History of Probability and Expectation

Correspondence between Blaise Pascal (1623-1662) and Pierre de Fermat (1601-1665)

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Fermat to Pascal (1654)

Monsieur

If I undertake to make a point with a single die in eight throws, and if we agree after the money is put at stake, that I shall not cast the first throw, it is necessary by my theory that I take 1/6 of the total sum to be impartial because of the aforesaid first throw.

And if we agree after that that I shall not play the second throw, I should, for my share, take the sixth of the remainder that is 5/36 of the total.

If, after that, we agree that I shall not play the third throw, I should to recoup myself, take 1/6 of the remainder which is 25/216 of the total.

And if subsequently, we agree again that I shall not cast the fourth throw, I should take 1/6 of the remainder or 125/1296 of the total, and I agree with you that that is the value of the fourth throw supposing that one has already made the preceding plays.

But you proposed in the last example in your letter (I quote your very terms) that if I undertake to find the six in eight throws and if I have thrown three times without getting it, and if my opponent proposes that I should not play the fourth time, and if he wishes me to be justly treated, it is proper that I have 125/1296 of the entire sum of our wagers.

This, however, is not true by my theory. For in this case, the three first throws having gained nothing for the player who holds the die, the total sum thus remaining at stake, he who holds the die and who agrees to not play his fourth throw should take 1/6 as his reward.

And if he has played four throws without finding the desired point and if they agree that he shall not play the fifth time, he will, nevertheless, have 1/6 of the total for his share. Since the whole sum stays in play it not only follows from the theory, but it is indeed common sense that each throw should be of equal value.

I urge you therefore (to write me) that I may know whether we agree in the theory, as I believe (we do), or whether we differ only in its application.

I am, most heartily, etc.

Fermat.

Pascal to Fermat (1654)

Monsieur,—

1. Impatience has seized me as well as it has you, and although I am still abed, I cannot refrain from telling you that I received your letter in regard to the problem of the points [1] yesterday evening from the hands of M. Carcavi, and that I admire it more than I can tell you. I do not have the leisure to write at length, but, in a word, you have found the two divisions of the points and of the dice with perfect justice. I am thoroughly satisfied as I can no longer doubt that I was wrong, seeing the admirable accord in which I find myself with you.

I admire your method for the problem of the points even more than that of the dice. I have seen solutions of the problem of the dice by several persons, as M. le chevalier de Méré, who proposed the question to me, and by M. Roberval also M. de Méré has never been able to find the just value of the problem of the points nor has he been able to find a method of deriving it, so that I found myself the only one who knew this proportion.

2. Your method is very sound and it is the first one that came to my mind in these researches, but because the trouble of these combinations was excessive, I found an abridgment and indeed another method that is much shorter and more neat, which I should like to tell you here in a few words; for I should like to open my heart to you henceforth if I may, so great is the pleasure I have had in our agreement. I plainly see that the truth is the same at Toulouse and at Paris.

This is the way I go about it to know the value of each of the shares when two gamblers play, for example, in three throws, and when each has put 32 pistols at stake:

Let us suppose that the first of them has two (points) and the other one. They now play one throw of which the chances are such that if the first wins, he will win the entire wager that is at stake, that is to say 64 pistols. If the other wins, they will be two to two and in consequence, if they wish to separate, it follows that each will take back his wager that is to say 32 pistols.

Consider then, Monsieur, that if the first wins, 64 will belong to him. If he loses, 32 will belong to him. Then if they do not wish to play this point, and separate without doing it, the first should say “I am sure of 32 pistols, for even a loss gives them to me. As for the 32 others, perhaps I will have them and perhaps you will have them, the risk is equal. Therefore let us divide the 32 pistols in half, and give me the 32 of which I am certain besides.” He will then have 48 pistols and the other will have 16.

Now let us suppose that the first has *two* points and the other *none*, and that they are beginning to play for a point. The chances are such that if the first wins, he will win all of the wager, 64 pistols. If the other wins, behold they have come back to the preceding case in which the first has *two* points and the other *one*.

[1][The editors of these letters note that the word *parti* means the division of the stake between the players in the case when the game is abandoned before its completion. *Parti des dés* means that the man who holds the die agrees to throw a certain number in a given number of trials. For clarity, in this translation, the first of these cases will he called the problem of the points, a term which has had a certain acceptance in the histories of mathematics, while the second may by analogy he called the problem of the dice.]

But we have already shown that in this case 48 pistols will belong to the one who has *two* points. Therefore if they do not wish to play this point, he should say, “If I win, I shall gain all that is 64. If I lose, 48 will legitimately belong to me. Therefore give me the 48 that are certain to be mine, even if I lose, and let us divide the other 16 in half because there is as much chance that you will gain them as that I will.” Thus he will have 48 and 8, which is 56 pistols.

