pacific Islanders in the group, (e) median or mean age, (f) inclusion of MSM (men who have sex with men), (g) inclusion of MPHs, and (h) inclusion of IDUs. We computed analyses in both the intervention groups, as well as control groups in which no HIV-prevention intervention was delivered (e.g. wait-list control).¹

Method

Meta-analysis of completion

Review and inclusion criteria

To capture interventions designed to increase condom use, we combined the key words *AIDS* and *HIV* with *intervention*, *behavior*, *knowledge*, *education*, *prevention*, *condoms*, *communication*, *attitudes*, and *message*. The search comprised the period between 1988 and 2006. We electronically searched Pubmed, PsycINFO, Medline, Cambridge, ERIC, and Dissertations Database, as well as relevant conference databases. We also manually searched all available issues appearing during or after 1980 of the journals *AIDS*, *AIDS Education and Prevention*, *AIDS and Behavior*, *American Journal of Public Health*, *Basic and Applied Social Psychology*, *Health Psychology*, *Journal of the American Medical Association*, *Journal of Applied Social Psychology*, *Journal of Consulting and Clinical Psychology*, *Journal of Personality and Social Psychology*. Finally, we checked prior meta-analyses, examined cross-references in the obtained reports, and requested reports from researchers in this area.

The selection criteria were strict because, due to objectives not relevant to this article, we needed to identify studies that had baseline measures of knowledge, motivation, or condom use. In addition, given the focus on attrition, we required

precise reports of the sample sizes at the beginning and end of the intervention. Various other criteria were relevant as well.

- (1) Given our hypotheses, we synthesized studies that provided information on the levels of baseline knowledge, motivation, or condom use of the audience upon entering the intervention. These measures could be retrieved only when researchers reported the baseline levels of one of these measures for all commencers rather than only posttest completers.
- (2) To be eligible, studies had to include at least one standardized behavioral intervention designed to increase condom use among recipients. In addition, reports often included comparison and control conditions. Groups that researchers treated as “comparison” conditions but that participated in an intervention were considered treatment groups. We considered control groups only those not exposed to any kind of HIV-related intervention at the time of the study (e.g. waiting-list groups; education programs on other health topics; for similar criteria in a different meta-analysis, see Albarracín et al., 2003, 2005). These controls provide an estimate of what occurs in the absence of systematic exposure to an HIV-prevention intervention.
- (3) Studies were eligible only when they provided statistics to calculate either or both acceptance or completion. In practice, however, all studies reporting acceptance also reported completion. Completion rates required the initial number of participants and the number of completers of the intervention or the immediate post-test. Acceptance rates required initial number of participants and the numbers of people targeted for recruitment, but are not considered in the current analyses.
- (4) The need of measures of completion excluded reports containing pre- and post- test information from different audiences.
- (5) We only included studies conducted in the United States.

Coding of moderators

Independent raters coded relevant characteristics of the reports and methods used in the study. After the initial training, the overall inter-coder agreement was 95%. Occasional disagreements were resolved by discussion and further examination of the studies. For all variables, reliabilities were superior to r and $\kappa = 0.70$. Of interest to the current article, we recorded the presence or absence of the different intervention components. (a) HIV information and/or information about condom use, (b) attitudinal arguments, such as statements about the positive implications of using condoms for the health of the partners and for the romantic relationship, (c) normative arguments asserting support for condom use on the part of friends, family members, or partners, (d) threat arguments, such as discussions about the recipient's personal risk of contracting HIV or other sexually transmitted infections (STIs), (e) behavioral strategies such as training negotiation skills (e.g. role-playing condom-use negotiation) and condom-use behavioral skills (e.g. opening wrapper without tearing it, unrolling condom in proper direction), (f) HIV counseling and testing, and (g) provision of condoms. We also recorded demographics of the participants as well as specific characteristics and behaviors that are associated with HIV-infection risk. These characteristics were recorded for the commencers and completers of the

intervention or of the immediate follow-up. Specifically, we retrieved: (a) sample size; (b) percentage of females in each group; (c) mean or median age; and (d) percentage of participants of European, African, Latin-American, Asian, and Native-North-American descent. Further, we registered the inclusion of various behaviorally-at-risk groups, including men who have sex with men, intravenous-drug users, and multiple-partner heterosexuals. We classified each treatment group according to whether (a) the setting of the recruitment and intervention comprised clinical or community settings (community-based organizations, streets, bars, work settings, or schools), (b) whether the intervention was group based, (c) whether the source was a professionally-trained expert, (d) whether the intervention included gender-matched sources, and (e) whether the intervention included ethnically matched sources. Further, we recorded (a) the planned number of sessions, and (b) whether participants were paid for their participation.

