

Comment on Norsen’s defense of Einstein’s “box argument”

Abner Shimony<sup>a)</sup>

*Departments of Philosophy and Physics, Boston University, Boston, Massachusetts 02215*

(Received 9 August 2004; accepted 10 September 2004)

[DOI: 10.1119/1.1811623]

In 1927 Einstein<sup>1</sup> presented a precursor of the much better known argument of Einstein–Podolsky–Rosen (EPR)<sup>2</sup> that the quantum mechanical description of physical reality is either incomplete or violates locality by tacitly assuming superluminal causality in the measurement process. He proposed a thought experiment in which the support of the wave function of a ball is contained within two boxes,  $B_1$  and  $B_2$ , that are carried arbitrarily far from each other. When a measurement is performed to detect whether the ball is in  $B_1$ , not only is a positive or negative answer found regarding that box, but simultaneously a negative or positive answer is obtained regarding box  $B_2$ . He concluded that either the wave function is an incomplete description of the ball, requiring supplementation by additional information concerning its location, or else an experimental intervention in  $B_1$  immediately causes a determination of its presence or absence in  $B_2$ , which would constitute non-local causation.

Norsen<sup>3</sup> has deplored the neglect of Einstein’s “box” argument, claiming not only that it is valid, but is superior to EPR because of its simplicity: it involves only a single system, considers only the position of that system rather than a pair of non-commuting properties, and above all avoids the need to invoke counterfactual reasoning. (EPR not only infers the position of particle I from the position of particle II, or vice versa if position is measured on one of the particles, but they infer the momentum of one particle from the momentum of the other if momentum is measured on either.)

However, there is a serious flaw in Norsen’s defense of the box argument in my opinion. If  $Q_1$  is the projection operator representing the physical proposition that the ball is in  $B_1$  and  $Q_2$  is the projection operator corresponding to the ball’s location in  $B_2$ , then the logical structure of the lattice of projections in the quantum mechanics of localizable systems ensures that  $Q_1$  and  $Q_2$  are orthogonal, that is,

$$Q_1 Q_2 = Q_2 Q_1 = 0, \tag{1}$$

independently of the quantum state of the ball. Hence any state that is an eigenstate of  $Q_1$  with eigenvalue 1 or 0 will also be an eigenstate of  $Q_2$  with respective eigenvalue 0 or 1. (The relation of  $Q_1$  and  $Q_2$  to  $B_1$  and  $B_2$ , which is accepted intuitively by physicists, is generalized and presented with mathematical rigor in Mackey’s discussion<sup>4</sup> of projection-valued measures on Borel spaces.) Because Eq. (1) is independent of the quantum state of the ball, the inference from a measurement yielding the location or non-location of the ball in  $B_1$  to the respective non-location or location in  $B_2$  does not rely on any contingencies, and therefore it is fair to say that it is a matter of logic rather than of causality. Hence if one rejects Einstein’s contention that quantum mechanics

gives an incomplete description of the ball, in the sense that its location is definitely in  $B_1$  or  $B_2$  even when the wave function does not vanish identically in either box, one is not forced to accept non-local causality as the only alternative. The logical determination of the truth value assigned to  $Q_2$  when the truth value of  $Q_1$  is determined by measurement (or conversely) does not constitute an act of causality.

This statement is made with a minimum commitment to any explanation of the relation between the quantum theory of measurement and relativistic space–time structure. One possible explanation is the proposal by Fleming and Bennett<sup>5</sup> that the objective quantum state has a different representation as a wave function in each reference frame. The only claim made here, contrary to that of Norsen,<sup>3</sup> is the non-causal character of the mutual determination of the truth values of  $Q_1$  and  $Q_2$ .

A corollary of my claim is that the EPR argument for the incompatibility of locality with the completeness of a quantum mechanical description of physical reality is definitively stronger than the box argument. The quantum state studied by EPR is an entangled two-particle state, which is a contingency. If the two-particle state were a product state, then the strict correlation of the positions of particles 1 and 2 and the strict correlation of the momenta 1 and 2 could not be derived, and therefore the strict relation of the projections onto regions of position space and the strict relation of the projections onto regions of momentum space are not matters of logic. Therefore EPR is justified in concluding that the only way to save local causality is to postulate the incompleteness of the quantum mechanical description, and, as we now know, this conclusion opens the path to Bell’s theorem and to the experiments that strongly point to non-locality in the physical world.

A final remark concerns Norsen’s argument that the EPR argument is inferior to the box argument because of its reliance on counterfactual reasoning. Although many scholars accept that characterization of EPR’s argumentation, it has been plausibly maintained by d’Espagnat<sup>6</sup> and Shimony<sup>7</sup> that EPR’s reasoning for the strict correlation of the positions and the momenta of the two particles can be carried out by ordinary inductive logic, using four subensembles of the entire ensemble of particle pairs, with no reliance on counterfactual conditionals.

In spite of the foregoing criticisms, Norsen<sup>3</sup> deserves recognition for his examination of the box argument, which has subtleties worthy of further study, particularly concerning the temporal relation of logically equivalent events in regions with spacelike separation.

<sup>a)</sup>Electronic mail: shimony@verizon.net

<sup>1</sup>For example, see Max Jammer, *The Philosophy of Quantum Mechanics* (Wiley, New York, 1974), pp. 115–117; L. de Broglie, *The Current Interpretation of Wave Mechanics: A Critical Study* (Elsevier, Amsterdam, 1964), pp. 24–26; L. E. Ballentine, “Einstein’s interpretation of quantum mechanics,” *Am. J. Phys.* **40**, 1763–1771 (1972).

<sup>2</sup>A. Einstein, B. Podolsky, and N. Rosen, “Can quantum mechanical description of physical reality be considered complete?” *Phys. Rev.* **47**, 777–780 (1935).

<sup>3</sup>T. Norsen, “Einstein’s boxes,” *Am. J. Phys.* **73**, 164–176 (2005).

<sup>4</sup>G. Mackey, *Induced Representations of Groups and Quantum Mechanics* (Benjamin, New York, 1968), p. 47.

<sup>5</sup>G. Fleming and H. Bennett, “Hyperplane dependence in relativistic quantum mechanics,” *Found. Phys.* **19**, 231–267 (1989).

<sup>6</sup>B. d’Espagnat, “Nonseparability and the tentative descriptions of reality,” *Phys. Rep.* **110**, 201–264 (1984).

<sup>7</sup>A. Shimony, “The logic of EPR,” *Ann. Fond. Louis de Broglie* **26**, 399–410 (2001).

## Paradoxical games and a minimal model for a Brownian motor

R. Dean Astumian<sup>a)</sup>

*Department of Physics and Astronomy, University of Maine, Orono, Maine 04469-5709*

(Received 12 May 2004; accepted 29 July 2004)

I give an extended analysis of the very simple game that I previously published that shows the paradoxical behavior whereby two losing games randomly combine to form a winning game. The game, modeled on a random walk, requires only two states and is described by a first-order Markov process. © 2005 American Association of Physics Teachers. [DOI: 10.1119/1.1801191]

### I. INTRODUCTION

In 1986–7 my colleagues and I posed the following “paradox.”<sup>1,2</sup> Consider a random walk on a cycle of bases governed by one of two sets of transition constants for stepping between the bases. Using either set alone, the walk is biased to favor completion of a counterclockwise cycle. However, either periodic<sup>1</sup> or random<sup>2</sup> alternation between the two sets causes net clockwise cycling!

To illustrate as simply as possible this paradoxical behavior, I published<sup>3</sup> a very simple game played with a checker stepping on part of a checkerboard. The stepping is decided by the roll of a pair of dice. Alternation between two sets of rules for the stepping is achieved by flipping a coin. In a recent paper<sup>4</sup> Martin and von Baeyer discussed these “Astumian games” and asserted that my analysis was flawed, and that the game does not display the paradoxical behavior that I claimed. They further claimed that “the inherent symmetry of Astumian’s game prevents it from achieving its purpose.” Here, I give an extended description of my game and show that the analysis of Martin and von Baeyer only treats a special case of my model that is different than the case on which I focused, and that, in general, my game does show the paradoxical behavior first demonstrated in Refs. 1 and 2.

The game is illustrated in Fig. 1. A checker is placed on the middle square of a sequence of five squares. The object is to reach the square marked “win” before reaching the square marked “lose” by rolling the dice and displacing the checker according to one of two possible sets of rules. Apparently, Martin and von Baeyer<sup>4</sup> misread the rules of my game and took a move to mean only a physical displacement of the checker rather than a turn consisting of rolling the dice and following the instructions on the relevant rule table. In their picture, the checker is displaced either to the left or right on each move and the sum of the transition probabilities to the left and to the right from any square is constrained to be one. In my game, however, which is inspired by a Monte Carlo simulation of a random walk, there are three possibilities for the checker on each move: step left, step right, or stay put. When we play a single game, both interpretations lead to the

same ratio of wins to losses. For game 1 we calculate  $(8 \times 2) / (4 \times 5) = 16/20$ , which is equal to the ratio calculated in Ref. 4 equal to  $((2/7) \times (2/3)) / ((5/7) \times (1/3)) = 4/5$ . The identical ratio of wins to losses holds for game 2, so clearly each game is one that on average the player loses.