Let us now suppose that the first has but *one* point and the other *none*. You see, Monsieur, that if they begin a new throw, the chances are such that if the first wins, he will have *two* points to *none*, and dividing by the preceding case, 56 will belong to him. If he loses, they will he point for point, and 32 pistols will belong to him. He should therefore say, “If you do not wish to play, give me the 32 pistols of which I am certain, and let us divide the rest of the 56 in half. From 56 take 32, and 24 remains. Then divide 24 in half, you take 12 and I take 12 which with 32 will make 44.

By these means, you see, by simple subtractions that for the first throw, he will have 12 pistols from the other; for the second, 12 more; and for the last 8.

But not to make this more mysterious, inasmuch as you wish to see everything in the open, and as I have no other object than to see whether I am wrong, the value (I mean the value of the stake of the other player only) of the last play of two is double that of the last play of *three* and four times that of the last play of *four* and eight times that of the last play of *five*, etc.

3. But the ratio of the first plays is not so simple to find. This therefore is the method, for I wish to disguise nothing, and here is the problem of which I have considered so many cases, as indeed I was pleased to do: *Being given any number of throws that one wishes, to find the value of the first*.

For example, let the given number of throws he 8. Take the first eight even numbers and the first eight uneven numbers as:

2, 4, 6, 8, 10, 12, 14, 16

and

1, 3, 5, 7, 9, 11, 13, 15.

Multiply the even numbers in this way. the first by the second, their product by the third, their product by the fourth, their product by the fifth, etc.; multiply the odd numbers in the same way: the first by the second, their product by the third, etc.

The last product of the even numbers is the *denominator* and the last product of the odd numbers is the numerator of the fraction that expresses the value of the first throw of *eight*. That is to say that if each one plays the number of pistols expressed by the product of the even numbers, there will belong to him [who forfeits the throw] the amount of the other's wager expressed by the product of the odd numbers. This may he proved, but with much difficulty by combinations such as you have imagined, and I have not been able to prove it by this other method which I am about to tell you. but only by that of combinations. Here are the theorems which lead up to this which are properly arithmetic propositions regarding combinations, of which I have found so many beautiful properties:

4. If from any number of letters, as 8 for example,

A, B, C, D, E, F, G, H,

you take all the possible combinations of 4 letters and then all possible combinations of 5 letters, and then of 6, and then of 7, of 8, etc., and thus you would take all possible combinations, I say that if you add together half the combinations of 4 with each of the higher combinations, the sum will be the number equal to the number of the quaternary progression beginning with 2 which is half of the entire number.

For example, and I shall say it in Latin for the French is good for nothing. If any number whatever of letters, for example 8,

A, B, C, D, E, F, G, H,

be summed in all possible combinations, by fours, fives, sixes, up to eights, I say, if you add half of the combinations by fours, that is 35 (half of 70) to all the combinations by fives, that is 56, and all the combinations by sixes, namely 28, and all the combinations by sevens, namely 8, and all the combinations by eights namely 1, the sum is the fourth number of the quaternary progression whose first term is 2. I say the fourth number for 4 is half of 8.

The numbers of the quaternary progressions whose first term is 2 are

2, 8, 32, 128, 512, etc.

of which 2 is the first, 8 the second, 32 the third, and 128 the fourth. Of these, the 128 equals:

+ 35 half of the combinations of 4 letters
+ 56 the combinations of 5 letters
+ 28 the combinations of 6 letters
+ 8 the combinations of 7 letters
+ 1 the combinations of 8 letters.

5. That is the first theorem, which is purely arithmetic. The other concerns the theory of the points and is as follows:

It is necessary to say first: if one (player) has one point out of 5 for example, and if he thus lacks 4, the game will infallibly be decided in 8 throws, which is double 4.

The value of the first throw of 5 in the wager of the other is the fraction which has for its numerator the half of the combinations of 4 things out of 8 (I take 4 because it is equal to the number of points that he lacks, and 8 because it is double the 4) and for the denominator this same numerator plus all the higher combinations.

Thus if I have one point out of 5, 35/128 of the wager of my opponent belongs to me. That is to say, if he had wagered 128 pistols, I would take 35 of them and leave him the rest, 93.

But this fraction 35/128 is the same as 105/384, which is made by the multiplication of the even numbers for the denominator and the multiplication of the odd numbers for the numerator.

You will see all of this without a doubt, if you will give yourself a little trouble, and for that reason I have found it unnecessary to discuss it further with you.

6. I shall send you, nevertheless, one of my old Tables; I have not the leisure to copy it, and I shall refer to it.

You will see here as always, that the value of the first throw is equal to that of the second, a thing which may easily be proved by combinations.

You will see likewise that the numbers of the first line are always increasing; those of the second do the same; those of the third the same.

But after that, those of the fourth line diminish; those of the fifth etc. This is odd.

