"Although Newton was able to unravel the mathematical secrets of the laws of nature, the laws of human nature seem to have eluded his grasp."

When Lions Battle

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"When lions battle, jackals flee." So wrote Isaac Newton to Gottfried Leibniz as their public and vitriolic feud over priority in discovering calculus began. If this quotation sounds unfamiliar to historians of science, it is because it comes from a collection of letters recently discovered in a London estate sale that is already having a tectonic effect on our current understanding of the Newton-Leibniz dispute. Indeed, the date on the previously quoted letter makes it clear that Newton and Leibniz were in fact *discussing matters with each other* as early as 1677, a turn of events that no one has previously postulated. And this, as we shall see, is only the very tip of the iceberg. The battle over priority in the discovery of calculus is arguably the most well-studied and bitter scientific dispute in history. The debate continued for centuries after the original disputants' deaths with charges and recriminations and bitterness flying back and forth across the English Channel as British mathematicians repudiated the calumnies of the Leibnizian Continentals and hurled brickbats of their own. It is only in the past thirty or so years that a consensus view on the three-century-old conflict has developed. (See Hall [3].) Our discovery shatters that consensus and suggests a shocking new explanation of events in the calculus priority war.

The Consensus View

The current consensus holds that in 1665–66, his *annus mirabilis*, Isaac Newton working alone and not telling anyone what he'd done worked out the details of differentiation, integration, and the inverse relation between them. He recognized the inherent difficulty of integration and developed series methods for approximating definite integrals. By no later than October 1666 he was essentially in possession of the ideas and techniques that comprise the first two semesters of the college-level calculus course. (See Westfall [4].) Leibniz traveled a similar path in the years 1673-76, at least as regards differentiation and integration. In 1676 Newton, in response to a request from Leibniz, wrote him two well-known letters containing some hints about differentiation and integration, but mostly concerned series manipulations and representations. Also, during a 1676 visit to London, Leibniz examined letters and draft publications about calculus written years earlier by Newton and shown to Leibniz by Newton's correspondent, John Collins. Leibniz's access to these documents and letters formed the basis for the charges leveled against him many years later that he had plagiarized the calculus from Newton.

Leibniz published first, in 1684 and 1686. Newton was at that time fully engaged in producing his masterwork, the *Principia*. Not eager to enter a priority dispute with Leibniz, but equally unwilling to forego his portion (which he counted the lion's share) of the credit, he inserted a comment into the *Principia* stating that he had told Leibniz ten years previously about his calculus discoveries. And there matters might have rested had not John Wallis and then Nicholas Fatio de Duillier taken it into their heads to publicly pick a fight with Leibniz, asserting not only Newton's priority, but also the inherent superiority of Newton's methods. Leibniz responded in print with others, most especially Johann Bernoulli, coming to his defense. Eventually Leibniz and Newton strayed from their initial positions of publicly recognizing the other's independent discovery and each accused the other of outright plagiary. The conflict lasted beyond the deaths of the main antagonists and English mathematicians scorned Continentals (most of whom were Leibniz supporters) and vice versa for a century. The accepted modern view is that Leibniz and Newton each came to his respective understandings of calculus independently of the other, but even as the opinions of most scholars have converged on this version of events, nagging questions remain. To what degree were the subordinates (e.g., Bernoulli, Wallis) campaigning with their masters' consent? And how was it that both Newton and Leibniz moved so far from their early positions of mutual respect to ones of such reckless animosity?

Priority disputes between seventeenth-century scientists were common as a result of the structure of scientific practice at the time. In the Middle Ages one gained scientific prestige by publicly posing problems to stump others and, conversely, solving the challenges posed by others. It was an advantage to keep one's methods to oneself. University positions were awarded to winners of public problem-solving competitions. As the Scientific Revolution took root, practice moved towards today's model of journal publication of ideas, methods, and discoveries, but in Newton's day the scholarly world was still in transition and nearly every scientist was

