Statapult Model for Accuracy

Megan Rose Bryant
Anthony Finch
Department of Mathematics
William and Mary

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Contents

1 Executive Summary

This experiment was conducted to create a model that would allow for the accurate and consistent operation of the Statapult at a variety of ranges, using only the limited resources made available to us. A 2^{4-1} factorial experiment of resolution IV was conducted varying the factors Pullback Angle, Release Angle, Eye Hook Position, and Tension Arm Position. The rubber band and cup position factors were held constant. A regression model was derived from experimental data and used to construct a firing table with settings that were selected to minimize theoretical variance. The result was a highly consistent model which proved to be somewhat inaccurate at hitting specified distances. Further experimentation would improve the model and allow for the creation of a model that would excel at both accuracy and consistency.

2 Introduction

The Statapult is a variation on the traditional catapult and is an object of particular complexity. The goal of this experiment was to analyze each of the six factors to determine which main factors and factor interactions are most significant in determining firing accuracy. This information was then utilized to create a working regression model from which a set of firing tables was generated. These firing tables were then field-tested in a competition for accuracy and consistency at various ranges. Therefore, our design was constructed to create the most accurate and consistent model while recognizing constraints placed on the experiment in terms of limited resources.

3 Experiment Design

3.1 Potential Factors

The Statapult has a total of six factors available for consideration. Each of these factors has a range of levels associated with it. These factors are illustrated visually in 1.

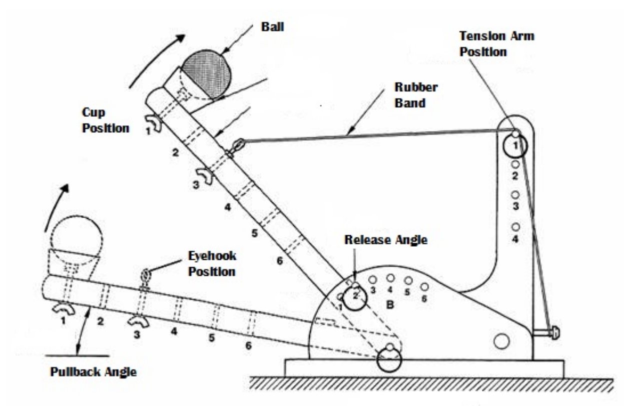

Figure 1: Design and Layout of Statapult

1. Pull Back Angle: 90 Degrees - 180 Degrees

2. Release Angle: 1 - 6

3. Cup Position: 1 - 5

4. Eye Hook Position: 2 - 6

5. Tension Arm Position: 1 - 4

6. Rubber Band used: Band 1, 2, or 3

3.2 Heuristic Designs

In order to design an efficient and effective experiment we used all process knowledge available to us. To this end, we conducted some prelimanary research on existing heuristic designs for standard catapult accuracy.

There exists a wealth of knowledge concerning catapult accuracy. This is due to its long and distinguished history in warfare; the first catapult can be traced back to 399 B.C.E. While the Statapult derives its design from a more modern concept, this archetype and the overall evolution of the catapult can give aid us in our search for significant factors.

The overall development of the catapult focused primarily on the factors that we have called pullback angle, eye hook position, tension arm position, and release angle. The factor that we have designated as cup position seems to have garnered very little attention in the overall design process. The tension medium (in this case a rubberband) is also treated as significant, but that medium has changed dramatically over the millenia. Therefore, the data regarding its significance lends little in the way of helpful process knowledge.

We used these historical precedents regarding catapult accuracy when considering which experimental design to select in section 3.3 on page 3.

3.3 Design Selection

It was essential that we select the most appropriate, cost effective design in order to succeed in constructing an accurate firing model given our limited resources. Therefore, we had to carefully consider which experimental design best met our resource restrictions while ensuring that our model would be both effecient and effective.

We began by considering our resource restrictions, the foremost of which was our run limitations. We were limited to a maximum number of 20 runs. Therefore, any design must allow for this restriction. This immediately rendered some of the designs infeasible. Specifically, a 2^6 design considering all six factors would require a prohibitive 64 runs, and a 2^5 design would require 32.