	If each wagers 256 on					
	6 throws	5 throws	4 throws	3 throws	2 throws	1 throw
First throw	63	70	80	96	128	256
Second	63	70	80	96	128	
Third	56	60	64	64		
Fourth	42	40	32			
Fifth	24	16				
Sixth	8					

	If each wagers 256 on					
	6 throws	5 throws	4 throws	3 throws	2 throws	1 throw
First row	63	70	80	96	128	256
First two throws	126	140	160	192	256	
First three throws	182	200	224	256		
First four throws	224	240	256			
First five throws	248	256				
First six throws	256					

7. I have no time to send you the proof of a difficult point which astonished M. (de Méré) so greatly, for he has ability but he is not a geometer (which is, as you know, a great defect) and he does not even comprehend that a mathematical line is infinitely divisible and he is firmly convinced that it is composed of a finite number of points. I have never been able to get him out of it. If you could do so, it would make him perfect.

He tells me then that he has found an error in the numbers for this reason.

If one undertakes to throw a six with a die, the advantage of undertaking to do it in 4 is as 671 is to 625.

If one undertakes to throw double sixes with two dice the disadvantage of the undertaking is 24.

But nonetheless, 24 is to 36 (which is the number of faces of two dice)[2] as 4 is to 6 (which is the number of faces of one die).

This is what was his great scandal which made him say haughtily that the theorems were not consistent and that arithmetic was demented. But you will easily see the reason by the principles which you have.

[2][Clearly, the number of possible ways in which two dice can fall.]

I shall put all that I have done with this in order when I shall have finished the treatise on geometry[3] on which I have already been working for some time.

8. I have also done something with arithmetic on which subject, I beg you to give me your advice.

I proposed the lemma which everyone accepts, that the sum of as many numbers as one wishes of the continuous progression from unity as

$$1, \quad 2, \quad 3, \quad 4,$$

being taken by twos is equal to the last term 4 multiplied into the next greater, 5. That is to say that the sum of the integers[4] in A being taken by twos is equal to the product

$$A \times (A+1).$$

I now come to my theorem:

If one he subtracted from the difference of the cubes of any two consecutive numbers, the result is six times all the numbers contained in the root of the lesser number.

Let the two roots R and S differ by unity. I say that $R^3 - S^3 - 1$ is equal to six times the sum of the numbers contained in S.

Let S be called A, then R is $A+1$. Therefore the cube of the root R or $A+1$ is

$$A^3 + 3A^2 + 3A + 1^3.$$

The cube of S, or A, is A^3, and the difference of these is $R^3 - S^3$; therefore, if unity he subtracted, $3A^2 + 3A$ is equal to $R^3 - S^3 - 1$. But by the lemma, double the sum of the numbers contained in A or S is equal to $A \times (A+1)$; that is, to $A^2 + A$. Therefore, six times the sum of the numbers in A is equal to $3A^3 + 3A$. But $3A^3 + 3A$ is equal to $R^3 - S^3 - 1$. Therefore $R^3 - S^3 - 1$ is equal to six times the sum of the numbers contained in A or S. *Quod erat demonstrandum*. No one has caused me any difficulty in regard to the above, but they have told me that they did not do so for the reason that everyone is accustomed to this method today. As for myself, I mean that without doing me a favor, people should admit this to he an excellent type of proof. I await your comment, however, with all deference. All that I have proved in arithmetic is of this nature.

9. Here are two further difficulties. i have proved a plane theorem making use of the cube of one line compared with the cube of another. I mean that this is purely geometric and in the greatest rigor. By these means I solved the problem: "Any four planes, any four points, and any four spheres being given, to find a sphere which, touching the given spheres, passes through the given points, and leaves on the planes segments in which given angles may be inscribed;"[5] and this one: "Any three circles, any three points, and any three lines being given, to find a circle which touches the circles and the points and lives on the lines and are in which a given angle may be inscribed."

I solved these problems in a plane, using nothing in the construction but circles and straight lines, but in the proof I made use of solid loci,[6]—of parabolas, or hyperbolas.

[3][Perhaps the manuscript which Leibniz saw, but which is not now extant.]
[4]["... des nombres contenus dans A."]
[5]["... capable d'angles donnés."]
[6][A common name for conics.]

Nevertheless, inasmuch as the construction is in a plane, I maintain that my solution is plane, and that it should pass as such.

This is a poor recognition of the honor which you have done me in putting up with my discourse which has been plaguing you so long. I never thought I should say two words to you and if I were to tell you what I have uppermost in my heart,—which is that the better I know you the more I honor and admire you,—and if you were to see to what degree that is, you would allot a place in your friendship for him who is, Monsieur, your etc.

Fermat to Carcavi (1654)

Monsieur,

1. I was overjoyed to have had the same thoughts as those of M. Pascal, for I greatly admire his genius and I believe him to be capable of solving any problem he attempts. The friendship he offers is so dear to me and so precious that I shall not scruple to take advantage of it in publishing an edition of my Treatises.

