Discussion on "Problems of Heavy Electric Traction".

L. B. Stillwell: Probably no subject among the many which members of the Institute are called upon to consider is of greater practical interest at the present moment than that which is discussed in some of its phases in the interesting paper presented by Messrs. Lyford and Smith. Many points of view are possible, and the interdependence of questions involved in the selection of electric equipment to replace steam equipment in heavy electric traction service is such as to call for the exercise of great care and well-balanced judgment in deciding the important questions which require decision.

It is especially important that at a time when engineers whose experience in the field of traction is chiefly or wholly in that particular department which deals with tramway service, are for the first time called upon to advise in the broader field of heavy traction, they should at every step verify their theory by practical demonstration in order to make sure that nothing essential to a correct solution of the new problems is overlooked. The speaker feels, therefore, that the paper which Messrs. Lyford and Smith have presented is one of considerable practical value. He will not undertake to discuss it in detail, but has selected from the many interesting points discussed or touched upon in the paper a few which seem especially to suggest comment.

He desires to state at the outstart that the data set forth in Figs. 9 and 10 of the paper do not represent in respect to performance of the rheostatic control during acceleration the actual performance of the equipment used in the Subway. It is to be noted by inspection of the curves in Figs. 9 and 10 that the increments of current which follow the cutting out of successive rheostatic steps are very far from equal in amount, while not less than one second is lost in passing from series to parallel connection. Ideal performance would imply a uniform rate of increase of the current input from starting to the instant when the last resistance is cut out. Obviously, the curves shown are far from realizing this condition, and apparently they were constructed from runs made before the rheostats were properly adjusted. It is possible also that the irregularity of input increments is exaggerated by the fact that the measurements in these particular tests were made by simultaneous readings of non-recording ammeters and voltmeters. In the majority of tests which have been carried out by the engineers of the Interborough Company they were able to use, thanks to the courtesy of the General Electric Company, a most satisfactory outfit of automatic recording instruments which recorded simultaneously and continuously the current and pressure input; they also automatically constructed the time-velocity and time-distance curves; but these curves are not so satisfactory owing to the fact that their ordinates were determined by a Boyer speed recorder which, as has been pointed out frequently, is not an entirely satisfactory instrument, particularly at low speeds.

The theoretical acceleration up to the point where all rheostats were cut out of circuit, as shown in Figs. 9 and 10, is apparently about 1.33 miles per hour per second, while the actual acceleration fell to 1.2 miles per hour per second. The theoretical acceleration for which the equipment of the Subway now in operation was adjusted was 1.50 miles per hour per second in the case of cars carrying no load, 1.30 miles per hour per second with a load of 9800 pounds per car, and 1.22 miles per hour per second with a load of 15 000 pounds per car.

The engineers of the Interborough Company have constructed many time-velocity curves, both actual and theoretical. They have considered various formulas, notably those used by the Baldwin Locomotive Works, the formula proposed by Mr. W. J. Davis, Wellington's formula, and a formula suggested by Mr. John Lundie. They have also made use of a curve suggested by the speaker's assistant, Mr. H. N. Latey, which is based upon consideration of the several formulas referred to and also upon numerous results of tests made by the writer and some of his assistants at various times during the last seven years. In the opinion of the speaker, all of the formulas referred to give for speeds under about 25 miles an hour values of train resistances too low for electric trains having from one-half to all axles equipped with geared motors. The fact that the Baldwin formula gives for all speeds values much lower than those derived by using the Davis formula is probably accounted for by the fact that the former is based upon tests of trail-cars only. Every one who has attempted to derive train friction from actual tests knows the difficulty of repeating, even once, the conditions obtained in an initial test. There are so many varying factors which enter into the train resistance that even at any given speed it is difficult to make a series of tests and obtain results which will check with any satisfactory degree of approximation. In the case of service like that of the local trains in the Subway, where the larger part of the power is used in local runs averaging about 1700 feet in length south of 96th Street and a little over 2000 feet north of that point, and where consequently fully two-thirds of the total energy delivered to trains is dissipated in braking, a considerable difference between the assumed and the actual train friction does not involve a corresponding difference in the resulting calculations of the power required. In the case of longer runs, it is obviously more important to have accurate quantitative knowledge of train friction.

For preliminary calculations under conditions of speed, length of run, and proportion of motor-equipped axles such as exist in the case of the Subway, the speaker believes that the assumption of an average of 13 lb. per net ton was reasonable

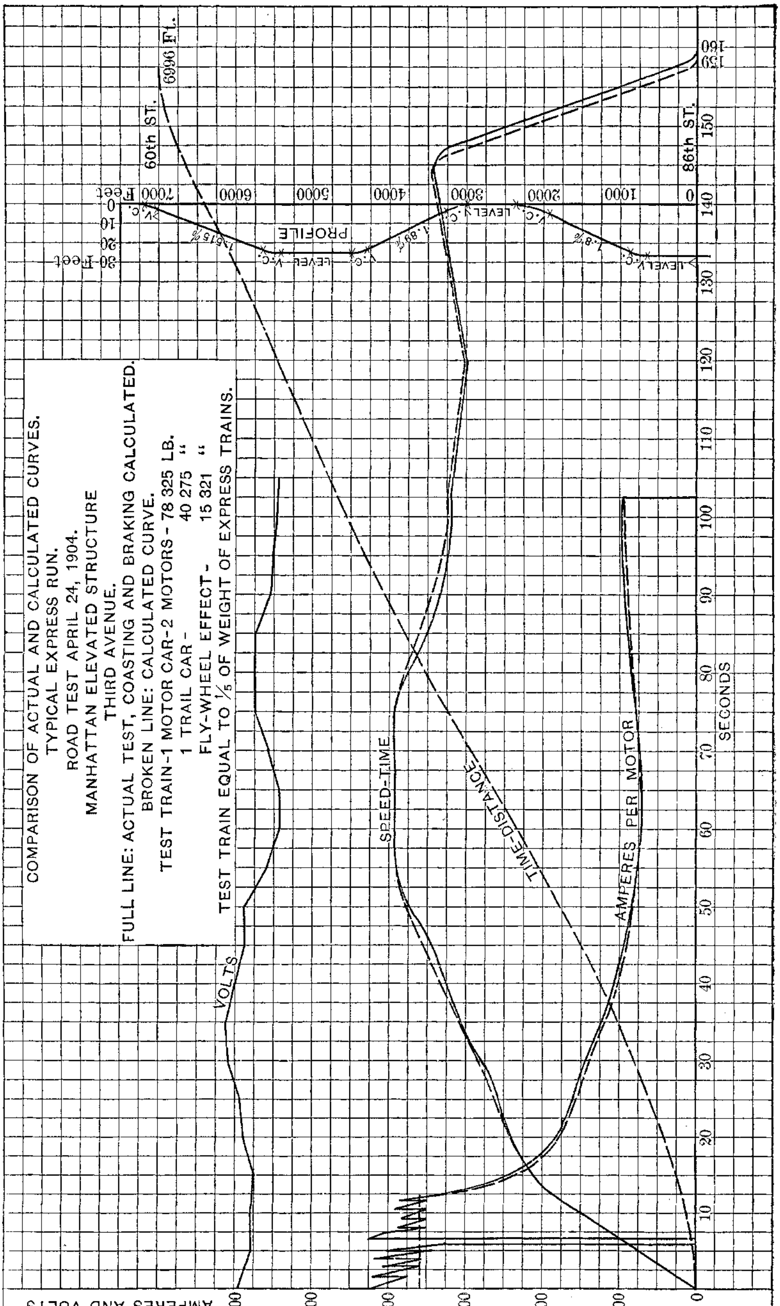
COMPARISON OF ACTUAL AND CALCULATED CURVES.
TYPICAL EXPRESS RUN.
ROAD TEST APRIL 24, 1904.
MANHATTAN ELEVATED STRUCTURE
THIRD AVENUE.
FULL LINE: ACTUAL TEST, COASTING AND BRAKING CALCULATED.
BROKEN LINE: CALCULATED CURVE.
TEST TRAIN-1 MOTOR CAR-2 MOTORS - 78 325 LB.
1 TRAIL CAR - 40 275 "
FLY-WHEEL EFFECT - 15 321 "
TEST TRAIN EQUAL TO 1/5 OF WEIGHT OF EXPRESS TRAINS.
PROFILE
SPEED-TIME
TIME-DISTANCE
AMPERES PER MOTOR
VOLTS
SECONDS
AMPERES AND VOLTS
MILES PER HOUR
60th ST.
6996 Ft.
86th ST.
Feet