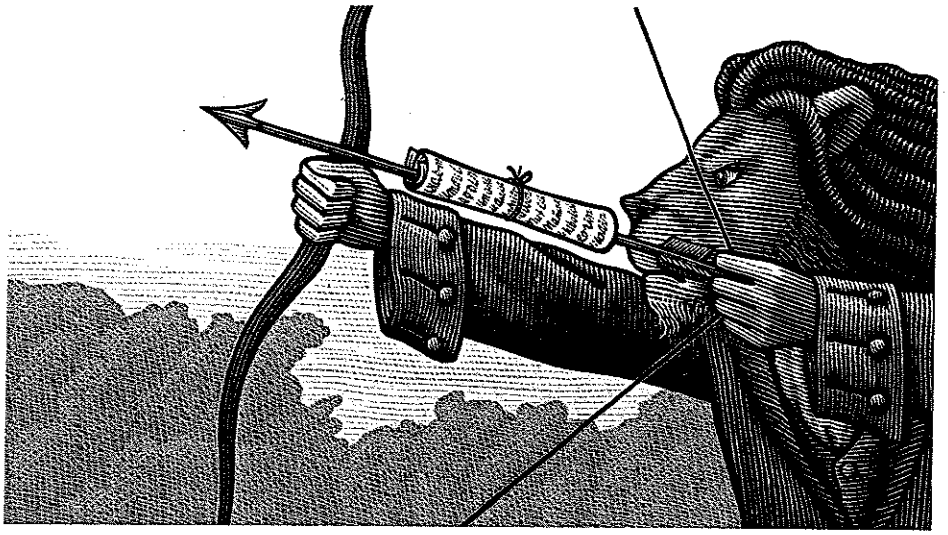

Illustration by Greg Nemec

involved in one or more bitter conflicts. Newton, for example, famously battled Flamsteed, Hooke, and Huygens, in addition to Leibniz. (Newton is perhaps not a reasonable example, being nearly as exceptional for his pugnacity as for his genius.) The important point here is that priority disputes were common enough that it would have been clear to both Newton and Leibniz that they were a hindrance to scientific progress, leading us again to the question of how these masters let such a thing happen. As we shall explain, it appears that although Newton was able to unravel the mathematical secrets of the laws of nature, the laws of human nature seem to have eluded his grasp.

The Missing Letters

The description for item 56AZ1/CHB-02 at a December Christie's auction was innocently labeled as "Early 18th Century Gamebooks and Seasonal Almanacs" and attributed to the library of one Lord Roswell Stephens of Sussex. As it turned out, Stephens, an avid sportsman who was known to walk with a limp due to the loss of several toes in two separate hunting accidents, was the brother-in-law of Hans De Berger, accountant and minor partner in the London accounting firm of Sokal and Conduit. Although spelled differently, this Conduit was indeed the same family as Catherine Conduitt, the niece and, for twenty years, housemate of none other than Sir Issac Newton. Conduitt and her husband John took care of Newton in his old age and, in fact, were alone with him when he died. After his death they purchased Newton's papers from his estate. Most of those papers eventually ended up in the Cambridge University Library. Item 56AZ1 is indeed a small box full of eighteenth-century gamebooks (records of game shot on the Stephens estate), but one item tucked inconspicuously near the bottom was in fact a packet of correspondence that had been mislabeled. Given the contents of these letters and the esteem of the Conduitts for Newton, it is reasonable to surmise that they intentionally hid them from public view after Newton's death.

The packet contains thirty-four letters, all addressed to Isaac Newton. The author of the majority of the letters is Leibniz, although two are signed by Leibniz's famous bulldog, Johann Bernoulli. The first letter is dated January 1677 and was written by Leibniz to thank Newton for his two letters of 1676. The final letter is dated just two days before Leibniz's death in 1716. A book [1] containing reproductions and translations of all thirty-four letters will appear soon, as will a more thorough article [2]. Although scholarly etiquette suggests restraint, the explosive contents of these letters demands that we offer at least a preview of the radical new interpretation of events that is surely to emerge. We begin with an excerpt from the very first letter (translated from the original Latin):

January 13, 1677
My Dearest Newton,

I must express my profound gratitude for your letters of June and October sent on to me by Oldenburg. I have as yet only scratched the surface of the wondrous mysteries whose depths are revealed in them. I am most eager to apply myself to a thorough study of your wonderful ideas, but I felt that I must stop, take pen in hand and acknowledge your generosity. Too, I wish to express my gratitude in a more substantial fashion by explaining to you some of my own notions regarding tangents and quadratures. I suspect, from hints I discern from my first perusal of your letters, that some of these ideas are already known to you.