In addition to run restrictions, we were further limited by our access to the statapult. In order to maintain a competitive enviroment, we were limited to cursory access prior to a brief 48 hour testing window. This fact, combined with our limited number of runs, left us little room for error. Therefore, we were cautious in our design selection and careful to maintain as much flexibility as possible.

Even with these limiting factors there were several designs from which we could choose. As stated previously, our run restrictions did not afford us the ability to run a full 2^6 model. We could, however, run a smaller model using fewer factors. If we had been able to select two or three factors to control, we would have been able to run either variations of either a 2^4 or a 2^3 factorial design. This is where the process knowledge we gained from section 3.2 on 3 was essential, as it helped us in selecting the most important factors.

Once we selected the factors that we wished to control, we had to decide how to prioritize replicates. Replication would give us both a better estimate of our error and our factor effects, but it is costly in terms of in runs. With 20 runs, we would be able to run two replicates of a 2^3 design or one replicate of a 2^4 design. Replicates may prove to be essential in creating an accurate model, but they would be obtained at the cost of varying an additional factor.

To counter this, we considered running fractional factorials, which would allow us to screen factors for effect. The downside to this type of design is that certain factors and interactions were aliased with one another. We attempted to alleviate this problem by selecting a design of appropriately high resolution.

The resolutions available to us depended entirely on the number of factors that we decided to vary. By selecting three factors and running a half fraction factorial, we could afford ourselves the possibility of replicates, but we would do so at the cost of a higher resolution; a 2^{3-1} design has a resolution III, meaning that main effects would become aliased with two factor interactions. Additionally, we would have fewer factors with which we could control the firing process, and we would be limited to a smaller range of settings from which to choose. However, by selecting four factors to vary, our design was of resolution IV. This meant our two factor interactions were only aliased with each other, not with any of our main effects. This was achieved at the cost of doubling the number of runs in our factor screening, which limited our replication ability.

In order to maximize our ability to meet variations on testing day, we reserved the option of running our experiment sequentially. Instead of deciding on the full course of our design before experimentation began, we elected to run the second half of our experiment after completing an initial analysis of the first. This enabled us to use the first set of runs to screen for significant factors. The second set of runs could then be used as a confirmation experiment or, if the initial runs led to inconclusive results, more data for analysis. For example, if we observed very large center point effects and one of our effects did not seem significant, we could project our data into a 2^3 experiment and run axial points to estimate the curvature effects of the space. Sequential experimentation can be an extremely useful tool in screening experiments with limited runs.

By running a sequential experiment, we gained a lot of freedom in our design and experimentation process, but there was also a distinct possibility that this would introduce noise into our model (since the rubber band could stretch or warp in the time between our trials). We controlled for this noise by blocking by groups of runs and operator; the two block effects were confounded with one another since we had no reason to want to estimate their effects separately. Therefore this effect was confounded with the largest interaction possible (the $ABCD$ interaction). The *sparsity of effects* principle led us to believe that at least the highest order interaction term may be sacrificed to confounding without a significant negative impact on the model.

Regardless of our secondary design choice, we were likely be left with four additional runs. These were kept in reserve, in case some unexpected circumstances required an extra run. If no such erroneous runs occurred, we hoped to use these runs for center-point runs.

3.4 Selection of Excluded Variables

We knew based on our initial design selection process that we would need to exclude at least two factors from variability in order to conduct a successful experiment. The decision of precisely which factors to exclude from our model was a difficult one which required a great deal of time and careful consideration.

The first factor that we chose to exclude was done for relatively straightforward reasons; we eliminated the cup position factor since varying it would limit our level selection of the eyehook position. Selecting any level of eyehook position that was less than the cup position level would result in a malfunction and interfere with our firing abilities. Since our process knowledge developed in section 3.2 on page 3 gave us good cause to believe that eyehook position was significant, the exclusion of cup position was a natural selection.

Selecting a second variable for elimination was not so effortless. In the end, we concluded that the rubber band factor was the most natural. This choice however, required careful consideration and thought. In addition to our heuristic process knowledge, we knew that *Hooke's Law* in Physics gave us keen insight into the mechinations of tension. It told us that the tension produced in a spring is equal to: $F = -kx$, where F is the force produced, k is a spring constant, and x is a distance of displacement. We extrapolated this law to our situation by considering the rubber band factor to be somewhat equivalent to that of a spring. Therefore, we concluded that, in effect, a change in the rubber band should be equivalent to a change in k, the spring constant.