Effect size calculation and analysis

For each paper, we retrieved each available intervention and control condition. In addition, whenever the report distinguished samples, we attempted to treat each sample separately. In three papers, however, interventions were collapsed into a single group. Hence, all statistics for these reports represent an average. In no case did we merge interventions and control groups.

As measures of completion, we recorded the number of commencers and completers of the intervention. When available, these data allowed us to compute the proportion of completion in a sample. These indices were then converted into odds (proportion of declinations). Odds of 1 correspond to equal probability of completion. Odds greater than 1 correspond to more likely completion than drop out. Odds smaller than 1 correspond to more likely drop out than completion.

We first calculated weighted-mean odds as estimates of the degree of completion and performed corrections for sample-size bias. As described before, proportions of completion were converted into odds, and then the odds were log transformed (see Haddock, Rindskopf, & Shadish, 1998). We used Hedges and Olkin's (1985) procedures to correct the effects for sample-size bias,² as well as to calculate weighted-mean effect sizes, confidence intervals, and homogeneity statistics. Calculations of the between-subject variance followed procedures developed by Hedges and Olkin (1985).

Computations of effect sizes were performed using fixed-effects procedures. (Hedges & Olkin, 1985; Hedges & Vevea, 1998; Rosenthal, 1995; Wang & Bushman, 1999; but see Hunter & Schmidt, 2000; Raudenbush, 1994). The weights for fixed-effects followed Hedges and Olkin's computational formulas, and were used because we assume that the variance in effect sizes can be explained by study moderators. The moderator analyses included regressions using fixed-effects weights, with the error of the beta weights being corrected based on Hedges and Olkin's (1985) recommendations.

Meta-analysis of behavior change

Review and inclusion criteria

We conducted a review of reports that were available by September of 2003. First, we conducted a computerized search of Medline, PsycINFO, ERIC, Social Science

Citation Index, and Dissertation Abstracts International using a number of keywords, including *HIV (AIDS) messages*, *HIV (AIDS) communications*, *HIV (AIDS) interventions*, *HIV (AIDS) prevention*, and *health education and HIV (AIDS)*. Second, we manually searched all available issues appearing during or after 1985 of the journals, *AIDS*, *AIDS Education and Prevention*, *AIDS Research*, *American Behavioral Scientist*, *American Journal of Community Psychology*, *American Journal of Nursing*, *American Journal of Public Health*, *Basic and Applied Social Psychology*, *Communication Research*, *Communications*, *Health Communication*, *Health Education Quarterly*, *Health Education Research*, *Health Psychology*, *Journal of the American Medical Association*, *Journal of Applied Communication Research*, *Journal of Applied Social Psychology*, *Journal of Consulting and Clinical Psychology*, *Journal of Personality and Social Psychology*, *Journal of Sex Research*, *Medical Anthropology*, *Morbidity and Mortality Weekly Report*, *Qualitative Health Research*, and *Social Science and Medicine*. We also checked cross-references in the obtained reports, sent requests for information to researchers funded by National Institutes of Health (NIH), and contacted selected experts and agencies who could provide relevant materials.

We used several eligibility criteria to gather an optimal sample of studies that could serve our objectives well, as explained below.

- (1) Studies were included if they described the outcomes of a behavioral intervention to promote the use of condoms. We excluded interventions to promote abstinence from sex, except when they also included a condom-use component. The interventions sometimes included other targets, such as drug-related behaviors.
- (2) The studies we included concerned outcomes of different types of interventions. Therefore, we included simple communications, as well as interventions in which recipients engaged in behaviors as part of the intervention (i.e. role-playing, practicing condom-use related skills, and HIV counseling and testing).
- (3) We only included studies that provided information to calculate the effect of interventions over time and excluded reports without a pretest. Most of the reports obtained pre and posttest measures on the same sample, but others used independent samples at each time (for an explanation of the advantages of the use of independent samples for longitudinal studies, see Cook & Campbell, 1979).³
- (4) We only included studies conducted in the United States.