Imagine a vanishingly small increment of x, which we will call the differential of x, and the corresponding increment in y.
...

After this follows a surprisingly modern sounding explanation of differentiation. The next several letters discuss Leibniz's discoveries in calculus and contain essentially everything one learns in a standard first course. Several of the letters refer to letters of Newton; e.g.,

June 12, 1677
My Dearest Newton,

Yes, it seems that your fluxions are identical to my ratio of differentials. As you say, I was confused by your notation, even

more was I confused by your language. You seem to be conceiving of these curves as being generated by moving points while my methods dispense with that notion and treat the curve as a static object. ...

Much of the rest of this letter, and large portions of the next five, concern the relative advantages and disadvantages of the two different notations they developed. Eventually they apparently agreed to disagree, each preferring his own notation. It is fascinating to observe this discussion (or at least Leibniz's half of it) because it reveals the differences between their intuitions, which are hinted at in the passage above. Newton had a movie running in his head of a particle traversing a path and the tangent was the direction the particle would fly off were it not constrained to the path. Leibniz had no such dynamical intuition, or at least did not exploit one in his exposition, which reads very much like that found in most modern textbooks. It is also clear from these letters, all written long before the public controversy began, that Leibniz acknowledges that Newton was in possession of the calculus long before he was and there is no hint that Newton believes anything other than that Leibniz was an independent, but second, discoverer. The air of mutual respect between these two seventeenth-century geniuses is unmistakable; but it was about to change.

Letter twelve is one of the few letters not from Leibniz, and contains the first clues as to why the recipient of all of these letters went to such lengths to conceal their existence. It was penned by Johann Bernoulli. Bernoulli, of course, would eventually make his reputation applying and defending the calculus he learned from Leibniz, but at the time of this writing he was a mere 23 years old and living obscurely in the shadow of his established brother Jakob.

February 12, 1690
Dear Sir,

I do not need to tell you how great is your reputation as a geometer and philosopher, with your recent publication Philosophiae Naturalis Principia Mathematica being only the latest evidence. Perhaps the greatest compliment I can pay you is to say that you are held in the highest esteem by the great Leibniz, my own teacher, friend and mentor. And this is why I humbly write to you with a request for some assistance with the famous problem of Galileo on the shape of the hanging chain...

Originally posed by Galileo, it is widely known that the problem of finding the proper equation of the catenary curve (as it had come to be called, *catena* is Latin for "chain") was something of an obsession for the older Jakob Bernoulli. Apparently Newton obliged the request because it was later that year, in what is now a famous effort to one-up his brother, that Johann burst onto the intellectual scene by publishing the correct solution as his own. Any doubts that Newton was indeed the legitimate author are put to rest in Bernoulli's follow-up letter where he begs for Newton's "eminent indulgence" to let the deception persist a bit longer in the cause of what amounted to a fraternal practical joke. From a June 18, 1690 letter, the younger Bernoulli writes that

...in my more mathematically naïve days, my older brother persuaded me of the convergence of the harmonic series, a "fact" that I publicly put forth on many occasions to his hysterical delight and my later embarrassment. Thus it is as a form of brotherly revenge that I have created the charade of easily solving the problem of the hanging chain that has vexed poor Jakob for so many years, and, on my most profound honor as a gentleman and a philosopher, I certainly promise to expose the true author of the solution in a timely way.

Yours most humbly,
Johann Bernoulli

But the promised announcement did not come—or at least did not come quickly enough for the ornery English mathematician—and the reason for this may be the same reason why Johann Bernoulli did not go to his mentor Leibniz for help in the first place. Put simply, Bernoulli most likely never had any intention of revealing the truth on this matter. Having smelled a rat, Newton hatched his own plan for a very particular kind of justice—a plan that would require the unknowing assistance of Gottfried Leibniz. The next letter is from Leibniz and reads with a tone of caution and confusion.