If it does not shock you, you could both help in bringing out this edition, and I suggest that you should be the editors: you could clarify or augment what seems too brief and thus relieve me of a care which my work prevents me from taking. I would like this volume to appear without my name even, leaving to you the choice of designation which would indicate the author, whom you could qualify simply as a friend.

2. Here is the course which I have thought out for the second Part which will contain my researches on numbers. It is a work which is still only an idea, and for which I may not have the leisure to put fully on paper; but I will send a summary to M. Pascal of all my principles and first theorems, in which, I can promise you in advance, he will find everything not only novel and hitherto unknown but also astounding.

If you combine your work with his, everything will succeed and soon be completed, and we will thus be able to publish the first Part which you have in your care.

If M. Pascal approves of my overtures which are based on my great esteem for his genius and his intellect, I will first begin to inform you of my numerical results. Farewell.

I am, Monsieur, your very humble and obedient servant.

Fermat.

Pascal to Fermat (1654)

Monsieur,

1. I was not able to tell you my entire thoughts regarding the problem of the points by the last post,[7] and at the same time, I have a certain reluctance at doing it for fear lest this admirable harmony which obtains between us and which is so dear to me should begin to flag, for I am afraid that we may have different opinions on this subject. I wish to lay my whole reasoning before you, and to have you do me the favor to set me straight if I am in error or to indorse me if I am correct. I ask you this in all faith and sincerity for I am not certain even that you will be on my side.

When there are but *two* players, your theory which proceeds by combinations is very just. But when there are three, I believe I have a proof that it is unjust that you should proceed in any other manner than the one I have. But the method which I have disclosed to you and which I have used universally is common to all imaginable conditions of all distributions of points, in the place of that of combinations (which I do not use except in particular cases when it is shorter than the general method), a method Which is good only in isolated cases and not good for others.

I am sure that I can make it understood, but it requires a few words from me and a little patience from you.

2. This is the method of procedure when there are two players. If two players, playing in several throws, find themselves in such a state that the first lacks two points and the second three of gaining the stake, you say it is necessary to see in how many points the game will be absolutely decided.

It is convenient to suppose that this will he in four points, from which you conclude that it is necessary to see how many ways the four points may be distributed between the two players and to see how many combinations there are to make the first win and how many to make the second win, and to divide the stake according to that proportion. I could scarcely understand this reasoning if I had not known it myself before; but you also have written it in your discussion. Then to see how many ways four points may be distributed between two players, it is necessary to imagine that they play with dice with two faces (since there are but two players), as heads and tails, and that they throw four of these dice (because they play in four throws). Now it is necessary to see how many ways these dice may fall. That is easy to calculate. There can be *sixteen*, which is the second power of *four*; that is to say, the square. Now imagine that one of the faces is marked *a*, favorable to the first player. And suppose the other is marked *b*, favorable to the second. Then these four dice can fall according to one of these sixteen arrangements.

a	*a*	*a*	*a*	*a*	*a*	*a*	*a*	*b*	*b*	*b*	*b*	*b*	*b*	*b*	*b*
a	*a*	*a*	*a*	*b*	*b*	*b*	*b*	*a*	*a*	*a*	*a*	*b*	*b*	*b*	*b*
a	*a*	*b*	*b*	*a*	*a*	*b*	*b*	*a*	*a*	*b*	*b*	*a*	*a*	*b*	*b*
a	*b*	*a*	*b*	*a*	*b*	*a*	*b*	*a*	*b*	*a*	*b*	*a*	*b*	*a*	*b*
1	1	1	1	1	1	1	2	1	1	1	2	1	2	2	2

and, because the first player lacks two points, all the arrangements that have two *a*'s make him win. There are therefore 11 of these for him. And because the second lacks three points, all the arrangements that have three *b*'s make him win. There are 5 of these. Therefore it is necessary that they divide the wager as 11 is to 5.

There is your method, when there are two players, whereupon you say that if there are more players. it will not be difficult to make the division by this method.

3. On this point, Monsieur, I tell you that this division for the two players founded on combinations is very equitable and good, but that if there are more than two players, it is not always just and I shall tell you the reason

for this difference. I communicated your method to [some of] our gentlemen, on which M. de Roberval made me this objection:

That it is wrong to base the method of division on the supposition that they are playing in *four* throws seeing that when one lacks *two* points and the other *three*, there is no necessity that they play *four* throws since it may happen that they play but *two* or *three*, or in truth perhaps *four*.

Since he does not see why one should pretend to make a just division on the assumed condition that one plays four throws, in view of the fact that the natural terms of the game are that they do not throw the dice after one of the players has won; and that at least if this is not false, it should he proved. Consequently he suspects that we have committed a paralogism.

