

collision. Of course, the causes which lead to collisions are many and various; but in every instance where a new brake is employed, when an accident occurs, the explanation always reported is that the *brake failed*; and, therefore, it is necessary that it should be tried long enough and under such widely varying conditions, that those who operate it shall acquire sufficient confidence in it to apply their common sense to causes of trouble and accident, and to determine whether it was the failure of the apparatus or the conditions under which the apparatus was used, which brought about the trouble. Therefore, I may say that the reason why there is no more definite data available now is largely disclosed by the fact that we have not thought it worth while to go after it and get it, until we were assured that the device was in a fair way to receive practical application. I may say, as Mr. Parke has explained, that, so far, everything is moving well, and that we hope at some time in the future something more satisfactory may be presented in the way of data.

PRESIDENT SCOTT:—If there is no further discussion the meeting will adjourn.

Meeting adjourned.

DISCUSSION COMMUNICATED AFTER ADJOURNMENT BY W. S. FRANKLIN.

I beg leave to call the attention of the members of the INSTITUTE to a discussion which will perhaps help to convey my meaning in the statement I made regarding the failure of the principle of limits. If an infinitesimal variation of condition can produce a finite variation in results, it is evident that a finite variation of conditions might produce a stupendous variation in results. In *Science*, Vol. XIV., p. 496, September 27, 1901, I pointed out in detail how this principle may enable us to control meteorological phenomena, and I pointed out also just what kind of knowledge we must obtain in order to realize this possibility. In this discussion I mentioned perhaps too indulgently the smoking cannonading of Burgomeister Stiger, which had attracted considerable attention during 1900 and 1901.

COMMUNICATION AFTER ADJOURNMENT BY R. A. PARKE.

Having had no opportunity to examine Mr. Keiley's paper before it was presented at the December meeting, I was not prepared at that time to call attention to one or two features which appear to me to call for a word of caution. Mr. Mailloux referred to the difficulties he had discovered in making any general use of formulæ for train stops, in his discussion of Mr. Keiley's paper; but it may prove advantageous and useful to point out more specifically the reasons why such formulæ are unreliable beyond the very narrow range of conditions from which they appear to have been derived.

It will be noted, first, that, in deriving the formula, it is stated on page 21 that, after the car was brought up to speed, the speed

"was found to fall off at the rate of about 0.16 miles per hour per second." It is, of course, a well-recognized fact that train resistance or car resistance is a function of the speed, and any formula which contains a constant train resistance can be applicable only to conditions which include the speed to which such resistance applies. Also, the distance run during the interval that the brakes were applied depends, in the formula, upon the initial speed at the instant of application and a constant retardation R , which is designated at the "rate of braking, in miles per hour per second, from setting of brake-shoes to stop." This quantity R is, in reality, different for every different speed, and, if determined from stops made from a speed of from 15 to 18 miles per hour, the stopping distance given by the formula would be very much too great for a speed of 35 or 40 miles per hour.

The formula for the total distance (on page 23) contains a constant quantity N ; that is, one independent of the speed. The use of the constant in such a formula necessarily introduces error in the lengths of stops computed for low initial speeds, which increases as the initial speed becomes lower. It will readily be seen that the formula would require that the car should still run a definite distance if the brakes were applied when the speed is practically zero. Obviously, no formula of general application for stopping distance in terms of the speed can include a term containing the zero power of the speed.

These statements are not intended to imply that arbitrary formulæ of this kind are without practical utility. Such formulæ are very useful and even necessary in reducing the length of stops from known speeds to that of a standard speed, not greatly differing from those from which the test stops were made. Where competitive tests of different brakes are to be made, under definite conditions, for stops from a given speed, the actual stops will generally be made from speeds near, but usually a little more or less than that which it is desired to realize, as it is extremely difficult to attain the exact speed desired at the time that the brakes are applied. It is then necessary, for the purpose of comparison, to reduce the actual stops to the corresponding distance for the required speed, and this may be done very satisfactorily, and with but little error, by the use of such formulæ.