FIG. 1.

and as satisfactory as the use of any of the formulas which have been considered. By way of supplementing the paper of Messrs. Lyford and Smith, Fig. 1 representing what might be called the typical express run of the Subway, has been plotted. Upon this figure, the theoretical run is shown, also the actual test run; the theoretical run having been based upon an assumed train friction of 13 lb. per net ton, with proper correction for grade and for variation in applied pressure. It is to be noted that the agreement shown between theory and test is close. From the fact that the assumed train friction in this instance appears to be sufficiently accurate for practical purposes, it is not to be inferred that the same train friction would show equally satisfactory results for speeds materially

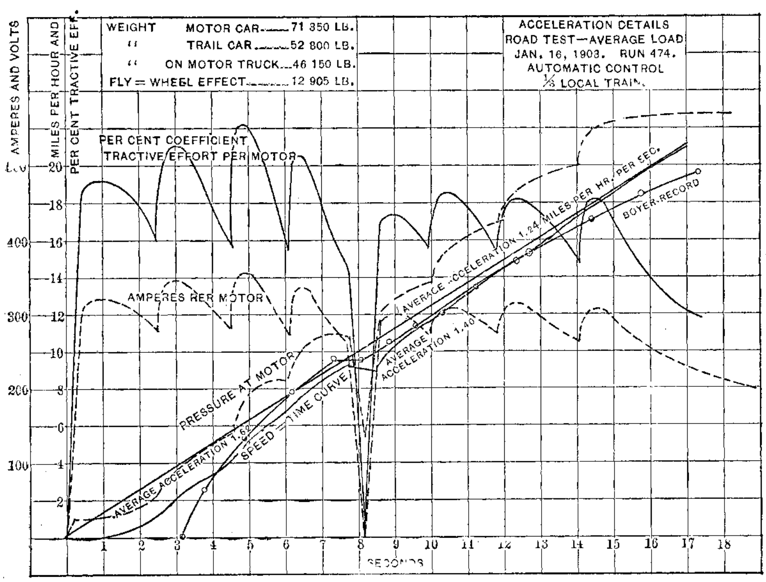

FIG. 2.

lower or materially higher than the speed illustrated in the diagram of the typical run. It is interesting to note that the formulas of Davis, Lundie, and Smith all indicate a train friction of from 12 to 13 lb. per ton at a speed of from 40 to 45 miles per hour. The speaker believes, however, that within the limits of speeds considered by Messrs. Lyford and Smith, the assumption of 13 lb. per ton would give results substantially as satisfactory as those derived from any of the formulas which have been considered.

The speaker calls attention to the fact that while the time-velocity curves shown in the paper which has been presented are so drawn as to indicate a fairly uniform rate of acceleration during the period of rheostatic control, yet this rate is in reality by no means uniform. To illustrate this, he calls attention to Fig. 2, which has been plotted upon a larger scale in order to show clearly the increments of current, the total loss of current for a period approximating a second in passing from series to parallel, and the resultant relations of the actual speed-time curve and the theoretical speed-time curve. The actual speed-time curve as drawn has been derived from the current and pressure readings, and by calculation from the speed and torque curves of the motors. The speed-time curve as determined by the Boyer speed recorder is also shown. It is to be noted that for an average acceleration of 1.24 miles per hour per second the actual acceleration during a part of the period was as high as 1.62 miles per hour per second. Subsequent to the tests from which the results shown in Fig. 2 have been plotted, marked improvements have been effected by changes in the rheostatic connections; as a result of these changes the loss of current and consequently of acceleration while passing from series to parallel connections can now be avoided.

It is to be noted that the current input and consequently the acceleration can be made more uniform by increasing the number of steps of the rheostatic control, but to do this would involve additional complication of apparatus. Incidentally it might be noted that the ideal rheostat would be something of the nature of a liquid rheostat.

Another point to which the speaker would call attention is the fact that the time-velocity curve during braking is never a straight line, owing to the fact that unless braking pressure be decreased as the speed decreases, its effect in checking speed will increase from the point of brake application to the end of the run. He has found that the relation of the average rate of braking to the maximum rate of braking in cases where brakes were applied at speeds of from 25 to 30 miles an hour was approximately three to four; *e.g.*, the average rate of braking will be 1.5 miles per hour per second when the maximum is two miles per hour per second. Recently one of the air-brake companies has produced a graduated release which operates to decrease the brake pressure as the speed of the train decreases, thus obtaining more nearly uniform rate of braking during the entire period of brake application.

Another point to which the speaker believes attention has not been called is the fact that the weight available for adhesion upon the forward and rear axles of a truck equipped with two motors is not equal. This results from the fact that the tractive effort due to the motors is exerted on a line passing through the centers of the two axles, while the resistance due to inertia of the car body is applied to the truck through the king-bolt at a point which in the case of the trucks adopted for the Subway is nearly 12 inches above the horizontal line through the axle centers. Figs. 3 and 4 show diagramatically the relations of the forces involved. It is to be noted upon examina-

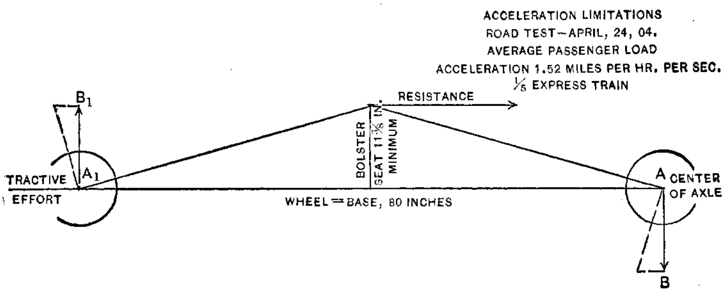

FIG. 3.

$$A\,B = A_1\,B_1 = T\,E\,\frac{11.375}{2\times 40} = 0.142\;T\,E$$

Data. 2-Car Test Train. 2 Motors.

Weight of Motor Car,	39.16 tons.
" " Trail Car,	20.14 "
" " Test Train,	59.30 "
" on Motor Truck,	48 885 lb.
Fly-Wheel Effect per Motor,	3.83 tons.
Theoretical Acceleration $T\,E$	20.5%
Maximum $T\,E$ 12 550 lb. —	25.7%

Corrected Fig. 3.

Weight on Rear Axle, 26 226 lb. — $T\,E$ 23.9%
" " Front Axle, 22 658 lb. — $T\,E$ 27.7%

Additional Correction Fig. 4.

Weight on Rear Axle, 25 649 lb. — $T\,E$ 24.4%
" " Front Axle, 23 235 lb. — $T\,E$ 27.0%

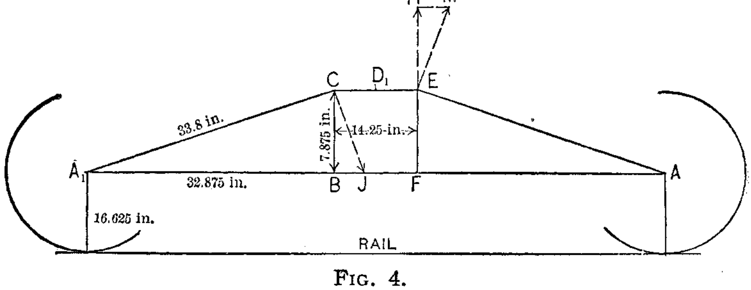

FIG. 4.

$$\text{Vertical force at } C = \frac{T\,E}{2}\times\frac{16.625}{33.8}\times\frac{32.875}{33.8} = 0.239\;T\,E$$

$$\text{Vertical force at } A_1 = \frac{0.239\;T\,E\times 14.25}{80} = 0.046\;T\,E$$

tion of the figures referred to that the resulting tendency to tip the truck, reducing weight effective for adhesion in the case of the front wheels and increasing it in the case of the rear wheels, is quantitatively of considerable importance. It is interesting to note also that this tendency to tip the truck is opposed to, and to some extent neutralized by, the fact that the force with which the motor fields revolve in the same direction as their armatures tends to tip the transom, and consequently the truck, in the opposite direction. Taking both of these tendencies into account, it appears that in a certain case for which calculations have been carefully made the weight upon the rear axle effective for adhesion exceeds the effective weight upon the front axle by more than 10 per cent.