I replied to him that I did not found my reasoning so much on this method of combinations, which in truth is not in place on this occasion, as on my universal method from which nothing escapes and which carries its proof with itself. This finds precisely the same division as does the method of combinations. Furthermore, I showed him the truth of the divisions between two players by combinations in this way. Is it not true that if two gamblers finding according to the conditions of the hypothesis that one lacks *two* points and the other *three*, mutually agree that they shall play four complete plays, that is to say, that they shall throw four two-faced dice all at once,—is it not true, I say, that if they are prevented from playing the four throws, the division should be as we have said according to the combinations favorable to each? He agreed with this and this is indeed proved. But he denied that the same thing follows when they are not obliged to play the four throws. I therefore replied as follows:

It is not clear that the same gamblers, not being constrained to play the four throws, but wishing to quit the game before one of them has attained his score, can without loss or gain be obliged to play the whole four plays, and that this agreement in no way changes their condition? For if the first gains the two first points of four will he who has won refuse to play two throws more, seeing that if he wins he will not win more and if he loses he will not win less? For the two points which the other wins are not sufficient for him since he lacks three, and there are not enough [points] in four throws for each to make the number which he lacks.

It certainly is convenient to consider that it is absolutely equal and indifferent to each whether they play in the natural way of the game, which is to finish as soon as one has his score, or whether they play the entire four throws. Therefore, since these two conditions are equal and indifferent, the division should he alike for each. But since it is just when they are obliged to play the four throws as I have shown, it is therefore just also in the other case.

That is the way I prove it, and, as you recollect, this proof is based on the equality of the two conditions true and assumed in regard to the two gamblers, the division is the same in each of the methods, and if one gains or loses by one method, he will gain or lose by the other, and the two will always have the same accounting.

4. Let us follow the same argument for *three* players and let us assume that the first lacks *one* point, the second *two*, and the third *two*. To make the division, following the same method of combinations, it is necessary to first discover in how many points the game may he decided as we did when there woe two players. This will be in three points for they cannot play three throws without necessarily arriving at a decision.

It is now necessary to see how many ways three throws may he combined among three players and how many are favorable to the first, how many to the second, and how many to the third, and to follow this proportion in distributing the wager as we did in the hypothesis of the two gamblers.

It is easy to see how many combinations there are in all. This is the third power of 3; that is to say, its cube, or 27. For if one throws three dice at a time (for it is necessary to throw three times), these dice having three faces each (since there are three players), one marked *a* favorable to the first, one marked *b* favorable to the second, and one marked *c* favorable to the third,—it is evident that these three dice thrown together can fall in 27 different ways as:

a	*a*	*a*	*a*	*a*	*a*	*a*	*a*	*a*	*b*	*b*	*b*	*b*	*b*	*b*	*b*	*b*	*b*	*c*	*c*	*c*	*c*	*c*	*c*	*c*	*c*	*c*
a	*a*	*a*	*b*	*b*	*b*	*c*	*c*	*c*	*a*	*a*	*a*	*b*	*b*	*b*	*c*	*c*	*c*	*a*	*a*	*a*	*b*	*b*	*b*	*c*	*c*	*c*
a	*b*	*c*	*a*	*b*	*c*	*a*	*b*	*c*	*a*	*b*	*c*	*a*	*b*	*c*	*a*	*b*	*c*	*a*	*b*	*c*	*a*	*b*	*c*	*a*	*b*	*c*
1	1	1	1	1	1	1	1	1	1	1	1	1			1			1	1	1	1			1		
				2						2		2	2	2		2						2				
								3									3			3			3	3	3	3

Since the first lacks but one point, then all the ways in which there is one *a* are favorable to him. There are 19 of these. The second lacks two points. Thus all the arrangements in which there are two *b*'s are in his favor. There are 7 of them. The third lacks two points. Thus all the arrangements in which there are two *c*'s are favorable to him. There are 7 of these.

If we conclude from this that it is necessary to give each according to the proportion 19, 7, 7, we are making a serious mistake and I would hesitate to believe that you would do this. There are several cases favorable to both the first and the second, as *abb* has the *a* which the first needs, and the two *b*'s which the second needs. So too, the ace is favorable to the first and third.

It therefore is not desirable to count the arrangements which are common to the two as being worth the whole wager to each, but only as being half a point. For if the arrangement *acc* occurs, the first and third will have the same right to the wager, each making their score. They should therefore divide the wager in half. If the arrangement *aab* occurs, the first alone wins. It is necessary to make this assumption.

There are 13 arrangements which give the entire wager to the first, and 6 which give him half and 8 which are worth nothing to him. Therefore if the entire sum is one pistol, there are 13 arrangements which are each worth one pistol to him, there are 6 that are each worth Y2 a pistol, and 8 that are worth nothing.

