Discussion.

Mr. Butter. Mr. BUTTER remarked that if he had gone into matters of mere mechanical detail, the Paper might have been more interesting, but it would not have had the permanent value which he believed it now possessed. If during the discussion any errors in the deductions should be discovered, the Paper would still have served a good purpose in bringing the matter before the Institution, because there were various questions requiring investigation, to which no answer had yet been given, and which he trusted would receive due consideration and thorough criticism at the hands of the members. He had received various communications from gentlemen who were unable to be present; and in one of them there was a misconception, which he thought it right to remove, with reference to the value of the formula $V = \frac{v\,w}{W}$, the formula for finding the velocity of recoil from the equality of momenta. He had stated in the Paper that w represented the weight of the shot, and had incidentally remarked that it was usual to include in that weight half the weight of the charge. Some persons included the whole weight of the charge; and to avoid misconception he desired to state that, although he had only alluded to it as including the weight of the projectile, yet throughout the whole of the calculations it included half the charge as well. The writer of the letter had attributed the increase of velocity of recoil in the case of pebble powder to the fact, that a larger charge was used to get the same initial velocity of the projectile than with R. L. G. powder, and therefore w possessed a higher value when pebble powder was used than in the case of R. L. G. powder. That was no doubt the case to a certain extent; but he had gone over the matter to find out what was the extent to which it would affect the question, and he had ascertained that in the case of the 10-inch gun fired with a 300-lbs. projectile, and 60 lbs. of R. L. G. powder, and with 70 lbs. of pebble powder, the respective velocities of recoil were 14·3 and 14·5, whereas by the actual curves taken from the gun the velocities were 14·3 and 16·7; so that it would be seen that the addition of the weight of the charge to the formula did not in any material sense alter the result arrived at from the actually observed curves. The only alteration in the ratio was that, instead of being as 3 to 4, it was as 3 to 3·98. Neither half the charge nor the whole charge was sufficient to account for the large increase in the velocity. It had been long suspected that the formula itself failed to give the true velocity with the change of powder, and in order to endeavour to increase the value, the whole weight of the charge (which was manifestly incorrect) had been taken, and a constant in addition. In some French publications it was stated that w should include the weight of the shot and charge, and also a certain constant; that constant he had found to be $\sqrt{\frac{4}{3}}$ from the considerations which had been set forth in the Paper. With regard to the use of the term "Elastic field gun-carriage," it was elastic only in the sense of admitting a change of form, and of absorbing force by that change of form. Of course when a spring was compressed in one direction it absorbed a certain amount of force; then it reacted and gave the force out again. The complete elastic action was a compression and a restitution; in the application of the elastic principle to the field carriage, it was only the first part of that action, the absorption of force, as in the case of a spring made of steel or india-rubber; the restitution was absent from it, and that was considered to be rather desirable than not. Mr. Butter.

Colonel SHAKESPEAR said the question of the recoil of guns should be regarded as a question of war, and not treated as if the guns were at Shoeburyness. Heavy guns recoiled up-hill on a platform, but field guns recoiled upon the bare ground, and most frequently down-hill. In regard to the former, he had no doubt that the Author was right, but he had hardly grasped the requirements of light field guns. He might be permitted to mention some incidents that had occurred in his own experience, which would show that the recoil of a field gun was a very immaterial point. Whereas a heavy gun was always on a known platform, the field gun was always on unknown ground, and generally on ground falling from the enemy. Sometimes the gun was placed on ground sloping towards the enemy, but rarely on the top of a hill where it was flat; and the recoil was much greater when the ground was declining towards the rear, and the reverse when towards the front, as then the trail was forced against the hill. A great deal also depended on the length of the trail. A horse artilleryman cared very little about the recoil of his gun, but he regarded the construction of the carriage as a material point. The late Field-Marshal Sir Hew Ross once told him a story about Norman Ramsey's Horse Artillery at Fuentes de Onoro which was practically captured. He formed his guns at close interval, and galloped through the enemy's cavalry, going over very rough ground; there was a great deal of bumping, but he got away, and no harm was done. He might also mention his own ex- Colonel Shakespear.

Colonel Shakespear. perience at the battle of the Alma. After watching the Russian cavalry all the morning, and protecting the left flank of the English army; on seeing the Russian cavalry move off, the Artillery galloped down a slope of rough ground to cross the river, going at full pace. Just before getting to the river the near wheel of the left gun caught the angle of a wall. The gun went over, and the horses and men fell down; but they all got up again unharmed. He remembered that on an occasion at Aldershot one of the leading horses was ill, and another had been put in its place. It was a wagon horse, with upright shoulders and upright pastern. At the pace they were going that horse fell and the others rolled over him, and the gun fell on its side, but all got up again unhurt. These facts showed that the material was the great thing to be considered in the construction of a Horse Artillery gun-carriage, and that the question of recoil was of no importance in the field. No doubt some field gun-carriages might be improved, but they would have to go through the same kind of rough usage as before, for war was not likely to be quieter work in future than it had hitherto been. He had spoken of Horse Artillery, as it was with that arm he was most experienced. A team of six horses was the greatest number manageable per gun, and there ought not to be more than 30 cwt. behind the six. It had occurred to him that by good mechanism there might be an advantage in taking weight out of the carriage and putting it into the gun; but 30 cwt. must not be exceeded for the horses.

Mr. Cowper. Mr. E. A. Cowper observed that although the Author had stated that a sufficient number of experiments had not yet been made to enable one to calculate in every possible way, the different strains and results, still he had gone a long way towards pointing out the right direction, in which the desired information might be ascertained, and also the way in which the strains on a field gun-carriage could be greatly reduced. He had spoken of a wave line, in place of a steady line being produced by the indicating instrument. Mr. Cowper, before commencing the use of the indicator, had taken care to ascertain, that a line drawn through the waves represented truly the same power as the waves themselves, and he had come to the same conclusion as the Author. It was merely the fluctuations of the spring of the indicator, moving with the weight of the piston, as in the ordinary balance of a watch; a certain weight vibrating with a certain strength of spring. It was a simple point, and he was glad that a right understanding had been come to upon it. He felt considerable interest in the arrangement for a "hydraulic buffer." He took, however, some exception to the term "elastic" field gun-carriage; it was more like a "dead resistance" field gun-carriage. It was, in fact, a gun-carriage with a certain amount of "give" in it. The gun could run back on the carriage a certain distance, before it made the carriage run; and however strong the gun-carriage might be made, it would be less strained by the gun in its recoil, if a certain amount of dead resistance was opposed to the gun before it struck the carriage. He did not find in the Paper the name of the inventor who first brought forward the "hydraulic buffer." Dr. Siemens was consulted some time ago by the authorities as to the best mode of causing a resistance, and he suggested that the best way would be, to put in a hydraulic buffer. Mr. Cowper took the more interest in the subject because many years ago the same idea had occurred to him in reference to a totally different matter. In 1846 a bad railway accident occurred from a train running into a station buffer. He then schemed a buffer to allow 9 or 10 feet recoil, with a water cylinder and piston to run back in the same way. Dr. Siemens, however, knew nothing of the plan he had proposed. The resistance, of course, could be increased, as had been clearly pointed out in the Paper, by the addition of taper rods through the piston, so that, as the piston went home, the area through the holes was reduced, because the rod, being larger, occupied more space. The resistance was kept up throughout the whole length of the stroke, although the stroke was much quicker at the commencement and slower at the end. In that way more work was done in the same length. If complete experiments were made to ascertain all the facts affecting the recoil of guns, he should like much to analyse them, and see if the result of all the forces, after proper deductions had been made, agreed with the results obtained. He would ask the Author to furnish such results to the Institution if they should be obtained, for he thought that further experiments were necessary, and he felt sure that they would now be made in the cause of science and the practical interests of the country. Several data were wanting. One of them was the gas-pressure acting inside the gun against the breech, until such pressure had died away. He wanted to see the line continued until it showed no pressure in the gun. It was certain that after the shot had left the gun, the gun acted as a rocket; there was an enormous quantity of dense compressed gas equal to the weight of the powder inside, which issued from the muzzle with great force, and continued to press back and cause an acceleration of speed in the recoil. That force had not yet been calculated. Then there was a deduction to be made on account

Mr. Cowper.