It is apparent in the paper of Messrs. Lyford and Smith that in making their plans they have kept in mind the power supply with due regard to the fact that high accelerations cause great fluctuation of power demand on sub-stations and power plant. Probably that is one reason why they have been content to limit the acceleration to a little more than one mile per hour per second. Obviously, this could be increased at a future time by increasing the motive power equipment and by reinforcing the power supply. It seems to the speaker that the time will come when such increase would be desirable, because one of the things which should not be forgotten is the fact that the more important side of the account in a railway project of this kind is the earning power of the property and this is obviously increased rapidly by higher speed in service.

C. O. Mailloux: When he first looked the paper over, the speaker was led to expect important new disclosures of simple methods of predetermination in handling electric traction problems. In that respect, his expectations were not fully realized. So far as methods of procedure are concerned, he found little, if anything, that is not already known to and practiced by every engineer who has had to deal with similar problems. He experienced a sensation of relief, however, when he discovered that the most valuable lesson taught by this paper, was that the theoretical methods, which the authors apparently disparage, even though they still have to use them, are capable of giving results which differ from those obtained in actual tests by only a few per cent. This appeared to him to be all the more remarkable when the theoretical method was presumably handicapped to some extent by simplifications, made to save tedious calculation.

In reference to the discrepancy between theory and practice, Mr. A. H. Armstrong had told the speaker more than three years ago that the predeterminations of the energy consumption, in watt-hours per ton-mile, when made by reference to hypothetical "typical" service runs, assuming the line to be level and to have no curves, came close enough to the results found in actual working to be considered sufficiently reliable and satisfactory for first approximations, except in special unusual cases. The authors themselves, he observed, are careful to make an exception of the cases presenting unusual conditions.

Some of these unusual cases are apt to occur in any problem, and this method is then absolutely unsafe and unreliable. As an illustration he mentioned the case where the run includes several acceleration cycles, or where there are curves of such sharp radius between the stops that it is necessary to apply the brakes, and to take off the current, and to accelerate again after the curve has been passed. In such cases it will be found that the watt-hours per ton-mile, calculated from a "typical" service run, are very much lower than what actual practice will show. There is also another case, perhaps of the contrary kind, where there is a down grade and an up grade, forming a "hollow," on the middle of the run. In that case one might possibly find just the contrary result, because the momentum acquired while running on the down grade will be partly utilized in running on the up grade, and the time during which current has to be applied may be shorter; and the energy consumption will then be a little lower per ton mile than the theoretical run would show.

The confirmation of theory by practice, which is shown in the paper must be gratifying to every consulting engineer. Having had and still having the feeling that our theoretical methods are really crude and incomplete and are in great need of development and correction, he felt grateful to the authors for their having taken pains to gather and to present the very interesting additional data contained in their paper which corroborate theory so satisfactorily, even in its present incomplete and imperfect state.

The most novel feature of the paper was the new formula for train resistance proposed under the name of the "Smith" formula. On page 157 of Mr. W. C. Gottshall's "Railway Economics," reference is made to a so-called "tentative or provisional formula," suggested by the speaker. As stated in that book, this formula resembles, in mathematical form, the formulas of Davis and of Wellington, although it gives results which are quite different. It is, like these formulas, and also like that of Mr. Smith, of the general form $f = A + B\,V + C\,V^2$. The third term in such formulas which, as we note, expresses the portion of the train resistance that is due to air-friction, is, in the case of his formula, based wholly on the celebrated laboratory experiments of Professor W. F. M. Goss, made at Purdue University, some years ago, with small car models enclosed in a conduit and subjected to the action of a rapidly moving current of air. Moreover, the first term in this formula, instead of being constant, varies with the weight per axle and with the condition of the track. The formula takes the general form

$$f = \left(\frac{A}{\sqrt{W}} + g\right) + 0.15\ V + \frac{(0.02\ N + 0.25)}{N\ W}$$

in which,

A = a constant depending upon and varying with the diameter of car wheels and journals, its value ranging between 6 and 9.

g = a constant depending upon the condition of the track. Its value varies between 2 and 5 for *first-class track*.

W = the total weight per car in tons of 2000 pounds.

N = number of cars per train.

The reason why the first term was made more complex was that he was trying to find a formula having a greater range of usefulness which would apply to city lines and interurban lines, especially lines with poor track or with grooved rails, or dirty rails. He found there was a certain more or less constant element in such cases or an increase in the resistance, due to the rail itself. This fact had already been noted by others. particularly Doctor P. H. Dudley, the celebrated specialist in track construction. With this formula, by applying suitable values to the constant g (which may be taken at any value between 2 and 15 if necessary), one may get a very high value for the train resistance even for the lowest speeds. In fact, one may, with this formula thus applied, at speeds of a few miles per hour, obtain values of 8, 10, or 15, or even more, lb. per ton for single light cars, which we know is much more nearly what is obtained on cars running on grooved rails, especially when the grooves are filled with dust. His " proposed " formula has, in that respect, a certain advantage over all others.

He quoted from page 157 of the work just mentioned: " For approximate calculations corresponding to average conditions, in the case of eight-wheel cars, the formula could take the following simplified form:

$$f = 3.5 + 0.15\ V + \frac{(0.02\ N + 0.25)}{W}\ V^2$$

The formula referred to was made public in lectures at Lehigh University in April and May, 1903.

Being naturally curious to know how this formula compared with the Smith formula, he took one of the curve sheets, used at these lectures, on which several train resistance curves are plotted according to various formulas, including his "tentative" formula; the example illustrated on this sheet being the case of a train of five 45-ton cars. The results were so interesting as to induce him to present them. On plotting on this sheet, in dotted lines, the curve obtained by the Smith formula, taking A = 110 sq. ft. as is done by Mr. Davis, for cars of 45 tons, he found that the curve obtained with his own formula which is marked " proposed " on the curve sheet, comes very close to the Smith curve.

In the case of 45-ton cars, of rapid-transit type, his formula, after clearing fractions, takes the form $2.7 + 0.15\ V + 0.00156\ V^2$ for a train of five cars.

COMPARISON OF TRAIN-RESISTANCE FORMULAS.

Speed miles per hour	Value of Train Resistance by Smith Formula	Goss-Mailloux Formula	Difference
	$(3+.167\,V+0.001223\,V^2)$	$(2.7+.15\,V+0.00156\,V^2)$	
10	4.79	4.36	+0.43
20	6.83	6.32	+0.51
30	9.11	8.60	+0.51
40	11.64	11.12	+0.44
50	14.41	14.10	+0.31
60	17.43	17.31	+0.12
70	20.69	20.84	—0.15
80	24.20	24.68	—0.46
90	27.96	28.83	—0.87
100	31.[illegible]	33.31	—1.35

A comparison of the curves and of the calculated values shows in the case of a five-car train, that the two curves come very close (Fig. 5.) The Smith formula gives results which are slightly

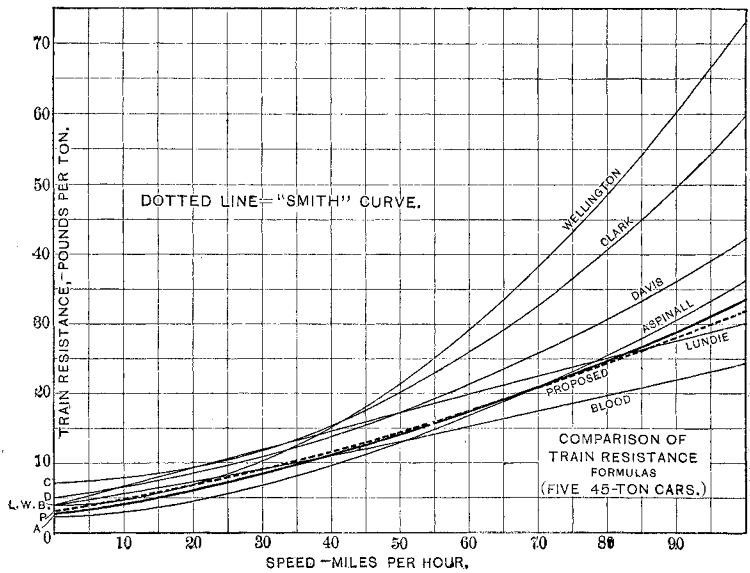

FIG. 5.

higher at low speeds, which are coincident at about 70 miles per hour, and which are slightly lower at higher speeds. The approach of the two curves to each other, in this case, is remarkable. In the case of a single car of light weight (25 tons) which represents an extreme condition, the discrepancy between the two formulas is somewhat greater. The Smith formula then gives results which are much lower at higher speeds, so that for higher speeds, the " proposed " curve, based on his formula, would pass about midway between the Smith and Davis curves.