Coding of moderators

Two independent raters coded characteristics relevant to the report and the methods used in the studies. Inter-coder agreement for all categories included in the coding sheet was 85%, and inter-coder-reliability coefficients. Disagreements were resolved by discussion and further examination of the studies.

We recorded the type of intervention and strategy used in each case. Passive strategies included (a) factual information (i.e. mechanisms of HIV, HIV-transmission, HIV-prevention), (b) attitudinal arguments, such as discussions of the positive implications of using condoms for the health of the partners and for the

romantic relationship, (c) normative arguments about support of condom use provided by friends, family members, or partners, (d) threat-inducing arguments, such as discussions about the recipients' personal risk of contracting HIV or other STIs. We also recorded the use of (a) behavioral-skill training (e.g. condom-use skills such as practice with unwrapping and applying condoms; interpersonal skills such as role playing of interpersonal conflict over condom use and initiation of discussions about protection; self-management skills such as practice in decision-making while intoxicated, avoidance of risky situations), (b) HIV counseling and testing involved the administration of a seropositivity test as well as the counseling in place, and (c) condom provision.

We also recorded characteristics of the participants, including demographics of the target group as well as specific characteristics and behaviors of the target group that are associated with HIV-infection risk. To describe the target population, we retrieved the: (a) percentage of males in each group; (b) mean age; (c) percentage of European Americans, African-Americans, Latino American, Asian American, and Native Americans; (d) inclusion of men who have sex with men, multiple-partner heterosexuals, and intravenous-drug users. Moreover, we classified each intervention group according to (a) the setting of the intervention (i.e. community vs. other). We also recorded (a) whether exposure to the communication was individual or in groups, (b) whether the facilitator was a professional expert, (c) whether the facilitator was gender-matched to the participants, (d) the duration of the communication in hours, and (e) whether participants were paid in exchange for participation.

Retrieval of effect sizes and analysis

Two raters calculated effect sizes independently. Disagreements were checked with a third researcher and resolved by discussion. Raters were instructed to calculate effect sizes representing change from the pretest to the most immediate posttest. When there was more than one measure of behavior, we first calculated effect sizes for each one and then obtained the average, which was used as the effect size for that particular variable.

To represent change from pretest to posttest measures, we used Becker's (1988) g , which is calculated by subtracting the mean at the posttest from the mean at the pretest and dividing the difference by the standard deviation of the pretest measure. This measure controls for the inflation in the standard deviation following treatment (for an excellent analysis of the problem, see Carlson & Schmidt, 1999). Effect sizes were also derived from exact reports of t -tests, F -ratios, proportions, p -values, and confidence intervals. To derive effect sizes for within-subject studies, one needs the correlation between posttest and pretest measures to calculate effect sizes. Because some reports did not offer this information, we adopted procedures recommended by Becker (1988) as well as by Dunlap, Cortina, Vaslow, and Burke (1996). We explain these procedures when they become relevant.

We also estimated effect sizes when a report contained inexactly described p -values – such as when the authors indicated that a given finding was not significant at 0.05 – using the appropriate within- or between-subjects procedures. Thus, a reported nonsignificant finding was estimated to have a probability of 0.99, whereas a significant finding was estimated to have a probability at the level of the cutoff value used in the study (e.g. 0.05 or 0.01). However, because the use of such reports

may lead to incorrect estimations, we conducted separate analyses on the set of exactly-reported effect sizes, as well as on all the effect sizes, including the ones estimated on the basis of inexactly-reported p values. Because both sets of analyses yielded similar results, we only report the results that included all effect sizes.

We calculated effect sizes representing change in attitudes, norms, control perceptions, intentions, behavioral skills, knowledge, perceived severity, and perceived susceptibility, although only condom-use behavior was considered in this article. Condom-use measures included assessments on subjective frequency scales, as well as reports of the percentage and number of times participants use condoms over a period of time. For example, the Community Demonstration Projects Research Group (Centers for Disease Control, 1993) asked participants "When you have vaginal sex with your main partner, how often do you use a condom?" (p. 11), and participants provided their response on a scale from 1 (*every time*) to 5 (*never*). To obtain a more precise report of condom use, Ploem and Byers (1997) asked participants to report the frequency of sexual intercourse over the previous 4 weeks, as well as the number of occasions of sexual intercourse for which condoms were used. The researchers then derived the percentage of condom use for each participant. Similarly, Belcher et al. (1998) asked participants to list the first name of all of their sex partners in the previous 90 days. For each name listed, participants were then asked to identify the partner's gender, the partner type (regular, casual, or new), the total frequency of vaginal sex, the frequency of condom-protected vaginal sex, the total frequency of anal sex, and the frequency of condom-protected anal sex. Percentages were again derived based on relative frequencies.

