

THE OBJECTIVES OF HIGHWAY RESEARCH

Paper by Thomas H. MacDonald, Chief of the U.S. Bureau of Public Roads, Presented Before the Advisory Board on Highway Research, National Research Council, November 8, 1923.

The United States is now carrying on a highway improvement program which, measured in terms of expenditures, approximates a billion dollars annually. As in nearly every thing else of concern to the general public, we have gone in for quantity production. A large yearly mileage of new roadways has been demanded that a place might be found upon which to operate the quantity production of the motor vehicle. So large are the annual programs of new construction in many States that even minor modifications in specifications, or designs, have a very large financial aspect.

In the face of large annual increases in the mileage of improved roads, handicaps exist and have existed continually to the satisfactory and economical use of the motor vehicle because of insufficient mileage of highways improved to even reasonably acceptable standards. Very recently a well known manufacturer stated that a million new vehicles would be placed on the highways in 1924. Even allowing for the retirement of a large number of those now in service, the net increment is such that there is the certainty that much greater service will be required from highways already seriously over-loaded structurally and congested to the point of discouraging the reasonable and desired use of the motor vehicle.

All this has occurred without warning and without precedent. The whole development of highway transport is in the making, bringing with it hundreds of problems, social, economic and engineering. This condition must be recognized, accepted with just weight, and solved. There is much greater reason to doubt if the condition now existing will be correctly measured than to doubt the ability to apply proper remedies when the individual problems are correctly analyzed. In no other development, ancient or modern, affecting in such a major degree the whole structure of our social and economic life, has the engineer been given so commanding an opportunity for leadership and for carrying into effect policies formulated by his profession.

The opportunity is real, will the engineer succeed or fail? If he is making progress, if he is succeeding, if he finally proves beyond all doubt the ability of the engineer to master and thus to lead in a new and major translation in our national life, it will be through research. Research that is as big and broad, as sympathetic and intelligent as the present existing conditions and opportunities for advancement and progress, justify.

A satisfied confidence in present practices and a lack of interest in engaging in and supporting broad highway research is not only a failure in a public duty but it is the worst possible betrayal of the engineering profession itself. But past history is not entirely reassuring. There is a question which for years has been to me a most perplexing one, and I may add it has not been an entirely happy one.

This same question has been active in the minds of a great many, if not most, of the engineering profession and common reference is made to it in engineering literature.

It is this: Considering the study, preparation and experience required to fit one adequately to practice the profession of engineering, the hard work, both mental and physical, usually involved in the practice of the several branches of engineering, the sincerity and persistency of purpose required for any successful accomplishment, and the responsibilities of all kinds imposed, - why has not the engineer been accorded more readily and more consistently leadership and the rewards, not necessarily material, which generally accrue to recognized authority.

And the answer, which is reasonably sufficient to me but which I do not insist that anyone else shall accept, is this, the lack of engineering research. As we review engineering history, this lack and often entire absence of anything approaching adequate research has been until very recent years characteristic of both individuals and institutions.

For example, agricultural experiment stations have been established through Federal and State Legislation and have been supported for many years to promote the development of the science of agriculture. These experiment stations are established in every State but there are only a very few Educational Institutions which have established engineering experiment stations and most of these have had serious trouble to maintain a meager existence. Why research in engineering lines has not been more fully developed is most difficult to understand. Undoubtedly

our great natural resources have lulled us into the belief that it was not necessary to use them with the economy which ought to follow proper research, yet this certainly cannot be the only reason when we consider the tremendous fertility of the lands which has been ours for the taking, and yet we find a widespread and persistent extension of agricultural research in the States where there has been no engineering research developed.

I am fully convinced that engineers have been so busy with the day's work that they have not insisted upon the development of an adequate engineering research until the fact was accomplished, and that this is, to a greater degree than any other reason, responsible for the failure of engineers up to this time to secure that degree of recognition justified. There is now the very hopeful indication that this recognition is coming slowly but surely and undoubtedly the motivating reason behind this changing attitude of the public toward the engineer is due to the greater interest, activity and insistence of the engineers in the establishment and conduct of engineering research.

You will find in this statement no striving for effect, no unfriendly criticism, but rather an expression of the deepest concern that the great needs and great opportunities for research that are presented shall receive full recognition from the engineer. And what is true of the necessities and fruitful potentialities in the field of engineering research is equally true in that of other professions, for example, the economist, who is concerned with the economic and social development of this nation.

Consider the highway transport field. The civil, the mechanical, the automotive and the transportation engineer, the economist, and the chemist, all have presented to them broad opportunities for direct research while, although more indirectly and perhaps somewhat more removed in time there are other professions that may very conceivably find worthy phases for research endeavor.

There are two major objectives of highway research, first, the isolation and definition of each problem, and second, the solution of the problem. These objectives are supplemental and sequential. All too frequently the first is neglected to the undoing of the second. Perhaps I may even be justified in saying that engineers have frequently in the past made grave errors, and, judging the future by the past, will continue to make grave errors in big enterprises or in the solution of big problems in one of the following ways:

- a. Insufficient and inaccurate fundamental data.
- b. Limitations in scope of data secured.
- c. Limitations imposed by legislation or other authority.
- d. The misapplication or maladjustment of data in itself correct.

Examples might be multiplied of failure or lack of obtainable success in engineering projects through an incorrect or inadequate analysis of the whole problem involved. More specifically failures might be cataloged under one of these four reasons. It might be urged that engineers are not responsible for the limitations imposed by legislation or other authority but are they not responsible when, as is

frequently true, they cut and trim estimates and designs below safe practice to meet these limitations? More than this, it matters not what the reasons are or what handicaps exist, the engineer in charge will be held in the court of public opinion for any failures.

There are these two groups of highway research objectives, those having for their purpose the correct analysis of each problem and second, those having for their purpose the finding of the remedy or the solution when the problem shall have been correctly isolated and analyzed. Just at this time, of the two, the first is the more important. Real progress is always slow. Consider the years of research required to isolate the yellow fever germ and in the meantime the lives that were lost from this cause unknown and unchecked. Once the cause was known the remedy has followed swiftly to the everlasting credit of the medical profession. So, too, the opportunity is offered in the highway field to do a big work now in research. Much progress has been made but we have only started. Nor can the research worker stop when he shall have reached both of these objectives for he must then insist upon the practical application of the solutions he finds in the field of highway transport. The most perfect solution is only valuable when applied. After all the real objective of highway research is the most economical, in every sense of the word, and efficient highway transport service possible for the use of the public as a whole. The opportunities are unlimited. There is not a single phase in which there is not the necessity for major research. In the field of finance,

no rational policies of providing the funds have been worked out. We have only fairly started upon the improvements in the design of road surfaces. We need to develop better materials, cement, asphalt, aggregates, particularly sands.