Then in this case of division, it is necessary to multiply

	13	by one pistol which makes	13
	6	by one half which makes	3
	8	by zero which makes	0
Total		Total	16

and to divide the sum of the values 16 by the sum of the arrangements 27, which makes the fraction 16/27 and it is this amount which belongs to the first gambler in the event of a division; that is to say, 16 pistols out of 27.

The shares of the second and the third gamblers will be the same:

There are	4	arrangements which are worth 1 pistol; multiplying,	4
There are	3	arrangements which are worth 3/2 pistol; multiplying,	1 ½ ½
And	20	arrangements which are worth nothing	
Total	27	Total	5 ½

Therefore 5 ½ pistols belong to the second player out of 27, and the same to the third. The sum of the 5 ½, 5 ½, and 16 makes 27.

5. It seems to me that this is the way in which it is necessary to make the division by combinations according to your method, unless you have something else on the subject which I do not know. But if I am not mistaken, this division is unjust.

The reason is that we are making a false supposition,—that is, that they are playing three throws without exception, instead of the natural condition of this game which is that they shall not play except up to the time when one of the players has attained the number of points which he lacks, in which case the game ceases.

It is not that it may not happen that they will play three times, but it may happen that they will play once or twice and not need to play again.

But, you will say, why is it possible to make the same assumption in this case as was made in the case of the two players? Here is the reason: In the true condition [of the game] between three players, only one can win, for by the terms of the game it will terminate when one [of the players] has won. But under the assumed conditions, two may attain the number of their points, since the first may gain the one point he lacks and one of the others may gain the two points which he lacks, since they will have played only three throws. When there are only two players, the assumed conditions and the true conditions concur to the advantage of both. It is this that makes the greatest difference between the assumed conditions and the true ones.

If the players, finding themselves in the state given in the hypothesis,—that is to say, if the first lacks *one* point, the second *two,* and the third *two*; and if they now mutually agree and concur in the stipulation that they will play *three* complete throws; and if he who makes the points which he lacks will take the entire sum if he is the only one who attains the points; or if two should attain them that they shall share equally,- in this case, the division should be made as I give it here. The first shall have 16, the second 5 ½, and the third 5 ½ out of 27 pistols, and this carries with it its own proof on the assumption of the above condition.

But if they play simply on the condition that they will not necessarily play three throws, but that they will only play until one of them shall have attained his points, and that then the play shall cease without giving another the opportunity of reaching his score, then 17 pistols should belong to the first, 5 to the second, and 5 to the third, out of 27. And this is found by my general method which also determines that, under the proceeding condition, the first should have 16, the second 5 ½, and the third without making use of combinations, for this works in all cases and without any obstacle.

6. These, Monsieur, are my reflections on this topic on which I have no advantage over you except that of having meditated on it longer, but this is of little [advantage to me] from your point of view since your first glance is more penetrating than are my prolonged endeavors.

I shall not allow myself to disclose to you my reasons for looking forward to your opinions. I believe you have recognized from this that the theory of combinations is good for the case of two players by accident, as it is also sometimes good in the case of three gamblers, as when one lacks *one* point, another *one*, and the other *two*,[8] because, in this case, the number of points in which the game is finished is not enough to allow two to win, but it is not a general method and it is good only in the case where it is necessary to play exactly a certain number of times.

Consequently, as you did not have my method when you sent me the division among several gamblers, but [since you had] only that of combinations, I fear that we hold different views on the subject.

I beg you to inform me how you would proceed in your research on this problem. I shall receive your reply with respect and joy, even if your opinions should be contrary to mine. I am etc.

Fermat to Pascal (1654)

Monsieur,

1. Our interchange of blows still continues, and I am well pleased that our thoughts are in such complete adjustment as it seems since they have taken the same direction and followed the same road. Your recent *Traité du triangle aritbmétique* and its applications are an authentic proof and if my computations do me no wrong, your eleventh consequence[9] went by post from Paris to Toulouse while my theorem, on figurate numbers,[10] which is virtually the same, was going from Toulouse to Paris. I have not been on watch for failure while I have been at work on the problem and I am persuaded that the true way to escape failure is by concurring with you. But if I should say more, it would he of the nature of a Compliment and we have banished that enemy of sweet and easy conversation.

It is now my turn to give you some of my numerical discoveries, but the end of the parliament augments my duties and I hope that out of your goodness you will allow me due and almost necessary respite.

2. I will reply however to your question of the three players who play in two throws. When the first has one [point] and the others none, your first solution is the true one and the division of the wager should he 17, 5, and 5. The reason for this is self-evident and it always takes the same principle, the combinations making it clear that the first has 17 changes while each of the others has but five.

3. For the rest, there is nothing that I will not write you in the future with all frankness. Meditate however, if you find it convenient, on this theorem: The squared powers of 2 augmented by unity[11] are always prime numbers. [That is,] the square of 2 augmented by unity makes 5 which is a prime number;

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[8][Evidently a misprint, since two throws may he needed.]