Mr. Cowper. of the friction of the powder-gas against the sides of the bore of the gun while driving the shot, and while escaping after the shot was out; that was a friction tending to urge the gun forward, and not backward. Then there was the friction of the gas check, also tending to urge the gun forward. The gas check was driven against the bottom of the shot or shell with considerable force, and there was a great deal of friction. Then there was the friction of the shot itself while rushing along, also tending to pull the gun forward. There was further the pressure required to set up the quick rotation of a shot in rifled ordnance, due to the *vis inertia* of the shot, and the friction caused thereby against the grooves in the gun, which also tended to pull the gun forward. The last, he was persuaded, amounted to a considerable deduction from the force tending to cause recoil. He might mention that, having had to make the working drawings of the first rifling machine constructed, for boring out the first Lancaster gun, according to Lancaster's design, he saw the evil that might result from giving too much pressure against the grooves, particularly if the rifling was with an increasing pitch. He felt a great interest in the gun, and took a fortnight to bore it out. In practice it was found that from the rapid twist given to the shot towards the muzzle of the gun, some damage was done to the muzzle from the tearing action of the shot going out; that showed the enormous friction against the sides of the gun, which should be taken as a reduction from the total recoil. It was well known that plugs inserted into the shot to take the grooves in the gun were sometimes torn off, and this also proved the severity of the strain. He agreed with the Author that simply gauging the greatest pressure could not be correct, and that it was necessary to take into account the pressure acting against the inside of the breech for each instant, until the whole force ceased. The total of all those efforts, minus the efforts to produce a forward motion, would be the amount of force causing the recoil. In conclusion, he might say that a most interesting experiment would be, to separate the forces tending to pull the gun forward, from those tending to cause recoil, and that might be done by separating the breech from the bore of the gun, by making the breech-piece like a piston, to fit inside the bore, the piece having as much weight behind it as the total weight of the gun; the friction of the shot, gas check, &c., would all act on the bore of the gun to pull it forward to a certain extent, and the motion could be measured. There was another point that he should like to see brought out, namely, the exact times of the pressures in the gun, compared with the exact times of recoil.

He could not help taking a little exception to the apparatus for registering the recoil as presenting some chances of shake and elasticity. He should like to see a hole drilled in the trunnion itself, aud the pencil placed in it, and the paper placed on the cylinder opposite to it, so that there should be no elasticity between the paper and the gun. He should like to see the powder-pressure registered accurately for time on the same paper, or another paper geared to it, to learn precisely what the pressure was, at each instant of time, in comparison with the exact time of recoil. He could not think that the gun ever moved instantaneously at a high speed; it must, he thought, show a curve on a properly constructed instrument for the starting, as well as the stopping, of the recoil. In that way it might be found out exactly when the recoil commenced. He thought that the gun began to move, as soon as the gas-pressure inside it was equal to the friction of the moving mass. The exact times of the pressures, and of the shot passing the "pressure-plugs," could be registered by pencils stationed over the paper cylinder, and acted upon electrically, from wires reaching down inside the pressure-plugs to very near their ends, so that a slight motion for "contact" could be got from the wires. Mr. Cowper.

Professor UNWIN said the Paper contained a vast number of extremely interesting facts which the members might all accept whether they agreed with the Author's *rationale* or not. The facts had been observed in circumstances of great difficulty, and apparently with great care; and the Paper certainly showed that in the development of artillery at Woolwich, valuable observations had been made. But there could be no doubt that some statements in the Paper appeared to put in question the equality of action and reaction; and if that had to be given up, he was afraid that a good deal of mathematics would have to be re-written. He was disposed to accept every fact that the Author had stated as being accurately recorded, without, however, accepting his explanation. The Author had pointed out that the velocity of the recoil of the gun had been calculated by assuming the equality of the forward momentum given to a shot, and the backward momentum given to the gun. According to the formula, Prof. Unwin.

$$W\ V = w\ v,$$

and that result appeared to be based upon the third law of motion. In the interpretation of that, the whole question to be determined was what value was to be assigned to the symbols. With regard to the two V's there could be no doubt; but about the two W's

Prof. Unwin. there might be a difference of opinion. All that the law of equality of momenta taught was that the whole forward momentum given to the shot, to the powder charge, and to the air surrounding the shot must be equal to the whole of the backward momentum given to the gun, to the gun-carriage, and to any bodies which were put into motion backwards; and the equation would not be rightly formed unless there were introduced on the one side, in addition to the weight of the shot, that portion of momentum which belonged to bodies accompanying the shot in forward motion; and, on the other side, not only the weight of the gun, but the weight of those bodies which accompanied the gun in backward motion. Taking the case of a ballistic pendulum, the gun was so suspended that in that case the conditions were most nearly approached in which W might be taken to be the weight of the gun alone, and w the weight of the shot alone. Another extreme case would be obtained if a gun were supposed fixed absolutely rigidly by a sufficiently large block to the earth. In that case, the shot would have the same forward momentum as before; and there would, apparently, be no backward momentum at all. But, of course, the backward momentum would have to be calculated by making W equal to the whole mass of the earth, or, at all events, to a large portion of it. The actual case of a gun and a carriage was intermediate between those two cases, the W was not simply the weight of the gun, but the weight of the gun *plus* certain weights which, in the ordinary use of the equation, had not been introduced into it.

The gun-carriage by its frictional connection with the ground imparted some momentum to the earth while acquiring its maximum velocity of recoil, and on the other hand the shot and powder-gas escaping from the gun gave forward momentum to a mass of air, the weight of which was not negligable in comparison with the weight of the shot. Probably the larger volume of powder-gas in the case of pebble powder, and its slower escape from the gun, caused a larger mass of air to be put into forward motion, and thus the greater recoil with pebble powder might be explained. It might be pointed out that the equality of the forward and backward momentum was independent of all such mutual actions between the shot and the gun, as the friction of the shot on the gun, or the pressure against the rifling. Those actions were balanced. The forward drag of the shot on the gun was equal to the backward drag of the gun on the shot, and the forward and backward momentum were equally affected.

Dr. Siemens. Dr. Siemens thoroughly agreed with the mathematical proposition put forward by Professor Unwin, which, indeed, admitted of no doubt. At the same time, as Mr. Cowper had already pointed out, there were great deductions to be made. All the friction had to go in reduction of recoil, and that friction must necessarily be largest at the commencement of the action when the charge was rammed tight home. Then, again, the friction of the gun-carriage upon the ground might be very considerable, and that had to go in reduction; so that theory and practice, as propounded, and so well argued by the Author, seemed to agree nearly enough for general acceptance. He should like to add a word with regard to an observation from Mr. Cowper regarding his connection with the question of hydraulic compressors. All that Dr. Siemens could claim was the mere suggestion of hydraulic compression for gun-carriages, and that had been gracefully acknowledged by the then head of the department (Colonel, now Sir Andrew, Clarke, K.C.M.G., C.B., Assoc. Inst. C.E.), in a Paper, read about a year after the suggestion was made, before the British Association. The fact of his suggestion, however, in no way detracted from the great merit due to the officers of Woolwich, and especially to the Author, for the thorough way in which the hydraulic pressure had been worked out for stationary guns, and had been now brought forward as applicable to field guns. He could not help thinking that the term "elastic gun" was unfortunate, because it gave a wrong idea. Although the Author had explained that it meant only one portion of the elastic action without the elastic rebound, it was essentially an inappropriate name. One question which presented itself was whether the hydraulic compressor applied to a field-gun was a complication. It was, unquestionably an additional part; but he did not think that every additional part to a machine meant a complication. If by the addition of 1 cwt. 1 ton could be saved without subjecting any portion of the material to a greater strain, that really meant simplification; an effect was produced with less weight of machinery. The hydraulic cylinder with a single piston was an extremely simple mechanism. No valves were connected with it; there was nothing that could possibly take harm; and he believed that Colonel Shakspear would have found that a gun of that description would not come to any harm from the rough usage to which he had alluded; and after it had fallen would have been more readily put on its wheels again than the older form. The Author's proposal was a move essentially in the right direction—the lightening the material of the field-gun—and he thought it was done without complication, if complication meant liability to get out of order. Dr. Siemens.