The speaker believed that a formula built as his was, "from the ground up," entirely on hypotheses and assumptions, is, to some extent, vindicated and possibly justified when we find that it comes so near to "one which gives results close to actual practice" as the authors say of the Smith formula. Nevertheless, he preferred to retain the same conservative attitude which he had had from the beginning in regard to his own formula, and to continue to characterize it as merely "tentative or provisional," as being suitable and convenient, perhaps, for approximative calculations, possibly quite accurate in certain specific cases, but undoubtedly subject to revision and rejection, ultimately. He had made a somewhat close and careful study of the subject of train resistance, more especially during the last two years, in preparing lectures delivered at various times and places on that subject; and the more he studied it, the more skeptical he become in regard to the reliability or value of any formula offered as a universal solution, covering all cases.

Mr. J. A. F. Aspinall, in his celebrated paper on train resistance, which is in many respects the most complete presentation of the subject, read and discussed before the Institute of Civil Engineers in November, 1901, gives a list of 55 train-resistance formulas, including five of his own. Since reading this paper, the speaker had come across dozens of other formulas which are not in his list. It may be said of many of them that they "give results close to actual practice" in some specific case, at some particular speed, with a certain kind or length of train, or under other peculiar conditions; but not one of them, so far as he knew, fits all cases, even approximately, as becomes painfully evident when one sees the way in which the curves wander over the curve sheet when plotted together on the same sheet and according to the same scale. In his opinion, train resistance is one of the details of electric railroad engineering of which we have the least knowledge, and, unfortunately, one on which we need the most light. He expressed this opinion strongly in a letter written last April to Professor H. H. Norris, Supt. of Tests for the Railway Test Commission at the St. Louis Exposition. He quoted from this letter a portion which he considered almost as pertinent to the discussion of this paper as if it had been written for this special purpose:

"The more perfect the apparatus that can be devised for making graphical records of all kinds, the more complete and the more valuable will be the results of the tests made; for, these results if published, can be made available in such manner as to be of the greatest utility, later on, to others engaged in studying various details of the theory and practice of electric train movement. . . . . . . one of the most important problems now before the railroad engineers is that of finding a formula, either rational or empirical, for *train resistance*. . . . . if the Railway Test Commission did absolutely nothing more than to solve this mystery, it would well nigh immortalize itself; failing to do this, if it could render us the important service of eliminating say 50 or 60 per cent. of 200 or more train resistance formulas now disseminated in the railroad engineering literature, it would still have done good and valuable work. Knowledge, in this case, consists just as much in eliminating and blotting out what is obsolete, useless and misleading, as in discovering and adding something new and unknown. . . The ideal thing, of course, is to do both."

He was glad to learn from Professor Norris, recently, that his suggestions have borne fruit and that special tests are in progress at the present time, with the special object of throwing additional light upon that still obscure point—atmospheric resistance—which is the *bete noire* of all train resistance formulas, though it is not by any means the only point on which we need to be further enlightened. The most "popular" formulas, we must not forget, still ignore "starting friction" as if it did not exist. We know, however, from the results of actual tests as well as from theoretical considerations of the laws of friction, that it *does* exist, and that train resistance is relatively high at very low speeds, or at the instant when the train begins to move, and that it then decreases quickly to a minimum value which occurs at a certain relatively low speed, after which it increases again more slowly. The discrepances which are observed in the early portions of the acceleration curves, between the so-called theoretical curves, and the curves derived from tests, are partly due, and perhaps in some cases largely if not wholly due, to the existence of starting friction, which is absolutely ignored in making the theoretical curves. This means that the curve of train resistance, plotted as a function of the speed, is really a "two-branch" curve; this fact is admitted and shown graphically by certain authors, including Wellington, Aspinall, and Dennis; yet all "our" formulas, as well as theirs, neglect entirely the first branch, showing the starting friction. That is why he could not help regarding them all, including Mr. Smith's and his own, as mere makeshifts. He desired to point out that it would be necessary to do something more than merely to devise and introduce a new set of constants in a Maclâurin series of the form $Y = A + B\,X + C\,X^2 +$etc. in order to accomplish something representing a material advance in our methods of dealing with train resistance in railroad engineering. We already have too many formulas of that kind. Moreover, it was not difficult to foresee that the physicist and the mathematician will probably contribute at least as much as—and perhaps much more than—the engineer, to the solution of the problem of finding a general formula for train resistance—if one be ever found. There is valuable work to be done by the engineer in obtaining data for theoretical analysis and in making tests for the purpose of proving or disproving theory.

A very interesting, and perhaps the most amusing thing to him, in this paper, was the cynical commentary of the authors on his own paper of June, 1902, which is characterized as being "scholarly and somewhat bewildering," with the hint added that its value is principally, if not wholly, "academic." He was sure that he did not deserve the implied compliment in the words "scholarly" or "academic" with which the rest of the comment is sugar-coated. There was some humor there and he did not think that the joke was altogether on him. At the time the paper was presented, certain "academicians" thought it altogether too "elementary," and, indeed, considered it anything but "academic." It is some consolation to know, under the circumstances, that requests are still coming from all sides and from many countries, for copies of the reprint of this paper. although no copies are available. He found gratifying evidence in Europe this summer, that this same paper, so far from being regarded either as bewildering or academic, was used as a practical hand-book. He found equally gratifying evidence that the work was also appreciated in this country, in the scores of letters received from American engineers during the last two years. He read extracts from some of these letters.

One of these letters, dated December 3, 1902, was from an electrical engineer who stated that he had "found this paper exceedingly interesting," and particularly so, because he had "recently carried out a similar series of calculations for the Manhattan Elevated Co." He added: "Had your paper been in my possession at that time, it would have been of great value to me in this work, and I am sure that this paper will become a hand-book of great value to all who are engaged in work of this nature." Another letter, received from "8 Bridge Street," New York, under date of December 29, 1902, contained a request for two more copies of the reprint of the paper on "Speed-Time Curves" stating that the first copy was practically worn out from having been used "a very great deal." The third letter, acknowledging receipt of the two letters in question, said:

"As I wrote you the other day, I have already had occasion to make use of these and have found them of great service to me in various railway problems. I most heartily concur with the expressions that I have heard from many engineers that this presentation of the subject is the most exhaustive and thorough that has ever been attempted."

The speaker thought that further evidence on this point was unnecessary.

His paper was intended to cover only one detail of the art of predetermination in electric traction problems—the plotting of speed-time curves. It merely sought to present means whereby an approximative calculation or even a "guess," in regard to the equipment required for a given electric railroad service, could be tested or analyzed, with more or less precision, according to the importance of the case. He did not, at that time, and did not, at the present time, take the position that either this method or any other theoretical method known to him can be followed with confidence or safety, without the exercise of some judgment. It was very natural, therefore, that he should concur fully with the authors in the opinion expressed on page 692 that "important details are frequently settled by quite simple processes of reasoning." The paragraphs (a to f) on pp. 692 and 693 might, almost be characterized as "axioms," for they are so self-evident as to require no demonstration or argument. Every engineer using a little common sense, would, he thought, do these things naturally, as a matter of course. He could not believe that any engineer having had even a little experience in any kind of work, would neglect to canvass the situation thoroughly at the outset, so as to ascertain the requirements of the case, or that he would fail to consider the possibilities as well as the limitations created by them and also to appreciate the extent to which they influence the character of the equipment, in a given case. In all the cases that have come to his knowledge the process recommended by the authors was substantially that followed. In almost every case he had found certain features or requirements which decide and settle some point in a peremptory manner, rendering discussion or analysis not only useless but even ridiculous. It usually needs but very little study, and very little calculation, if any, to ascertain the limits between which the choice of motors must be made, and even to narrow these limits until one knows that the choice lies between two or three sizes only.

