This Grad Student Studied Parapsychology — And You Won't Believe What He Found!

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Parapsychology is a scientific field studying effects that would be extremely important to our understanding of the world, but are widely considered to be nonexistent. As an attempt to examine the scientific method, I conducted a parapsychology experiment to see if I could telepathically influence people's minds. Participants were given thirty seconds in which to click a tally counter while I either mentally impelled them to push it more or did not. Analysis of the results found that people in the control group pushed the button statistically significantly more frequently than people subject to the treatment condition — I am supernaturally unpersuasive. I consider several possible explanations for this effect, including experimenter influences, failure of blinding, coincidence, and actual telepathic faculty on my own part. I discuss the implications of this experiment on my personal belief in psi and on my attitudes towards science as a tool for uncovering the truth.

Introduction

Parapsychology is the study of psychic phenomena, of mental capabilities beyond those explained by science. These are collectively referred to as psi. Many topics fall within its umbrella; the ones most relevant to this research are: [4]

- Telepathy — Transmission, reception, and influence of thoughts
- Psychokinesis — Exertion of physical force through mental power
- Precognition — Divining inaccessible information about the future

Parapsychology promises revolutionary insights with incredible applications. Telepathy could allow for rapid and surreptitious communication, precognition could open new categories of computation and math, and remote viewing even attracted CIA attention with Project Star Gate [10]. New forces might be discovered, and a full science of parapsychology would touch nearly every other discipline. Personally, as a magician, I would be fascinated to learn about a real version of the abilities I present only the facsimile of possessing.

There's extensive research into parapsychology by major academics such as Daryl Bem, and it's published in dedicated peer-reviewed parapsychology journals, with forays into top conventional psychology ones [8]. However, parapsychology is overwhelmingly considered pseudoscientific. Its claimed results tend not to replicate, its mechanisms are hazy and frequently in contravention of existing beliefs about physics, and its papers are often plagued by statistical malpractice. Plainly, it's a field dedicated to studying an effect that isn't real.

I agree. I am skeptical of psi (mostly; I confess to having occasionally idly tried to move objects with my mind). However, I think that the methodological criticisms of the field of parapsychology also apply within the domain of conventional science. Consider the replication crisis in social psychology, in which statistical fraud has produced a wealth of groundless scholarship. One article considers parapsychology as a control group of sorts for science, studying a domain where there is no effect [2]. That they nonetheless produce positive and negative results at a similar rate to scientists in legitimate fields paints a concerning picture of the ability of scientists to find affirmative outcomes if and only if there is a true effect.

Felix Planer, the author of a book on esoteric beliefs, writes that "[I]f the existence of PK [psychokinesis] had to be taken seriously […] no experiment could be relied upon to furnish objective results, since all measurements would become falsified to a greater or lesser degree, according to his PK ability, by the experimenter's wishes." [1] He needn't have referenced psychokinesis — experimenter effects are a well-established phenomenon in which the results of an experiment tend to be biased towards those favored by the researcher [5]. Science doesn't need psi to have a problem.

That's why I'm researching parapsychology. Science is a method, not a domain, and I want to conduct an experiment in a strange domain following scientific protocols to observe how they function and how well I am able to follow them. That there is likely no true effect is to my advantage, because it allows me to focus my analysis more on the procedural elements of science. And if I do get positive results, that will be all the more interesting.

Experimental Design

Am I able to telepathically influence people's behavior?

This is the question I will be researching, and to find out, I have developed a protocol to isolate a causal effect between my thinking a command at someone and their obeying it. I want this

procedure to be quick, to pose no risks to the subjects or to myself, to produce data more granular than a binary did or did not follow, and provide little room for experimenter influence or mechanisms of causal effect other than psi. At a high level, my procedure is as follows:

1. Greet and explain that the subject will have thirty seconds to click a tally counter as many or as few times as they like, and that interval will start and end with a musical note
2. I give them the tally counter and get out of their sight
3. I start the experiment sound file, randomly assign the subject, and record their assignment
4. A note cues the subject that the test period has begun
5. Treatment conditions
    1. Control: I read whatever's on my computer screen for thirty seconds
    2. Experimental: I intently think at the subject a psychic command to push the button for the next thirty seconds, while avoiding giving any audible indicators of my mental focus
6. A note cues the subject and myself to stop
7. The subject leaves and I record the number of times the counter was clicked