Rev. JAMES WHITE, M.A. (Head Master of the Royal Naval Rev. J. White.

Rev. J. White. School), thought that the effect of the air had been completely taken into consideration by the manner in which the results had been obtained—from observing the velocity of the recoil and the velocity of the shot. In considering those elements account had been taken of the effect of the air, both upon the carriage and the shot, at least as far as the force of velocity was exerted, for the force of velocity was determined by the medium through which the projectile passed and also through which the carriage recoiled. He did not think that, apart from the effect of velocity, much effect was produced by the explosion of powder upon the air, at least not much effect in producing a recoil upon the gun. He made those observations for the following reasons. He had witnessed many of the experiments made with Colonel Moncrieff's gun-carriage. That carriage, which was remarkable in many ways, was also particularly interesting as exhibiting many new conditions. He referred to the counterweight-carriage and the hydro-pneumatic carriage. In both, especially in the counterweight-carriage, there was no resistance proper to the recoil at first, or almost none; the centre of gravity of the whole mass hardly moved at the first motion of the gun. That was one reason why it was unsuccessful in solving the problem which had been attempted frequently before without success, namely, that of meeting recoil without dangerous results. At the first motion of the gun there was hardly any resistance; the centre of gravity of the whole mass was hardly moved, and the counterweight merely got a very slight horizontal motion. It was only when the recoil advanced that the counterweight lifted, and the centre of gravity of the whole mass altered. When the gun was fired with a blank cartridge there was very little recoil indeed, the gun hardly moved, and from that it seemed clear that the action of the exploding gas upon the air produced but a very small effect. Therefore the effect of the air and the gas upon it might be considered as accounted for in the velocity which resulted both in the shot and in the gun; he did not imagine that any other estimate of it was required. He might also observe that the expanding gas, when there was a blank charge, produced very little recoil, and the reason seemed clear; the gas, when it expanded, did not meet with a large mass of some resisting medium, but met with a highly elastic medium—the air, and it diffused itself through the air. Therefore he did not think that the resistance or the effect of the air need be taken further into consideration in estimating the velocities of the gun in its recoil and of the projectile in its flight. Gun-cotton, although it strained the gun heavily, produced but very slight recoil, because its action was so rapid. In Colonel Moncrieff's carriage hardly any recoil was produced, not sufficient to carry out experiments to show what the effect would be when gunpowder was used. The action of the exploding gunpowder upon the air was not really the action developed in a rocket; that was brought about slowly, and therefore it produced a continual push, as it were, to send the rocket forward. That was not the case in the explosion of a cartridge in a gun. The gas in the rocket was produced during a good number of seconds of the rocket's flight, and the time of moving the projectile was estimated in ten-thousandths of a second, so that there was a very great difference between the two. He thought that in estimating the effects of recoil all that was necessary with regard to the air was to take into consideration the velocity imparted to the shot and the velocity of the recoil of the gun. It appeared to him also that the important question of the laws of motion was hardly affected by the subject under consideration. The great law of motion—that action and reaction were equal—was not touched. The action of the gunpowder upon the shot gave a certain momentum, but it did not follow that the effect upon the gun was also a corresponding momentum, because the reaction had other things to do besides producing the motion of a certain mass; it had to heat a much larger body than the shot, and some of the reaction was expended in that way. Other molecular changes might be brought about. No doubt action and reaction were equal; but it appeared that the subject had been dealt with in the Paper entirely from the point of observation, and that the great laws of motion, especially the important law of action and reaction, were not touched by the question under discussion. There seemed to be some confusion with regard to the expressions "statical" and "dynamical" force. The two things were very different; but in touching those subjects, it should be remembered that these things were taken as they were in idea, and not as they were in fact. Though statical and dynamical force were very dissimilar in idea, sometimes in practice there was a border-line between them which it was not easy to distinguish. For example, if a weight was laid upon a spring, that was a statical force, pressure; yet, however gently the weight might be laid upon the spring, there was always something of a dynamical effect produced—the spring would always go a little below the point at which it would remain. Sometimes the force, quasi-statical, involved a dynamical force operating very slowly. It was a confusion in terms; it did not necessarily imply confusion in ideas. Rev. J. White.

Rev. J. White. Persons who taught mathematics were apt to forget that they were dealing with ideal conditions, and that practically men might make use of terms which to teachers might seem confused, but which did not necessarily imply a confusion of ideas on the part of persons using them. Although it was desirable that language should be always correct, and it should be remembered that statical and dynamical forces were distinct, it was almost as important for professors to remember that in teaching mechanics especially, they were dealing with conditions that never actually existed. For example, every manual of mechanics began by informing the learner that a bar was spoken of as perfectly rigid, and that there was no such bar in nature as that which he was desired to imagine.

Mr. Bousfield. Mr. W. R. Bousfield said that his criticisms might be considered a little uncharitable in view of the great practical value of the Author's Paper; but he could not help thinking, in regard to the theory of the subject, that the Author had gone astray. The Author had explained how the powder acted on the shot. No doubt it produced a compression on the base, as he had stated, and no doubt also, it produced a permanent compression in some cases if the pressure were very great; but he could not agree with what the Author said in speaking of the action of the powder-pressure upon the gun. He imagined it would be very difficult to ascertain in practice at what point motion was set up. He also demurred to the way in which the Author had spoken of overcoming inertia. He spoke of time being an element in overcoming inertia. Of course, it was an essential element; but in the sense meant by the Author he thought it was not essential. Take the case of a constant force acting upon a certain mass. The overcoming of inertia meant the setting up a certain velocity in the mass. Whether the first ten-thousandth of a second were taken or the ten-thousandth of a second after the force had been acting two or three minutes, the amount of velocity set up in that short interval of time was exactly the same; the effect of overcoming inertia was exactly the same in the same interval at whatever point of time it was taken. Inertia (if the word had any scientific meaning—which he much doubted) was no greater when the force began to act than when it continued to act. It was a simple question of being able to perceive the motion which was set up. He thought that the way in which the overcoming of inertia had been spoken of rather tended to lead astray than to give any true theoretical view of the subject. Of course, in considering the impact of an inelastic body, mathematicians divided the time of the impact into two periods, the period of compression and the period of restitution; but, in this case, the two bodies were close together and re-acting on one another. The Author appeared to have taken some formula, or principle of equality of momentum, (a principle which did not exist in a scientific point of view), and did not find it fit in with the facts before him; he had therefore adopted the theory that the velocity of the gun did not begin to be set up during the first part of the powder action; but that the effect of the powder-pressure was simply to stretch the gun longitudinally, and that the velocity of the gun was due to the effect of this longitudinal stretching after the shot had left the gun, and all external forces had ceased to act. That was equivalent to saying that a system of particles with various strains acting between them could be moved as a whole by the inter-action of these forces inside the system of particles. This was a startling proposition to any one acquainted with the fundamental principles of mechanics, that the recoil of a gun could be due to some state of strain which existed in it, and that that state of strain could set up a velocity independently of any external force. He did not think that in order to explain away an unscientific formula and its discrepancies, mathematicians were called upon to accept any theory of that kind, which was far more unscientific than the formula itself. What was the true way of looking at the subject? Instead of going back upon some principle of equality of momenta or equality of energies (of which he knew nothing), or some principle of inertia, it would be better to fall back upon the laws of motion referred to by Mr. White, which were sufficient to account for all the facts observed. Newton's second law of motion was that change of momentum was proportional to the acting force. No doubt the principle of equality of momenta might be supposed to be an expression of that second law; but it was nothing of the kind. He thought that the Author had left out of consideration two or three most important elements which entered into the subject. One reason why the formula taken by the Author was not correct was that the effect of the resistance of the ground, or of a trunnion, or of whatever resistance might be acting upon the gun, had not been considered. Secondly, the Author had only taken into account (in the case where he had taken the weight of the gun as being one hundred and thirty-four times the weight of the shot) the weight of the gun itself and not the weight of the carriage. Thirdly, he had neglected the effect of the discharge of the gaseous contents of the gun, which Mr. White thought might rightly be neglected, but which Mr. Bousfield thought ought to be taken into consideration. The time during which the forces were acting ought to be divided into Mr. Bousfield.

