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The Mathematics of Gambling

Physical Prediction of Roulette I

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by Edward O. Thorp

It was the spring of 1955. I was finishing my second year of graduate physics at U.C.L.A. In the course of the next year I would make three decisions that would shape my life for the next 23 years. I married (my present wife), I changed my field of study from physics to mathematics, and I began to toy with the fantasy that I could shatter the chains of poverty through a scientifically based winning gambling system.

I was living in Robison Hall, the student-owned cooperative. For \$50 a month and four hours work a week, we got our room and board. I had lived in the co-ops for nearly six years of undergraduate and graduate work, on a budget of about \$100 a month. Part of this came from scholarships and, in the early years, I got some help from home. But I was basically self-supporting like most of the other 200 or so co-op residents.

I attended classes and studied from 50 to 60 hours a week, generally including Saturdays and Sundays. I had read about the psychology of learning in order to be able to work longer and harder. I found that "spaced learning" worked well: study for an hour, then take a break of at least ten minutes (shower, meal, tea, errands, etc.). One Sunday afternoon about 3 p.m., I came to the co-op dining room for a tea break. The sun was streaming through the big glass windows (Robison, designed by Richard Neutra in the '30s, was very radical for that time. It had so many big sheet glass windows that it was

often called "the glass house"). My head was bubbling with physics equations, and several of my good friends were sitting around chatting.

In our mutual poverty the conversation readily turned to fantasies of easy money. We began to speculate on whether there were a way to beat the roulette wheel. In addition to me, the group included math majors Mel Rosenfeld and Andy Bruckner (now professors of mathematics at U.C. Santa Barbara), Tom Scott, and engineering major Rick Rushall. After all these years it's hard to be sure of exactly who said what, but we began the discussion by acknowledging that mathematical systems were impossible. We've shown this in the last three columns.

Then we kicked around the idea of whether croupiers could control where the ball will land well enough to significantly affect the odds. I will show in a later column that this is impossible under the usual conditions of the game. (The incredible thing is that logical reasoning could even be used to settle such a question.) It was a short brainstorming step to wondering whether wheels were imperfect enough to change the odds to favor the player. Those in the group who "knew" assured me that the wheels are veritable jeweled watches of perfection, carefully machined, balanced and maintained. This is false. Wheels are sometimes imperfect enough so they can be beaten. I had no experience with gambling, or with casinos, or with

roulette wheels, so I accepted the mechanical perfection of roulette wheels.

But mechanical perfection, for a physicist, means predictability. You can't have it both ways, I argued. If these wheels are very imperfect the odds will change enough so we can beat them. If they are perfect enough we can predict (in principle) approximately where the ball will land. Suddenly the orbiting roulette ball seemed like the planets in their stately and precise, predictable paths. In my mind there was that intuitive "click" of discovery that I would experience again and again. Unknowingly, I had just taken the first step on a long journey in which I would discover winning systems such as those for blackjack and for the options market, and I would accumulate a wealth I never imagined.

Mechanical perfection, to a physicist, means predictability

One side argued that it is a long way from prediction in principle to practical prediction. My group said that, over and over, the story of science has been a rapid leap from a theoretical vision ($E=MC^2$) to an unexpected practical result (nuclear power plants). By now our initial group of people agreed that the idea had merit and might well work. The novel debate attracted listeners, some of them cynical. They challenged us to prove the idea worked. The ten minute "study break" had run into a couple of hours. We adjourned with the half definite idea of "doing something."

In the following weeks the idea kept coming back to me: measure the position and velocity of the roulette ball at a fixed time and (maybe) you can then predict its future path, including when and where the ball will spiral into the rotor. (The rotor is the spinning circular central disc where the ball finally comes to rest in numbered pockets.) Also measure the rotor's

position and velocity at a (possibly different) fixed time and you can predict the rotor's rotation for any future time. But then you will know what section of the rotor will be there when the ball arrives. So you know (approximately) what number will come up!

You can see that the system requires that bets be placed *after* the ball and rotor are set in motion and somehow timed. That means that the casinos have a simple, perfect countermeasure: forbid bets after the ball is launched. However, I have checked games throughout the world, including Reno, Las Vegas, London, Venice, Monte Carlo, and Nice. Only in a few cases were bets forbidden after the ball was launched. A common practice instead was to call "no more bets" a revolution or two before the ball dropped into the center.