We calculated weighted mean effect sizes to examine change over time in intervention and control groups, and performed corrections for sample-size bias to estimate d . We used Hedges and Olkin's (1985) procedures to correct the effects for sample-size bias,⁴ calculate weighted mean effect sizes, d , confidence intervals, and homogeneity statistics, Q , which test the hypothesis that the observed variance in effect sizes is no greater than that expected by sampling error alone. Calculations of the between-subject variance followed procedures developed by Hedges and Olkin (1985). For within-subjects designs, we calculated the variance of effect sizes using Morris' (2000) procedures. Specifically, we performed calculations for the variance of within-subject effect sizes using three alternate correlations between pre and posttest measures (see also, Albarracín et al., 2003). Thus, we assumed $r = 0$ and $r = 0.99$ as the most extreme values, and also imputed correlations from Project RESPECT (see Kamb et al., 1998), which provided moderate values of this association. Because results were similar regardless of the correlation we used, we present only the ones with the imputed correlations (see also Albarracín et al., 2003).

Computations of effect sizes were performed using fixed-effects procedures. In the first case, one assumes a fixed population effect and estimates its sampling variance, which is an inverse function of the sample size of each group. The inverse of the effect size's variance is used to weigh effect sizes before obtaining average values. Thus, effect sizes from studies with larger N s are considered more precise and carry more weight than effect sizes obtained from studies with smaller sample sizes. These procedures are powerful and produce narrow confidence intervals (Rosenthal, 1995; Wang & Bushman, 1999). Presumably, fixed-effects models are reasonable when one assumes that effect sizes vary as a result of a few, identifiable study characteristics, whereas random-effects models are appropriate when variation derives from multiple, unidentifiable sources (Raudenbush, 1994).

Results and discussion

We included 29 research reports that included completion data from a total of 59 independent groups. We also included 146 reports that included behavior change data from a total of 353 independent groups. All references appear in the Appendix. Tables 1 and 2 present descriptive statistics for the meta-analyses of completion and behavior change. The studies had great gender and ethnic diversity and the interventions included various methods to elicit change in behavior.

Overall effects: What would we gain if the same set of interventions were applied across all groups?

We examined if the proportion of males in the samples of intervention recipients is positively correlated with the degree of completion or behavior change obtained. One complication, however, is the potential presence of a similar correlation between the proportion of men in a sample and naturally-occurring outcomes (e.g. changes in behavior). As a result, any association between a sample characteristic, such as gender, and either intervention completion or behavior change in intervention groups must be examined in comparison with the same association in control groups. We thus analyzed the effect sizes reflecting completion and behavior change as a function of each sample characteristics of interest and a dummy code representing experimental vs. control condition (experimental = 1; control = 0). An interaction term was included in the regression equation to determine if the influence of each

Table 1. Meta-analysis of completion.

Variable	Statistic	<i>k</i>
Percent of interventions that included information about HIV	85	52
Percent of interventions that included attitudinal arguments	30	53
Percent of interventions that included threat arguments	49	53
Percent of interventions that included behavioral strategies	46	52
Percent of interventions that included HIV testing	29	53
Percent of interventions that included condom distribution	26	53
Mean percent of male participants	35.08	59
Mean age	23.98	59
Mean percent of European-American participants	21	58
Mean percent of African-American participants	59	53
Mean percent of Latino-American participants	13	55
Mean percent of Asian-American participants	4	55
Mean percent of native American participants	0	55
Percent of groups that included MSM	9	59
Percent of groups that included IDUs	5	59
Percent of groups that included MPHs	17	59
Percent of interventions conducted in a community setting	85	52
Percent of group-based interventions	53	49
Percent of expert delivered interventions	77	53
Percent of interventions that included a gender matched facilitator	100	
Percent of interventions that included an ethnically matched facilitator	NI	NA
Planned number of sessions	3.6	53
Percent of interventions in which participants were paid	22	59

k = number of groups.

Table 2. Meta-analysis of behavior change.