Furthermore, by *Hooke's Law*, the interactions between our tension instrument, the rubber band, and the other stretching factors should be significant. Thus, we should expect the interaction effects between rubber band, pullback angle, eyehook position, and tension arm position to be significant in our model. Therefore we must be expecially considerate when choosing a resolution so as to avoid alias important effects. It was expected that these interactions may be so large as to violate the *sparsity of effects* principle, causing large interactions on at least the third-order level.

We decided to exclude the rubber band factor from varying and keep it at a constant level. This introduced the question of which rubber band to use. Upon inspection, it became clear that the rubber bands available were all deteriorating and in danger of snapping under duress. We briefly considered blocking by rubber band to mitigate the ramifications of our chosen rubber band breaking during or before our final trials. In the end, however, we decided that the benefits of controlling this factor outweighed the potential risks.

4 Final Design

For our final design, we decided on an experiment with four factors varied and two controlled, run sequentially. This design is formally known as a half fraction factorial of 2^{k-1}. We decided to vary the following factors: pullback angle, release angle, eyehook position, and tension arm position. The rubber band and cup position factors were held constant. The levels of the factors are included in section 4.1.

The design was run sequentially and blocked by operator. An initial analysis was conducted after the first eight runs. This data is included in appendix 6.1 on page 6.1. The analysis of variance revealed several potential factors of interest, including the alias chain including both the AB and the CD interactions. The data seemed to indicate that there was a strong possibility that the more important factor was the AB interaction, but the aliased effect was so significant that we decided that we could not risk any assumptions about which effect was important. Therefore, we elected to conduct a foldover and ran the other eight treatment combinations in order to de-alias the two factor interactions. The four runs held in reserve for unexpected complications were not needed and we were able to utilize these to conduct a replicated center point.

4.1 Factor Levels

The factors were set to the following levels in our finalized design:

Factor	**Type**	**Uncoded Levels**	**Center Point**
Pull Back Angle	Variable	160 - 180	170
Release Angle	Variable	3, 5	4
Cup Position	Constant	1	NA
Eye Hook Position	Variable	2, 6	4
Tension Arm Position	Variable	2,4	3
Rubber Band used	Constant	1	NA

4.2 Design Matrix

The following design matrix was employed in our finalized design. The treatment combinations are listed in their run order, which was randomized. They are delineated by block.

A	B	C	D	Treatment Combination
1	-1	-1	1	AD
1	1	1	1	ABCD
1	-1	1	-1	AC
-1	-1	-1	-1	(1)
1	1	-1	-1	AB
-1	1	1	-1	AD
-1	1	-1	1	BD
-1	-1	1	1	CD
1	1	-1	1	ABD
-1	-1	-1	1	D
1	1	1	-1	ABC
-1	-1	1	-1	C
-1	1	-1	-1	B
1	-1	-1	-1	A
1	-1	1	1	ACD
-1	1	1	1	BCD

5 Methodology

5.1 Data Collection

Our final experiment consisted of an initial $\frac{1}{2}$ fractional factorial of the 4 factors identified above, a foldover on Pull Back Angle, and 4 centerpoint runs. Our first fractions of the factorial were run in random order, but we elected to run 2 centerpoint trials after each half of the factorial to track any changes that may be induced in our model due to stretching of the rubber band. In addition, we blocked both halves of the factorial by operator. The first two center points were run at the end of the first block and operated by our first operator. Similarly, the second pair of center points were run at the end of the second block and performed by our second operator.

Our original intent in running a sequential experiment was to give ourselves more freedom in the creation of our model; had our initial runs justified it, we would have dropped an unimportant variable or simply attributed certain effects to a given interaction and then neglected to break the alias chains. This would have permitted us to use the rest of our runs to either run a full axial range of axial points or to replicate our first $\frac{1}{2}$ fraction, enabling us to observe the effects of the various factors on our variance.