[9][From the *Traité du triangle arithmétique*,—“Each cell on the diagonal is double that which preceded it in the parallel or perpendicular rank.”]

[10][*I.e.*, the theorem that $A_n(A + 1)$ is double the triangular number $1 + 2 + 3 + \cdots + A$.]

[11][*I.e.* $2^{2} + 1$. Euler (1732) showed the falsity of the statement.]

The square of the square makes 16 which, when unity is added makes 17, a prime number;
The square of 16 makes 256 which, when unity is added, makes 257, a prime number;
The square of 256 makes 65536 which, when unity is added, makes 65537, a prime number;
and so to infinity.

This is a property whose truth I will answer to you. The proof of it is very difficult and I assure you that I have not yet been able to find it fully. I shall not set it for you to find unless I come to the end of it.

This theorem serves in the discovery of numbers which are in a given ratio to their aliquot parts, concerning which I have made many discoveries. We will talk of that another time.

I am Monsieur, yours etc.

Fermat.

At Toulouse, the twenty ninth of August, 1654.

Fermat to Pascal (1654)

Monsieur,

1. Do not be apprehensive that our argument is coming to an end. You have strengthened it yourself in thinking to destroy it and it seems to me that in replying to M. de Roberval for yourself you have also replied for me.

In taking the example of the three gamblers of whom the first lacks one point, and each of the others lack two, which is the case in which you oppose, I find here only 17 combinations for the first and 5 for each of the others; for when you say that the combination ace is good for the first, recollect that everything that is done after one of the players has won is worth nothing. But this combination having Made the first win on the first die, what does it matter to the third gains two afterwards, since even when he gains thirty all this is superfluous? The consequence, as you have well called it "this fiction," of extending the game to a certain number of plays serves only to make the rule easy and (according to my opinion) to make all the chances equal; or better, more intelligibly to reduce all the fractions to the same denomination.

So that you may have no doubt, if instead of *three* parties you extend the assumption to *four*, there will not be 27 combinations only, but 81; and it will he necessary to see how many combinations make the first gain his point later than each of the others gains two, and how many combinations make each of the others win two later than the first wins one. You will find that the combinations that make the first win are 51 and those for each of the other two are 15, which reduces to the same proportion. So that if you take five throws or any other number you please, you will always find three numbers in the proportion of 17, 5, 5. And accordingly I am right in saying that the combination ace is [favorable] for the first only and not for the third, and that *cca* is only for the third and not for the first, and consequently my law of combinations is the same for three players as for two, and in general for all numbers.

2. You have already seen from my previous letter that I did not demur at the true solution of the question of the three gamblers for which I sent you the three definite numbers, 17, 5, 5. But because M. de Roberval will perhaps be better satisfied to see a solution without any dissimulation and because it may perhaps yield to abbreviations in many cases, here is an example:

The first may win in a single play, or in two or in three.

If he wins in a single throw, it is necessary that he makes the favorable throw with a three-faced die at the first trial. A single die will yield three chances. The gambler then has 1/3 of the wager because he plays only one third.

If he plays twice, he can gain in two ways,-either when the second gambler wins the first and he the second, or when the third wins the throw and when he wins the second. But two dice produce 9 chances. The player than has 2/9 of the wager when they play twice.

But if he plays three times, he can win only in two ways, either the second wins on the first throw and the third wins the second, and he the third; or when the third wins the first throw, the second the second, and he the third; for if the second or the third player wins the two first, he will win the wager and the first player will not. But three dice give 27 chances of which the first player has 2/27 of the chances when they play three rounds.

The sum of the chances which makes the first gambler win is consequently 1/3, 2/9, and 2/27, which makes 17/27.

This rule is good and general in all cases of the type where, without recurring to assumed conditions, the true combinations of each number of throws give the solution and make plain what I said at the outset that the extension to a certain number of points is nothing else than the reduction of divers fractions to the same denomination. Here in a few words is the whole of the mystery, which reconciles us without doubt although each of us sought only reason and truth.

3. I hope to send you at Martinmas an abridgment of all that I have discovered of note regarding numbers. You allow me to be concise [since this suffices] to make myself understood to a man [like yourself who comprehends the whole from half a word. What you will find most important is in regard to the theorem that every number is composed of one, two, or three triangles;[12] of one, two, three, or four squares; of one, two, three, four, or five pentagons; of one, two, three, four, five, or six hexagons, and thus to infinity.

To derive this, it is necessary to show that every prime number which is greater by unity than a multiple of 4 is composed of two squares, as 5, 13, 17, 29, 37, etc.

Having given a prime number of this type, as 53, to find by a general rule the two squares which compose it.