The situation is dramatically different when we combine games 1 and 2 by flipping a coin after each roll of the dice to select the rule set by which the next decision is made. The correct way of calculating the result of combining the two games by a coin flip is to average the frequencies for stepping left and right. The coin flip randomizes the chance of playing by rule set 1 or by rule set 2 on each roll of the dice. Starting at the middle there is a 50/50 chance of playing by rule set 1, for which the frequency of displacement to the right is  $2/36$ , and a 50/50 chance of playing by rule set 2 for which the frequency of displacement to the right is  $8/36$ . Thus the net chance of being displaced to the right on the next roll is  $10/72$ . Exactly the same  $10/72$  holds for the frequency of displacement to the right from a white square. The average frequency for displacement to the left from either white or black is  $9/72$ . When we play the combined game we win more often than we lose  $[(10 \times 10) / (9 \times 9)]$ , i.e., 55% wins expected], even though when playing either game alone we lose more often than we win  $[(8 \times 2) / (5 \times 4)]$ , i.e., 45% wins expected].

### II. KINETIC BARRIER REPRESENTATION

In Fig. 2 the game is translated into a “kinetic barrier” diagram<sup>5</sup> commonly used to describe biochemical processes. In this picture, the mechanism by which the paradoxical results of Ref. 2 arise becomes clearer. If we play either game alone, the net tilt from right to left leads inexorably to more losses than wins. However, things are very different if we flip a coin before each roll of the dice to determine whether the next decision will be made according to rule set 1 or rule set 2, effectively averaging the frequencies. In the combined game, the frequency for a transition to the left from either a black or white square is  $5/36$  and for a transition to the right

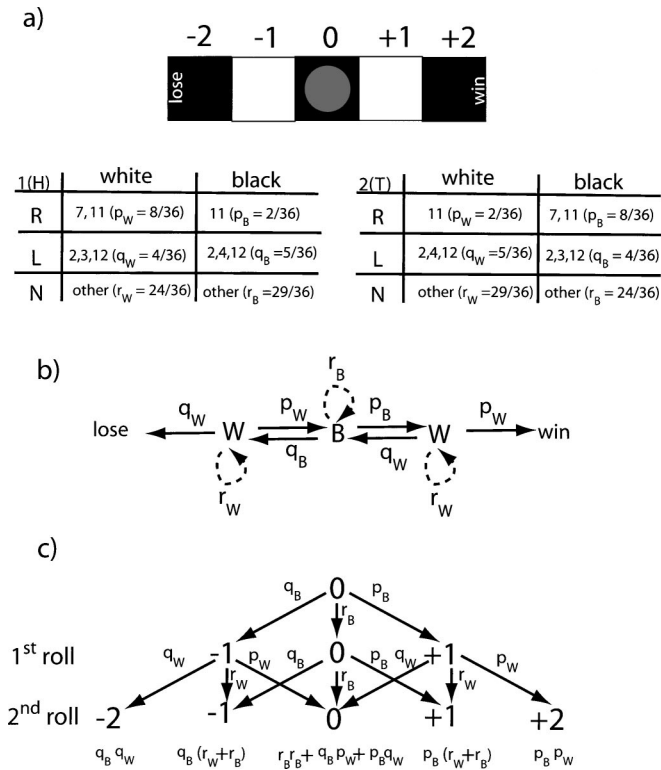


Fig. 1. (a) The simplest Astumian Game is played with a checker on five squares of a checker board. The stepping of the checker toward a “win” or “loss” is governed by the roll of a pair of dice according to one of the two rule tables labeled 1(H) and 2(T). For each roll there are three possibilities—step left (L), step right (R), or step in place (N). The rolls that trigger each response depend on whether the checker is on a black (B) or white (W) square. Thus, when on the center black square playing game one, if an 11 is rolled (2 ways out of 36 possible rolls), the checker is moved to the white square on the right; if a 2, 4, or 12 is rolled (5 rolls out of 36), the checker is moved to the white square on the left; and on any other roll, the checker remains on the central black square and the dice are rolled again. When the checker is on either white squares, the checker is moved one square to the right on a roll of 7 or 11 (there are 8 ways out of the 36 total possible rolls), and on a roll of 2, 3, or 12 (4 rolls out of 36) the checker is moved to the left. In the randomly combined game a coin is flipped before each roll. If the coin lands on heads, the following roll is played according to Table 1(H), and if it lands tails, the following roll is played according to Table 2(T). (b) A random walk diagram for the game. The dashed circular arrows indicate the “self transitions” with probabilities  $r_B$  on a black square and  $r_W$  on a white square. (c) A probability tree for the random walk. Martin and von Baeyer (Ref. 4) analyzed only the special case that  $r_B = r_W = 0$  for both games.

from either color square is  $4.5/36$ . Thus the kinetic barrier diagram for the combined game now tilts to the right, leading inexorably to more wins than losses.

### III. THE GAME AS A RANDOM WALK

In Fig. 1(b), the game is illustrated as a random walk on a one-dimensional lattice with two states, “Black” (B) and “White” (W). The probabilities for a transition to the left are denoted by  $q_B$  and  $q_W$ , and the probabilities for a transition to the right are denoted by  $p_B$  and  $p_W$ . The  $r_{B(W)} = 1 - p_{B(W)} - q_{B(W)}$  denote the probability that no transition occurs on a given move, and so are a measure of the lifetime on a particular square. In the literature a move in which no transition occurs is often termed a “self-transition.”

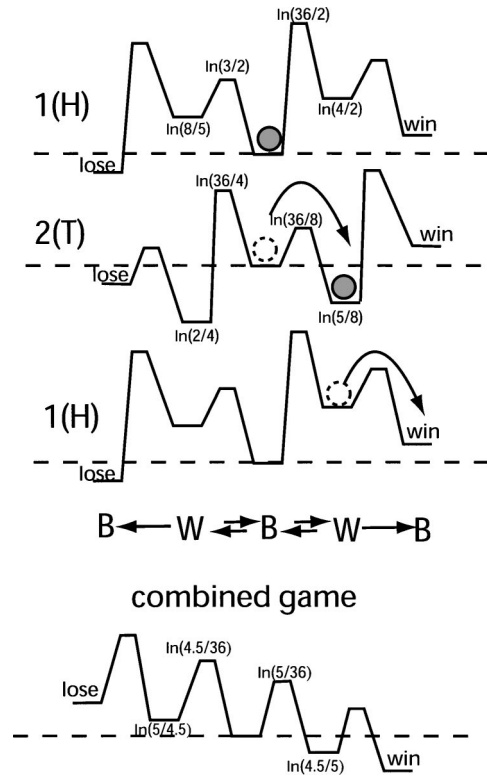


Fig. 2. Kinetic barrier diagram representation of the game. The barriers are proportional to the log of the inverse frequencies, and the difference in levels between adjacent B and W squares depend on the log of the ratio of frequencies as shown. We arbitrarily set the level of the starting square to zero. The net tendency is to move from high to low, so it is apparent from the net tilt to the left that both game 1 and game 2 on average are losing propositions. When we combine the games by flipping the coin, the frequencies are averaged, resulting in the kinetic barrier diagram for the combined game, which tilts to the right and is clearly, on average, a winning proposition.

Figure 1(c) shows a “probability tree” for the Astumian game. The ratio of wins to losses for either game is  $p_{B,i}p_{W,i}/(q_{B,i}q_{W,i})$ , with  $i = 1, 2$  denoting game 1 or 2. The random combination of the two games by flipping a coin results in averaging the  $p$ 's and  $q$ 's so in the combined game the ratio of wins to losses is  $(p_{W,1} + p_{W,2})(p_{B,1} + p_{B,2}) / [(q_{W,1} + q_{W,2})(q_{B,1} + q_{B,2})]$ . For the paradox, we need

$$p_{W,1}p_{B,1} < q_{W,1}q_{B,1}, \quad (1a)$$

$$p_{W,2}p_{B,2} < q_{W,2}q_{B,2}, \quad (1b)$$

$$(p_{W,1} + p_{W,2})(p_{B,1} + p_{B,2}) > (q_{W,1} + q_{W,2})(q_{B,1} + q_{B,2}), \quad (1c)$$

which is very easily met, for example, with the values given in the tables in Fig. 1(a). Taking the case, however, that self-transition probabilities from black and white in each game are the same,  $r_{B,1} = r_{W,1} = r_1$  and  $r_{B,2} = r_{W,2} = r_2$  ( $r_1 = r_2 = 0$ , the case treated by Martin and von Baeyer, is a special case of this) and writing the  $q$ 's in terms of the  $r$ 's, Eqs. (2a), (2b), and (2c) lead to (3a), (3b), and (3c), respectively,