Unfortunately, we were unable to pursue either of these courses of action. Our model yielded a single significant interaction term B. The remaining factors and interactions were not significant at the $\alpha = 0.05$ level. They were, however, close enough to significant to suggest that replication would make them so. We ran into difficulty with the aliased two factor interaction $AB = CD$. It was appearing to be significant, yet it was unclear which of the two interactions was the true contributor. We had reason to believe that the AB interaction was likely the cause of this significance, but the evidence was not strong enough to solidify this claim. Therefore, since the alias effect was so large, we deemed it too much of a risk not to break this alias chain. Thus, we elected to conduct a foldover that allowed us to break all of our alias chains and indentify the truly significant effects.

Great care was taken in recording the distances of each run. The presently non-firing member of the team acted as observer and recorder of data. To ensure accuracy, the test were filmed and reviewed after each shot. Therefore every run was confirmed by camera footage. All but one of the runs were conducted without incident. When conducting run 16 with the pullback angle at 180 degrees and all other factors at the low level, the shot missed the table entirely and struck the observer. In this case, we were forced to estimate the distance based upon how far away the observer was standing from the table. Fortunately, the ball struck at a height very near to the surface of the table, so there was no need to estimate the additional distance it may have travelled before landing.

6 Statistical Analysis

6.1 ANOVA

	P-Value
Model	0.000
Blocks	0.005
Linear	0.000
A = Pullback Angle	0.000
B = Release Angle	0.000
C = Eyehook Position	0.000
D = Tension Arm Position	0.000
2-Way Interactions	0.000
A = Pullback Angle*B = Release Angle	0.000
A = Pullback Angle*C = Eyehook Position	0.002
A = Pullback Angle*D = Tension Arm Position	0.001
B = Release Angle*C = Eyehook Position	0.001
B = Release Angle*D = Tension Arm Position	0.005
C = Eyehook Position*D = Tension Arm Position	0.073
3-Way Interactions	0.006
A = Pullback Angle*B = Release Angle*C = Eyehook Position	0.003
A = Pullback Angle*B = Release Angle*D = Tension Arm Position	0.031
B = Release Angle*C = Eyehook Position*D = Tension Arm Position	0.027
Curvature	0.003
Error	
Lack-of-Fit	0.624
Pure Error	
Total	

Our final ANOVA yielded significant p-values for almost all of our effects, including our interaction effects. This is also evident from the interaction and main effects plots included in appendix 3 on page 15. We elected to keep the CD interaction in the model despite the fact that it was technically not significant at the $\alpha = 0.05$ level. We did, however, eliminate the ACD and $ABCD$ interaction terms as neither neared the significance cutoff at the $\alpha = 0.05$ level. These terms constitute the Lack-of-Fit error that appears in the table above.

The Normal Probabilty Plot of our final model for the standardized effect (see Figure 5 on page 15) illustrates the high degree of significance observed for almost all of the terms included in our final model. This is in stark contrast to the normal probability plot generated in original analysis of variance (viewable in appendix 4 on page 15).

It is evident from the data that this system is an exception to the *sparcity of effects* principle and the assumption that significant factors are restricted primarily to lower-order interactions. We were therefore prevented from eliminating any further terms to gain more degrees of freedom for our error estimates, and were left with the large, somewhat dubious estimate arising from our 4 centerpoint runs.

Our analysis of the residuals indicated some predicitable issues. Since we were forced to run a full foldover design, our residuals were entirely derived from our centerpoints and elimination of two of our higher-order interaction terms. This explains their abnormality, which is visually apparent in the 4-in-1 plot (Figure 6 on page 16) of our distribution of residuals. Our 'verus-fits' and 'histogram' exhibit somewhat extreme outliers, which is to be expected from a single replicate experiment. What is more interesting, however, is that our residuals 'versus order' depict a fairly steady trend. This is cause for some concern as it appears that there was some small, additional error that was not captured by our blocks. This likely reflects the degradation of the rubber band throughout our trials in combination with a difference between operators.