Answering one of the questions propounded on page 695; viz., how much of this tedious calculating is necessary? He should say, simply, "*just what and only what cannot be avoided.*" His experience showed that the "amount" of calculating and its "tediousness" both depend greatly on the man who is doing the calculating.

To take one extreme case, a person who is totally inexperienced, even though competent technically, might have to make the "tedious calculations" an almost endless number of times, for innumerable hypothetical cases, based on as many kinds of equipment, and yet he might not be able to avoid making a serious mistake. To take another extreme case, an engineer having had wide experience with very similar or perhaps almost identical cases, and having good judgment, might see and might be able to give "offhand" without any calculating whatever, the best solution of the problem. He may have, from experience, from data, and from statistics in his possession, his intelligence, etc., that ability to grasp and "size up" the situation which make the case both clear and simple to him and enables him to do by intuition, let us say "guess," if we prefer, what others could not accomplish even with the most comprehensive calculations. Between these two extremes, there are almost as many cases as there are kinds of men; but, after all, they all proceed by substantially the same process, within their limitations of intellectual equipment, knowledge, and experience. That process is essentially *empirical*, when it can not be *rational*; and in this case, it can not *become* rational until the *theory* has been further developed, *no matter by what means*. To put it in simple plain language, that process is simply this: " *look the case over carefully*; *take up what you 'figure out,' what you 'guess,' or what you 'hope' or presume* (either from your knowledge or experience, or that of others), *will best suit it*; *and then use the best means at your command to ascertain how nearly or how badly it fits*; *and if, at first, you don't succeed, 'try, try again.'* " If he understood the authors, this description also covered their method of procedure fairly well. He had had to do this repeatedly in cases where the path was unbeaten and uncertain, as it is in analyzing problems in electric traction. So have, doubtless, many others. But he had, each time, done it from necessity, from want of a better way, more than from choice. He realized that there *ought* to be a more direct way, a more *rational way*; and when once that rational way was accessible, he followed it.

He had no greater fondness for tedious calculation than the average engineer. At the same time, he did not believe that it should be shirked so long as it can accomplish a useful purpose. The "limit," in this case, depends largely on the *thoroughness* with which the engineer is accustomed to do his calculating and the *degree of precision* with which he can *content* himself. He knew a *few* engineers who are, perhaps, open to criticism for *excess* of precision in their work; but he also knew *many*, indeed *very many*, who are too much open to criticism for exactly the opposite reason.

He greatly favored and gladly welcomed any new developments in methods of predetermination, whereby the work is simplified. He probably disagreed with most of the others, however, in stating that he anticipated more progress in methods from the *theoretical* than from the *practical* side. He said this, bearing in mind the great value of the statistical method in furnishing clues and short cuts whereby a desired result can be obtained, or whereby the possibilities can be ascertained in a much more simple and direct manner. The very excellent paper by Mr. A. H. Armstrong, on the "heating of railway motors," read before the INSTITUTE, was a magnificent example of the manner in which statistics furnish clues to newer and simpler methods; but such papers and presentations of statistics or the conclusions based upon them, do so merely to the extent that they simplify, develop, perfect or correct *theory*.

In conclusion, he recalled some remarks made by him at the Niagara Falls convention, in the discussion of Mr. F. W. Carter's paper on predeterminations in railroad work, which show that he did not believe specially in plotting speed-time curves for a pastime, even though he were the author of an "academic" paper on that subject. He quoted, emphasizing a few words: "Consequently, it is not enough to have a means of *readily plotting* speed-time curves. We want more than a means of readily plotting the *subsidiary curves*,—we want means of *obviating* the plotting of them, and of obtaining, nevertheless, the *results which they would give us*, and which we now have to obtain by plotting them and laboriously integrating them by mechanical methods." The thought in his mind, at the time he said this, was that these curves, including the speed-time curve itself, though desirable for graphically *picturing* the results, are not necessarily indispensable as instruments or means for *obtaining* these results in the first place. He already had this thought even at the time his paper was read, as the discussion shows.

He believed this now just as fully as he did two years ago. Indeed, he believed this now even more fully than he did when he said it, because he had since that time made some progress of a satisfactory character in that direction, the results of which will be made public in due time.

H. Ward Leonard: The title of the paper of the evening is "Heavy Electric Traction." The speaker's remarks will bear more on what might be termed Heaviest Electric Traction. As the title of the paper seems to make it proper, the speaker would like to touch upon the subject of operation of the heaviest electric traction, and bring out some figures which have been obtained from various sources, and should prove of interest.

One of the points upon which electrical engineers would like information in considering the possibilities of trunk line electric traction, is the power required. The speaker has arrived at an approximate average figure for this of about 125 h.p. per mile of line. This has been obtained by taking the total maximum horse power of the steam locomotives and the total mileage of the railroad systems.

In the case of the Pennsylvania road, the figure is in the neighborhood of 550 h.p. per mile of line, and in the case of a few roads which have very heavy freight service, such as the Pittsburg & Lake Erie, and the Bessemer & Lake Erie, rough estimates show as high as 1000 h.p. per mile.

The steam locomotive of the latest type and largest power costs with its tender about 5c. per lb., but when the weight upon the drivers only is considered, the cost figures 9c. per lb Now with the price per lb., which has been reached in electric generators, as a basis of calculation, it is reasonable to suppose in the case of the heaviest electric locomotives, where the horse power will be relatively low compared with the weight, that they can be made to sell for less than 9c. per lb. on drivers when they have been brought to standard types.

The speaker thinks that the question of the application of the electric locomotive is one which is largely dependent on the horse power per mile of steam service which it can replace, and that in cases where 250 h.p. per mile of line is employed, the electric locomotive can replace the steam locomotive with a very decided economy. In this connection it will be of interest to state that in the case of the many modern types of freight locomotives, the cost of maintenance per mile run is as much as the cost of the fuel per mile run. This, at first sight, seems almost impossible, but it is a fact that $3500 per annum is the cost of fuel, and $3500 per annum is the cost of maintenance for locomotives of the heaviest type, such as the 2–10–2 type of the Santa Fé locomotives.

Referring to some of the comments which have been made about short and ready methods of reaching approximate results, the speaker uses one which perhaps may be useful in calculating the horse power at the draw-bar: Multiply the miles per hour at which the train is moving by the pull in pounds at the draw-bar and double that product; this will equal the watts. This is correct within a very small fraction. Striking off the three right-hand figures gives the kilowatts; and by increasing the last result by one-third, the horse power is given. For example, a locomotive of 30 000-lb. draw-bar pull running at 20 miles an hour, gives 1200 kw. or 1600 h.p. at the draw-bar.

The speaker wishes to draw attention to the fact that this paper, and the comments which have been made upon it, illustrate the importance of having for heavy electric traction and especially for the *heaviest* electric traction, such methods as will secure the maximum possible draw-bar pull from certain definite horse power and weight on drivers.

It is a fact which most people do not appreciate that a freight locomotive of the most modern type, which will develop 1400 h.p. going at 20 miles an hour on a level, when it comes to the heaviest grade, and its speed falls to six miles an hour, will then be producing in the neighborhood of only 700 h.p. That is just the time when, instead of producing half the horse power that is produced on the level, there is needed a locomotive that will produce three to six times the horse power ordinarily required on the level. In this regard the electric locomotive has important characteristics that a steam locomotive lacks. It has the ability, for the short time required to cover grades a few miles in length, to respond to a very great demand for overload capacity or overload currents without the heating effects accumulating sufficiently to harm it. In the case of heavy grades of 2 and 2.5 per cent. the horse power of the locomotive, necessary to maintain the speed of the train and prevent traffic congestion, is from three to four times the horse power on the level. Three steam locomotives, applied to a train on grades of this character, do not produce more than one and one-half times the horse power that is produced by one of these locomotives on the level. And yet the demand for power is, of course, greatest on grades.