$$(p_{W,1} + p_{B,1}) < (1 - r_1), \quad (2a)$$

$$(p_{W,2} + p_{B,2}) < (1 - r_2), \quad (2b)$$

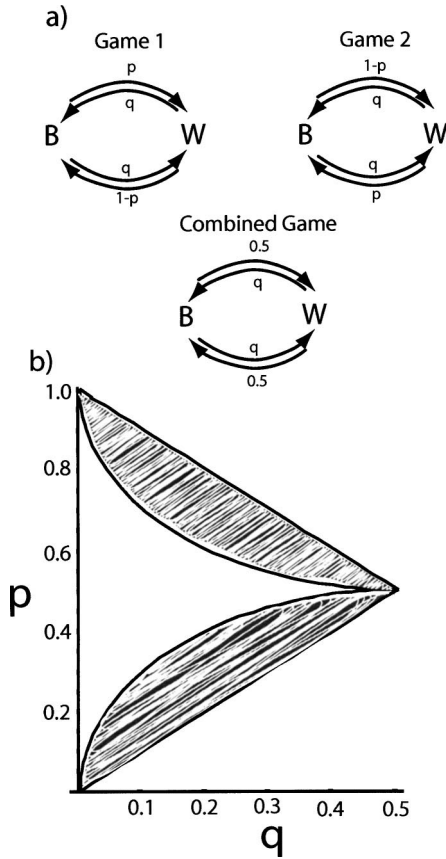


Fig. 3. (a) Compact cycle diagrams for the Astumian game with the very simple two-parameter case discussed in Sec. IV. (b) A graphical illustration of the region in  $\{p, q\}$  parameter space in which the paradoxical behavior occurs. The total transition probability out of either state must be less than 1, that is,  $p+q < 1$  and  $(1-p)+q < 1$  so that only the region above the line  $p=q$  and below the line  $p=1-q$  is consistent with the transition probabilities for this model. The two games are individually losing only when  $q^2 > p(1-p)$ , and because the combined game is winning for all  $q < 0.5$ , the shaded areas define the region where the paradoxical reversal from losing to winning by a random combination occurs.

$$(p_{W,1} + p_{W,2} + p_{B,1} + p_{B,2}) > (2 - r_1 - r_2), \quad (2c)$$

which is a contradiction. Thus, the paradoxical reversal of two losing games to a winning game by a random combination is impossible for games where the self-transition probabilities from black and white are the same in each game.

#### IV. TWO PARAMETER MODEL FOR THE PARADOXICAL BEHAVIOR

As a very simple case we can demonstrate the “paradox” with only two parameters. Let  $q_{B,1} = q_{W,1} = q_{B,2} = q_{W,2} = q$ ,  $p_{B,1} = p_{W,2} = p$  and  $p_{W,1} = p_{B,2} = 1-p$ , which leads to  $r_{B,2} = r_{W,1} = 1-(p+q)$  and  $r_{B,2} = r_{W,1} = p-q$ . We require  $p+q < 1$  and  $(1-p)+q < 1$ . In either individual game, the ratio of wins to losses is  $p(1-p)/q^2$  while in the combined game the ratio of wins to losses is  $0.5/q^2$ . In Fig. 3(a) a compact cycle representation, used in early studies of the paradox,<sup>6</sup> is given. The completion of a clockwise cycle corresponds to a win, and the completion of a counterclockwise cycle corresponds to a loss. The region of parameter space  $\{q, p\}$  in which the paradox occurs is graphically illustrated in Fig. 3(b), where we plot the curves  $q=p$ ,  $q=1-p$  [this defines

the region in which  $p+q < 1$  and  $(1-p)+q < 1$ ] and  $q^2 = p(1-p)$ , which defines the region in which the two games are individually biased to favor counter-clockwise cycling. The paradox occurs in the shaded region because for all  $q < 0.5$  the combined game is biased to favor clockwise cycling.

#### V. GENERALIZED ASTUMIAN GAMES

Imagine that instead of moving on only five squares, the checkerboard rank extends indefinitely from the starting point in both directions, and we take a model with general forward ( $p_{B,i}$  and  $p_{W,i}$ ) and backward ( $q_{B,i}$  and  $q_{W,i}$ ) frequencies between zero and one, where  $i=1, 2$ , or  $c$ , with  $c$  denoting the combined games. This is a “modulo-2” game because there are only two color squares: black and white.

We allow the possibility of flipping two different, possibly biased, coins to determine which of the two games is played on the next roll; one coin if the checker is on the black square, with probability to land heads  $h_b$ , and the other if the checker is on the white square, with the probability to land heads  $h_w$ . For any of the games (including the combined game) the average gain,  $G_i$ , for the  $n+1$ st roll is

$$G_i(n+1) = (p_{B,i} - q_{B,i})P_B(n) + (p_{W,i} - q_{W,i})(1 - P_B(n)). \quad (3)$$

$P_B(n)$ , the probability that the checker is on a black square after the  $n$ th roll, can be calculated from the recursion relation

$$P_B(n) = (1 - p_{B,i} - q_{B,i})P_B(n-1) + (p_{W,i} + q_{W,i})(1 - P_B(n-1)), \quad (4)$$

which is the fraction on black that stays on black plus the fraction on white that moves to black in a given roll. The probability that the checker is on a white square is obviously  $P_W(n) = 1 - P_B(n)$  so the game involves only a single variable. This game is a first-order Markov chain because  $P_B(n)$  depends only on the probability one step before,  $P_B(n-1)$ .

The stable solution to Eq. (4) occurs when  $P_B(n) = P_B(n-1)$ . Hence, we find

$$P_B^{ss} = \frac{p_{W,i} + q_{W,i}}{p_{W,i} + q_{W,i} + p_{B,i} + q_{B,i}}. \quad (5)$$

We substitute Eq. (5) into Eq. (1) and find the average displacement once the stable solution has been reached:

$$G_i = \frac{p_{W,i}p_{B,i} - q_{W,i}q_{B,i}}{p_{W,i} + q_{W,i} + p_{B,i} + q_{B,i}}. \quad (6)$$

Clearly, the average gain is zero (a condition known as detailed balance) if and only if the product of the forward frequencies is equal to the product of the backward frequencies.

For the combined game the frequencies are the weighted averages of the frequencies for games 1 and 2,

$$p_{W,c} = h_w p_{W,1} + (1 - h_w) p_{W,2}, \quad (7a)$$

$$q_{W,c} = h_w q_{W,1} + (1 - h_w) q_{W,2}, \quad (7b)$$

$$p_{B,c} = h_b p_{B,1} + (1 - h_b) p_{B,2}, \quad (7c)$$

$$q_{B,c} = h_b q_{B,1} + (1 - h_b) q_{B,2}. \quad (7d)$$

As pointed out in Ref. 2, the combined game need not display detailed balance even if the individual games do. Consider the case with frequencies  $p_{B,1} = 25/36$ ,  $p_{W,1} = 1/36$ ,  $q_{B,1} = 5/36$ ,  $q_{W,1} = 5/36$ , and  $p_{B,2} = 1/36$ ,  $p_{W,2} = 25/36$ ,  $q_{B,2} = 5/36$ , and  $q_{W,2} = 5/36$ . Clearly both games 1 and 2 individually obey a detailed balance (the product of the forward frequencies equals the product of the reverse frequencies), but the combined game with  $h_w = h_b = 0.5$  ( $p_{B,c} = 13/36$ ,  $p_{W,c} = 13/36$ ,  $q_{B,c} = 5/36$ , and  $q_{W,c} = 5/36$ ) does not.

We could derive a condition for the detailed balance for the combined game directly by requiring that  $p_{W,c} p_{B,c} - q_{W,c} q_{B,c} = 0$  and substituting the transition probabilities from Eq. (5). A more elegant approach is to directly impose microscopic reversibility. Thus, starting on black in game one, the probability to switch to game 2, move right, switch to game 1, and move left must equal the probability to move right, switch to game 2, move left, and switch to game 1, its microscopic reverse. That is,

$$(1 - h_b) p_{B,2} h_w p_{W,1} - q_{B,1} (1 - h_w) q_{W,2} h_b = 0. \quad (8)$$

Equation (8) is the same condition as that derived for detailed balance using a diagram method for an enzyme kinetic model.<sup>6</sup> The left-hand side is proportional to the average displacement, so it is obvious both formally as well as intuitively<sup>7</sup> that the deterministic strategy of always playing game 1 when on black ( $h_b = 1$ ) and game 2 when on white ( $h_w = 0$ ) maximizes the win rate, and is better than any random strategy with  $h_b = h_w$ .

## VI. RELATIONSHIP TO SIMPSON'S PARADOX

The condition for the paradoxical conversion of two losing games into a winning game by statistically combining them is  $G_1, G_2 < 0$  and  $G_c > 0$ . This reversal is reminiscent of the well-known Simpson's paradox,<sup>8</sup> which refers to the reversal of the direction of a comparison or an association when data from several groups are combined to form a single group.<sup>9</sup>

A classic example<sup>10</sup> involves a drug test in which 30 males take a drug, and 18 recover (60%), but in a control group of 10 males with the same illness whom do not take the drug, 7 recover anyway (70%). Out of 10 females who take the drug, only 2 recover (20%), while in a control group 9 out of 30 who do not take the drug recover (30%). Thus, it is clear that there is no statistical evidence that the drug helps either males or females. However, when we combine the data, we come up with a total of 40 people who took the drug, out of which 20 recover (50%), whereas in the aggregate control group of 40 people with the illness who did not take the drug, only 16 recovered (40%). From this perspective it appears that the drug is helpful.