Mr. Bousfield. two periods: first, the period while the shot was in the gun up to the point when it left the muzzle, and next the period after the shot had left the gun; for he believed that while the gun was being emptied of gases there was a considerable effect. Taking the expression of the second law of motion, that change of momentum was proportional to the acting force, what was the change of momentum? Taking the direction of motion of the shot as the positive direction, the change of momentum during the time the shot was travelling from the breech to the muzzle would be

$$m\,v - M\,V,$$

where m M were the masses of the shot and gun respectively, and v V their respective velocities at the moment the shot left the muzzle. The so-called principle of equality of momenta assumed that $m\ v\ -\ M\ V\ =\ 0$; but Newton's second law said that this expression, which was the change of momentum, was proportional to the whole forces acting during the time. By a simple application of that law the following equation therefore was obtained:—

$$m\,v - M\,V = \int_{t_0}^{t_1} (R - S)\,\delta\,t,$$

where R was the resistance acting upon the trunnions, and S the resistance of the air to the shot, whilst in the bore of the gun; t_0, t_1 being the respective times of the explosion and of the shot emerging. When the shot had left the gun, the motion of the gun was given by the formula—

$$M\frac{d^2 x}{d\,t^2} = P - R,$$

P being the pressure on the breech due to the reaction of the escaping gases. Thus the time when the gun had its maximum velocity was determined by putting

$$\frac{d^2 x}{d\,t^2} = o\,;$$

in other words, it was a maximum when R, the resistance on the trunnion, was equal to P, the internal pressure on the breech, of the escaping gases. After this the velocity of recoil diminished, and was given at any future time t by the equation

$$M\frac{d\,x}{d\,t} = M\,V + \int_{t_1}^{t} (P - R)\,\delta\,t.$$

If the resistance S and P of the air were neglected, then the velocity V at any time t would be given by

$$m\,v - M\,V = \int_{t_0}^{t} R\,\delta\,t.$$

Since the area of the curve obtained by the Author from the pressures of his hydraulic buffer was the $\int R\,\delta t$, obviously the last formula could be applied as an approximation, which he ventured to think would be nearer and more generally applicable than putting in the arbitrary factor $\frac{3}{4}$ suggested by the Author. Mr. Bousfield.

In that way, without upsetting the theory of conservation of energy and other theories, the facts could be accounted for on ordinary mechanical principles without supposing that a system of particles acted on by no external force, but simply by internal strains, could generate motion on its own account. He might be permitted to make an observation on the form of the cylinder which the Author used. He thought it might be advantageous, instead of having holes in the piston with gradually tapering rods put into them in order to regulate the pressure as the gun went backwards, to have a return tube for the water alongside the hydraulic cylinder, with a series of holes, along the cylinder into this return tube, which would be gradually closed as the piston receded.

In accounting for the difference of recoil with R. L. G. powder and with pebble powder, the Author stated that, with slow burning powder, the recoil was perhaps a third greater. The Author had stated that the R. L. G. quick-burning powder set up a molecular action, and that a great deal was wasted in that way. No doubt, as Mr. White had stated, much of the energy which might go into the recoil was used up as heat. Of course there was a greater compression of the gun than of the shot, and compression always meant, to a certain extent, in dealing with elastic bodies, generation of heat, and this would take place to a greater extent on the large mass of the gun than it would on the shot; that, however, was small, and might be left out of the account. He believed that the difference in the times was due to the fact that the pebble-powder was not completely consumed at the time the shot left the gun, and that the result was due to the second of the two periods to which he had referred. A considerable quantity of the powder not having been burnt when the shot left the gun, it still remained to be burned, and it was the additional action due to this burning after the shot had left the gun that set up the additional recoil. The weights of the powder used in the Author's experiments, 60 and 70 lbs. respectively, would account for it to a great extent, because the energy communicated to the shot being the same in both cases, Mr. Bousfield. it followed that a larger energy would be communicated to the gun in the latter case. As to the practical value of the Paper there could be no doubt. He had only ventured to remark upon the theory of the question, because he thought progress was impeded when a wrong theoretical view was taken.

Commander Dawson. Commander W. DAWSON, R.N., from what he could make out in the Paper, considered that the forces and strains on the carriage were to be measured by the length and time of the recoil. It appeared to him that the reverse of that proposition was true—that the strains and forces of recoil upon the carriage were in an inverse ratio to the length and time of recoil. If two railway trucks were placed on two pairs of rails, and if one was struck with such a sudden impact as to shiver the structure, and the other was pushed gently with a force the sum of which would equal the impact, he submitted that the recoil of the one which was pushed gently would be longer by many hundred yards than the recoil of the other which had been struck by a sudden impact so as to injure the carriage. In which case were the greatest forces and strains on the wagon? Was it not on the one that had been struck sharply and had a short recoil, and not upon the one which had a long recoil? It was a great loss to the country, and to individuals, that seamen who had to use carriages and guns were brought so little in contact with engineers who made them, and from whom they might learn a good deal, especially in the matter of taking observations. Interesting results often passed before their eyes which, for want of accuracy of observation, were of no value to the world at large nor to themselves. If they were taught to make accurate observations they might perhaps be able to give engineers more useful hints than they could give at present. It had been his lot to have the responsible charge of firing nine thousand or ten thousand shot, not at Shoeburyness or Woolwich, but under more trying circumstances on board ship. He had seen accidents occur, carriages disabled, platforms disabled, the deck beams and planks injured, and the like, and he had observed that when those accidents occurred there was generally a very short, not a long recoil. There were of course occasions in which the recoil was long, and in which the tackle of the gun gave way; but in the majority of cases when the carriages or decks were injured the recoil was short. That, he believed, was due to the degree of violence with which the gunpowder acted. The same charge of gunpowder did not always produce the same amount of recoil; it depended upon varying circumstances, such as the condition of the atmosphere, and the number of charges

Commander Dawson.

that had been discharged from the gun during the same day—in other words, upon the heat of the gun. If the gun were warm the gunpowder acted very violently and the recoil was greater. Again, if the gun were elevated a few degrees it would be found that the strains and forces of recoil were much greater than when the gun was fired horizontally. If the platform opposed much friction, or was on a plane inclined upwards at the rear of the gun, the force of recoil would be greater, the strains upon the carriage would be greater, and when the carriage gave way, it was generally all those circumstances acting in conjunction over and above the ordinary action of gunpowder that brought about the result. Every sportsman knew that in discharging his fowling-piece rapidly it became heated, and kicked more violently, and if he fired it in an elevated position his shoulder felt the recoil more than when he fired horizontally. There had been some recent experiments in reference to recoil, of which he had hoped to find details in the Paper, but the subject remained, for all practical purposes, uninvestigated; and that was his apology for giving his experience as a seaman. The statement in the Paper to which he took exception was, "That slow-burning powders, by acting more gradually in overcoming the inertia of the gun than quick-burning powders, induce a greater energy of recoil, in the proportion of the longer intervals of time during which they act upon the gun; and this notwithstanding that the energies imparted to the projectiles are the same." The remarks of Mr. White as to gun-cotton completely answered that statement. Gun-cotton was quick in combustion, and the recoil was small. The power that it exerted was tremendous, so that the recoil could not be taken as a measure of the straining or force which the gun-carriage had to bear. With regard to the practical proposal of the Author, there could be no question as to its value; but he hoped that engineers would take into consideration the difficulties that had to be encountered, and remember that they were not always playing at soldiering in the marshes at Woolwich. Whatever elastic buffer might be used, its strength should be computed, not with reference to a cold gun fired horizontally, but to its action when fired in a heated condition, at a high elevation, or with an inclined plane under the base of the carriage. It had been suggested that a spare buffer might be carried, in case of failure of the first; but he should prefer making the first buffer so strong that it would not get out of order.