Every prime number which is greater by unity than a multiple of 3, is composed of a square and of the triple of another square, as 7, 13, 19. 31, 37, etc.

Every prime number which is greater by 1 or by 3 than a multiple of 8, is composed of a square and of the double of another square, as 11, 17, 19, 41, 43, etc.

[12][*I.e.*, triangular numbers.]

There is no triangle of numbers whose area is equal to a square number.

This follows from the invention of many theorems of which Bachet vows himself ignorant and which are lacking in Diophantus.

I am persuaded that as soon as you will have known my way of proof in this type of theorem, it will seem good to you and that it will give you the opportunity for a multitude of new discoveries, for it follows as you know that *multi pertranseant ut augeatur scientia*.

When I have time, we will talk further of magic numbers and I will summarize my former work on this subject.

I am, Monsieur, most heartily your etc.

Fermat.

The twenty-fifth of September.
I am writing this from the country, and this may perhaps delay my replies during the holidays.

Pascal to Fermat (1654)

Monsieur,

Your last letter satisfied me perfectly. I admire your method for the problem of the points, all the more because I understand it well. It is entirely yours, it has nothing in common with mine, and it reaches the same end easily. Now our harmony has begun again.

But, Monsieur, I agree with you in this, find someone elsewhere to follow you in your discoveries concerning numbers, the statements Of which you were so good as to send me. For my own part, I confess that this passes me at a great distance; I am competent only to admire it and I beg you most humbly to use your earliest leisure to bring it to a conclusion. All of our gentlemen saw it on Saturday last and appreciate it most heartily. One cannot often hope for things that are so fine and so desirable. Think about it if you will, and rest assured that I am etc.

Pascal.

Paris, October 27, 1654.

Fermat to Pascal (1660)

Monsieur,

As soon as I discovered that we were nearer to one another than we had ever been before, I could not resist making plans for renewing our friendship and I asked M. de Carcavi to be mediator: in a word I would like to embrace you and to talk to you for a few days, but as my health is not any better than yours, I very much hope that you will do me the favor of coming half way to meet me and that you will oblige me by suggesting a place between Clermont and Toulouse, where I would go without fail towards the end of September or the beginning of October.

If you do not agree to this arrangement, you will run the risk of seeing me at your house and of thus having two ill people there at once. I await your news with impatience and am, with all my heart,

Yours ever,

Fermat.

Pascal to Fermat (1660)

Monsieur,

You are the most gallant man in the world and assuredly I am the one who can best recognize your qualities and very much admire them, especially when they are combined with your own singular abilities. Because of this I feel I must show my appreciation of the offer you have made me, whatever difficulty I still have in reading and writing, but the honor you do me is so dear to me that I cannot hasten too much in answering your letter.

1 will tell you then, Monsieur, that if I were in good health, I would have flown to Toulouse and I would not allow a man such as you to take one step for a man such as myself. I will tell you also that, even if you were the best Geometrician in the whole of Europe, it would not be that quality which would attract me to you, but it is your great liveliness and integrity in conversation that would bring me to see you.

For, to talk frankly with you about Geometry, is to me the very best intellectual exercise: but at the same time I recognize it to be so useless that I can find little difference between a man who is nothing else but a geometrician and a clever craftsman. Although I call it the best craft in the world, it is after all only a craft, and I have often said it is fine to try one’s hand at it but not to devote all one’s powers to it.

In other words, I would not take two steps for Geometry and I feel certain you are very much of the same mind. But as well as all this, my studies have taken me so far from this way of thinking, that I can scarcely remember that there is such a thing as geometry. I began it, a year or two ago, for a particular reason; having satisfied this, it is quite possible that I shall never think about it again.

Besides, my health is not yet very good, for I am so weak that I cannot walk without a stick nor ride a horse, I can only manage three or four leagues in a carriage. It was in this way that I took twenty-two days in coming here from Paris. The doctors recommended me to take the waters at Bourbon during the month of September, and two months ago I promised, if I can manage it, to go from there through Poitou by river to Saumur to stay until Christmas with M. le duc de Roannes, governor of Poitou, who has feelings for me that I do not deserve. But, since I go through Orleans on my way to Saumur by river and if my health prevents me from going further. I shall go from there to Paris.

There, Monsieur, is the present state of my life, which I felt obliged to describe to you so as to convince you of the impossibility of my being able to receive the honor you have so kindly offered me. I hope, with all my heart, that one day I shall be able to acknowledge it to you or to your children, to whom I am always devoted, having a special regard for those who bear the name of the foremost man in the world.

I am, etc.

Pascal.

De Bienassis, 10th August, 1660

[All but the last two letters were translated from the French by Professor Vera Sanford, Western Reserve University, Cleveland, Ohio, and appear in A Source Book in Mathematics *(ed. D E Smith). The last two were translated by Maxine Merrington and appear in* Games, Gods and Gambling *by F N David.]*