The paradoxical result—that the drug is deleterious when considered from the perspective of males or females individually, but helpful when considered from the perspective of the overall population—arises from a hidden correlation. Males, who are in general more likely to recover than are females, make up most of the experimental cohort, but females make up most of the control cohort. There is no para-

dox when this difference is recognized—the drug should not be administered to males or to females. Note that there could be other “lurking” variables, such as age, which might reverse this conclusion. This example should serve as a warning on how easy it is to fool ourselves with statistics unless we understand all potential correlations between the data.

The reversal of losing games to form a winning game when combined arises from a similar “hidden” correlation. In the simplest Astumian game in Fig. 1, the checker tends to reside on the black square for more rolls than on the white square in game 1H, but on the white square for more rolls than on the black square in game 2T. Thus, it is more likely for a checker on a white square to switch to game 2, where a right transition is more likely than a left one, and for a checker on a black square to switch to game 1, where once again a transition to the right is more likely than one to the left. The combination gives rise to a net tendency to move right, even though in either game alone the tendency is to move left.

When we strip away the veneer of dice or coin games or of datasets, and we simply consider the counterintuitive behavior in terms of numbers, we see that the condition for Simpson's paradox to occur is to find numbers such that  $a/A < b/B$  and  $c/C < d/D$ , but  $(a+c)/(A+C) > (b+d)/(B+D)$ . For the drug example,  $a$  is the number of males who took the drug and recovered and  $A$  is the total number of males who took the drug, etc. The requirement for the Astumian game to demonstrate the paradoxical result is  $p_{W,1} p_{B,1} / (q_{W,1} q_{B,1}) < 1$  and  $p_{W,2} p_{B,2} / (q_{W,2} q_{B,2}) < 1$ , but  $p_{W,c} p_{B,c} / (q_{W,c} q_{B,c}) > 1$ . Obviously this requirement can be converted into the identical form for Simpson's paradox. Thus we conclude that the result that two losing games can combine to form a winning game is, in fact, a variation of Simpson's paradox.

## VII. MINIMAL MODEL FOR A BROWNIAN MOTOR

The Astumian games clearly demonstrates that a first-order Markov chain, a two-state (modulo-2) system, is sufficient to demonstrate the paradoxical behavior that two losing games can be combined to form a winning game. This game is the discrete-time analog of the continuous time two-state chemical kinetic mechanism,<sup>6</sup> which is the minimal model for a Brownian motor.<sup>11</sup> Nonetheless, in addition to Ref. 4, other authors have incorrectly claimed that the minimal model for demonstrating the paradoxical reversal of the direction of a flow requires a second-order (three-state or modulo 3) system.<sup>7,12,13</sup> Why have so many authors arrived at the same erroneous conclusion? By reviewing these papers we find that in all an unjustified constraint is placed on the transition coefficients, namely that the sum of the transition constants out of a state is required to equal one. In other words, the system is not allowed to remain in the same state in any iteration. But in general, a particle undergoing a one-dimensional motion on a lattice can move forward, backward, or stay in the same place; a stock price tomorrow can be greater than, less than, or equal to the price today; and in a game of chess, a player can win, lose, or draw. From where did the idea come that there are only two possibilities?

Perhaps the answer lies in the recent work of Juan Parrondo who introduced games based on tossing one of several biased coins, in which also two losing games combine to form a winning game (or *vice versa*).<sup>14</sup> Because the games

are played with a coin, each flip of the coin results in a binary decision: win–lose, or left–right. To achieve the required asymmetry, one of the games Parrondo described involved a rather complicated rule where the coin to be flipped is determined by the amount of money the player has on that turn. However, Parrondo’s “coin flip” games can be mapped onto a Markov cycle<sup>12,13</sup> identical to the cycles used in the original formulation of the paradox,<sup>2</sup> except that the sum of the two transition rates out of each state must equal one. The authors<sup>12,13</sup> have pointed out that these resulting cycle models are time symmetric with fewer than three states, and based on this made the claim that a three-state model (or equivalently a modulo three system) is the minimal model

$$(B,H) \xrightarrow{1-h_B} (B,T) \xrightarrow{p_{B,2}+q_{B,2}} (W,T) \xrightarrow{h_W} (W,H) \xrightarrow{p_{W,1}+q_{W,1}} (B,H),$$

with the reverse of the sequence,

$$(B,H) \xrightarrow{(p_{B,1}+q_{B,1})} (W,H) \xrightarrow{1-h_W} (W,T) \xrightarrow{(p_{W,2}+q_{W,2})} (B,T) \xrightarrow{h_B} (B,H).$$

The ratio of the probabilities for these two sequences is

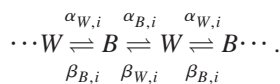
$$\frac{(1-h_b)(h_w)(p_{B,2}+q_{B,2})(p_{W,1}+q_{W,1})}{(1-h_w)(h_b)(p_{B,1}+q_{B,1})(p_{W,2}+q_{W,2})}. \quad (9)$$

If the escape frequencies are different for some states than for others, this ratio need not be unity even for a random combination ( $h_w=h_b$ ) of the games, and the combined game can thus be time asymmetric. The original continuous time<sup>6</sup> and discrete time<sup>3</sup> Astumian games are composed of two individual games, each of which is losing and time symmetric, and yet which randomly combine to form a winning game with broken time symmetry.

When we insist that the lifetimes in the states are the same [that is,  $(p_{W,1}+q_{W,1})=(p_{B,1}+q_{B,1})$  and  $(p_{W,2}+q_{W,2})=(p_{B,2}+q_{B,2})$ ], we lose the time asymmetry in the combined two-state game; clearly in this case, for  $h_b=h_w$  the ratio of the probabilities for forward and reverse sequences is unity. As we have seen in Sec. III, with this constraint the paradoxical reversal by a random combination of the “games” is impossible. Time asymmetry can be restored by adding another state which builds asymmetry into one of the individual games, but this addition certainly does not lead to a minimal model as claimed in Ref. 12.

## IX. FROM CHEMICAL KINETICS TO GAMES

The game was inspired by the earlier work on random walks on biochemical cycles,<sup>2,6</sup> which are represented by chemical kinetic models such as shown below.



The time evolution of the system is given by

$$\frac{dP_B}{dt} = (\alpha_{W,i} + \beta_{W,i})(1 - P_B) - (\alpha_{B,i} + \beta_{B,i})(P_B), \quad (10)$$

where we have used conservation  $P_B + P_W = 1$ . The system above describes a two-state cycle that could equally well be

capable of demonstrating the paradoxical result by which two losing games randomly combine to form a winning game. However, for the Astumian games, with unequal lifetimes in two states, the combination itself breaks time symmetry, and so a two-state model is sufficient.

## VIII. BREAKING TIME SYMMETRY

Any two-state Astumian game is clearly time symmetric—there is no way to distinguish a sequence, say  $B \rightarrow W \rightarrow B$ , from its reverse. When we combine the games, however, we must specify both the state of the checker ( $B$  or  $W$ ) and the state of the coin ( $H$  or  $T$ ). Let us compare a sequence:

written in the compact cycle form shown in Fig. 3(a). The subscript  $i$  denotes the chemical state, for example, phosphorylated ( $i=1$ ) or dephosphorylated ( $i=2$ ). The chemical energy supply (for example, the hydrolysis of Adenosine Triphosphate, ATP) powers the random flipping between the two chemical forms.

Here, the  $\alpha$ ’s and  $\beta$ ’s are rate constants, with dimensions of inverse time, rather than dimensionless transition probabilities. Such biochemical cycles are often simulated by a Monte Carlo approach<sup>2</sup> in which each transition is modeled as a Poisson process. For the two-state model, the probability  $P(B, t + \Delta t | W, t)$  that a system in state  $W$  at time  $t$  has made at least one transition to a surrounding state  $B$  by time  $t + \Delta t$  is

$$P(B, t + \Delta t | W, t) = \int_t^{t+\Delta t} (\alpha_W + \beta_W) e^{-(\alpha_W + \beta_W)t} dt = 1 - e^{-(\alpha_W + \beta_W)\Delta t}. \quad (11)$$

For sufficiently small  $\Delta t$  there will be at most one transition during the interval so the transition probabilities to the left and right are simply this total probability multiplied by the branching probabilities,

$$p_{W,i} = \frac{\alpha_{W,i}}{\alpha_{W,i} + \beta_{W,i}} (1 - e^{-(\alpha_W + \beta_W)\Delta t}), \quad (12a)$$

$$q_{W,i} = \frac{\beta_{W,i}}{\alpha_{W,i} + \beta_{W,i}} (1 - e^{-(\alpha_W + \beta_W)\Delta t}), \quad (12b)$$

$$r_{W,i} = 1 - p_{W,i} - q_{W,i}, \quad (12c)$$

with similar results, derived in the same way, for  $p_{B,i}$ ,  $q_{B,i}$ , and  $r_{B,i}$ . The evolution of the system is then simulated by iterating Eq. (4) with these values for the transition probabilities over many time steps. This procedure effectively converts the ordinary differential equation (10) to the computationally amenable iterative form of Eq. (4). For this procedure to reliably reproduce the solution of Eq. (10), it is

essential that  $\Delta t$  be very small, and hence that the  $r$ 's are all close to 1.

