



RESPONSE TO COMMENT ON KAHN AND DAVIDSON

## The Reality of Type 2 Diabetes Prevention. Diabetes Care 2014;37:943–949

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We appreciate the opportunity to respond to Gregg et al. (1) regarding our commentary on community-based lifestyle modification programs to prevent diabetes (2). Gregg et al. believe that we have a disagreement over the interpretation of the extant data and that in our analysis we ignored other important findings of the landmark trials.

Gregg et al. (1) cite the 2012 systematic review by Ali et al. (3) (which we discussed in our article) as evidence that in community programs “an average 4% weight loss was observed” (1), which Gregg et al. believe is “comparable” to the major prevention trials. What Gregg et al. (1) ignore is the fact that the duration of the intervention and follow-up in 24 of the 28 studies in the Ali article was *less than 1 year*—most were 3–9 months; only 4 had a duration of 1 year. As we showed (2), which was also confirmed in a more recent meta-analysis of lifestyle interventions (4), community-based studies lasting 1 year were able to achieve a weight loss compared with control subjects that is less than half that achieved in the Diabetes Prevention Program (DPP) and ~30% less than in the Finnish Diabetes Prevention Study (DPS). And that is in the first year, when weight lost is at its maximum.

This shortfall in weight reduction is critically important because the inability to come close to replicating the landmark

trials and the fact that weight regain virtually always occurs means that the degree of diabetes prevention likely to be achieved in a community setting will be miniscule at best.

Gregg et al. (1) go on to state that physical activity and diet quality are “independently” important and they cite a Centers for Disease Control and Prevention Web site (lacking peer-review and no reference list at this writing) and a 2008 review article that provides only a cursory assessment of community-based prevention. While it is true that the Chinese study (5) achieved great success without weight loss, the intervention was so poorly described that no other study we know of has even mentioned the Chinese approach as the template for a community program. In addition, the design, conduct, and analysis of the study are questionable (2,6). To our knowledge, there is no study conducted in Americans or Europeans that has shown that diabetes prevention can be achieved by means other than weight loss.

Gregg et al. also state that the “58% relative reduction in diabetes incidence” (1) in trials is a very high bar to achieve in a community setting and other prevention efforts with weaker effect sizes are widely recommended. However, their characterization of the DPP and DPS is misleading. As we pointed out (2), the 58% reduction was over the 2.8 years of

the DPP follow-up and 3.2 years in the DPS, not for all time or for even 5 years of prevention efforts. Had either the DPP or DPS continued for a longer period, the extent of the reduction would certainly be far less. Thus, community programs do not have “a large margin” (1) to be as effective as other prevention efforts in medicine.

Moreover, and most important, the major randomized controlled trials have not demonstrated any meaningful clinical outcome. After more than a decade of follow-up, both the DPP and DPS have yet to show any impact from lifestyle modification on diabetes-related complications as a result of the approximate 4-year delay in the diagnosis of diabetes (7,8). What then do we expect to happen in a community program that does not achieve close to the weight lost in the randomized controlled trials? Would less delay be valuable?

Finally, Gregg et al. justify the need for community prevention on the “high prevalence of prediabetes” (1), which to them is a high-risk state. But most people with prediabetes will never develop diabetes and the term itself has virtually no evidence base (9,10). The landmark trials were conducted on subjects who were mostly at the cusp of the diagnostic threshold, not the majority of those with prediabetes who meet the current definition but whose dysglycemia is modest.

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All told, therefore, we are not intrinsically against lifestyle modification but continue to believe that it is unacceptable to pour money into programs that have no evidence they can achieve a meaningful clinical outcome in a population that as a whole is not at high risk. In this age of health care budget constraints, there are countless more evidence-based efforts that should take precedent.

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