6.2 Regression Model

Our final regression equation was, in coded units:

```
Distance = 60.063 +11.937PullbackAngle -21.312ReleaseAngle
           -11.438EyehookPosition -12.062TensionArmPosition
           -12.937PullbackAngle*ReleaseAngle
           -5.563PullbackAngle*EyehookPosition
           -6.937PullbackAngle*TensionArmPosition
           +6.688ReleaseAngle*EyehookPosition
           +4.312ReleaseAngle*TensionArmPosition
           -1.812EyehookPosition*TensionArmPosition
           +5.062PullbackAngle*ReleaseAngle*EyehookPosition
           +2.437PullbackAngle*ReleaseAngle*TensionArmPosition
           +2.563ReleaseAngle*EyehookPosition*TensionArmPosition
           +11.19CtPt
```

6.3 Firing Tables

We had initially hoped to use Minitab's "Response Optimizer" to generate our firing tables, but found that it lacked flexibility. It was unable to handle both center points and a continuous variable. Furthermore, it failed to provide any information on the variance of the factor combinations, which was unacceptable. We therefore decided to create our firing tables using Microsoft Excel. This afforded us more intimate control over the model and allowed us to efficiently generate tables with a continuous factor.

We began with our Minitab generated regression equation. Utilizing this model created some difficulty, however, when attempting to account for our center point. In order to effectively incorporate this data,we estimated the effect of the center point linearly by setting its effect to the following

$$CtPt = \frac{4 - |x_1| - |x_2| - |x_3| - |x_4|}{4}$$

where the x_i variables are the coded settings for their respective factors. Because of the way we constructed this center point variable, none of our observed trials were affected in any way. The purpose of using this equation for the center point was to extrapolate the partial center point settings, and it was necessary for us to do this in order to estimate the effects of varying the pullback angle to hit various ranges.

The resultant modified regression equation we used to generate our firing tables was:

```
Distance = 60.063 +11.937PullbackAngle -21.312ReleaseAngle
           -11.438EyehookPosition -12.062TensionArmPosition
           -12.937PullbackAngle*ReleaseAngle
           -5.563PullbackAngle*EyehookPosition
           -6.937PullbackAngle*TensionArmPosition
           +6.688ReleaseAngle*EyehookPosition
           +4.312ReleaseAngle*TensionArmPosition
           -1.812EyehookPosition*TensionArmPosition
           +5.062PullbackAngle*ReleaseAngle*EyehookPosition
           +2.437PullbackAngle*ReleaseAngle*TensionArmPosition
           +2.563ReleaseAngle*EyehookPosition*TensionArmPosition
           +11.19(4 - |PullbackAngle| - |ReleaseAngle|
           -|EyehookPosition| - |TensionArmPosition|)/4
```

We were then then able to use this equation to isolate the pullback angle. This was accomplished by modifying our regression equation so that pullback angle was the sole dependent variable.

This methodology was used to calculate the necessary pullback angle for every settings group at each range interval. We then eliminated all groups of settings which were deemed implausible. This included any settings which required a pullback angle above 185 or below 150. These cutoff ranges were chosen based on the physical limitations of the Statapult to a maximum of 185 and we deemed 150 degrees to be a significant departure from the range which we were comfortable extrapolating. Since the coded variable for 150 would be -2, twice our largest distance from the center point.

We then analyzed the theoretical variance for our predictions for each range. This was accomplished using the formula for variance:

$$Var(\hat{Y}) = \frac{26.23}{16}(1 + x_1^2 + x_2^2 + x_3^2 + x_4^2 + x_1^2 x_2^2 + \cdots)$$

where we include the square of every term in the model.

We used this variance equation to select the lowest-variance estimate for each point. This group of settings is the group presented on our firing table (see figure **??** on page **??**). This methodology gave a large advantage to settings which were contained the most settings at the center; thus our methodology unintentionally emphasized extrapolations over observed groups of factor settings.

Our final firing table is displaying in Appendix A.2: Firing Tables on page 13, and it includes estimated Confidence Intervals using the computed ranges and estimated variances.

6.4 Accuracy Analysis

Depicted below is a table of our runs when targeting 3 ranges.