Mr. Walker.

Mr. B. WALKER said the first object of the Paper appeared to be to bring before the members a new arrangement of field gun, which

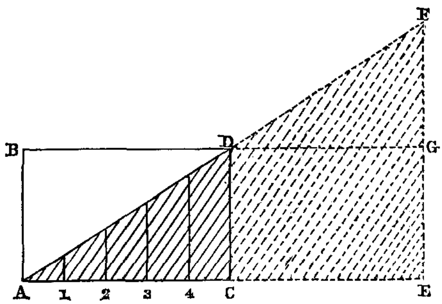
Mr. Walker. the Author called an elastic field gun, and which was to weigh less than field guns had hitherto weighed, to cost less, and to be less liable to get out of order. He thought the carriage illustrated in Fig. 8 was an admirable specimen, and likely to do good service. It had just been suggested that the buffers should be made so strong that a spare one would not be wanted. No doubt that would be very desirable, if it were possible to make a mechanical instrument that would never go wrong, but it would be a very difficult problem. Dr. Siemens had claimed to be the first to mention to the authorities of the Carriage Department the use of water as a means of controlling the violent action of gunpowder. The Department had wisely asked his advice, and had suggested the use of galvanism or electricity. Dr. Siemens told them that he had too much knowledge of the uncertainty of electricity and galvanism to recommend its adoption, and he suggested water as a means of moderating the action of gunpowder. It would be well within Dr. Siemens' knowledge at that time that water had been used to check the violent jerky action of large steam valves, by allowing the water to pass through a specified orifice, the same water being used to control the closing of the valve by letting it go through another orifice in a definite time. He had no doubt the Author was also well acquainted with a contrivance called a cataract motion. There was another contrivance, namely, two or three pumps working into a vessel, the water passing out of a small definite hole. If the pumps went too fast the pressure was increased, and by that means the throttle valve of the steam engine was closed. The Author and Dr. Siemens would no doubt both have the notion that the water would have to pass through a specified orifice, and so regulate or absorb the violent jerky action of the gun. It would be difficult to imagine any contrivance in which the parts were less liable to get out of order, or in which the arrangement was more simple and ingenious. He had long been familiar with the fact that in using a hydraulic pump, or any means for getting pressure, a vibratory action was shown by the indicator, such as that referred to by the Author, but he had never before seen the matter brought out so simply and plainly. Mr. Walker had frequently noticed when testing boilers or hydraulic cylinders, that a slight movement of the hand-pump gave a vibratory motion to the pressure-gauge, sending it above the pressure that could possibly be reached in the cylinder, and likewise depressing it an equal distance below. He had himself followed the practice of shutting the valve between the pump and the pressure-gauge to reduce the vibratory action on the pressure-gauge. He had found, over and over again, that he did not get all the pressure that he should have got according to the indicator card, and he believed that some light was thrown upon the question by the facts mentioned by the Author. Mr. Walker

Mr. W. E. RICH said he had studied the question of recoil and how best to resist it some years ago, when engaged on designs for Moncrieff hydro-pneumatic gun-carriages made by his firm for the English Government, and also some 40-ton gun-carriages on the hydraulic disappearing principle for Russia. He thought that the recoil of guns should be taken up by a resistance very small at first, and increasing rapidly. The resistance which took up recoil in ordinary guns might be considered practically uniform, disregarding the resistance of the molecules and particles of the gun itself. Clarke's system of hydraulic buffers seemed to him, in its usual application, to be subversive of the principles that ought to govern such designs, inasmuch as the resistance was enormous to begin with, and rapidly fell with a hollow curve to a small amount towards the end. In the Russian carriages of which he had spoken, the recoil was taken up by loading a safety valve with springs increasing the pressure as the gun went down, and in the hydro-pneumatic Moncrieff carriages the recoil was taken up by a pressure increased by the compression of the air in a cylinder partly filled with air and partly with water. Mr. Rich.

Sir FREDERICK BRAMWELL, Vice-President, remarked that many subjects had been brought forward in the Paper into which he was not prepared to enter, but there was one point upon which he should like to speak, and that was upon the question of the possible strain on the trunnion of a gun. He must confess that he at one time entertained the opinion that in no case could that strain possibly exceed that due to the area of the bore multiplied by the maximum pressure per square inch, and he had suggested as a test that if a gun had a bore of, say, 100 inches area, and a maximum pressure of, say, 17 tons per inch, then if that gun was placed over a well with its mouth downwards, and its trunnions were loaded with a weight which, added to that of the gun, exceeded in the least the 1,700 tons of pressure on the whole area of the bore, the gun could not rise; or in other words, that it was clear the maximum strain on the trunnion could not exceed in the case supposed the 1,700 tons. Further consideration of the matter had, however, led him to the conclusion that this strain might be considerably exceeded, and that, in a certain assumed event, which, however, could never happen in practice, the strain might be doubled; and in order to show the principle he had in his mind he Sir F. Bramwell.

Sir F. Bramwell. would ask the members to consider the assumed instance, which was that the maximum powder-pressure might be generated all at once without requiring any appreciable interval of time before that pressure was reached, and, secondly, that it might remain constant for the brief period during which the stretching (of which he was about to speak) of the material of the gun longitudinally between the base of the bore and the trunnion was going on.

Fig. 1.

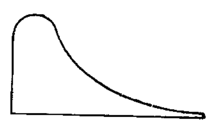


Let the vertical line A B in the Figure represent the maximum powder-pressure, and continuing uniformly during the whole time that the elastic material of the gun was extending say through A C to the point C, where it was to be assumed it would be capable of resisting a strain equal to A B, then the work done by the pressure would be represented by the parallelogram A B D C; but the work expended in the elastic material, if, as was assumed, the resistance offered to stretching increased regularly as shown by the vertical lines 1, 2, 3, 4 would only be that represented by the cross-lined triangle A C D, or one half the work done; the stretching of the material must therefore be continued until the area of the triangle representing the work of resistance of the spring was equal to that of the parallelogram representing the work done by the pressure on the bore moving through the space allowed by the yielding of the material, and this would obviously be when the strain upon the material was double that upon the bore as shown by the vertical line E G F, for not till then would the triangle A F E be equal to the parallelogram A B G E. He was well aware that in practice no such double strain would arise. The pressure was not generated absolutely instantaneously, the trunnions were not rigidly fixed, and the weight of the elastic material set in motion had to be considered; but nevertheless he thought he had stated enough to show how it was possible that the strain produced

Sir F. Bramwell.