Variable	Statistic	<i>k</i>
Percent of interventions that included information about HIV	46	283
Percent of interventions that included attitudinal arguments	16	283
Percent of interventions that included threat arguments	45	283
Percent of interventions that included behavioral strategies	72	283
Percent of interventions that included HIV testing	20	283
Percent of interventions that included condom distribution	46	131
Mean percent of male participants	43	342
Mean age	26	243
Mean percent of European-American participants	38	317
Mean percent of African-American participants	47	322
Mean percent of Latino-American participants	14	291
Mean percent of Asian-American participants	4	258
Mean percent of native-American participants	0.47	257
Percent of groups that included MSM	13	353
Percent of groups that included IDUs	18	353
Percent of groups that included MPHs	18	353
Percent of interventions conducted in a community setting	21	283
Percent of group-based interventions	77	283
Percent of expert delivered interventions	51	217
Percent of interventions that included a gender matched facilitator	41	171
Percent of interventions that included an ethnically matched facilitator	55	111
Duration of intervention in hours	8	217
Percent of interventions in which participants were paid	37	283

k = number of groups.

sample characteristic on completion or behavior change was influenced by the intervention. A significant interaction indicates that the simple slopes for the effect of the sample characteristic on the effect size significantly differ across treatment and control conditions. All regressions were weighted by the inverse of the variance of the predicted effect sizes, with the standard errors appropriately corrected using fixed-effects models.

A summary of the data from the two target meta-analytic databases appears in Tables 3 and 4, presented for both intervention and control groups. We comment only on significant differences, which are highlighted in the table. These comments are also summarized in Figure 1, which depicts HIV disparities along with the likely effects of the interventions on either closing or perpetuating existing social gaps. The data in the first row of the tables indicate that the proportion of males in the synthesized samples. As a result of the interventions, samples with greater proportions of men had more completion but less behavior change. Overall then, interventions appeared to reduce the natural (control group) noncompliance of male groups and are probably well poised to reduce the male burden in HIV present in the US. As a result, the HIV prevalence among women may increase.

The data for the racial/ethnic composition of the synthesized samples are also interesting. As shown in the second horizontal panel of Tables 3 and 4, greater proportions of Latinos were more positively correlated with completion in interventions than controls. These results suggest that when it comes to completion, the analyzed set of experimental interventions could serve to reduce the HIV burden for Latinos. In contrast, the data for behavior change suggest that this set of

Table 3. Simple regressions of completion on demographic and behavior risk characteristics.

	Interventions	Controls	Difference between correlations
Proportion of men	0.14***	-0.53***	***
<i>Race/ethnicity</i>			
Proportion of participants from European-Americans	0.25***	-0.53***	***
Proportion of participants from African-Americans	-0.54***	-0.13	<i>ns</i>
Proportion of participants from Latinos	0.33***	-0.33**	*
Proportion of participants from Asian/Pacific Islander backgrounds	0.12***	0.54***	<i>ns</i>
Mean or median age	0.23***	0.02	<i>ns</i>
<i>Behavioral risks</i>			
Sample included MSM ^a	-0.03	-0.07	<i>ns</i>
Sample included MPHs ^a	-0.20***	0.43***	***
Sample included IDUs ^a	-0.06*	-	-

Entries are standardized regression coefficients.

MSM, men who have sex with men; MPH, multiple partner heterosexuals; IDU, injection drug users.

^a1 = yes, included, 0 = no, not included.

* $p < 0.05$. ** $p < 0.01$. *** $p < 0.001$.

interventions is well poised to reduce disparities for African-Americans but produces no bias for Latinos. Specifically, the proportion of African-Americans was more positively correlated with behavior change in intervention than control groups.

Two additional sets of analyses are also relevant to our analysis of the prospects of available interventions for reducing social disparities in HIV. First, older age was more positively correlated with behavior change in intervention groups than control groups. Second, samples including MPHs had less completion as a result of interventions than samples not including this population. Third, the inclusions of MSM and IDUs were more negatively correlated with behavior change in intervention than control groups. The implication from these data is that the current intervention set is better suited for older than younger samples, and better suited for groups other than MSM and IDUs. As young people, MSM and IDUs continue to be disproportionately affected by the epidemic, these data suggests that this set of interventions may be ill-armed to close the age and behavioral risk gaps in the US.

