



December 2, 2003

Comments on the Draft NIH Strategic Plan for Obesity Research

On behalf of The North American Association for the Study of Obesity (NAASO) we wish to thank the National Institutes of Health for this opportunity to provide comments on draft NIH Strategic Plan for Obesity Research. NAASO is the only member-based scientific society dedicated to the study of obesity. Its membership is comprised of 1,600 leading scientists and clinicians in the field. We are committed to research on the causes, treatment and prevention of obesity.

NAASO is pleased to see the effort being made by the NIH to address this important problem. We applaud the determination to work with scientists and to advance the field of obesity research. Nothing could be more important at this time than a full scale, institute wide commitment towards obesity research.

The Strategic Plan has several strengths. First, it is appropriately broad in scope ranging from molecular to translational research. Such a scope is necessary in trying to research such a complex condition like obesity. Second, the trans-NIH nature of the plan is exemplary. It underscores the need for making obesity research a priority across Institutes and centers. While this approach runs the risk of no single Institute championing obesity research, it has the benefit of prompting numerous Institutes and centers to become more active in obesity research. This multidisciplinary approach is critical for successful short- and long-term advances in the field and has the full support of NAASO. Other advances outlined in the draft that NAASO supports include a focus on proteomics and support for young investigators that will be critical to the success of a long-term strategy addressing this issue.

Unfortunately, we also believe the Strategic Plan falls slightly short in four general areas: it seems to lack a dramatic new vision for the field; it does not go far enough to address a systemic, albeit unintentional, bias against obesity research; some important areas of research are not properly emphasized; and, there is an imbalance between treatment and prevention.

The Plan, by its very nature, is broad and general. As such, it provides an outline that is useful to guide future funding priorities but it is lacking in dramatic new directions for the field. Moreover, the document ignores institutional and administrative issues that might hamper systematic implementation of this agenda. Coordination among Institutes is critical if the plan is to avoid each Institute acting independently, resulting in a piecemeal approach. This plan must include practical methods for implementation. NAASO urges the inclusion of set-aside monies for obesity in each Institute, RFAs or other approaches that systematically insure obesity is addressed across the NIH.

Another major hurdle for the Plan is getting the message to study sections. In the past it has not been possible to get much funding for work with diet, activity, and obesity end products in the epidemiological area. For instance, the EDC study sections want CVD or cancer or other chronic endpoints. Study sections must accept obesity as an endpoint. In order for the plan to succeed, the active support of study sections is vital. NIH funding priorities for research in obesity must be communicated and accepted by study sections.

There is not much emphasis on understanding mechanisms regulating energy balance, either on the regulation of food intake or on the regulation of energy expenditure. This is very exciting area of research that is developing rapidly (adipokines, gut hormones and CNS signaling pathways). These systems are incredibly complicated and undoubtedly regulated by genes and gene products in ways that are just now being worked out. Basic mechanisms do not seem to be well covered in either Section A or B, which focus more on treatment.

We note the lack of research into important questions relating to obesity across the entire life-cycle. There should be an emphasis in pediatric, adolescent, adult as well as geriatric medicine. The approach to minimizing the development of obesity (infants and children) should not be expected to be the same as that used to decrease weight loss (adults). The physiological mechanisms occurring during the dynamic as opposed to static phases of obesity are very different. Socioeconomic factors and their impact on obesity were only casually mentioned but this is crucial to our understanding of preventive measures.

The list of large NIH-funded studies addressing various aspects of prevention and treatment of obesity and its complications does not include any sense of what is being done in smaller projects that focus on basic mechanisms of disease. One gets the sense that the NIH is primarily funding large, multi-center clinical trials. Too much is being allocated to these large clinical studies while a larger number of smaller basic studies would do more to advance our understanding of the problem. There should be some discussion of the optimum balance and what strategies should be used to achieve the right balance across the various Institutes. Finally, there is no mention of the role of stress in obesity – this topic could be incorporated under both the lifestyle goal and the biological role sections.

Specific Comments

A. Preventing and Treating Obesity through Behavioral and Environmental Approaches to Modify lifestyle (pp. 16-25).

The third bullet (p. 17, line 13) says, “Determine age, sex, and race/ethnicity specific estimates of physical activity, dietary intake, and body composition in the US population.” This is already being done. The more important issue is that monitoring and surveillance need to be continuous and methodologies improved.

The long-term goals (p. 17, lines 23-39) are heavily focused on genetic aspects. The fourth bullet (lines 34-36) calling for observational studies to identify potentially modifiable behavioral and environmental determinants should not be restricted to children and adolescents. This is a pressing need for adults as well. Worksite interventions will need to rely on an expanded set of targets that promise success in prevention of weight gain and sustained weight loss.