April 1, 1691
My Dearest Newton,

Yes, I do agree that not all of our colleagues appear to understand the significance of our discoveries on tangents, quadrature and series. And I also agree that it is unfortunate the degree to which the philosophical community wastes its time bickering over priority. However, I'm not sure I understand your suggested solution to these problems. Is it true that you are suggesting that we engage in a faux public dispute in order to foster a wide and vigorous dissemination of our techniques that might simultaneously convey a gentle lesson about priority disputes? Is that what you mean by "When lions battle, jackals flee?" I do agree that you and I are in a position to give some direction to our colleagues and I do feel a duty to do my best to diminish the occurrence of controversies between philosophers, but what you appear to be suggesting seems to me to at least have the potential to make things much worse. Do you really think that a messy

public battle between us will have the desired effect? Do please write more clearly about what you intend. I'm puzzled and anxious, I remain

Your most affectionate and honored friend,
Leibniz

Over the course of the next few letters Newton and Leibniz work out their scheme with Leibniz, at first reluctant, eventually becoming convinced and enthusiastically encouraging Newton. The plan calls for Newton to convince John Wallis to "stir the coals" with a strongly worded nod to Newton's priority in his forthcoming *Algebra*. Then Leibniz is to counter by enlisting his most outspoken student to come to his defense. "I would agree with your suggestion," Leibniz writes, "that the younger Bernoulli is an excellent candidate for the part." Thus, the greatest scientific priority dispute in history was born of an object lesson gone awry and orchestrated by the author of the *Principia* himself. However, the *Principia* and its author did not take into account the principles of thermodynamics, a mistake that would prove nearly fatal as events began to heat up.

Shortly after Wallis's opening salvo appeared in 1693 Newton received the following formal sounding letter signed by his friend and containing a gift of "mutual respect."

March 15, 1693
Dear Sir,

As these arguments about priority begin to proliferate, it seems important to reaffirm our mutual respect and admiration, both for the truths hidden in nature and for the intellectual integrity you and I have exhibited for each other in our respective pursuits. In this spirit, and knowing of your ongoing researches in alchemy and related matters, I have enclosed a vial of a remarkable metal that I think you will find worthy of more investigations. I offer it to you along with my utmost esteem and regard. Know, Sir, that I am,

Yours most sincerely,
Leibniz

The content of this letter becomes chilling when one ponders the date. Newton's ongoing researches into alchemy are well-documented, and so is his mental breakdown of 1693. Speculations of a relationship between the two are common as it is known that Newton would often ingest different alchemical ingredients as part of his experiments, but this disclosure surely heightens every suspicion. Was Newton unwittingly poisoned? Or, if we adopt a more sinister mindset, was this a *deliberate* attempt at foul play? To make sense of what follows one has to imagine events as they appeared to Newton and Leibniz at this point. The arrangement these two had made had been to start a faux dispute about *priority*, but Wallis implicitly made the debate one about *plagiarism*, a charge that Newton must have assumed Leibniz was not prepared for. Leibniz gives no indication of his thinking in his subsequent letters, content to carry on in the role of willing accomplice. Newton, meanwhile, was mentally incapacitated for much of this year and was unable to engage in any sort of meaningful correspondence.

Newton recovered his health early the following year, and, as we shall see, determined to his own satisfaction that it was indeed some ingredient contained in the alchemical peace offering that had been his undoing. Taking stock of the situation, Newton saw that the budding calculus priority debate was still gaining momentum, and after a quick recalculation, he recognized a golden opportunity. Whereas Johann Bernoulli was the original target of his retribution, Newton readjusted his vengeful sights on none other than the celebrated Leibniz and adapted his scheme accordingly. In 1695 Wallis in the preface to his *Mathematical Works* states, much more plainly than he had in his *Algebra* only two years before, that Newton preceded Leibniz and, in fact, had helped the latter achieve his results. In 1699 Newton's young protégé Nicholas Fatio de Duillier openly questioned (in a volume published by the Royal Society) whether Leibniz was a plagiarist or merely "a second inventor." He also pointedly contrasted Leibniz's "eager zeal" for credit with Newton's "modesty." The gloves had most definitely come off. Experts have long wondered how much influence Newton exerted over Fatio's publication; it seems clear now that it was considerable.

Leibniz, unaware that Newton is orchestrating the new more pointed attacks, begins expressing reservations, and he urges Newton to reconsider their plan and to stop "making goats of our friends and defenders." Newton agrees—or at least pretends to—and as a final gesture proposes that he and Leibniz enter the fray and join the battle in person. The next part of the scheme, and Leibniz's growing doubts about it, are clearly laid out in a letter dated August 1703.