Recruitment

For this experiment, I recruited almost exclusively people at Carnegie Mellon University. They were primarily students, though several subjects were professors, and some may have been merely on campus but presently unaffiliated with the university. This was a highly educated and likely unusually intelligent class of subjects. I did not record demographic data, though from memory, participants skewed young — indeed, I don't believe any of them were over fifty — and were disproportionately Chinese and Indian, representative of the demographics of Master's students at Carnegie Mellon. I believe gender balance was fairly even.

Certain esoteric concerns regarding recruitment are also worth noting. I did not ask my subjects about their attitudes towards psi, even though it is frequently postulated in parapsychological research that skepticism towards psi can inhibit its functioning [3]. Additionally, most people who took part in the experiment were acquaintances or strangers. Few were friends and none were among the people closest to me. If telepathic connection is possible, it may be more readily established between minds already tuned to the same frequency, as it were [7]. These both, then, could be significant covariates.

I attempted to get as many people as possible to take part in the experiment in the time I had, and so opportunistically recruited most heavily from among my classmates. I was unable to conduct the experiment until I had established the procedure and until I had settled on a method and obtained the necessary materials, so my recruitment was laggardly at the beginning. It also slowed on the last few days of data collection, as I relaxed without the risk of having a critically small sample size and exhausted the supply of willing classmates. In total, I had 49 subjects.

Materials

I used a tally counter as shown in Figure 1 to be a button that records the number of times it is pressed. I had considered implementing a digital counter, but the mechanical device required no technical implementation, had less risk of error either in function or by the user, and would not tie up my computer during testing. The tally counter proved perfect for the task.

I also needed a way to mark the bounds of a 30-second interval while first having a short delay in which I could conduct my randomization. For this, I composed a short track on Apple

Figure 1: The tally counter

GarageBand consisting of a piano note at 15 seconds, another identical piano note at 45 seconds, and silence otherwise. In retrospect, I should have had different sounds, and found a way to play these from a source other than my computer. The inadequacy of this method of cuing the start and end of the interval resulted in a handful of mangled data points.

Instructions

I did not have a fixed script for introducing subjects to the experiment and giving them instructions, and it evolved over the course of data gathering, though more or less attained fixity at something close to the following:

> I'm conducting an experiment that will just take a minute of your time. This here is a tally counter. You push this button to increment it, and the thing along the side here resets it — you don't use that; I'll do that after we're done. [Give tally counter to the participant.] In this experiment, after a brief delay, you will hear a noise like this. [Play sound.] That marks the start of a thirty second interval which ends when you hear the same note. During that interval, but only during that interval, you may press the button to increment the tally counter as many or as few times as you like. At the end, I'll recollect the tally counter and mark down how many times you pressed it. For experimental integrity, you'll need to sit so that you can't see me. Do you have any questions? Are you ready?

Even once my instructions standardized, there was still variation in subjects' knowledge going into the experiment. Some subjects were aware of the purpose of the experiment, something I had explicitly mentioned in earlier versions of the instructions, and some had watched the experiment being conducted on previous subjects.

Randomization

Proper randomization is vital for this experiment to control for pre-randomization inconsistencies in the experimental procedures. This ensures that many covariates are not confounders. Inconsistencies in the instructions and in knowledge of the experiment, having taken effect prior to random group assignment, could not have had causal influence on my results. Similarly, I can rule out effect from the room I conducted the experimented in (though I did record that), the time it was conducted, individual variation in test subjects such as their belief in psi, and many other factors.

Additionally, randomization also reduces the avenues for experimenter effects. I could often tell whether a subject was likely to push the button a large or small number of times. For instance, one subject asked me what was the most number of times someone had clicked the button, and unsurprisingly she recorded a very high number of presses. If I did not randomize properly, it would be easy to produce whatever results I desired. I also designed my procedure to randomize late, as close to the effect administration interval as possible, so as to limit my post-randomization degrees of freedom.