Analyses after the group specific strategies are considered: What would we gain after applying the most efficacious strategies for each recipient population?

Of course one could argue that a selective application of the more efficacious interventions for a particular group might be an adequate way of resolving disparities. For example, expert facilitators have been shown to stimulate more change than peers and this difference is even stronger among African-Americans. There are also important differences in how the groups of interest in this article respond to the use of different information contents. Figure 2 shows decision trees constructed based on the data from the second meta-analysis (Albarracín et al., 2005) and describes various important interactions identified previously. Moreover,

Table 4. Simple regressions of behavior change on demographic and behavior risk characteristics.

	Correlation in intervention groups	Correlations in control groups	Difference between correlations	Are described association in intervention groups and difference with the association in control groups retained after controlling for group specific strategies?
Proportion of men	0.06***	0.17*	ns	The correlation in intervention groups becomes nonsignificant, but is still not significantly different from the control correlation
<i>Race/ethnicity</i> Proportion of participants from European-Americans	-0.32***	0.22**	***	The correlation in the intervention groups becomes nonsignificant but is still more negative than the control correlation
Proportion of participants from African-Americans	0.30***	0.16*	***	The correlation in the intervention groups becomes significantly negative and significantly more negative than the control association
Proportion of participants from Latinos	-0.02	-0.18*	ns	The correlation in the intervention groups remains nonsignificant and nonsignificantly different from the control correlation
Proportion of participants from Asian/Pacific Islander backgrounds	0.02	-0.41***	***	The correlation in the intervention groups remains nonsignificant and significantly more positive than the control correlation
Mean or median age	0.35***	0.25**	**	The correlation in the intervention groups becomes nonsignificantly negative and significantly more negative than the control correlation
<i>Behavioral risks</i> Sample included MSM ^a	-0.27***	0	***	The correlation in the intervention groups becomes nonsignificant and not significantly different from the control correlation
Sample included MPHs ^a	0.09***	0.12	ns	The correlation in the intervention groups becomes nonsignificant but is still not significantly different from the control correlation
Sample included IDUs ^a	-0.17***	0.28*	***	The correlation in the intervention groups becomes nonsignificant but is still significantly more negative than the control correlation

Entries are standardized regression coefficients.

MSM, men who have sex with men; MPH, multiple partner heterosexuals; IDU, injection drug users.

^a1 = yes, included, 0 = no, not included.

* $p < 0.05$. ** $p < 0.01$. *** $p < 0.001$.

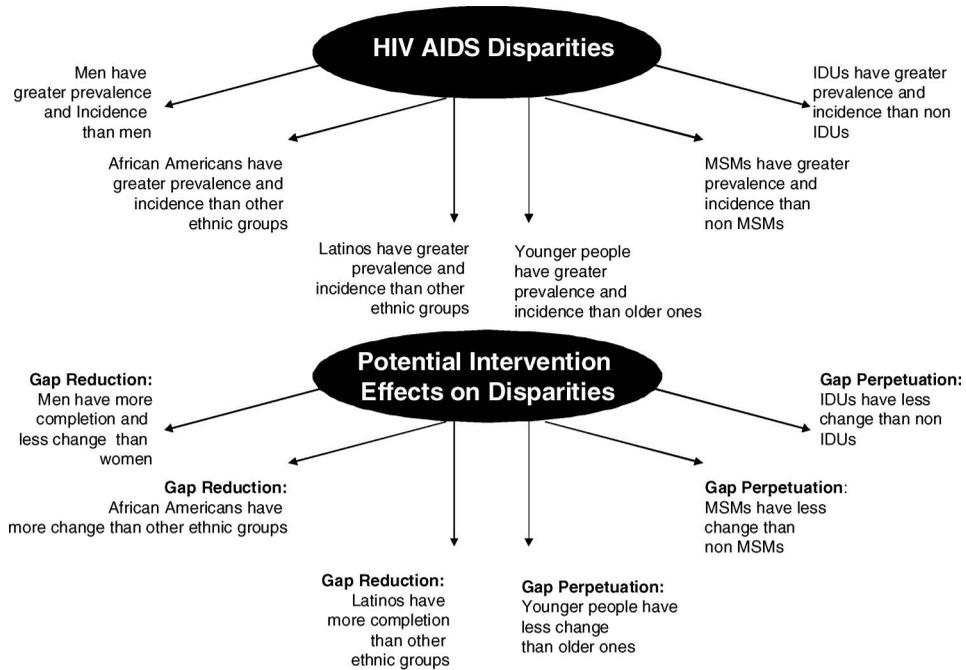


Figure 1. Summary of disparities across all interventions.