Heretofore in discussing the possible application of electric locomotives to trunk line use, the question has been narrowed down to the consideration as to whether or not fuel can be saved—whether power can be generated at a distant station and transmitted more cheaply to the locomotive than by having independent sources of power in each locomotive. The point has also been much discussed whether or not some labor cost may not be saved in driving the electric locomotive. These points are trivial in connection with this problem. Assuming $100 of receipts for a railway, the fuel cost represented in the earning of that $100 is between seven and eight per cent. The total cost of everything which can be charged up against the power, including the shops and everything of that kind, will not run as high as 20 per cent. Given a road where the movement of freight is pressing, and the road is demanding more locomotives to haul the traffic, it is evident that if there can be applied to the train twice the horse power now obtained from steam locomotives the receipts can, in a given length of time, be doubled and at a comparatively slight additional expense.

W. S. Franklin: It seems to the speaker that in a matter like this—of the relation between train speed and train friction—in which there is the element of chance, that one of the most important things to be found out in tests is what one might call the *probable variation* (analogous to probable error of a measurement, only here it is not an error but an actual variation of the thing measured).

To make the thing more definite, the speaker thinks that there is an important element of reality in what many of you are pleased to call the discrepancy among these various curves of train friction. There is no theoretical relationship between friction and speed; and there can be no ultimate theoretical solution of this problem. There is too great an element of chance involved. Strictly speaking, it is a matter which depends upon the conjunction of an infinite number of fortuitous elements, and such a problem cannot be rigorously formulated.

The speaker suggests that one of the most important practical results from a set of tests would be the establishment of the *probable variation* of friction in different runs, so that the designer of a car equipment would know the factor of safety to allow in choosing the motor to be used in order to make provision for the probable discrepancy between the calculated amount of power—the actual mean amount of power observed during the many runs of the test—and the actual amount of power likely to be consumed in a series of runs in service.

In a test involving many individual runs, not only should the mean be taken and used as an engineering datum, but also due attention should be paid to the discrepancies, and these discrepancies should be formulated by the laws of probabilities and the result used as an important engineering datum.

The discrepancy among different friction speed curves, in so far as these curves do not go beyond the limits of the observations upon which the curves are based, must be ascribed to chance except in so far as the different conditions are specifiable under which the observations are made. The speaker doubts very much whether any full specification of this kind could be made. He is inclined to look upon the discrepancies of these curves as largely fortuitous. You will note indeed that these discrepancies are slight in the observed parts of the curves. The exterpolated parts of the curves are absolutely meaningless.

A. H. Armstrong: In looking over this paper it is noted that two thirds of it is devoted to discussing two train formulas, one devised by one of the authors and the other by Mr. Davis. It is not the intention in taking up the discussion to determine whether the formula devised by Mr. Davis or by Mr. Smith, or the indiscretion perpetrated by Mr. Mailloux two years ago and admitted by him a few minutes since, is correct. But an attempt will be made to prove that given any formula of reasonable accuracy, holding true for speeds up to 40 miles an hour, it would be impossible with careful and accurate calculations to arrive at the results noted in this paper.

Turning first to page 704, there is given in Figs. 7 and 8, in full lines, a reproduction of some speed-time curves taken at Schenectady, and in dotted lines the calculated results of the Davis and Smith formulas. Referring to Fig. 8, two things are noted, one of which is that the 670 amperes does not agree with the 42 miles an hour with the equipment cited when running at 570 volts. The actual speed given with this current, according to the speed-torque curves of the motor is 38.8 miles, or, in other words, it practically duplicates the Davis curve given on the previous page. To get a speed of 42 miles at 570 volts requires a minimum input of 600 amperes, or 70 amperes less than that given in the curve. Any calculations given here are open to some criticism, as they were made while coming down from Schenectady on a train running at 60 miles an hour; and those who have worked a slide rule under like conditions know the difficulty of arriving at accurate results. Referring to Fig. 7, the minimum current is given as about 720 amperes, and corresponds very closely to the actual speed; but it must be pointed out that the better showing of the Smith formula, on curve Fig. 8, is not due to the somewhat lower friction of 2.5 lb. per ton at 40 miles an hour, but to the inconsistent method of calculating the two curves. For instance, in Fig. 7, the current represents a constant rate of 720 amperes, while the time-speed curve still shows an increasing speed for at least 40 seconds during the latter part of the time the power is applied, and gives results which are somewhat inconsistent.

In dealing with the speed-time curves given in this paper, two or three sources of inaccuracy are present which perhaps have not been fully taken account of by the authors. The first three sets of curves are stated to be plotted from readings taken every five seconds by the observers watching a more or less dead-beat ammeter. The Arnold-Potter tests were taken with two-second readings, with one man reading, another jotting down the results, and a second group of observers reading maximum and minimum current values and time at which they occurred in order to arrive at the rheostatic peaks. Even with the precautions taken in the Arnold-Potter tests, wattmeter readings show that the product of volts and amperes was from 5 to 10 per cent. below the carefully calibrated wattmeter results. What the results would have been with five-second instead of two-second readings is open to a good deal of comment, and the curves in Figs. 2 and 3 cannot be looked upon as forming a very accurate basis with which to compare calculated results. Referring to Fig. 4 it is noted that three results are compared, one tested and two calculated, with very different braking rates. No obvious reason is given for the rates being different, and the effect of the more rapid braking rate is equally apparent as a more rapid accelerating rate, so that the theoretical value of the Smith formula plotted to the maximum braking rate would give a somewhat lower energy input. The 94 watt-hours per ton-mile given would drop below that figure and show a still greater discrepancy with the test value of 99 watt-hours if the more rapid braking were used for all three curves.

Referring to Fig. 5, it is not apparent why the authors should have allowed the calculated run in dotted lines to fall so far below the actual acceleration observed in the test run, while on the next page in comparing the Smith formula the actually observed acceleration rate is closely followed. The effect of using a lower rate of acceleration with the Davis curve is to cut out any coasting and thereby to increase considerably the watt-hours per ton-mile, not on account of the small increased pounds per ton friction, but due to the absence of coasting. On Fig. 6 it is found that the Smith formula enables the run to be made in some five seconds less than the actual test run, giving a calculated value of 72 watt-hours as compared with 79.4 by wattmeter in the test. If the time had been increased to the 136 odd seconds of the test run, the 72 watt-hours would still further be reduced by the introduction of more coasting. so that as an estimate the comparison would be for 66 or 68 watt-hours calculated against 79.4 by wattmeter, a result which is not very close even for an approximation.

On Fig. 9, page 706, it is noted that the theoretical calculated energy consumption of the train is some 10 per cent. higher than the test results, although the dotted line shown is at a lower friction rate than the test value; in other words, the formula used gave a lower friction value than was obtained in the test, but the calculated watt-hours per ton-mile were greater, which is inconsistent. On the next page, Fig. 10, the dotted line and full line show practically the same coasting curve, following very closely, and yet the calculated energy consumption is 83 watt-hours per ton-mile, against 77 in the test; and this obtains also with the higher average acceleration rate of the calculated curve. We should naturally expect that with the same friction rate, which is the only undetermined factor entering into the calculation of the speed-time curve, that the result would have been much closer than that given.

Passing on to page 711, it says, "It is safe to assume that a check made on the comparative determinations for square root of mean square current will indicate the correctness of the assumptions for the determination of the other requirements." The impression obtained by reading this page and others following is that the chief object in developing a series of speed-time curves was to obtain the square root of the mean square current, and that this current value was the controlling factor entering into the selection of the proper motor for the work. The details of the old controversy of the proper method of obtaining the capacity of railway motors will not be entered into here other than to state that the square root of the mean square current is ordinarily used in calculating the copper losses only of the motor. It ignores entirely the core losses of the armature and field and is not applicable to any other type of motor than the direct-current motor. It can not be used with the single-phase alternating-current motor, and furthermore excludes the brush-friction losses, the bearing losses, and all internal losses of the motor other than those depending on the current itself. Furthermore, the core losses and other incidental losses are not a direct function of the current employed; that is, with a given cycle, a certain relation may exist between the total losses and the square root of mean square current, and in that way the square root of mean square current may be used as a basis of the motor heating. In another cycle an entirely different ratio may exist; and any motor-heating predeterminations based solely upon the square root of the mean square current would fall through as being inaccurate and misleading if applied to general railway problems.