August 3, 1703
My Dearest Newton,

Very well then, we agree. You will assert your priority in your upcoming On Quadratures and I will review it in Acta Eruditorum and respond with my own claim. We will allow our surrogates to dispute our claims for a short period of time. Then we will report in letters published simultaneously and over both of our signatures in Acta Eruditorum and Philosophical Transactions our scheme and our intention. I, at least, will not encourage my friends to criticize you after our

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public claims, I tire of making them our fools and pawns. I confess to you that I feel no great pride in my actions in this conspiracy, I wish that we had never concocted this plot. I very much doubt that we have accomplished any part of our goal of diminishing priority disputes, I fear we have made things worse. I am

Your most humble Servant.
Leibniz

In public, events proceeded as described in this letter, at least up to the point of confession, which never came. Newton asserted in his 1704 *On Quadratures* that he had invented calculus in "1665 and 1666" and Leibniz in an anonymous review of that work published in *Acta Eruditorum* (the journal he had founded) described Newton's work as essentially the same as the differential calculus "discussed by its inventor G.W. Leibniz in these *Acta*." In private, Leibniz regretted this review even before it was published and the next several letters to Newton renounce it and urge Newton to join him in an immediate public confession and apology. It is not clear from Leibniz's letters what arguments Newton is making against this course, but Leibniz's letters exhibit growing shame and increasing frustration over the next few years and eventually, in 1707, he gives up.

September 21, 1707
Dear Sir,

I have burnt your letters. Please burn mine.

Leibniz

Warfare

One might think Newton would have been satisfied with this level of torment but there was more to come. Learning that his letters had been destroyed, Newton took advantage of the fact that his tracks were now untraceable and unleashed his harshest assault to date. In 1710, Englishman John Keill in his "On the Laws of Centripetal Force" (published in the Royal Society's *Philosophical Transactions*) included a blunt accusation that Leibniz had stolen his ideas from Newton and changed the notation to cover up his crime. Leibniz, a Fellow of the Royal Society, immediately demanded an apology. Newton, at this time, was President of the Royal Society and essentially allowed Keill to reiterate his claim in the form of a public document that was sent directly to Leibniz by the Society. Leibniz wrote again the Secretary of the Society demanding an apology and, meanwhile, sent a private letter to Newton.

December 14, 1711
Sir,

I am, as you well know, not a plagiarist. I have acted dishonorably in our schemes and I am guilty of deceptions of which I did not think I was capable. I should add that I am guilty of transgressions of which you yourself may not be aware. I am sorry, I am ashamed, and I am prepared to confess all. But I am not guilty of the crimes for which I have been charged by the Royal Society, and I ask your assistance in repairing my reputation.

Leibniz

Was this what Newton wanted? Was Leibniz finally owning up to a moment of weakness from 18 years earlier in which, as Newton saw it, Leibniz tried to win the calculus priority debate by incapacitating his rival? Uninterested in reconciliation at this point, Newton had Leibniz right where he wanted him and was not about to let up. The Royal Society formed a commission to study the dispute and its report, largely written by Newton himself, concluded that Newton had invented the calculus in the 1660s and communicated the essential ideas to Leibniz in the two letters of 1676. Leibniz had digested the ideas, made some modest improvements and published the ideas with a new notation as his own creation in the 1680s. A vicious and ugly public war of words ensued as scientists all over Europe joined in on one side or the other.

At this point the correspondence breaks off, and there is not another letter for nearly five years. In fact only two letters remain, both written by Leibniz in the year of his eventual death. The first echoes with the sound of contrition as the great German mathematician feels compelled to finally lay everything bare to perhaps the only intellectual equal he has ever known.