it has been shown that women change more in response to female experts, African-Americans change more in response to African-American experts (Durantini, Albarracín, Earl, & Mitchell, 2006), Latinos change more in response to Latino peers (Albarracín, Albarracín, & Durantini, 2008), MSM change more in response to all experts, and IDUs and MPHs respond better to experts accompanied by members of the recipients risk group. If one took into account these interactions, any remaining difference in the effect of a sample characteristic would imply that even after using the most appropriate known interventions biases in the effect of that characteristic remain. We were thus interested in performing these analyses.

The completion data were not sufficient to answer the question of potential disparities even after using the most promising intervention types for specific groups. However, the behavior change dataset is large and thus enabled such analyses. Specifically, we repeated the analyses described in Table 4 for the treatment groups only and after incorporating each sample characteristic along with the use of an expert or a peer facilitator, the similarity of the facilitator and the recipients in terms of ethnicity, gender, and risk group, the use of fear-inducing arguments, the use of normative arguments, the use of attitudinal arguments, the provision of information, the use of arguments to increase self-efficacy, the provision of condoms, the inclusion of behavior skills training, the inclusion of an HIV test, the duration of the intervention, and the use of a group or an individual delivery format. In addition to these moderators, each analysis included all interactions between the sample characteristic of interest and each of the other moderators. The regression weight for each sample characteristic (e.g. proportion of men in the sample) was then compared with the same weight in the control condition using confidence intervals constructed with the corrected standard errors. These supplementary analyses indicated that

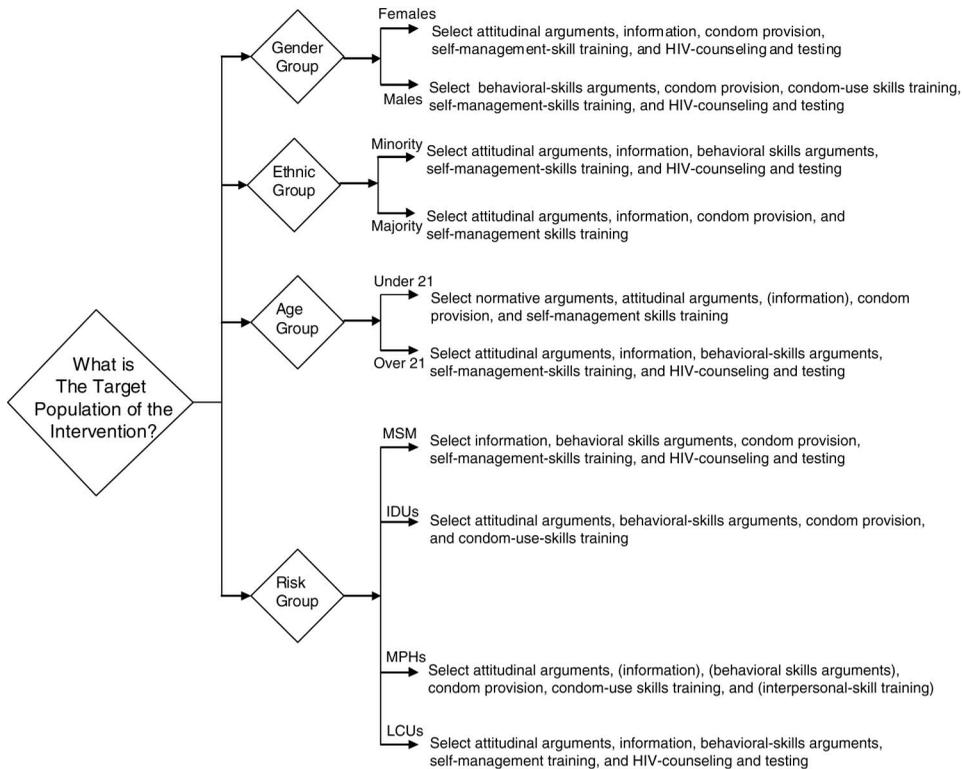


Figure 2. Summary of strategies that are effective for different groups (adapted from Albarracín et al., 2005).

once the best possible combinations of recipient and strategies were taken into account the aforementioned findings replicated with three exceptions. First, the behavior-change bias for MSM disappeared. In addition, African-Americans changed their behavior less than other groups and younger recipients changed their behavior more than older ones. These conclusions are summarized in Figure 3.