To conduct my randomization, I used random.org, which promises high quality random number generation. I wanted to randomize in a way that I knew I would not allow for experimenter effect. In particular, I needed a method that would also minimize the risk of esoteric influence on my part. A coin toss, for instance, can be somewhat controlled by some magician's tricks

(though not ones I myself have any practice at), and because this experiment is examining psi, I also can't rule out the possibility of telekinetic influence on the coin, similar to what Felix Planer feared [1]. A website seemed sufficiently out of my control to be trustworthy for these purposes.

Blinding
This experiment was single-blind. Double-blinding would have been impossible, seeing as intention on the part of the experimenter is the key independent variable. The protection of single-blinding varied from subject to subject. For the first several subjects, I conducted the experiment in a room with a screen I could stand behind out of view of the subject. However, in later iterations, no such barrier was available, and I had to make do with merely crouching out of sight (hopefully) behind the subjects. Because the sound cues for the interval played from my computer, where I also carried out the randomization, I was unable to be very far away from the test subject without risking them missing the interval. Most subjects never learned which group they were in, and those that did learned only after their data was collected and recorded.

Effect Administration
For subjects in the treatment group, I thought at them as hard as I was able to over the thirty-second interval a command to push the button. Because I am not sure of the proper way to do this, my method of application was inconsistent. For a significant majority of cases, I simply thought in words "push the button" over and over for thirty seconds. It varied whether I had my eyes open or closed for this, and how much I breathed. I pushed in several different directions: that the button itself was appealing, that the act of pushing it was appealing, or that pushing it would be for science and demonstrate something really cool. In some cases, I also mimed clicking the tally counter, as though there were some synchrony between my body and that of the subject.

I don't know which of these methods would be most effective, and I did not record which one I followed, nor did I record the subjective intensity of my treatment. I believe that in all cases, I was able to deliver sustained and intense mental focus towards encouraging the subject to push the button.

For subjects in the control group, I simply didn't think at them. To best ensure that I would not accidentally go against the intended treatment of the control group, I read. There was some variation in my handling of the control group as well, as I would varyingly read from a book or from what was on my screen, or examine other random things generated by random.org. It is possible that stray thoughts towards pushing the button may have entered my mind while some members of the control group were participating in the experiment, but I can say with confidence that such thoughts were never intense.

Data and Analysis

In total, I collected data from 49 people, reported in full in Appendix A. Of them, 29 were in the control group and 20 were in the treatment group. I chose to analyze the data using R because of my prior experience with it and its extensive free statistical packages. Across all participants, the median number of clicks was 60, and the mean was about 58.7. Standard deviation was 47.7. Two people pushed the button only once over the thirty-second interval, and one person managed 159 pushes, more than five per second.

Figure 2 shows box plots of the number of clicks by condition. The control group had a mean of 72.3 and a standard deviation of 50.8, whereas the treatment group had a mean of 40.0 and a standard deviation of 35.4. The control group was fairly symmetrically and widely distributed, but the treatment group was right-skewed, with most of the data points below 30 and a long

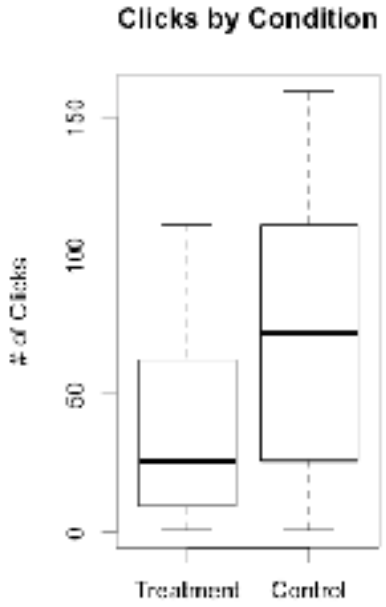

Figure 2: Results box plot

tail towards higher values. To analyze the significance of this apparent difference between the control and treatment groups, I used ordinary least squares. My residuals are depicted in Figure 3, and seem acceptably random, so I may look at the results, shown in full in Appendix B.

According to my linear model, people push the button about 39 times, and do so an additional 33 times if they are in the control group, significant to a $p \leq 0.05$ level. I examined the effect size in my data, and observed a Cohen's d of -0.79, with a 95% confidence interval ranging from -1.39 to -0.18, indicating a medium effect size. My thinking at someone to push the button has a statistically significant effect on the number of times they click it — and it lowers that number.