Years ago, I found considerable satisfaction in the discovery of a formula of the same general character as that given in Mr. Keiley's paper—except that it included no constant term. It seemed to correspond very satisfactorily with a considerable number of train stops under known conditions, and it was not until the formula was applied to considerably greater speeds that I became very suspicious of it. I then analyzed the matter very carefully and discovered the difficulty of obtaining a formula applicable to a wide range of conditions. Where train resistance and grade influence are omitted, I found a logarithmic expression for train stops, which may be employed for any initial speed,

Train resistance is so insignificant in comparison with that of the brakes, that suitable provision may be satisfactorily made for it in the constant coefficients of the formulæ; but the effect of grade is quite a different matter, and no general expression, short of an infinite series which does not satisfactorily converge, can be found to apply satisfactorily under the various conditions of regular practice. A complicated and cumbersome approximate expression, involving the cube of the initial speed, was derived; but a sufficient number of stops under sufficiently different known conditions was not available to determine satisfactorily the constant coefficients, and the formula would have been too complicated and tedious in its application for any general use, if the constants could have been determined. Therefore, while the formula in Mr. Keiley's paper may serve well for comparison of the performance of brakes at a given speed and under specific conditions, the absence of any suggestion in the paper that its usefulness does not extend to other conditions and speeds has led me to offer these words of warning to those who may be seeking some simple means of finding the stopping distance of cars or trains from various speeds, as such formulæ are not even approximately reliable under conditions materially different from those under which the constant coefficients were derived.

It appears to me that Mr. Keiley displays considerable courage in attempting to determine the commercial value of different brakes in even a single kind of service. The problem is a most intricate and illusive one, and the reduction to a matter of dollars and cents of the various elementary items entering unto the sum total, to be charged against each brake must, in any case, become a very complicated and difficult matter. For instance, the personal element of the operator of the brake is a most important one, and is so uncertain that the most efficient and economical brake may be caused to take almost any rank from the best to the worst.

Wheel sliding, for instance—which I do not find enumerated under the features of controlling importance in the paper—should condemn a brake possessing practically all the other virtues, but subject to that failing. Hand brakes of high efficiency are especially subject to this evil; because, if adapted to the use of men of moderate strength, wheel sliding is almost certain to result under the manipulation of men of greater strength, unless the greatest care is exercised in operation. In a certain series of competitive tests, a hand brake of especial design attained the most satisfactory results of any of a number tested. In these tests, the brake was operated by a picked man, whose discretion enabled him to make the most satisfactory stops without sliding wheels; but the daily pounding of wheels with worn-flat spots under a large proportion of the cars equipped with it upon the road, clearly indicated that, in the actual service of everyday use, the same brake was a most expensive luxury, in the hands of the average operator.

Also, in reference to the matter of accidents, the rating of a brake may be made or ruined by the character of the men employed, or by the price which a company is willing to pay its employees. Moreover, a brake might be in regular and extensive operation for a number of years without any accident, and so the charge against it in the accident column of Mr. Keiley's method, at the end of that time, would be nil; but the next year an accident might occur (and perhaps under conditions or from causes for which the brake ought to be held in no wise responsible, if the operator did not find it necessary to exculpate himself), of such magnitude that the average annual cost of accidents for all the previous years would thereby become considerable. It might easily occur, during a limited period of observation, that an inferior brake would show a clear accident record, while a superior brake, more simple and sure of operation, would be subject to a heavy charge for one or more accidents, which, if the actual facts and true conditions were all known, could not possibly be attributed to fault or characteristic of the brake or of its operation. It would therefore hardly seem that any figures of this kind could be accepted as really significant of the commercial value of any particular brake, unless made up from statistics from a considerable number of roads, operating under the various different conditions of practice, and extending over a long period of years.

DISCUSSION AT PITTSBURG, JANUARY 5, 1903.

PROGRAMME.

Mr. C. F. Scott, Introductory Remarks.

Mr. N. W. Storer abstracted Mr. J. D. Keiley's paper, "Some Brake Tests and Deductions Therefrom."

Mr. R. A. Parke presented his own paper, "Railroad Braking."

Mr. C. Renshaw abstracted the New York Discussion.

Upon motion of Mr. P. H. Thomas, a vote of thanks was unanimously extended to Mr. Parke for his very able and instructive paper.

Mr. Parke took exception to some of the general conclusions drawn by Mr. Keiley in his paper and stated that he had written a letter to the INSTITUTE setting forth at some length his views on the subject of this paper.

Mr. F. C. Newell stated that all of our data in regard to braking had been obtained from tests made under steam railroading conditions but that electric traction had introduced many new