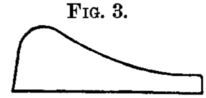
by the powder on the bore of the gun might be exceeded at the trunnion. With respect to one of the main points brought forward by the Author, viz., that with large grain powder it appeared there was a greater velocity of recoil than with pebble powder, the velocity of the shot being the same, he wished to remark that, as the Author had stated, there was about one-sixth more pebble than the R. L. G. powder employed, and that this in itself accounted for some of the difference; but irrespective of that he should be inclined to adopt Mr. Cowper's statement as to the effect of the gases in the gun escaping after the shot had left. It did not appear to him that the Author had given due value to that effect. He did not agree with Mr. Cowper as to the deductions to be made in the way of friction, because it appeared to him that friction equally operated in retarding the outgoing of the shot, and that when comparing the velocity of the shot into its weight, and the velocity of the gun with its carriage into their weight, it would be seen that friction had already been taken into account in both, and that therefore the deductions mentioned by Mr. Cowper ought not to be made. But he thought he was right in saying that when the R. L. G. powder was used, with rapid combustion, a pressure was generated in the gun greater than that generated by the combustion of pebble powder, but not lasting so long and having a low terminal pressure. In a diagram the R. L. G. powder might be represented by a curve, Fig. 2, and the pebble powder by a curve, Fig. 3. Where pebble powder was used there was, he thought, a larger pressure remaining in the gun at the time the shot left it, than there was at the corresponding time in the case of large grain powder. If that were so, ought there not properly to be taken into account, as one of the elements in causing the recoil of the carriage, the weight of the gases in the gun escaping after the shot had left with the enormous velocity due to the ultimate pressure of some 2 tons to the square inch? If that velocity was taken into account, and multiplied into the weight of the powder, would not that be quite sufficient to account for the extra recoil or velocity of the gun, when pebble powder was used, over and above the recoil that prevailed when rifle large grain gunpowder was used? He exhibited a model bearing on the subject—a gun weighing five times the weight of the shot. The shot and the gun were both mounted on wheels; there was a revolving cylinder alongside, and

FIG. 2.



R. L. G. Powder.

FIG. 3.



Pebble Powder.

Sir F. Bramwell. each traced its recoil upon the cylinder. He had before him two papers, the curves on which had been drawn by the action of the apparatus, but as they had only been obtained just before the meeting, he had not had time to examine them, and did not feel justified in drawing any conclusion from the records they afforded.

Mr. Barlow. Mr. W. H. BARLOW, Past-President, felt a little difficulty in reconciling the observations of Sir Frederick Bramwell with the intelligent remarks made by Mr. White. It was difficult to understand how altering the position of the trunnions of a gun could alter very materially the action of the gun upon its trunnions. It appeared to him, looking at the subject of the Paper generally, that if the endeavour was made to treat it mathematically, it became so minute in its conceptions and so difficult to follow, that he should prefer to be guided by the results of experiments rather than by the results of theory. He had been born and bred in a mathematical atmosphere, and he had seen the sinuosities of mind which were required to follow out calculations of that kind, and conditions came on which he might term a mathematical haze. He really thought that when mathematicians attempted to enter into those very fine points, and to measure time by millionths of a second, they were getting rather lost. Instead of entering into any arguments on theory, he should be prepared to see what would be the result of experiments on the Author's gun-carriage. It was quite new, and it involved principles that he did not think any one could exactly follow out at the moment, and he therefore preferred to wait till the Author had submitted it to experiments before determining what the results of those experiments would be.

Mr. Bousfield. Mr. BOUSFIELD said he had not intended to advance a complete mathematical theory, but only wished to show that, by omitting one side of the equation, the Author had been obliged to elaborate some molecular theory to account for the absence of it; and that, by putting in the other side of the equation, the facts might be accounted for by the second law of motion without going into any such theory.

Mr. Barlow. Mr. W. H. BARLOW replied that it appeared to him that Mr. Bousfield had treated the gun as being perfectly rigid, whereas, in fact, it was elastic.

Mr. Butter. Mr. BUTTER, in reply, said the discussion had taken two distinct lines: one, adopted by engineers, who were also mathematicians, was entirely practical, in exact conformity with the spirit of the Paper, and such as might be expected from those whose daily experience brought them into direct contact with the conditions governing action and reaction; while the mathematicians who had favoured him with their critical observations (except Mr. White) had insisted upon the sufficiency of mathematical expressions to meet the case, ignoring the difference between actual and ideal conditions; he had taken especial care to point out that the question was not ripe for exact mathematical investigation on account of the absence of sufficiently accurate observations, and on this account he had not wasted time nor confused the subject by introducing any of the higher modes of calculation. By purely empirical methods, embracing the results of reliable observation, and founded on the formula for the equality of momenta, which for generations had been used for finding the velocity of recoil, he had constructed a simple expression for practical use, of the sufficient accuracy of which for practical purposes he had no doubt. From the interest shown in the Paper it was manifest that another object he had in view would be attained, namely, the directing attention so forcibly to the subject as must lead to observations being made which would place the question in a much more satisfactory condition than he had been able to do from the poverty of his resources. Mr. Butter.

He would first direct attention to what he would call the "practical line," and afterwards deal with the "theoretical" one.

He must acknowledge Mr. Cowper's ready acceptance of his experimental results of the time and space values of pressure diagrams; these he knew quite well brought no new facts to light, but he was not aware that they had been so conclusively demonstrated by experiment. He was also gratified that Sir F. Bramwell and Mr. Cowper agreed with him that the maximum pressure of recoil might under certain conditions exceed the maximum powder-pressure on the bottom of the bore, and that the latter gentleman had stated his belief in the gradual increase to its maximum of the velocity of recoil—a fact which Mr. Butter believed had not been brought forward before. With respect to the invention of the hydraulic buffer, Dr. Siemens had undoubtedly suggested the idea of using fluids, and his thanks were due to him for the terms in which he had alluded to this part of the subject. Dr. Siemens had taken exception to the title adopted for the new carriage, but when it was first designed it had a steel spring inside the buffer the object of which was to restore the gun into the firing position, and thus the complete action was truly elastic. This spring might yet be found to be necessary, but if the reaction of the body of the carriage, assisted by gravity, was found to be sufficient to restore the gun into its original position, it would be better to dispense with the spring. On a due consideration of the

Mr. Butter. subject it would be found that no single word could be selected which would better express the object of the apparatus, which was to give that "yield" to the carriage, and by other means restore the "form," which were the peculiar features of elasticity. Colonel Shakespear and Captain Dawson had somewhat mistaken the sense in which the length of recoil was alluded to in the Paper. The carriage was not expected to affect the length of recoil whatever, but was designed to reduce the strain. Allusion had been made by Mr. Rich to a feature of the simple and the original form of the hydraulic buffer—the great pressure at the beginning of recoil—as being "subversive of the principles that ought to govern such designs." He was apparently not aware that one of the two buffers first constructed was made with taper bars to gradually contract the orifices on recoil, and to maintain an equal resistance; but this form was found to be less suited to the conditions of recoil of ordinary garrison carriages than the simple one on account of the higher pressure at the end of the recoil causing a jump of the front of the platform, and the buffer, which gave great resistance at the commencement of recoil, when the position of the gun over the front of the platform was most favourable to preserving the stability of the mounting, was adopted as being the most suitable. It must be remembered that the conditions of ordinary carriages and platforms were different from those associated with the Moncrieff and similar carriages, which were held securely from rising at the front ends. With regard to the theoretical line of argument, Mr. Barlow had justly remarked that "it appeared to him that Mr. Bousfield had treated the gun as being perfectly rigid, whereas, in fact, it was elastic." The Rev. James White had also said that "persons who taught mathematics were apt to forget that they were dealing with ideal conditions, and that practically men might make use of terms which might seem to teachers confused, but which did not necessarily imply a confusion of ideas on the part of persons using them," and that, "There was no doubt action and reaction were equal, but in the Paper it appeared to him that the subject had been dealt with entirely from the point of observation, and that the great laws of motion, especially the important law of action and reaction, were not touched by the question under discussion." These valuable remarks of gentlemen whose mathematical attainments were beyond suspicion, added to the equally important observations of a similar character by Mr. Cowper and Dr. Siemens, constituted in themselves a sufficient answer to Professor Unwin and Mr. Bousfield; but he would pursue the subject somewhat

Mr. Butter.