## X. CONCLUSIONS

The Astumian games were first conceived to illustrate the mechanism by which energy from a scalar (directionless) chemical reaction can be used to power vectorial transport of a Brownian motor that moves preferentially in one direction along a polymeric lattice. The dice play the role of thermal noise, allowing the occasional transition from one square to the next. The net tendency to lose reflects the effect of external forces that tend to cause the motor to lose energy. The coin toss mimics the input of energy from the nonequilibrium chemical reaction that powers the motor. The reversal from "losing" to "winning" addresses what I have always considered one of the deepest paradoxes associated with molecular motors and pumps. In any chemical state, the motor tends to move in the direction driven by any externally applied force. If the motor, powered by a nonequilibrium reaction, cycles through different chemical states, however, the direction can be reversed, and the motor can move energetically "uphill." The Astumian games show how randomly interleaving stochastic events of game 1 (one chemical state of the motor) with those of game 2 (a different chemical state of the motor) can reverse the predicted outcome of playing game 1 or game 2 alone. It is important to note that this reversal is a purely statistical result—there need be no direct mechanical interaction between the coin and the checker (or, more subtly, between a biomolecular motor and the chemical reaction, for example, ATP hydrolysis, that provides the energy for switching) other than that required to change the rules. A thermodynamic analysis of the simple two-state Brownian motor shows that the efficiency of energy conversion for biologically realistic values of the parameters can be greater than 75%.<sup>6</sup>

The paradoxical effect uncovered by Astumian *et al.*<sup>2</sup> has implications far beyond human games of chance. Every biological catalyst (enzyme) in our body is continually playing a game of chance, whereby the random bindings of different molecules to the enzyme, governed by thermal "Brownian" noise in the environment, determine whether the molecule (and hence the cell) wins (for example, increases the cells' energy capital) or loses (decreases the cells' energy capital) in that round. The ultimate fate of the cell, bankruptcy meaning death, is determined by playing in the limit of overwhelmingly large numbers, with many random games associated with every enzyme. Any help the cell can find, for example, by the paradoxical results described here, can move the cell toward survival and to evolutionary victory.

## ACKNOWLEDGMENTS

I am pleased to acknowledge very useful discussions with Professor Paul Camp (U. Maine), Ora and Jerome Percus

(New York University and The Courant Institute), Professor Rajiv Kohli (Columbia University), Professor Chris van den Broek (Limburgs University), Professor Hal Martin (Northern Michigan University), and Professor Hans von Baeyer (The College of William and Mary).

<sup>a</sup>Electronic address: astumian@maine.edu

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<sup>10</sup>This example is a realization of the simplest version of Simpson's paradox with dice. Take one 10-sided die, and one 20-sided die (available at many novelty and gaming stores). Your number on the 10-sided die is 1 (your opponent has 2–10), and on the 20-sided die your numbers are 1–16, and your opponents are 17–20. You each take one of the two dice and roll it as many times as it has sides. The winner is the one who rolls his/her numbers on their die more frequently. If you take the 10-sided die, you expect your number to come up once out of the ten rolls (10%), while one of your opponents numbers will on average appear on his/her die 4 out of the 20 rolls (20%). Clearly, you will generally lose this game. On the other hand, if you take the 20-sided die, your number will appear 16 times out of the 20 rolls on average (80%), but your opponents number on the 10-sided die will appear 9 out of the 10 times (90%). Once again, you lose. However, if you both roll the 10-sided die ten times, and the 20-sided die 20 times, you expect your number to appear a total of 17 times out of the total of 30 rolls (57%), and your opponents number will appear a total of 13 times in the 30 rolls (43%). You win! For more on Simpson's paradox, see the web site <http://plato.stanford.edu/entries/paradox-simpson/>.

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# Comment on “How to hit home runs: Optimum baseball bat swing parameters for maximum range trajectories,” by Gregory S. Sawicki, Mont Hubbard, and William J. Stronge [Am. J. Phys. 71 (11), 1152–1162 (2003)]

Robert K. Adair

Department of Physics, Yale University, P.O. Box 208121, New Haven, Connecticut 06520-8121

(Received 23 December 2003; accepted 22 July 2004)

[DOI: 10.1119/1.1794754]

In their baseball analysis of batting against fast balls and curve balls, Sawicki, Hubbard, and Stronge<sup>1</sup> concluded that given a definite bat swing velocity, an optimally hit curve ball will travel further than an optimally hit fast ball, although the fast ball will have a significantly higher velocity at the point of the bat-ball impact. Moreover, they find that the optimum launch angle for hitting long balls, that is, home runs, is near 25°. As they note, their conclusions are in sharp disagreement with the results of my analysis,<sup>2</sup> which concluded that a struck fast ball will go further and that the optimum launch angle is  $\approx 35^\circ$ .

In Ref. 1 it is assumed that the backspin of the ball projected from the bat must be greater when the overspinning curve ball is struck than when the backspinning fast ball is hit by the same bat swing. Moreover, they conclude that the backspin generates sufficient lift so that the struck curve ball, with a greater backspin but a lower initial velocity, sails further than the struck fast ball. The much larger lift that they assume also leads to a maximum flight distance from a lower angle initial trajectory.

Their model can be tested by comparing the range that they predict that balls will go with the ranges that are observed. From the larger lift from backspin in their model, the ball goes much further than the distances I calculate (see Fig. 2.5 in Ref. 2), distances that are in accord with observations of the game. Indeed, from the “fast ball” line of their Table II, they predict that under standard conditions a ball with an initial velocity of 44.3 m/s (99.1 mph) with an initial backspin of 1827 rpm will travel 134.8 m (442 ft) when hit from a height of one meter with a launch angle of 0.46 radians (26.35°). (The curve ball, driven at a launch angle of 24.3° with a velocity of 96.3 mph and a backspin of 2644 rpm, goes 455 ft.)

These are very long home runs. We know that only five of about 2000 home runs hit by the home team in 19 ball parks traveled that far,<sup>2</sup> and certainly most of these home runs were assisted by a following wind.

From these numbers and our observations, we conclude that the model of Ref. 1 must be incorrect. Balls projected with the backspins and velocities that they calculate do not go *nearly* that far in reality. From modern radar gun measurements of the velocity of pitched balls, we know that the fastest pitchers throw regularly with initial velocities of about 100 mph. (Add 2 mph to the radar gun readings because the guns pick up the ball after it travels at least 15 ft so as to differentiate the ball from the throwing hand.) Such a ball thrown overhand will usually have a backspin near 1800 rpm.

We know that outfielders with good, but not extraordinary

arms, throwing on the run, throw with initial velocities at least as large as assumed in the model and with about as much backspin. However, these outfielders can throw balls a maximum distance of only about 380 ft, from just in front of the warning track in center field to the plate on the fly.<sup>3</sup> From my own formulas, a ball hit with an initial velocity of 99.1 mph with a backspin of 1827 rpm will travel 354 ft if thrown at an optimum angle of about 35°. Using my model again, a ball hit with the batted curve ball parameters of Ref. 1, a velocity of 96.3 mph and a backspin of 2644 rpm, will travel about 343 ft. These distances are in accord with Fig. 2.5 of Ref. 2 and with my observations.

Although my calculation of baseball trajectories is not in accord with Ref. 1, I suggest that the primary error in their results is in their lift formulas. The lift force is in the direction of  $\omega_b \times V_b$ , where  $V_b$  is the velocity of the ball in still air and  $\omega_b$  is the angular velocity of the ball. With the axis of the spin normal to the velocity,  $r_b = 0.0368$  m as the ball radius,  $A = 0.00425$  m<sup>2</sup> as the cross sectional area of the ball, and  $\mu = 1.283$  kg/m<sup>3</sup> as the air density, they calculate the magnitude of the lift force,  $L$ , in a way that follows closely (see Ref. 1, Fig. 4) the results of Ref. 4 where (using different notation),

$$L \approx C_m \mu r_b \omega_b A V_b, \quad (1)$$

where the coefficient,  $C_m \approx 7.2 \times 10^{-7}$  with  $L$  in Newtons. But the wind tunnel measurements<sup>4</sup> extend only to velocities,  $V_b \approx 12$  m/s (40 mph), well below the drag crisis<sup>5</sup> velocities, while the values of  $L$  so calculated were used for the much higher velocities of batted balls. (The drag crisis denotes the transition region between a lower velocity fluid dynamic regime with high drag resistance to flow and a higher velocity regime where the resistance is smaller.)

My own lift force values (Magnus forces in Ref. 2) also follow Ref. 4 and Eq. (1) at low velocities, but are sharply reduced from the values of Eq. (1) at higher velocities<sup>6</sup> of the drag crisis. Although this reduction follows from the equations<sup>7</sup> that reflect my view of the probable relation of the Magnus force to the drag force, the reduced values also are required by the character of the observed flight of the ball. (My model leads directly to a reduced Magnus force or lift force in the drag crisis region and to the reversal of the force on smooth balls observed by Briggs,<sup>8</sup> a reversal that limits even Tiger Woods to 150 yd drives with smooth golf balls which duck downward when hit with backspin by a driver.)