The simple casino countermeasure meant that there were two problems: (1) find out whether exact enough predictions could be made to get a winning edge, first in theory and then in the casino itself, and (2) camouflage the system so the casinos would be unaware of its use. If we could solve the prediction problem, the camouflage was easy. Have an observer standing by the wheel recording the numbers that came up, as part of a "system." Many do this so it doesn't seem out of place. But the observer also wears a concealed computer device with timing switches. His real job is to time the ball and rotor. (Much later we settled on toe-operated switches, leaving both hands free and in the open.) The computer would make the prediction and transmit it by radio to the bettor. The bettor, at the far end of the layout, would appear to have no connection to the observer-timer. The bettor would have a poor view of ball and rotor and would not pay much attention to them. To further break any link between timer and bettor, I would have several of each, with identical devices. They would each come and go "at random."

The important bets have to be placed after the ball is launched. A bettor who only bet then, and who consistently won, would soon become suspect. To avoid that,

continued on next page

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I planned to have the better al-
so make bets before the ball was
launched. These would be limited
so their negative expectation
didn't cancel all the positive expecta-
tion of the other bets. I became a
radio amateur (W6VVM) when I
was 13 (back in 1945 when there
weren't easy novice class tests), so
I thought I could build the radio
link and other electronic gadgetry.

This left me with the prediction
problem to solve. More than a year
passed without much time for rou-
lette: I got my Master's degree in

Physics (June 1955) and wrote the
first part of my Ph.D. thesis on
nuclear shell structure (Mayer-Jen-
sen theory). The mathematical
problems that I ran into led me in
the fall of 1955 to take graduate
math courses. I needed so many
that I got my Ph.D. in math in-
stead! And early in 1956 I got mar-
ried. I had been working as a tutor
and one of my "students" was T.T.
Thornton. He was an independen-
tly wealthy, knowledge-loving bach-
elor of about 45, who had degrees
in English and chemistry. Now he

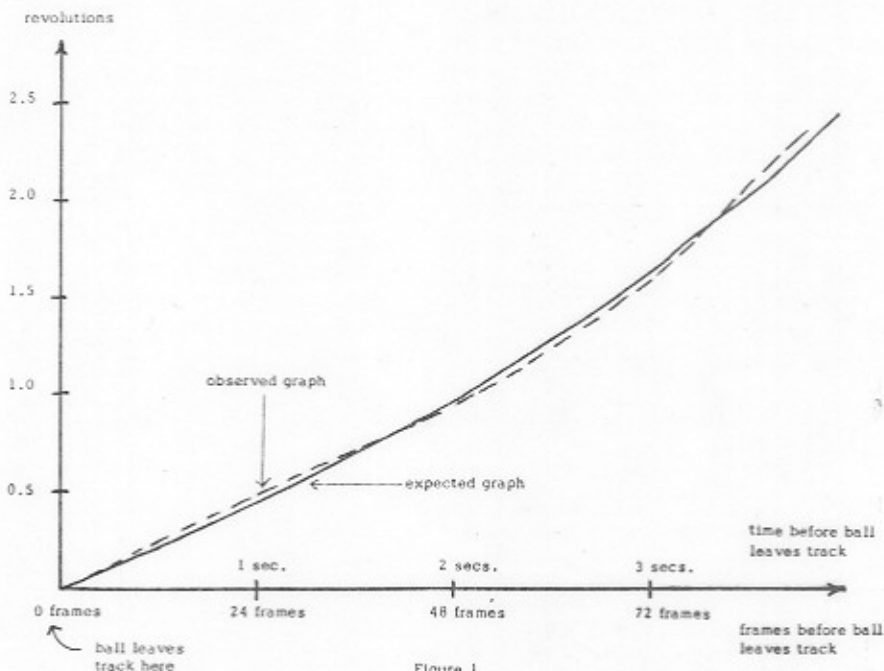


Figure 1.