Given the reliance on behavioral models, NAASO would like to see some emphasis on developmental work on models of behavior change. At present, there are a few previously developed models (e.g., Planned Behavior, Health Beliefs) that are used and adapted in these designs. The pressing need is to see if these models are really helpful and to develop additional behavior change models that are particularly well suited to weight-related interventions.

We would like to see bold, major initiatives in this section. For instance, do we need a national birth or family cohort with a major focus on diet, activity, and body composition and all the related factors? There is no natural laboratory that possesses all the elements that must be studied to understand the etiology of obesity and the relative role of the wide range of factors responsible for this. Similarly, are there gains to be made by studying the etiology of obesity across other nations and race-ethnic groups? So many conditions in the US do not vary in meaningful ways, and many studies conducted elsewhere might be highly relevant to the US since the dynamics of the environment, diet, and activity are not so much greater.

This agenda calls for more focus on prevention at the community and population level; however, the centers that focus on obesity are traditionally clinical in focus. Either new "prevention" center initiatives should be developed or existing obesity centers should include prevention components at the community and population level.

B. Preventing and Treating Obesity through Pharmacologic, Surgical or Other Biological approaches (pp. 26-39).

The goal of trying to use clinical trials to learn more about mechanisms of energy balance or variability in treatment response is laudable. However, the finding limits of 500 K per year preclude treating sufficiently large samples to tease out individual differences. Mechanisms, in addition to ancillary studies, are needed to encourage large, multi-center trials which are less focused on evaluating drug A versus drug B and more on identifying the heterogeneity of treatment response.

More needs to be known about widely used medications to treat obesity (e.g, phentermine) even if the FDA does not approve them for obesity indications.

C. Breaking the Link Between Obesity and its Associated Health Conditions (pp 32-39)

This section focuses on research to better understand the relationships between obesity and the multiple health problems associated with it. One approach is to look for ways to predict who will and who will not develop specific conditions, another approach is to predict which treatments will or will not be effective in a given individual. Both of these approaches will involve a search for predictive, novel biomarkers and genetic studies in various and diverse populations. Inflammatory markers are highlighted as an important area, but other types of "markers" are not mentioned. Perhaps this could be expanded and made more specific. The range of associated conditions is very broad ranging from CVD to urinary incontinence to depression. It is unlikely that all are related to inflammation.

The second major thrust for prediction is to evaluate the effects of differences in fat cell characteristics and distribution on risk of complications. This is related to the search for biomarkers as well as metabolic effects. More details on how these avenues should be approached would be helpful.

Another thrust is to study methods in which obesity can be dissociated from the complications (i.e., how to prevent the complications without removing the fat). This may include the development of drugs that block the development of the consequences of obesity. This requires studies to understand the basic pathogenesis and pathophysiology of the various complications, identifying promising targets for drug development and then developing and testing the drugs. We would like to see mention of pharmacogenetics here. There might be some discussion of the relative roles of the federal government and industry in this process.

D. Cross-Cutting Topics (pp. 40-49)

One type of interagency work that needs to be done relates to the food label. As a major potential vehicle for individual level behavior change virtually nothing is known about how it is used, how its use might be incorporated into interventions, etc. The vision for interdisciplinary work seems restricted to the basic science/clinical medicine pairing. Other potentially useful collaborations involve other disciplines such as human factors engineering, economics, anthropology and epidemiologic/clinical medicine alliances.

Current long-term efforts such as CARDIA or ERIC should be enhanced to further obesity research. This document is vague when it comes to evaluation of extant NIH programs and this should be strengthened.

Producing research is not sufficient to produce change. It would be useful if NIH worked with the Centers for Medicare and Medicaid Services to understand what is needed to consider reimbursement/coverage for obesity prevention and treatment by the medical care system.

While we appreciate the interest NIH exhibits in taking the lead in developing research measures for this group of underserved patients (e.g., body composition) it doesn't fully address the urgent need for medical devices among those who need them the most. Although discrimination is mentioned briefly in the document, the lack of basic medical technology (e.g., MRIs, X-ray, stress tests) for those greater than 300-350 pounds is unacceptable. NIH can play an important role by helping to develop cost-effective technologies that meet the medical needs of obese people.

NAASO commends the NIH for developing a strategic plan for obesity research. As the prevalence of obesity and its associated co-morbidities increase, research is critical to better understand, prevent and treat this serious and refractory condition.