farther as he felt it incumbent upon him, not only to answer the objections made, but to establish his views in a clearer light than was possible in the necessarily brief notice which the general character of the Paper permitted. Professor Unwin had stated that "the whole forward momentum given to the shot, to the powder charge, and to the air in front of the shot, must be equal to the whole of the backward momentum given to the gun, to the gun carriage, and to the whole mass of the air." Assuming for the moment that the ideal conditions upon which this statement was based correctly represented the actual conditions, Mr. Butter would still be justified in his attack upon the manner in which the formula had hitherto been constantly applied, for the air had always been omitted and generally only one-half the charge had been taken into account. But he further contended that it was incorrect to assume that action, without any limitation of conditions, must always expend itself in generating momenta, as generally understood, namely, the rate of motion of translation into the weight, and not including molecular motion. If this were true, action and reaction could not be equal under the conditions in which bodies were sometimes acted upon by a common force, such, for instance, as the varying conditions of their elasticity and of the amount of action converted into heat. Newton's law was so well expressed that all modern discoveries affecting the principle of the conservatism of energy served but to show its absolute truth; and when it was assumed that action, comprehended under the form of powder-pressure, set up between two bodies of whatever difference of weight, molecular and other conditions, must necessarily produce equality of momenta, which, in its applied sense, was but one out of several modes in which reaction might, and mostly did, manifest itself, he thought that statement must be regarded as being more opposed to Newton's law in its universal sense, including reactions of every kind, than was any statement which he had made. In point of fact, the formula of the equality of momenta (accepted as mathematically correct by Professor Unwin, but repudiated by Mr. Bousfield) could be true only when certain conditions existed, which conditions were purely theoretical. It was, nevertheless, invaluable for purposes of purely mathematical investigation, and in many cases where it was practically known the error could not be of importance. His position was this, that it had been, without qualification, wrongly applied to ascertain the highest velocity of recoil of the gun, or of the gun and carriage combined, and failed to give results in conformity with observed data. It would be well at this point to

Mr. Butter. recall what the observed facts were. First, there could be no doubt that, with the same total amount of powder-pressure acting between the gun and the shot up to the time of the shot leaving the muzzle, and any unequal pressure acting afterwards, there was about one-third greater energy of recoil with pebble than with R. L. G. powder, the shot in each case having the same energy; secondly, that the times during which the two kinds of powder acted in the bore were about as 3 to 4 respectively; and thirdly, that while the equation of momenta was applied to obtain the highest velocity of recoil of guns fired with charges of R. L. G. powder, and gave results fairly correctly, the introduction into the formula of the empirical value $\sqrt{\frac{4}{3}}$ rendered it possible, in the absence of more accurate knowledge of the exact conditions, to obtain the highest velocity of the same guns fired with pebble powder, adding the full weight of the charge to that of the shot in each case to give the value of w. These facts, he thought, must be beyond dispute, since they were confirmed, no less by careful and repeated observations upon the direct effects of the discharges, than by the results of independent calculations from other observed data, such as the time waves in the pressure-curves of recoil, and from the areas of the curves. In dealing with the causes of these facts, he was aware that nothing could be definitely established except by more extended observation and experiment, and by mathematical deductions from data so obtained. It was nevertheless permissible to advance speculative theories based upon *à priori* reasoning, if only for the purpose of inciting more competent persons to direct attention to the subject, or of indicating the direction in which experimental research might be expected to realise the necessary data, and it was upon these grounds that he had brought forward the theory of the action of discharge. The practical value of the Paper was in no sense bound up with the theory, its whole scope being to show that from actual observations, the mathematical method generally in use for finding the velocity of recoil did not give correct results, without the introduction of some constant. The causes of the insufficiency of the mathematical formula alone to solve the problem were no doubt both complex and of an indeterminate character; they must at least include the following:—First, the mode of "action" of the exploded powder; second, the loss of energy in the development of heat; third, the effect of elasticity in the masses of the shot and gun with reference to the time occupied in compression and restitution; fourth, the effect of the gas-pressure acting upon both the shot and the gun after the former had left the bore; fifth, the resistance of the atmosphere and of friction. The effect of the resistance of brakes, of the carriage moving up a slope against gravity, &c., which had been alluded to by several gentlemen, should not be included in finding the velocity of recoil; such velocity should be that of the gun and carriage moving as freely as possible in order to have the means of determining the total energy which the resistances of brakes, &c., had to meet in the length of recoil to which it was desirable to restrict the gun. The Paper being of an entirely practical character, but small attention had been paid to these causes as a whole, so little being known regarding them. The third was considered to be the one to which the least attention had been directed, and that which probably most affected the result, and hence it was embodied in the speculative theory which had, contrary to expectation, attracted the most attention. Had such a result been anticipated, much greater care and more elaboration would have been exercised upon it; as it was, he would endeavour to correct this omission, and in dealing with these causes, to show that the formula for the equality of momenta was, contrary to Professor Unwin's assumption, insufficient in itself to meet the case; the reasoning in itself would also prove that Mr. Bousfield's position was equally untenable, both these gentlemen having based their demonstrations upon the usual ideal conditions which would be shown to be different from what they most probably were in actual fact. In the first place the "action" of the powder-pressure did not exactly correspond with the pressure of impact, nor with that of steam or of compressed air; it would be found fully described in the "Researches on Explosives" alluded to in the Paper. The products of combustion consisted of two-thirds by weight of heavy liquid particles diffused, at first, in a finely divided state throughout the remaining one-third, which consisted of a highly elastic gas; the products of the first portions ignited were projected with enormous velocities along the lines of least resistance "through the interstices of the charge, or between the charge and the bore, and on meeting with any resistances their *vis viva* is reconverted into pressures," thus currents, or waves, of mingled *vis viva* and pressures were from the first set up. Unignited portions, no doubt, acquired a high momentum in the direction of the bore before ignition was completed, and continued the same action more or less, hence a succession of particles were hurled in waves against the base of the shot, and possibly in a less degree, as a whole, upon the gun; the result upon the shot was that "with rapidly burning, and in a less degree with the slower burning, powders, motion is communicated Mr. Butter.

Mr. Butter. to the projectile by a series of impulses more or less violent." The reactions to a great extent contributed to equalise the effects upon the shot and the gun, but even when no abnormal violent wave-actions existed, the chronoscope pressures, as a whole, obtained by the motion of the shot, were generally 5 to 7 per cent. greater than those of the crusher-gauge which gave the pressure on the gun; and yet the maximum pressure on the gun invariably exceeded that upon the shot, at times to the extent of 15 per cent., which showed the complicated nature of the action. With regard to the second cause of variation, the loss of pressure, by the communication of heat to the bore of the gun and to the shot, was so great and rapid that while the full theoretical effect was nearly realised in the largest guns, the amount secured in the smallest was only about 50 per cent., and this was due in great measure to the loss of heat. Since the effect of this loss was so rapidly manifested in the pressures, it might well be regarded as possible, that it operated as an additional disturbing element in the action of the products of combustion upon the shot and the gun. Taking, then, into careful consideration these well authenticated facts alone, it would be seen that there existed no justification for the assumption that the mathematical expressions of either Professor Unwin, or of Mr. Bousfield, were strictly applicable, but that, on the contrary, his assertion that the question was, for the present, outside the range of strict mathematical investigation was fully proved. The third was more immediately connected with the theory he had ventured to advance. This Mr. Bousfield had attacked in very summary fashion, and Professor Unwin feared it might lead, if true, to the rewriting of mathematics; but he maintained it would, if accepted, lead perhaps to mathematicians using greater caution in the application of well-known laws, until they were convinced of the identity of the conditions of the problem under investigation with those upon which the laws were based. It would, he thought, be conceded that both these gentlemen had assumed, as the basis of their mathematical expressions, that the shot and the gun were to be regarded as heavy particles, that was, capable of communicating any change of pressure instantaneously to their respective masses, and of having their rate of motion affected instantaneously thereby, whereas his position was that they were to be regarded as elastic bodies in the nature of steel springs of great resisting power, but nevertheless subject to the same mode of action under the influence of high pressures suddenly applied, and requiring the lapse of an appreciable interval of time, proportioned in some degree to their masses, to convey the pressure throughout the mass. It would thus be seen that the contention rested more upon physical conditions than upon mathematical laws. They claimed instantaneous effect from a given cause, he, a delayed effect, so far as motion of the whole mass was concerned. Grant his physical conditions to be true, his theory and the various terms he had used in formulating it would be perfectly clear and consistent with terms in common use in all text-books on dynamics, although it might not be regarded as in itself correct—in any case they could not fairly be judged from the mathematical point of view alone. It was curious that while, on the one hand, all persons devoted to physical research were convinced of the fact that instantaneous effects extending over large areas, from the operation of forces, was an impossibility, even the most rapidly acting, such as light and electricity, required some interval of time. Mathematicians still resented the introduction of time in connection with the transmission of pressures producing change of motion, mainly upon the supposition that it interfered with the equality of action and reaction, with which it had in reality nothing to do, although it might with equality of momenta in such cases as the discharge of a gun, which differed wholly from ordinary impact, in which, however different the interval of time might be to communicate motion to their respective centres of gravity, they would remain in contact until the action and reaction were completed, and thus would result in true equality of momenta; while in the case of the shot and the gun, if their elasticity were taken into account with reference to their enormous difference in mass, and the different intervals of time required to transmit suddenly applied pressures throughout them, so as to set up motion, it seemed to him clear, that the instant at which motion to the whole mass of the gun occurred, must be delayed beyond that at which the shot moved. In other words, the velocity at which the longitudinal vibrations travelled in the shot and the gun being regarded as nearly identical, it followed that they must traverse the shot in a much smaller interval of time than they did the gun. The resistance to motion in the small interval of time during which the powder-pressure rose to many tons per square inch, undoubtedly gave rise to a compression of the material of both shot and of gun to some extent, before either moved as a whole, that was with the acceptance of the conditions of elasticity and time before stated, and the wave of pressure was completed in the shot when it had only penetrated a short distance in the gun; after the shot had began to move, the generation of gas-pressure was so rapid that it continued to rise, and conse- Mr. Butter.