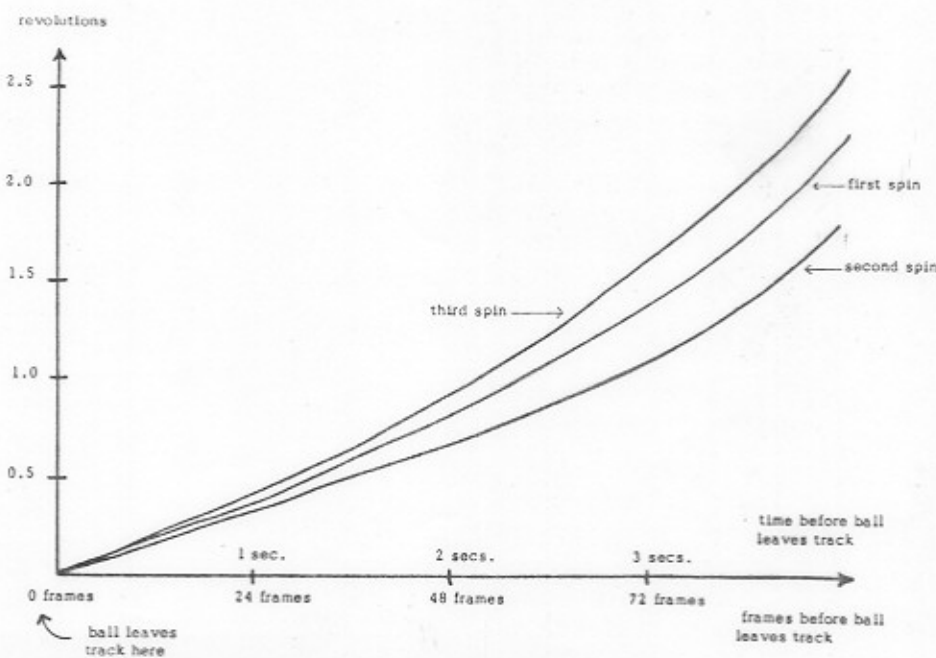


Figure 2.

was getting a degree in mathematics, just for the pleasure of it. He was an excellent student who didn't need a tutor but had hired me simply to learn faster and more efficiently.

We shared bits and pieces of our hopes, dreams, and enthusiasms.

After I had mentioned the roulette project, I was surprised and touched by his gift of a half-sized wheel. It was black plastic (bakelite?), made in France. I learned later that it cost the enormous sum of \$25. Though I had thought about the roulette system off and on, the gift of this wheel (sometime in 1958, I recall) got me to work more seriously on it. My first idea was to use a home movie camera to film the orbiting ball. I then plotted the amount the ball had traveled versus the number of the frame of the film. I expected that the pictures were taken at a uniform rate of 24 (?) frames per second so I could plot (angular) distance traveled versus time as in Figure 1. Instead of a smooth graph like the solid line in Figure 1, my first film showed a peculiar wavy structure, like the dashed line.

After thinking about this, I guessed that this was because the camera did not run at uniform speed. By taking a movie of a stopwatch that timed in hundredths of a second, I found that the camera did vary in speed. Photo stores confirmed this. The distortion of the curve in Figure 1 is analogous to the way a musical tone is distorted by a phono turntable whose speed varies slightly.

My next move was to take a movie of the rotating ball and the stopwatch. This gave me an accurate time for each frame. (I still have a roll of these pictures, postmarked January 16, 1959.) But there was still some "ripple" to the curves. (I later learned that even a slight tilt would cause this.) Worse, I found that the curves were not consistent from spin to spin. The situation was something like Figure 2. This meant the ball behaved differently from spin to spin. This meant that the distance it traveled varied even with the same initial velocity. This doomed predictability on my wheel. (Continued next month.)⁹³

BACCARAT CAPER

continued from page 44

win-or-loss units of 1, 3, or 5 based upon a particular set. The advantage lies in the fact that plus sets occur more often than minus sets.

Dick also slightly modified the D'Alembert so that when the pendulum did swing on the minus side, he edged up his units-per-sequence very slowly, but froze at level, or dropped very reluctantly, when the wins came along, keeping track of the profits until all losses were recovered and the commissions offset. He would quickly drop all the way back to his base bets when any profit was realized on the series, no matter how small. The real profits of the scheme occur not in an expanding progression and clearing it off, but by putting together a series of consecutive plus wins when operating at base betting levels. Also, even a long streak of Player decisions are not as damaging when viewed in set theory as opposed to progressing on individual plays, because the sets establish "plateaus" to keep the betting down on adverse runs.

Now, I only got Dick's permission to go this far in explanation because I didn't reveal his betting progression in exact detail. It would behoove the system buff to "take it from here," as this is the same springboard he used to develop what he's successfully using today.

Anyway, with over a half-million decisions as backup, Dick decided on an acid-test run. To keep his initial bets down, he opted for mini-baccarat tables because of the \$2 minimum and their higher frequency of play. He really put himself at a disadvantage here, because the casino assesses a minimum 25c commission on each Bank win (based on \$5). This forces the \$2 bettor to pay what appears to be a prohibitive vigorish on 12 1/4%, rather than 5%, when staking the basic \$2 on the Bank side. Even with this disadvantage, I'm happy to say that Dick "bailed out" every place he played. While the results varied, his extremes in this initial test showed a profit of \$500 in less than one hour's play in one downtown casino. His worst foray was

continued on next page

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