In connection with this a new factor may be mentioned which enters into the determination of motor capacity, which has been brought into prominence by the introduction of the single-phase railway motor. In direct-current motors it was thought when the copper, core, and brush-contact losses had been obtained that approximately all the internal losses of the motor had been noted; so it was put down that the total losses in the motor were those losses enumerated, plus a certain value for brush friction. In developing the single-phase motor with the 150 volts or so on the commutator which is incidental to such motors, the experiments encountered a brush friction fully three times as great as in the 600-volt direct-current motor, there being something like 1500 watts total loss in the motor tested which had never been counted on. The first motor that was built, in which no especial attention was paid to the brush-friction loss when mounted on a car and hauled as a trailer with no current whatever in the armature, would heat unduly due to the brush friction alone. That test was followed up by a series of experiments and improvements looking to a considerable reduction of this brush-friction loss until now it is within reasonable limits. This instance is cited to point out that the square root of mean square current is not a true criterion of the motor heating in service and that all the losses—not only the actual losses, but their subdivision and relation to each other—must be carefully determined before a proper motor can be selected for a given piece of work.

The authors have indicated their desire for some short-cut method that will avoid the laborious calculations involved in the preparation of speed-time curves in detail, but the approximate methods used indicate a lack of accuracy in the results obtained, making it an open question whether the careful preparation of speed-time curves is not necessary. No one appreciates the tedious nature of such calculations more than the speaker; but no rational method has yet been proposed which will do away with the necessity of such calculations for final results in the selection of train schedules and motor equipments. Not only is it necessary to calculate speed-time curves under the conditions imposed, but also to plot other curves with a modification of some of the conditions in order to determine the amount of leeway a given equipment possesses on either side of the limitations imposed by the specific problem in hand. General curves worked out throughout the range of limitations of rate of acceleration and braking and train friction will give reasonably accurate approximations to serve as a basis for detailed calculations, which must be gone through with in detail in order to get accurate results. Such accurate results are required, as it is folly to presume that less care should be exercised in the selection of the rolling stock of a road when its value exceeds that of the entire generating station and feeding system.

C. T. Hutchinson: The speaker has published two papers on the subject of predetermination of the operation of electric motors in railway service, both of which were based upon the assumption that certain average results could be depended upon for the ordinary type of railway motor in use, and that the results deduced from such averages would be found to correspond substantially with practical results. The principal object of the method proposed was to eliminate the tedious plotting of speed-time curves, and to outline a general method applicable to any motor to predetermine the temperature rise of a particular motor under service conditions. The paper this evening gives the speaker an opportunity to apply his method to some of the results given. The motor on which these results are based is the Westinghouse No. 86 motor, which has been tested more elaborately by Mr. Stillwell and his assistants in the Interborough Company. Through the kindness of Mr. Stillwell the speaker has had access to the results of these tests. The method proposed by the speaker, substituting the constants of this particular motor, is the basis of the statements that follow.

The speed-torque curves of this motor were first compared with the average speed-torque curves assumed in the papers referred to, and were found to be in practical agreement. The method proposed was then applied to the run shown on Fig. 1, using the Smith formula. The energy per ton-mile, as calculated by this method, came out 46 watt-hours; the result given by the authors of the Smith formula is 48.8 watt-hours,—a practical agreement. This result was obtained by a calculation taking probably five minutes, and should be compared with the time necessary to plot and integrate the speed-time curves shown in Fig. 1.

The method was then applied to a comparison of predicted and observed results in the typical local run of the Interborough service. This is the run shown in Figs. 9 and 10 of the paper. The energy per ton-mile calculated by this method was 75 watt-hours; the test results show an average of about 78; this agreement is as close as could be expected.

The most important matter to predetermine is, however, not the energy requirement, but the temperature elevation of the motor for any given run. This can be only predicted when the temperature that the motor attains, under approximately similar conditions, for a given energy loss is, known,—that is, when the radiation coefficient of the motor is known. The only way to determine this coefficient is by test. The speaker's method assumes that such test has been made, and that the value of this coefficient under different conditions is known. The temperature elevation of the motor will then be determined by the heat loss in the motor during the run. Hence if the average heat loss in the motor, as calculated, agrees with that shown by test, the temperature elevation will also agree. Applying this method, then, to the data of the typical local run of the Interborough system, and using the test records kindly furnished by Mr. Stillwell, the calculated core loss is 3360 watts; the test results give an average loss of 3460; the agreement is substantially exact, and therefore the temperature elevation predicted by the method in question would have been practically exact, assuming the radiation coefficient to have been properly determined by tests under the conditions of this run.

The greatest value of a method of this kind lies in calculating the results of a variety of runs, such as given in Tables 2 and 3. Using the method of the authors, the calculation of the results given in Table 2 alone would probably require the work of at least one man for the better part of a week. The speaker has applied his method to the same series of runs, and in less than an hour has obtained results of the character shown in the tables in question, together with other results giving, as he believes, data of far greater value. These results are, briefly, as follows:

Table 2 gives 17 separate runs and a summary for the round trip. For each trip it gives further the square root of mean square current, both for the actual run and for the typical run; that is, the average of all the separate runs. These figures are given to show that the use of a typical run in place of the separate runs is a justifiable assumption. This, by the way, is also the assumption made by the speaker in the papers referred to. These 17 runs, using curve-sheets already published by the speaker, gave an energy loss per ton-mile for the different runs varying from 60 to 87 watt-hours; multiplying the length of each run by the energy consumption per ton-mile, and taking the weight average, there results a figure of 66 watt-hours per ton-mile as the average of these separate runs. The quantity required for the typical run calculated in the same way is also 66 watt-hours per ton-mile. This result corroborates the result given by the authors, and is a sufficient demonstration that a typical run can be substituted for the individual runs in a case of this kind, with a high degree of accuracy as far as the energy consumption is concerned.

A more important matter is, however, the temperature elevation of the motor. The authors assume the same motor, to be used with the same gearing, etc., for these various runs, and assume the temperatures to which the motor will attain to be practically determined by the value of the square root of the mean square current. Applying the method of the speaker, and choosing an initial acceleration so that the temperature elevation for the type run shall be 75° cent., the temperature elevations for the individual runs will vary from 107° to 71°; the temperature elevation for a composite run, made up as indicated in Table 2, would be slightly greater than 90°. In other words, the substitution of a typical run for the separate runs is sufficiently accurate for the determination of the energy used, but is not satisfactory for the determination of the temperature elevation of motors, and therefore not satisfactory for the determination of the motor capacity, which is, of course, directly dependent upon the temperature elevation.

Speaking generally of the subject, it is the opinion of the speaker that the relative importance of the various phases that have been discussed this evening are somewhat as follows: Train resistance and plotting of speed-time curves of least importance; the determination of energy requirement of next greater importance, but still of slight importance compared to the third point,—that is, the predetermination of temperature elevation and of motor capacity. The third and most important point is practically left out of consideration by the authors of the paper.

W. N. Smith: Referring to the interesting point brought out by Mr. Stillwell in regard to adhesion during acceleration, which hitherto seems to have been overlooked, the speaker would recall to the attention of the INSTITUTE the interesting paper on braking contributed about two years ago by Mr. R. A. Parke, which shows how the converse of Mr. Stillwell's proposition holds true in a car truck when the brakes are applied.

As to the discussion of theory versus practice, this paper is not intended to be a statement that theory is of no account and that practice is the only rule to follow. It is not a question of theory versus practice; the paper shows that one bears out the other.

One of the points the authors wish to emphasize is the proper *interpretation* of theory in presenting a question of this kind to busy, hard-headed steam railroad men who have spent their lives in the railroad business and have neither time nor patience to listen to mathematical explanations of the points involved.

Concerning the train resistance formula, the authors do not claim any more for it than Mr. Mailloux or any one else can claim for any formula. It is by no means the last word on train resistance formulas; but the paper shows that it is based on results which are practical, and it certainly compares very favorably with others for the particular conditions of train weight and equipment.

As to the effect of sharp curves and grades, the particular problem here considered has very few curves to deal with which a train could not run around at practically maximum speed. Where they did exist, however, particularly in the case of the North Side Division, their influence on the train runs was carefully examined, as is shown in Part II. of the paper, with the result that the general typical run was found to compare sufficiently well with the actual plotted runs for all practical purposes.

Increased friction at the instant of starting may make a difference, but it is not in evidence in any of the practical test curves exhibited in the paper, nor is it shown in the curves presented by Mr. Stillwell.