The specific values of the drag coefficients that I adopted in the drag crisis region were constrained by my assumption that the coefficients,  $C_d$  and  $C_m$ , are functions only of the Reynolds number and that fly balls are known to travel ap-



preciably further in Denver than at sea level. Because the density of air is lower in Denver, a given Reynolds number will correspond to a higher velocity there. Hence, we expect that the drag crisis will be moved to higher velocities in Denver and for some formulations of the variation of the drag and Magnus (lift) forces with velocity in the drag crisis range, fly balls would be expected to travel less far in Denver than at sea level, contrary to observations. Also, both the drag and lift or magnus forces are expected to be proportional to the air density and thus are reduced at high altitudes. The reduction in drag increases the distance that balls travel, the reduction in lift will decrease the range. Hence, I find that balls hit in Denver according to the model of Ref. 1 and the drag and lift formulas do not travel farther than at sea level.

Their conclusion that balls batted from pitches with over-spin have more backspin than balls hit off of pitches with back spin is plausible and is in accord with my own formulas, but the detailed argument made in Ref. 1 in reaching this conclusion is not valid. They wrote that, “Although significant ball deformations can occur during batting, this analysis assumes rigid-body impact.” Hence, their spin transfer calculation, the results of which are central to their conclusions, cannot apply to baseball because baseballs are greatly deformed when struck by a bat under game conditions.

I cannot claim that my own results are definitive—indeed while discussing uncertainties in my book,<sup>2</sup> I wrote (p. 3)

“Some results are hardly more than carefully considered guesses: How much does backspin affect the distance a long fly ball travels?” Hence, although I am certain that Sawicki, Hubbard, and Stronge<sup>1</sup> are incorrect, I am not equally certain that my own analyses are wholly right. The science of baseball “ain’t rocket science.” It’s much more difficult.

<sup>1</sup>G. S. Sawicki, M. Hubbard, and E. J. Stronge, “How to hit home runs: Optimum baseball bat swing parameters for maximum range trajectories,” *Am. J. Phys.* **71**(11), 1152–1162 (2003).

<sup>2</sup>R. K. Adair, *The Physics of Baseball* (HarperCollins, New York, 2001), 3rd ed.

<sup>3</sup>In the first part of the 20th century, there were often throwing contests between games of Sunday double headers. The winners would sometimes throw the ball a distance approaching 400 ft, although a 380 ft throw would sometimes win the prize. The record, set by Glenn Gorbous, in Omaha in 1957, is 446 ft (see p. 104, Ref. 2). You can be sure that the throw at the Omaha altitude of 1040 ft was on a hot day with a following breeze—the ranges were always set so that the players threw with the wind—adding at least 30 ft to that for standard conditions.

<sup>4</sup>R. G. Watts and R. Ferrer, “The lateral force on a spinning sphere: Aerodynamics of a curve ball,” *Am. J. Phys.* **55**, 40–44 (1987).

<sup>5</sup>C. Frohlich, “Aerodynamic drag crisis and its possible effect on the flight of a baseball,” *Am. J. Phys.* **52**(4), 325–334 (1984).

<sup>6</sup>See Figs. 2.1 and 2.2 in Ref. 2.

<sup>7</sup>Reference 2, p. 24.

<sup>8</sup>L. J. Briggs, “Effects of spin and speed on the lateral deflection (curve) of a baseball and the Magnus effect for smooth spheres,” *Am. J. Phys.* **27**, 589–596 (1959).

## Reply to Comment on “How to hit home runs: Optimum baseball swing parameters for maximum range trajectories,” by Gregory S. Sawicki, Mont Hubbard, and William J. Stronge [*Am. J. Phys.* **71** (11), 1152–1162 (2003)]

G. S. Sawicki

*Department of Movement Science, University of Michigan, Ann Arbor, Michigan 48109*

M. Hubbard

*Department of Mechanical and Aeronautical Engineering, University of California, Davis, California 95616*

W. J. Stronge

*Department of Engineering, University of Cambridge, Cambridge CB21PZ, United Kingdom*

(Received 27 April 2004; accepted 29 July 2004)

[DOI: 10.1119/1.1796831]

### I. INTRODUCTION

We appreciate Professor Adair’s contributions to the understanding of baseball mechanics, and we commend his popularization of science through the example of baseball, which illuminates various principles of physics. In addition, we welcome his comment<sup>1</sup> as an expression of a genuine desire to increase our understanding of baseball mechanics. His comment has prompted us to reconsider carefully the implications of our model and its uncertainties.

We have long been uneasy, however, about the analytical models for various phenomena that have been proposed by Adair.<sup>2</sup> These include his models for ball-bat interaction and aerodynamic forces on a moving and spinning ball. Essentially, he has created “recipes” for batting and lift and drag. All models are approximations and, as we noted in our

paper,<sup>3</sup> “The spin decay time constant and the functional dependencies of the drag and lift forces on the Reynolds number and spin parameter are among the least well understood parts of [our] model.” Nonetheless, we believe our models taken together are more realistic than those of Adair<sup>2</sup> *in toto*.

To decide whether our model or Adair’s model of flight is more valid, we could launch baseballs with known velocities, launch angles and spin rates, and compare the measured and predicted ranges. Such experiments should be done. However, we find the arguments<sup>1</sup> presented by Adair regarding our model to be experimentally imprecise and based mostly on anecdotes and calculations from his model, about which we comment more in the following sections. We also disagree that “the science of baseball [is] more difficult... than rocket science,”<sup>1</sup> with the implication that it is espe-

Table I. Optimum control variables and maximum range for typical pitches.  $C_{Dmin}=0.15$ ,  $\rho=1.205 \text{ Kg/m}^3$ , and  $\mu=1.8\times 10^{-5} \text{ N-s/m}^2$ .

Pitch type	$V_{b0}$ (m/s)	$V_{B0}$ (m/s)	$\omega_{b0}$ (rad/s)	$V_{bf}$ (m/s)	$\omega_{bf}$ (rad/s)	$\zeta$ (rad)	$E_{opt}$ (m)	$\psi_{opt}$ (rad)	Optimal range (m)
fast	42.00	30.00	-200.00	44.46	194.75	0.4921	0.0277	0.1944	135.108
knuckle	36.00	30.00	0.00	44.09	232.30	0.4712	0.0259	0.1723	135.922
curve	35.00	30.00	200.00	44.23	267.64	0.4385	0.0227	0.1475	139.047

cially difficult to understand. We believe it's only baseball—the impact and flight of a near sphere in a fairly limited range of Reynolds numbers and spin parameters.

Our paper is based on published experimental data (our own and that of others). These data include (1) the pitched ball flight path angle at impact and its dependence on pitch speed; (2) the dependence of the ball-bat coefficient of restitution on impact speed; (3) the bat speed achievable by good batters; (4) the coefficients of friction between ball and bat; (5) the flight drag coefficient dependence on the Reynolds number  $Re=\rho v d/\mu$ ; and (6) the flight lift coefficient dependence on the spin parameter  $S=r\omega/v$ .

## II. LIFT

Of the two aerodynamic forces, we are most confident of our understanding of the source of lift and the variables on which it depends. The fluid mechanics literature<sup>4-6</sup> and numerous published studies<sup>7-10</sup> on baseball lift forces all agree: the lift (or Robins–Magnus) coefficient is, within the speed range relevant to baseball ( $0.5 < Re < 2.5 \times 10^5$ ), not (or at most weakly) a function of  $Re$ , but instead depends most strongly on the spin parameter  $S$ .

The spin parameters of optimally hit balls are typically small ( $S < 0.25$ ). Adair's point that the bilinear lift coefficient approximation (or fit) of  $C_L$  versus  $S$  (Fig. 4 in Ref. 3) must be wrong because it fits the low  $Re$ , high  $S$  data of Watts and Ferrer<sup>7</sup> is incorrect and irrelevant. All of the other experimental data used to generate this approximation lie very close to it; they are from experiments in the range  $0 < S < 0.47$  and for larger  $Re$  more typical of the flight of baseballs. Even the horizontal wind tunnel lift data of Briggs,<sup>11</sup> when suitably reinterpreted to account for the 0.1 m cylindrical wind shield on the ceiling (the wind shield reduces the time during which the ball is exposed to the lift force), result in lift coefficients that cluster around the bilinear fit in the practical ranges of  $0 < S < 0.3$ , and  $1.1 < Re < 2.2 \times 10^5$ . This latter range contains the drag crisis. Our paper<sup>3</sup> obtains optimal values for the variables controllable by the batter, undercut  $E$  and swing angle  $\psi$ . Our calculations do not depend on the fact that the experiments in Ref. 7 were done for  $Re$  too small and  $S$  too large to be strictly relevant to baseball or that our bilinear approximation fits their data. That a fit of the data in Ref. 7 alone can give a reasonable

picture of how lift behaves at larger  $Re$  and lower  $S$  is indicative of the fact that the dependence of lift on  $S$  is not a strong function of  $Re$ .