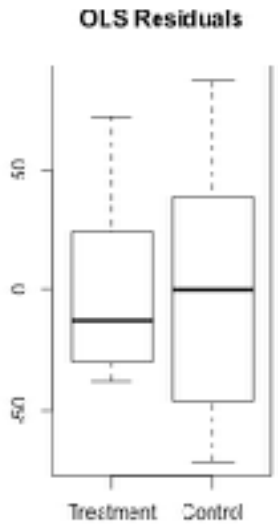

Figure 3: Linear model residuals

This is a surprising result, so I decided to run a few more statistical tests. This isn't proper experimental behavior, because trying tests until one produces the expected result is an avenue for p-hacking, but I have committed to relying on this first test, and merely include these others as curiosities.

I ran another linear model on the logarithm of the number of clicks. This one did not find a significant difference between the two groups. I also ran a permutation model and a Kolmogorov-Smirnov test to handle the possibility that the data is non-parametric. The permutation model had $p = 0.015$, and the Kolmogorov-Smirnov test produced $p = 0.059$. Different tests then produced different results, but the inconsistency wasn't huge. For the full output of these tests, see Appendix B.

From these data, we may conclude that I am supernaturally unpersuasive!

What Happened?

Okay, so that's not the end of the story. I have indeed found a significant result for my parapsychology experiment, and one in a surprising direction, but there's several possible explanations.

Mundane
This may simply be a coincidence. I got a significant result, but not at that low of a p-value. A significance threshold of $p = 0.05$ isn't that high, and had I chosen another test, I might not have gotten significant results. Moreover, it's not clear that the normality assumption holds well enough for my choice of a linear model to be acceptable, although I spoke with a statistics professor who assured me that my choice was reasonable. A number of people were possibly less flexible to influence of any sort, saying that they had pushed the button their favorite

number of times. If these people were numerous enough, my results may merely reflect the coincidence of their distribution.

In parapsychology research, great effort is expended to prevent subjects from being influenced by the researcher through conventional means such as visual or auditory cues, referred to as sensory leakage [6]. This causes a failure of blinding which creates a non-psi mechanism through which the treatment produces causal effect. I only had a screen separating me from the subject in a handful of trials, and so it's quite possible that the subject heard me or even saw me if they turned around too much.

I behaved in a visibly different way between the treatment and control conditions, being intensely focused in the former and relaxed in the latter, so had a subject seen me, their behavior could have been affected. I may have made perceptibly different sounds between the conditions. In the treatment condition, my focus may have caused my breathing to be harsher and louder, and it's even possible that my mental repetition of "push the button" accidentally slipped out as a whisper, though I don't recall this happening. Conversely, in the control condition, I might click on something on my computer to read it, which could have been an audible cue to the subject to click more.

There are stranger avenues for sensory leakage. In some cases, the subject could not see me, but could see other people who themselves could see me. It's possible that my focus during the treatment condition looked odd enough to be reflected as concern on the faces of people the subject could see, and so in that way sensory leakage accidentally occurred. Perhaps the intensity of the treatment condition caused me to sweat or emit pheromones, and that distracted the subjects.

There are also possible experimenter effects. My protocol was a good one, and eliminated most post-randomization degrees of experimenter freedom, but not all. The clearest case where I had a decision to make after randomization was how to handle a subject who did not register the bounds of the thirty-second interval. I had few enough subjects that I was reluctant to wholly throw out any data points, but without an established experimental procedure for handling these, I can't guarantee that no bias slipped in. Generally, if someone clicked only slightly beyond the final note, I would subtract out the number of clicks after the interval, and while I think I did this accurately, I might have been inconsistent. In case of larger errors, such as playing the track muted or the spectator only missing the first note and only starting to click after the second, I would inform the subject of this mistake using neutral language, and replay the track. I did not rerandomize the subject's group, so it's possible my notification or timing could be another avenue for bias.