Final note

Naturally, attempting to answer the question that guided this paper is not simple. To begin, the interventions examined here had to include an intervention delivered to groups and individuals and thus excluded interventions in which only a structural modification was tested. As structural interventions are proposed to be the answer to many structural factors related to the problem of health disparities, future research should conduct similar analyses of those interventions. Moreover, we offer some tentative answers, but recognize that more precise data might be collected by the public health system with good tracking of implementation of interventions in the US. This tracking must include an evaluation of behavior change as well as enrollment and attrition, all factors that are central to reaching a community and reducing the burden of a disease. Other increases in precision would be facilitated as more data accrue and permit similar analyses for all possible ways of crossing ethnicity, gender, and risk behavior, as well as for the possible factors underlying

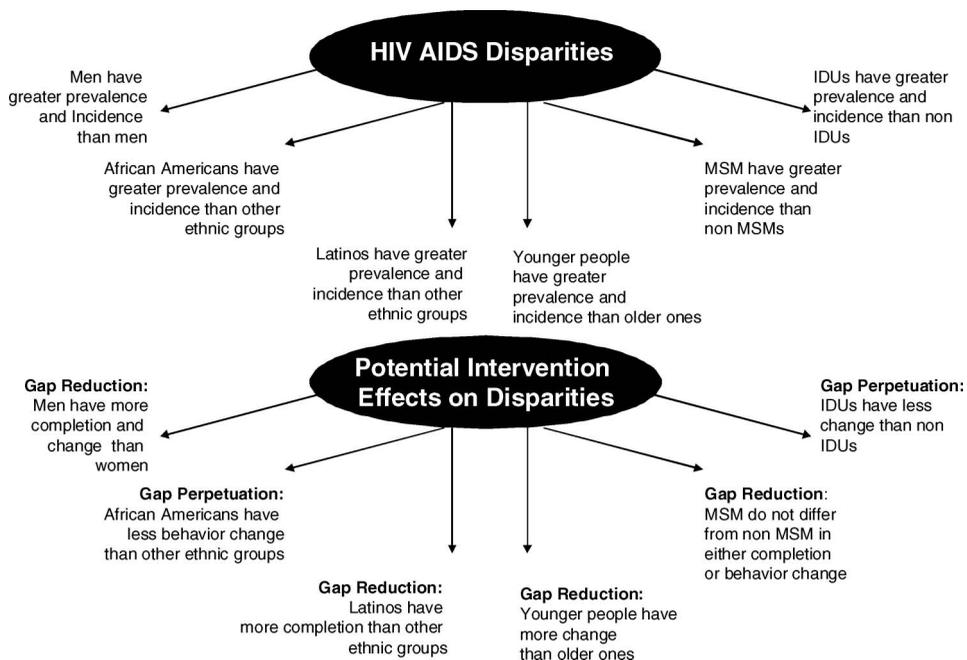


Figure 3. Summary of disparities after controlling for interactions between group characteristics and intervention/facilitator features (for behavior change data only).

these patterns (see Albarracín et al., 2003, 2005; Durantini & Albarracín, 2009; Durantini et al., 2006; Noguchi et al., 2007). We offer the present data as an inspiration and opportunity for the community of researchers and practitioners to engage in a critical debate: Should our interventions correct social disparities in HIV and what is the likely progress in the years to come?

Notes

1. Note that more detailed analyses of interactions among these behavioral and demographic variables would be ideal but is precluded by the lack of sufficient data in the areas of enrollment and completion.
2. When the N at the pretest differed from the N at the posttest, the smaller N was used.
3. Five hundred seventy four papers were excluded. Among those, 18.8% were theoretical or review papers, 16.8% were surveys, 8.7% were qualitative, 15.7% were reported interventions that did not target condom use, 12.5% had data on condom use interventions without a pretest, 11.9% reported otherwise usable interventions but the statistics could not be used to derive the effect sizes we needed, 12.0% reported no standardized intervention, 1.4% were not HIV related at all, and 2.1% had no outcome variable that we were interested in synthesizing.
4. When the N at the pretest differed from the N at the posttest, the smaller N was used.

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Appendix

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