Even in the somewhat extreme (compared to baseball) case of golf ball roughness, the careful and detailed study of golf ball aerodynamics by Smits and Smith<sup>12</sup> corroborates the interpretation that the lift coefficient is not a function of  $Re$  for  $Re > 90,000$  and supports the assumption that the lift coefficient is mainly a function of  $S$  for the range of  $Re$  in which baseball is played.

Although we agree with Adair that it is primarily the differences between our and his calculations of the lift that lead to the large differences in our predictions, we believe that our lift model is the best that is available. We find little or no support in the fluid mechanics literature for Adair's suggestion<sup>1</sup> (see Fig. 2.2 in Ref. 2) that the dependence of  $C_L$  on  $S$  should be attenuated or reduced to the extent shown by his model of lift at large velocities near the drag crisis. We urge the reader to scrutinize carefully Adair's equation<sup>2</sup> (p. 24) for how the lift (Robins–Magnus) force and the drag force are related and to consider whether there is any basis for this relation. We have not been able to find a derivation, experimental validation, physical explanation, or another reference anywhere in the literature to its use. Our view is that this equation is not valid.

## III. DRAG

We believe it is clear from the experimental data in Refs. 13, 8, and 10 that the baseball in high speed flight experiences the drag crisis; that is, the drag coefficient  $C_D$  undergoes an abrupt decrease at  $Re$  below or near  $1.5 \times 10^5$  and a gradual recovery for  $Re > 1.7 \times 10^5$ , although, as we noted in Ref. 3, the depth of the drag crisis appears to be different in spinning and nonspinning balls. In this sense the drag coefficient is a function of  $Re$  and  $S$ , but the lift coefficient is a function of only  $S$  to a good approximation. Nevertheless, all optimally hit long fly balls have considerable spin. For this reason we believe it is appropriate to assume a deeper drag crisis for batted balls.<sup>3</sup> This approximation might break down for line drives with little or no spin, but these hits are seldom long fly balls, the main subject of our paper.

It is curious that Adair takes care to have his “recipe” for the drag coefficient curve pass through the single data point

Table II. Optimum control variables and maximum range for typical pitches;  $C_{Dmin}=0.25$ ,  $\rho=1.205 \text{ Kg/m}^3$ , and  $\mu=1.8\times 10^{-5} \text{ N-s/m}^2$ .

Pitch type	$V_{b0}$ (m/s)	$V_{B0}$ (m/s)	$\omega_{b0}$ (rad/s)	$V_{bf}$ (m/s)	$\omega_{bf}$ (rad/s)	$\zeta$ (rad)	$E_{opt}$ (m)	$\psi_{opt}$ (rad)	Optimal range (m)
fast	42.00	30.00	-200.00	44.64	204.43	0.5380	0.0294	0.2363	124.362
knuckle	36.00	30.00	0.00	44.13	250.32	0.5153	0.0277	0.1972	124.929
curve	35.00	30.00	200.00	44.33	284.64	0.4880	0.0248	0.1807	127.517

of Briggs,<sup>11</sup>  $C_D=0.31$  at  $Re=2.09\times 10^5$ , while ignoring entirely Refs. 8, 10, and 13, which report drag coefficients consistent with an abrupt and possibly deep drag crisis.

We disagree with Adair that the main uncertainty in our flight model is the lift coefficient. Rather we think that the main uncertainty may be the depth of the drag crisis, that is, the value of the minimum drag coefficient. In our paper<sup>3</sup> we took this minimum value to be  $C_{Dmin}=0.15$ . This value is consistent with the results of Ref. 14 as discussed in Ref. 15. But on further reflection, there is relatively little experimental data for baseballs in the range  $1.6<Re<1.75\times 10^5$  that would allow a more confident estimate of  $C_{Dmin}$ . Thus we have done computational sensitivity studies, similar to those in our paper,<sup>3</sup> but with  $C_{Dmin}=0.25$ .

Tables I and II compare optimal batting strategies for three pitches that differ in speed  $V_{b0}$  and spin  $\omega_{b0}$ , with the bat speed  $V_{B0}$  constant, and show that the different optimal undercuts  $E_{opt}$  and swing angles  $\psi_{opt}$  produce different batted ball speeds  $V_{bf}$ , spins  $\omega_{bf}$  and launch angles  $\zeta$  and result in different optimal ranges. The results in Tables I and II can be compared directly to the results in the same format in Table II of Ref. 3. (Note that the results here are for a slightly different density  $\rho$  and viscosity  $\mu$  than in Ref. 3; see Sec. VII.)

The predicted range is very sensitive to increases in drag in the drag crisis region. For example, an increase of 0.1 in  $C_{Dmin}$  gives a decrease in range of all optimally batted balls of about 10 m. The optimal undercut  $E$  and swing angle  $\psi$  increase by about 2 mm and  $2^\circ$ , respectively, and the resulting launch angle increases by about  $2.5^\circ$ . The other essential features of our results remain unchanged.

#### IV. SPIN-DOWN

We believe Adair's estimate<sup>2</sup> of the spin-down time constant of 5 s to be unreasonably low, but this difference is not a source of major disagreement because we assume that the actual characteristic time is long enough that spin-down can effectively be ignored.<sup>3</sup>

What is the correct spin-down model? In the fluid mechanics literature, we are not aware of any relevant analytical or numerical solutions of the spin-down problem for a sphere with translational velocity. The solution of Ref. 16 for spin-down with little or no translational velocity was used in Ref. 17 as justification for neglecting spin-down entirely. These results<sup>16</sup> predict a baseball spin-down characteristic time of about 250 s. It is difficult to determine if the spin-down rate will increase or decrease when a large translational velocity is added. At zero or low translational  $Re$ , the shear stress is small but acts entirely around the ball. But at high  $Re$ , there is separation and the high shear stresses act only in the un-separated part of the boundary layer, but in an asymmetric way (some shear stresses accelerate and some decelerate).

Experimental spin-down rates for golf balls have been obtained in a wind tunnel<sup>12</sup> and in flight.<sup>18</sup> These results are consistent with measurements of a characteristic time for golf ball spin-down of about 16 s. Reference 18 showed clearly that increased dimple depth (increased roughness) increases the spin-down moment coefficient by  $\approx 10\%$ . Extending these results to baseball is not trivial, however. If we assume the baseball roughness is the same as that of a golf ball, then at 40 m/sec (corresponding to Reynolds numbers of the golf experiments), the characteristic time for baseball

spin-down is  $\approx 28$  s. If the equivalent roughness of the baseball is substantially less than the golf ball (which is conceivable), then the spin-down moment coefficient could be half of that in golf and the spin-down time could be 50 s or more. Thus we believe our assumption<sup>3</sup> of neglecting spin-down entirely may not be far off, even though with a spin-down time of 50 s the ball loses 10% of its spin in 5 s and this loss may be significant enough to include in a dynamic model of flight. It would be interesting to apply the technique of Ref. 18 to measure the baseball's spin-down in flight.

#### V. IMPACT

Adair<sup>1</sup> claims the rigid body impact model used in Ref. 3 is invalid because it does not account for the finite deflections that result from a bat striking a baseball—deflections that increase the moment of inertia of the ball and decrease the moment arm of frictional forces that act during the initial part of the collision period. During impact, the effect of finite deflections is to slightly increase the normal impulse  $p_s$  at which initial sliding is brought to a halt. Nevertheless, for all hard hit balls, the angle of incidence (measured from the normal) between bat and ball is well within the cone of friction,<sup>19</sup> so initial sliding is brought to a halt substantially before contact ceases; there is sufficient time for friction to halt initial sliding despite the changes in geometry. Because of the large baseball stiffness, the ball has almost recovered its spherical shape prior to separation from collision so that for impacts at a small angle of incidence, the changes in tangential and angular components of the ball velocity are not much affected by finite deflections.

To bring the initial sliding to a halt during the contact period, our rigid body theory<sup>3</sup> shows that the coefficient of friction  $\mu_f$  must satisfy

$$\mu_f \geq \frac{2}{7} \frac{|\dot{v}_1(0)|}{|\dot{v}_3(0)|} \frac{[1 - S_0 \sqrt{1 + (\dot{v}_3(0)/\dot{v}_1(0))^2}]}{(1 + e_*)}, \quad (1)$$

$$S_0 \equiv \frac{r_b \omega(0)}{\sqrt{\dot{v}_1^2(0) + \dot{v}_3^2(0)}} < 1,$$

where  $\dot{v}_1(0)$  and  $\dot{v}_3(0)$  are the incident tangential and normal components of the relative velocity between the centers of mass of colliding bodies,  $e_*$  is the coefficient of restitution, and  $S_0$  is the spin parameter at incidence. For our smallest measured coefficient of friction  $\mu_f=0.35$ , Eq. (1) indicates that the initial slip will be halted during the collision irrespective of the rate of initial spin and  $e_*$  if the angle of incidence is within  $50^\circ$  from the normal and  $-1 < S_0 < 1$ . These conditions are easily satisfied by almost all pitched balls that can be batted past the outfield.