August 12, 1716
Dear Newton,

I do not need doctors to tell me that I am coming into the twilight of my days and it is time to make peace with the moments in my life when I let my less noble self take charge of decisions to be made. It is thus with a sad, but honest, heart that I write to tell you of my real motives that dictated my dealings with you. Back in 1691 when you first proposed we stage our faux dispute, your admirable goal was to use it as a means to further proliferate the ideas and methods of our newly discovered geometry as well as provide a moral lesson to our colleagues about priority disputes. However, when you proposed your "scheme," I was at that time in a bitter personal dispute of my own with the young Johann Bernoulli. I shall not give you the details, but let it be said that he was

only an average intellectual talent, conspiring to make a name for himself, and constantly pestering me for solutions to problems that he was not able to solve with his own wit. On several earlier occasions I tried to orchestrate his mathematical demise, once stepping so low as to enlist his brother to instruct Johann that the harmonic series converged. But Johann persisted. (To this day, I am still baffled by how this mediocre mind was able to determine that Jakob's catenary was in fact a logarithmic semi-sum.)

Needless to say, in your plan I saw a way to pursue my own devilish goal of thwarting the career of the young Bernoulli. I regret this deceit most deeply, but what I most regret is the loss of our friendship. Although my motives were not pure, I am still confused as to why you forever postponed the time at which we were to announce the end of our charade. As punishment for my sins, I accept that I may never know the whole truth, but before departing this world I felt it best to let you know the truths of my own heart in this matter.

Yours most sincerely,
Leibniz

Leibniz's candor must have affected Newton deeply because the Englishmen responded in a timely fashion with a letter of his own. Reading Leibniz's next, and final, letter it is clear that in his reply Newton finally saw fit to confront his now dying rival with the accusations that Leibniz had tried to poison him years earlier.

November 12, 1716
My Dearest Newton,

You must accept that a man perched so precipitously on death's doorstep would have no cause to utter anything but what is true and pure. In this spirit, I must tell you that on this particular charge I am as innocent as I am shocked. As to how this confusion has come about, I have no proposition. I can only assert that, not only do I have no memory of sending you any sort of "gift" in 1693, I have never concerned myself with alchemy and would not have access to any sort of offering of this kind to send. In fact, I believe it was around this time that I swore off any possible researches in this area altogether when I learned that the ever-bothersome Bernoulli, while working on his medical degree, accidentally poisoned the family hound with some derived metal that rendered the dog raving mad for the remainder of its life. My decision then was to embrace the advice I gave Johann which was, as I recall, to 'deliver these wretched ingredients to some far away place before you kill someone.'

Toward the end of the letter, Leibniz makes one final plea for reconciliation:

..I am still shamed by our scheme, and my particular role in it, but I am now ready to wholeheartedly denounce it. My shame has until now prevented confession, but no longer. I have begun work on a manuscript describing our plot and our motivation and most abjectly apologizing for the foul deception we have perpetuated upon men who counted themselves our friends and admirers. I entreat you to accept my testimony above and join me in its publication. I will send it to you imminently. It will be published in my Acta as soon as possible. I remain, Sir,

Your humble servant
Leibniz

Forensic analysis of the August 5, 1693 letter containing the poisonous vial has already provided incontrovertible evidence that the letter most certainly did *not* come from Leibniz. Indeed, when we consider both motive and means, all leads now point squarely to Bernoulli as the guilty party. (As of this writing, the handwriting and the chemical composition of the ink are being compared to known samples of Bernoulli correspondence from 1693.) The implications here are, of course, stunning. Leibniz died two days after writing the letter quoted above. No trace of the manuscript he describes in the closing paragraph has ever been found and we assume he did not have the strength to pursue this final confession. Bernoulli went on to a celebrated career, teaching Liebniz's calculus to whoever would listen. As deplorable as we now see this snake in the garden to be, we must also give Bernoulli his due. Although he was the intended target of retribution of the two greatest minds of the 17th century, he somehow managed to slither his way out of the crossfire and ended up dancing on the heads and shoulders of both of the giants.

And what about Isaac Newton? Did he accept Leibniz's denials and, by necessity, then recognize the colossal error in his own calculus? We may not ever know the answer. If, however, we are to insist on a moral to the story then perhaps it should be this: the history of mathematics, like all human endeavors, is rich with miscalculations and misconceptions, and things are not always what they seem. Let us just affirm that skepticism should ever be our byword. ■

References

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[2] S.D. Abbott and S.F. Kennedy, "The Leibniz Letters to Newton 1677-1716", *Histeria Mathematica* [23], June 2007.

[3] A. Rupert Hall, *Philosophers at War*, Cambridge University Press, 1980.

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