As the end of the experiment approached, I could tell from eyeballing my data that there was a definite tendency for people in the control group to click more, and it's possible that this also colored my decisions. I was slower in collecting subjects at the very end of the experiment, and while this was mostly because I had run low on amenable classmates, perhaps it was also because I had noticed this pattern and was reluctant to gather more data that might disrupt it. Similarly, my choice to use the number of clicks rather than the logarithm of the number of clicks could been an unconscious decision to use the statistic that produces more significant results.

In their paper False-Positive Psychology, Simmons, Nelson, and Simonsohn examine ways that statistical chicanery can produce false positives, and list six rules for researchers to follow to minimize the risk of this [9]. I am pleased to say that I meet all of them, with the possible exception of some leeway in my termination of data collection. I have (just) 20 observations for each cell; I am open about what data I did and did not collect; I report both experimental conditions, including variations in administration of treatment and control; I report why I did not

eliminate observations; and I did not do analysis on covariates. I am comfortable saying that I have left relatively little room for experimenter effects.

Esoteric
Perhaps my results are the product of fraud. I assure you that I did not falsify my data, and did my best to perform this experiment honestly and diligently, but, well, that's exactly what I would say if I were a fraudster. Conceivably, I'm even conducting unconscious fraud, as a sort of extreme version of experimenter effects, where I could have misreported data and completely forgotten about it. This would be a remarkable result itself, but I don't believe it has happened.

My experiment is vulnerable to malicious interference. Sure, the tally counter seems to report correct numbers, but maybe that's only when I'm using it outside of the experiment. Similarly, perhaps random.org informs my subjects of what group they're in. My test subjects could have been deliberately notifying each other, conspiring to produce improbable results. I am disinclined to seriously consider conspiratorial hypotheses, however; why would my project matter enough to be worth the effort?

Or perhaps my experiment really does reveal psychic phenomena. True, the effect is in the opposite direction from what I would have expected, but there's reasonable parapsychological explanations for that. I am a skeptic, and skepticism is believed by some to inhibit psi [3] — could it in certain cases even reverse the effects? Maybe I was telepathically influencing the thoughts of my subjects, and was just conventionally unpersuasive through an unconventional medium. After all, my commands to push the button were the mental equivalent of shouting at the person. Had I spoken aloud instead, I would not be at all surprised to find that that people were less likely to push the button with someone they hardly know commanding in such a brutish manner. Lastly, this I would say is the most interesting possible result to my experiment. It could be that I possess some precognitive faculty, and used it to detect the outcome of the randomization for each subject and subconsciously tailored my instructions to push them towards the desired outcome.

Conclusions

As with any unusual result, the next step is to try a replication. Such a study should fix the problems of this one, having a preregistered target number of participants, one significantly higher than the 49 I used in this one. It should be conducted with better blinding to minimize the possibility of sensory leakage. I would establish procedures to handle errors so that I wouldn't have post-randomization degrees of freedom in how I handle those.

If such a replication still found a causal effect, further research would be warranted to home in further on the mechanism. Perhaps there was some form of sensory leakage even improved blinding did not prevent, or if it is really psi responsible, it would be important to learn more about how it functions.

In the meanwhile, I will learn from these results. The first lesson is that psi is more plausible than I had thought. I'm still a psi skeptic, even though an experiment I myself conducted shows a significant effect for telepathic influence, because I was quite confident that parapsychology is bunk. However, as a proper Bayesian, I have updated my beliefs in the direction of psi being real.

The second lesson is that a single experiment is no guarantor of truth. I do think I ran this experiment well, yet I got significant results in support of a proposition I really don't believe. There are flaws with my experiment, but I believe there's less room for experimenter effects in

my procedure than in most papers I've read. If even such a small quantity are enough to poison my experiment, how can I trust any other? I think the answer is that my epistemic standards must be high — any experiment may have its outcome be the product of subtle procedural flaws or simply coincidental. To ascertain the truth, I need large enough samples to sift signal from noise, and robust enough procedures to minimize the prospect of insidious experimenter effects. The mill of science requires a great quantity of grist, and its gears are exceedingly fragile.