For the three optimal hits in Table I, the fractions of the total compression impulse<sup>3</sup> required to halt the slip,  $p_s/p_c$ , are only 0.19, 0.13, and 0.04 for the fastball, knuckle ball, and curve ball, respectively. Much less than half of the maximum ball compression occurs before friction brings the initial sliding to a halt. If we assume a contact period of 1 ms and an initial normal relative velocity of 70 m/s, we estimate the maximum ball compression to be 0.011 m. Thus the period of slip will be slightly lengthened because of a  $\sim 10\%$  reduction in the moment arm for frictional forces about the center of mass, but this increased period is only a small part of the contact period.

Adair<sup>2</sup> (p. 109) presents “recipes” for changes in the velocity of the ball during impact with a stationary flat surface. These recipes are not applicable to batting a baseball because they take no account of the inertia of the bat and they neglect the initial spin of the ball. It is therefore difficult to understand Adair’s statement<sup>1</sup> that “[our<sup>3</sup>] conclusion that balls batted from pitches with overspin have more backspin than balls hit off pitches with back spin... is in accord with [his] own recipes,” when the recipes do not consider the initial ball spin. Furthermore, for elastic impacts, Adair’s recipes conserve energy whereas the equations in Ref. 3 exhibit a loss in kinetic energy. This loss of kinetic energy exactly equals the work done by friction—work that transforms some translational kinetic energy into rotational kinetic energy or vice versa.

The neglect of the initial ball spin causes significant errors in the launch angle.<sup>2</sup> As an example, in the direct impact of a spinning baseball with a bat of normal incidence, a tangential impulse  $p_t$  is required to create the angular impulse  $r_b p_t = I \omega_o = 2m_b \omega_o r_b / 5$  necessary to stop the spin, where  $m_b$ ,  $r_b$  and  $I$  are the ball mass, radius and moment of inertia, respectively. When  $\omega_o = 200$  rad/s this tangential impulse causes a tangential component of post-impact velocity  $v_t = p_t / m_b = 2.93$  m/s. For a normal rebound velocity of 45 m/s, this tangential velocity results in a deviation of the rebound path from the normal direction of 3.73 deg and a 7.5 deg difference in the directions of the paths of balls spinning in opposite directions. Adair<sup>2</sup> neglects this necessary consequence of Newton’s second law.

Our impact model is based only on Coulomb’s laws of friction and on Newton’s laws of motion. In contrast Adair<sup>2</sup> obtains tangential changes in velocity based on the assumption that the coefficient of restitution is the sole source of energy dissipation. We argue that slip during impact with friction is a secondary source of energy dissipation during collision, a source neglected in Adair’s model of batting.

## VI. ANECDOTES

Although Adair’s anecdotes about the limited range of thrown balls may seem compelling, it is well known that achievable human throwing velocities strongly depend on launch angle,<sup>20</sup> so that the launch velocities in the Omaha contests might well have been significantly less than those of the fastest pitchers (and optimally batted balls) if the thrown launch angles were large. His insistence that the optimal launch angle is near 35° makes us wonder whether he understands that our definition of optimal includes the requirement of the ball having been batted. The optimal batted conditions must trade off velocity, angle, and spin. Our calculations show explicitly how to do so.

We question whether Adair’s statistics<sup>1</sup> on the small percentage of long home runs are still (or ever were) valid. According to Ref. 21, Mark McGuire’s 70 home runs in 1998 included 20 that traveled more than 450 ft, four times more by a single person than IBM measured for the home teams (in less than all of the ballparks) of the league. In the only game one of us attended at Pac Bell Park (Aug. 9, 2003), Jim Thome hit a 441 ft home run. The distances we reported<sup>3</sup> as optimal (442 and 455 ft) are long, but are not outlandish by today’s standards. If the depth of the drag crisis turns out to be smaller than we supposed (see Table II), then these optimal ranges become 408 and 418 ft.

During our experiments<sup>8</sup> at the Olympic Games in Atlanta in 1996 where we measured the trajectories of pitched balls and used this data to infer the drag and lift forces, we observed, filmed, and digitized the 3D initial trajectory of a batted fastball hit for a home run. This home run had a launch velocity of 50.8 m/s (113.6 mph), s.d. 0.45 m/s and launch angle of only 13.7 deg, s.d. 0.3 deg! It cleared the 10 ft high left-center-field fence at Fulton County Stadium about 395 feet from home plate and traveled more than 400 ft. This precise experimental data point shows that balls hit not too much faster than those hypothesized by Adair and at a much lower launch angle can clear the fences, even near center field. Unfortunately, it was not possible to measure the spin of the ball, but it certainly must have been high because the resulting lift is the only force capable of delaying that ball’s rendezvous with gravity. We doubt that Adair’s lift model is capable of supporting this home run with a reasonable amount of spin.

## VII. CORRECTIONS

In the extensive review of our calculations and paper prompted by Adair’s comment,<sup>1</sup> we have found a misprint and two small errors, and have appreciated that we could have chosen a better set of nominal parameters. Although it did not affect our calculations, the radii of gyration for the ball and bat are transposed in Table I of our paper.<sup>3</sup> Rather than the intercept 0.09 as stated in Ref. 3, Eq. (29b), we inadvertently used 0.10 in our calculations and a constant dynamic viscosity  $\mu = 1.8 \times 10^{-5}$  N-s/m<sup>2</sup>, rather than the value  $\mu = 1.845 \times 10^{-5}$  N-s/m<sup>2</sup>, which would be calculated from the constant kinematic viscosity  $\nu = 1.5 \times 10^{-5}$  m<sup>2</sup>/s and density  $\rho = 1.23$  Kg/m<sup>3</sup> we reported.<sup>3</sup> We regret these discrepancies, although they change our results very little.

Also we chose<sup>3</sup> a nominal density  $\rho = 1.23$  Kg/m<sup>3</sup> which corresponds better to a chilly day at Fenway in April than to temperatures at which the game is more typically played (68 °F). In our results here we use the more realistic values  $\rho = 1.205$  Kg/m<sup>3</sup> and  $\mu = 1.8 \times 10^{-5}$ , the latter of which, unlike the kinematic viscosity  $\nu$ , is a function of the temperature only. This assumption makes  $Re = 2\rho v r_b / \mu$  depend only on velocity  $v$  and density  $\rho$  at higher altitudes but at the same temperature.

## VIII. CONCLUSIONS

We believe that Adair<sup>1</sup> may be too narrowly interpreting the main message of our paper.<sup>3</sup> We view its contributions to be the integration of the experimentally based impact and flight models such that it is possible to see how they fit together, and our recognition that there is an optimal batting strategy that is dependent on the bat-ball interaction and the flight of the ball. We reaffirm our conclusion that an optimally batted curve ball can travel farther than an optimally hit fast ball. It is apparently robust and remains so even if our value of  $C_{Dmin}$  proves to be too small by as much as a factor of two.

## ACKNOWLEDGMENTS

We thank LeRoy Alaways for his help in suggesting and finding the batted home run data. We also benefited from discussions with Jean-Jacques Chattot, Mohamed Hafez, Rabi Mehta, and Harry Soodak.

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- <sup>19</sup>The friction cone is a cone of half angle  $\theta = \tan^{-1} \mu$  around the normal at the contact point, within which the contact force vector must lie if there is no slip because the tangent (frictional) component must be less than  $\mu$  times the normal component.
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## Erratum: "A simple design for an electronic speckle pattern interferometer" [*Am. J. Phys.* **72** (11), 1380–1384 (2004)]

Thomas R. Moore

*Department of Physics, Rollins College, 1000 Holt Avenue, Winter Park, Florida 23789*

(Received 12 October 2004; accepted 15 October 2004)

[DOI: 10.1119/1.1830506]

Figure 5 of this manuscript was printed incorrectly. The correct figure is shown below.

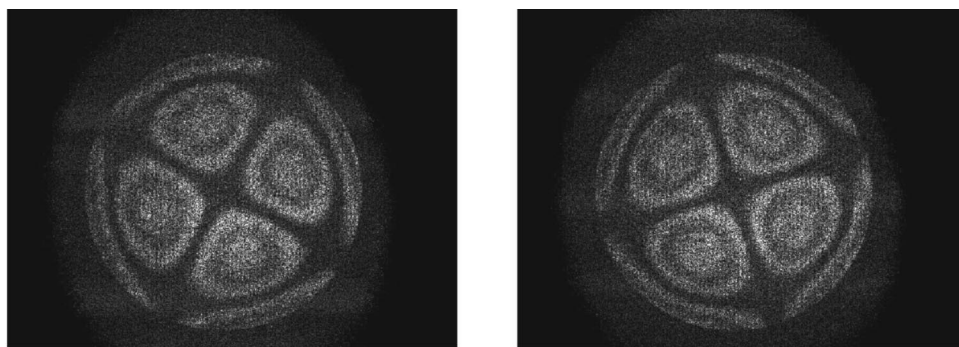


Fig. 5. Interferograms of a 17 cm circular plate vibrating in one of its normal modes. Theoretically the two modes occur at the same frequency, however, the degeneracy is broken by a slight asymmetry in the plate. The frequencies of vibration are 2133 and 2145 Hz.