Mr. Butter. quently still further to compress the material of the shot and the gun until the highest pressure was attained, at which point the shot had moved ½ inch with R. L. G., and 6 inches with pebble, powder. The use of the expression "overcoming inertia," to which so much objection had been made, was, when associated with the conditions of time and of elasticity, not therefore so unscientific as Mr. Bousfield asserted. Of course, the expression "overcoming inertia" could not, nor was it intended to, be confined to imparting motion to a body at rest, but would equally apply to one already in motion, the resistance under the same conditions to a change of motion being the same as would be opposed by a body at rest. This was essentially different from the definition of inertia given by Mr. Bousfield, who stated, "The overcoming inertia meant the setting up a certain velocity;" if this were so, then what should that resistance be called which bodies opposed to the setting up of any given rate of motion in very small spaces of time, and by virtue of which a 300-lbs. shot might, and did, oppose a resistance of 1,500 tons? Nor did his definition accord with that given in text-books on dynamics, for example, Weisbach stated, "If we raise a body slowly enough to be able to disregard its inertia." The idea conveyed here was the setting up of velocity without bringing inertia into action at all, it was when a force was applied to produce change of motion in a restricted interval of time that the resistance of inertia demanded consideration. The 300-lbs. shot lay in the bore perfectly free to move, except for a comparatively small amount of friction, and if a continuous pressure of no more than about 100 lbs. were applied to it for a sufficiently long interval it might ultimately attain to a velocity of 1,500 feet per second; but as this velocity has to be imparted in a very small interval of time, the pressure actually reached 1,500 tons, this pressure could only be reached by preventing the expansion of the powder gas, and this the shot did, although free to move with a pressure of only 100 lbs. Inertia therefore was that resistance associated with time which a body opposed to change of motion; and when the time was extremely small the property of elasticity and other phenomena were brought prominently into action, which was not the case when the time was unlimited. A pressure of 1,500 tons caused a compression of 0·066 inch in a cast-iron shot, and the energy requisite to produce this amount of compression was 10,225 foot-lbs.; from the moment of greatest compression this energy commenced to react upon the powder-pressure to increase the velocity of the shot, as in ordinary impact, while at the same time the powder-pressure acted as a directly accelerating force, imparting velocity as it would do to an inelastic body; the increase of velocity due to the restitution alone, assuming the shot to be perfectly elastic, was 45 feet per second. From this it would be seen that there was good reason for the statement that the muzzle velocity was due to completed impact, and to the accelerating force of the powder-pressure. The same actions occurred in the gun, but, still keeping in view the element of time in the propagation of waves of pressure, both the setting up of motion and the total compression and restitution must necessarily, by reason of the greater mass of the gun, be delayed beyond the periods at which they took place in the shot; the result of this would be that restitution in the gun must, at least, take place with a resisting powder-pressure considerably less than that upon which the shot acted, and this in a greater degree in the case of R. L. G. powder, since pebble-powder maintained for a much longer interval its higher pressure, and also acted in the bore a longer time (4 to 3). The late Mr. R. Mallet, M. Inst. C.E., had a similar view in principle to this, in his work "On the Physical Conditions involved in the Construction of Artillery," when speaking of "the rate at which force itself is transmitted through the mass,"[1] he said, "were it possible to have a gun of sufficient thickness, therefore, the impulsive effect of the explosion might have reached its maximum and vanished altogether before the strain visited on the interior portions had been transmitted to the exterior. This cannot occur in practice, but in no case is the whole of the impulse transmitted equally through the whole thickness of the gun." It might well be the case that the experiments quoted as to the position of the shot when recoil commenced might not be considered conclusive, since they had not been confirmed by the requisite number and variety of experiments, which rendered other data given in the Paper more reliable; but if these were rejected altogether, his position, as would be seen by the foregoing argument, would be affected only in degree and not in principle; and in any case the fact that one-third greater energy of recoil resulted from pebble than from R. L. G. powder had to be accounted for. The fourth cause of variation from the mathematical conditions of impact was that which several gentlemen had shown themselves disposed to refer the whole of the difference in the energies developed by pebble and R. L. G. powder; of reliable data in regard to the action upon the gun of the discharged gases after the shot had left the bore, there were none; all

Mr. Butter.

[1] *Vide* p. 70.

Mr. Butter. conclusions in respect to it must therefore be of a purely speculative character. Mr. White had shown by reference to various experiments that it could not be so great as had been imagined; it must be borne in mind that this cause operated, although in a less degree, in the case of R. L. G. as well as in that of pebble powder, so that to account for the difference of one-third in their respective energies a much larger proportion than one-third of the energy of recoil of pebble powder must be due to the back pressure of the liberated gas, and considering how small, comparatively with what it had been in the bore, this was to begin with, and how rapidly, by free expansion in the air, it would cease, it could scarcely be seriously maintained that the full difference was due to this cause alone, although he admitted it must have an important bearing upon it. It was needless to consider the effect of the resistances of friction and of air upon the shot and the gun respectively, as these had been generally accepted. Looking then at the whole question from the point of view of the physical conditions which would appear to govern it, he thought no grounds existed for the confidence with which Professor Unwin and Mr. Bousfield maintained the entire sufficiency of existing mathematical expressions to meet the case; and that there was ample justification for the introduction of some constant, to meet the unknown quantities, into existing mathematical formulæ. He was not disposed to defend himself from the charge of a misapplication of mathematical expressions to any greater extent than he had done. That relating to "inertia" was the most serious, and no doubt arose on the part of his critics from not fully realising the conditions he had in view when he used the expression.

Correspondence.

Mr. Bashforth. Mr. F. BASHFORTH remarked that there could be no doubt that great advantage would result from the introduction of some elastic material between the gun and carriage of field guns, provided the invention were simple and not peculiarly liable to be damaged on service. He thought a good result would have to be worked out by trial, because so little was known about the action of gunpowder. As to the kind of action of exploding powder on the gun, he believed it was for the most part correctly treated by Hutton, Didion, &c., in their accounts of the gun pendulum. There could be no doubt that, if it were possible to apply a pressure $P—p$ to the breech of a gun at all times equal to pressure P of the exploding powder on the base of the bore, less p the forward