Mr. Armstrong's criticism of Fig. 4 is covered by a note which says, "Power cut off at same distance from start. Average speed maintained constant by varying braking rate." Some of his criticisms in regard to the current curves are partly to be explained by the fact that some of the curves have been reproduced half a dozen times since originally made and are a little exaggerated, and in one or two cases are not quite exact. The curves made use of were originally made for the Arnold and Potter paper, and were taken from the TRANSACTIONS, and in this way it is likely that some inaccuracies have arisen which may explain some of the criticisms of Mr. Armstrong.

The authors are disposed to view such problems from the standpoint of the consulting engineer, and in the interest of the customer. Large enterprises of this character are likely to be handled by consulting engineers rather than designing engineers. It is hardly necessary to say that the consulting engineer cannot be expected to view the merits of any apparatus solely from the standpoint of the designer, because the minutiæ of design do not necessarily appeal to him. The consulting engineer is the interpreter to the customer, of the apparatus and methods advocated by the designing engineer. Whether or not the consulting engineer is interested in all the various intricacies of design which are of particular importance to the designing engineer, he must be able to present general results to his client in a clear, convincing, practical manner which will enable the client to follow the consulting engineer's reasoning and be governed by his conclusions.

Applying this principle to the question of the rating of motors, some of the gentlemen who have discussed the subject have brought into it a large number of the quantities which are taken into their calculations in the design of motors. They expect consulting engineers to follow through the various losses in the motor for various cycles of work, experimentally determining the degrees rise per watt loss, and finally the exact temperature rise for certain duty. While they admit that this process is cumbersome, they do not yet seem to have put forward any simple method of general application which it is reasonable to ask a consulting engineer or his client to follow in detail. Such experiments may be necessary for *designing* engineers and manufacturers, who must make the completest possible study of their apparatus and its possibilities; but when it comes to interpreting the results of such experiments, it is too much to expect that either the consulting engineer or his customer should be obliged to follow out the fine points of a separate laboratory test for every possible condition.

In contradistinction to such methods of arriving at results, satisfactory though they may be to some manufacturers and engineers, there has been developed a simple method of expressing the capacity of railway motors by means of determinations of the root of mean square current and the "equivalent volts" which appertain to any cycle of operation, representing an equivalent continuous load which a motor may safely carry in service without regard to the particular number of degrees temperature rise which may result from it.

It is found in practice that the exact temperature rise of motors in service is appreciably affected by such a slightly different condition as the position of the motor in the car truck. The motor which gets the greatest fanning effect by virtue of its position in the truck may be cooler than one not so favorably situated, by 10 degrees or more. Such results are likely to diminish considerably the value of tests for the exact determination of temperature rise.

Great exactness cannot be claimed for any of the assumptions made in electric railway predeterminations. All the variables are likely to be several per cent. out of the way; and temperature rise can certainly not be calculated with any greater degree of accuracy than pertains to the initial assumptions.

The method of using the root of mean square current for determining the sizes of electric railway motors, as proposed and demonstrated by Mr. Storer and Mr. Renshaw, has not only proved to be most valuable as a working basis by virtue of its simplicity, but has also proved out in practice remarkably well.

Referring to one of the last speakers, who showed the way in which his theoretical method of deriving results is corroborated, to a great extent, by the presentation in our paper: it seems to the speaker that his theory is now worth considerably more to a practical man than it was before this demonstration was made. His theory is undoubtedly a good one for its purpose, but the one thing to bear in mind is the relative ease with which its correctness can be demonstrated.

Methods that are largely graphical have an additional value in that they are very easily checked, and that arithmetical errors, if made, are found more readily than they would be if such a problem were worked out on the basis of a mathematical analysis requiring sheets of algebraic equations. A train schedule once settled on, and sub-stations, transmission line, and power-house located, a comparatively few days' work will enable a few skilled men to calculate and tabulate in permanent form the rapidly varying loads involved in such a problem, from which the determinations for lines and apparatus can be made at leisure.

E. E. Ries: On referring to curve 17, page 720, it will be seen that the changes in weight of the live load of this particular equipment do not materially affect the speed characteristics of the equipment; in other words, the electric train weighs 70.6 tons with seated load, and 77 tons with the standing load, a little over six tons more. This shows a very large disproportion between the dead weight of the train, and the live load or the passengers carried.

As the standing capacity of the subway cars employed in this test is greater than the seating load, it may be assumed that the total passenger weight in the two-car train referred to was not over 12 tons. This would make the dead weight itself 65 tons, or more than five times in excess of the maximum live or paying load. In other words, it required *only one-sixth* of the total power to transport the full passenger load, *whereas five-sixths of the power is expended in moving the empty train.* We have been accustomed to the waste which has taken place in the steam-engine, and to lay a great deal of stress upon the question of fuel consumption when discussing electric traction problems; now it would seem as if a strenuous endeavor should be made to lighten the excessive weight of the train itself as compared with that of its passengers.

O. S. Lyford Jr.: The paper was not intended to cover all the phases of the engineering necessary in selecting motors for such a project as referred to. Simply a few illustrations are given bearing upon the general points made. There is no desire to disparage the admirable work which has been done along purely theoretical lines, for all such work is of value in helping to reach the last analysis. It was desired simply to correct some impressions which have been given. There is no particular claim made to originality. The literature, as a whole, is lacking in evidence of the character given in the paper. No doubt other members have such data as given, which would be of great interest to the Institute. Recent proceedings of the Institute have brought out much practical information regarding transmission line construction, lightning protection, etc.; it is suggested that much information of a similar character regarding experience in electric traction could be furnished and would be of great value. The plans of the Railway Test Commission, if carried out, will add materially to the literature on the subject.

It is gratifying to know that Mr. Mailloux agrees with the authors of the paper that the amount of elaborate calculating to be done should be only that which cannot be safely omitted. It is a question whether we wish to obviate the necessity of plotting speed-time curves. A study of the actual characteristics of these curves is often very instructive. Moreover, the problem is not finished when the motors are selected. The current-time curves have to be studied very carefully in the consideration of sub-station and power-station loads.

With reference to Mr. Armstrong's comments, they are those common to the fine-toothed examination of each other's data frequently indulged in by the manufacturing companies. Irrespective of his adverse analysis the general facts are as given in the paper. The Arnold and Potter tests were made by the General Electric Company and can be analyzed more closely by the General Electric engineers than by the authors. It would be of interest to the Institute to have a similar comparison of theory and test by those best acquainted with the test data and the motors used. As to Mr. Armstrong's remarks concerning the use of square root of the mean square current in the determination of heating of motors, it may simply be said that this is a method which works fairly well.

Wm. McClellan (by letter): The engineering work in connection with railroad projects is for the purpose of settling three questions: the power-house equipment, the transmission line equipment, and the car equipment. These are in every way complex. For the first we need a daily time-power curve for the whole system; for the second a similar curve for each section of the road, the size of the sections depending on local conditions; and for the third we must consider the sections demanding greatest accelerations and prolonged heavy current supply. As a rule, no type-run will be sufficient to answer these questions. It is true, that there are some locations (chiefly between the Ohio and Mississippi) where the only variable is the distance between stations, in which type-curves might be serviceable. But in most cases, where the country is more or less rolling, such as we have in the vicinity of Philadelphia, with all kinds of combinations of curves and grades, the type-run would be of little avail.

The question, "How much tedious work is necessary?" presents itself to everyone, and the writer believes that tedious as most of us find it, the theoretical methods will prove the fittest by survival. Certainly the problems set by the electrical engineer are as complex as any set by the civil engineer, with his grades and alignment, and yet every one knows the amount of tedious work he is willing to devote. What we need now is data of all kinds. The engineers of the New York Central found this out soon after they started, and they had to get what they needed. A general method assumes the knowledge of enough data to make such generality possible; and it is for the lack of this data that frequently the theoretical method shows to poor advantage.

In this connection the writer would be interested to know upon what data the derivation of the Smith formula is based. The writer would also be interested in knowing why motor cars alone are absolutely necessary, as is intimated in the early part of the paper. It would seem entirely possible that some locomotives might be used advantageously. Many steam railroad men are now advocating a proper division of the work between motor cars and locomotives. In considering the great advantage of motor trains for much of our railroad work the peculiar field of the locomotive should not be forgotten.