Appendix A — Experimental Data

Parapsychology Full Data — Page 1

Condition (1 = experimental, 2 = control)	# Presses	Location	Notes
1	11	Heinz faulty lounge	
1	1		
2	26		
2	77	Near Morewood Gardens	
1	7	Hamburg basement	
2	155	6th floor Gates	(Sound didn’t go initially, had to restart after randomization)
1	25	8th floor Gates	
1	17	4th floor Gates	
2	84	5th floor Gates	
2	2	CIC	
1	107	Hunt Library	
1	95		
2	35		
2	64		
1	111		
1	32		
1	7		
2	72		
2	101		
2	132		
1	33	3rd floor Gates	
2	58	AB Classroom	
1	63		

Condition (1 = experimental, 2 = control)	# Presses	Location	Notes
2	139		
2	121		
2	52		
2	7		
2	69		
2	1		
2	20		
2	60		
2	104		
2	90		
2	141		
1	17		
1	62		
2	90	4th floor Gates	
1	24		
1	27	5th floor Gates	
1	1		
1	69		
2	159		
2	121		
1	62	8th floor Gates	
2	111		(Didn’t hear noise, restarted without rerandomizing)
2	3		
1	8		

Appendix B — R Analyses

```
> summary(Parapsych)
     Condition      Clicks
 Treatment:20   Min.   :  1.00
 Control  :29   1st Qu.: 17.00
                Median : 60.00
                Mean   : 58.69
                3rd Qu.: 95.00
                Max.   :159.00
```

```
> summary(Parapsych[Parapsych$Condition =="Control",])
     Condition      Clicks
 Treatment: 0   Min.   :  1.00
 Control  :29   1st Qu.: 26.00
                Median : 72.00
                Mean   : 72.31
                3rd Qu.:111.00
                Max.   :159.00
> summary(Parapsych[Parapsych$Condition =="Treatment",])
     Condition      Clicks
 Treatment:20   Min.   :  1.00
 Control  : 0   1st Qu.: 10.25
                Median : 26.00
                Mean   : 38.95
                3rd Qu.: 62.25
                Max.   :111.00
> parapsychOLS = lm(log(Clicks) ~ Condition, data = Parapsych)
> stargazer(parapsychOLS,type="text")

================================================
                        Dependent variable:
                    ---------------------------
                            log(Clicks)
------------------------------------------------
ConditionControl                0.566
                               (0.441)

Constant                      3.063***
                               (0.339)

------------------------------------------------
Observations                     49
R2                              0.034
Adjusted R2                     0.013
Residual Std. Error        1.517 (df = 47)
F Statistic              1.646 (df = 1; 47)
================================================
Note:                *p<0.1; **p<0.05; ***p<0.01
```

```
> parapsychOLS = lm(Clicks ~ Condition, data = Parapsych)
> stargazer(parapsychOLS,type="text")

================================================
                         Dependent variable:
                     ---------------------------
                                Clicks
------------------------------------------------
ConditionControl               33.360**
                               (13.133)

Constant                       38.950***
                               (10.104)

------------------------------------------------
Observations                      49
R2                               0.121
Adjusted R2                      0.102
Residual Std. Error         45.185 (df = 47)
F Statistic              6.452** (df = 1; 47)
================================================
Note:                *p<0.1; **p<0.05; ***p<0.01
> library(lmPerm)
> summary(lmp(Clicks ~ Condition, data = Parapsych))
[1] "Settings:  unique SS "

Call:
lmp(formula = Clicks ~ Condition, data = Parapsych)

Residuals:
   Min     1Q Median     3Q    Max
-71.31 -31.95  -6.95  30.05  86.69

Coefficients:
           Estimate Iter Pr(Prob)
Condition1   -16.68 5000   0.0146 *
---
Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1

Residual standard error: 45.18 on 47 degrees of freedom
Multiple R-Squared: 0.1207,     Adjusted R-squared: 0.102
F-statistic: 6.452 on 1 and 47 DF,  p-value: 0.01445

> ks.test(Parapsych[Parapsych$Condition=="Treatment",]$Clicks, Parapsych[Parapsych$Condition=="Contr
ol",]$Clicks)

        Two-sample Kolmogorov-Smirnov test

data:  Parapsych[Parapsych$Condition == "Treatment", ]$Clicks and Parapsych[Parapsych$Condition == "
Control", ]$Clicks
D = 0.38621, p-value = 0.05855
alternative hypothesis: two-sided
```

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