Target	Run 1	Run 2	Run 3	Average	Std. Dev.	Theoretical CI
32	45	40	40	41.67	2.89	(25.8,38.2)
64	58	60	61	59.67	1.53	(60.5,67.5)
98	84	81	81	82	1.73	(93.9,102)

In the end, none of our observed averages landed in our theoretical confidence intervals; our average deviation from our target range was 10 inches, and the average of our standard deviations within the shot groups was 2.05. If we model our deviations by their respective means, we get a standard error estimate of 2.62 ($2.62 = \sqrt{(2.89^2 + 1.53^2 + 1.73^2)/6}$. Note: we have 6 error degrees of freedom since we lose 3 degrees of freedom for the model).

This standard error is extremely small and indicative of our model's potential for accuracy. While our model did not accurately hit the desired ranges, its ability to cluster its shots intimates that there is hope that a small adjustment has the potential to correct our model. The trade-off between distance accuracy and small variance is one that was anticipated, but the severity of the effect was suprising nontheless.

7 Conclusions

7.1 Model Adequacy

We were able to design a model that yielded a high degree of consistency. This consistency was unfortunately achieved at the expense of distance accuracy. These results were typical of other teams as we saw an inverse relationship between accurate distance and small variance overall. This seems indicative of the overall trend between accuracy and clustering.

It was found that our model was most accurate when aiming for midrange targets such as Target II (see appendix A.2 on page 13). Overall, the results obtained during the firing competition closely mirrored the results that we observed when replicating our centerpoint. This leads us to conclude that, if we were able to run a replicate of our experiment to obtain better effect estimates, the overal accuracy of our model would improve.

We have concluded that one of the primary reasons for our model's inaccuracy is that our firing tables consisted primarily of extrapolated settings. A number of settings were not seen in our experiment trials, but were extrapolated based on our regression equation. As observed above, when the settings used were closer to those that were actually tested during our experimentation, the model was markedly more accurate. We believe that, if we had given tested levels more precedence over theoretical variance when crafting our firing table, our model would have been more accurate overall.

The consequence of priotizing tested settings would have likely made a significant increase in our variance. Thus, our model can be said to have performed extremely well in the capacity for which it was designed, but not for which it was actually intended. This discrepancy arose from our concern that our limitation on runs and lack of replication would result in a model with variance so large as to render it useless. This was supported by the large variance we obseved in the center points. We therefore underestimated the potential accuracy problems associated with using so many extrapolated settings for our final firing tables.

It should be noted that there were a number of external factors that likely influenced our model. For instance, our first (and most inaccurate) shot was performed with the rubber band affixed to the eyehook and the 'tail' (the longer end) pointing up. This is in contrast with both our experimental and sequential competitive shots which all had the 'tail' pointing down. Additionally, our first (and competitive) operator was noted as having moved the firing arm out of position immediately before firing on several shots. While it is possible that this error was captured in our model by the block effect, it is not guaranteed as it was not being controlled for. Such external factors may have had a significant impact on the accuracy of our model, both in terms of mean distance and variance. If these nuisance factors has been eliminated or adequately controlled for, our model would have performed more accurately. Further experimentation would allow us to account for such deviations in operation.

7.2 Model Refinements and Future Experimentation

Our final model was outstanding at minimizing the variance between shots and had the ability to closely replicate distance hit. However, as is with most things, there is room for improvement. We saw overall that this increase in consistency was inversely related to distance accuracy. Our model would benifit greatly from further experimentation.

In our initial analysis, we were unable to determine the dominant factor in the alias chain $AB + CD$. This was the primary motivating factor in conducting a full foldover instead of replication. Now that we have successfuly de-aliased the two factor interactions, our model can be replicated. This replication will require an additional 16 runs, but will likely result in a dramatic increase in accuracy. This is because the increase in replicate will allow us to better estimate error. Furthermore, this will allow us to model our variance as a function of each variable rather than on center points and sacraficed higher order interactions.

This, in turn, will give us empirical data upon which we may base our variance minimization operation. This new data, coupled with the theoretical values we previously used, should result in a better error estimate. It is important to note that this strategy will only improve our variance estimates because it is being conducted in conjunction with our obtained model; had we run replicates originally, our model would have been vulnerable to variance innaccuracies if the competitive targets did not closely mirror our experimental distances.

We can further improve our model by rederiving the firing tables with altered preferences. If we were to give more weight to tested settings over theoretical ones, our new table would likely be more accurate and more insulated against noise. This is a cheap improvement as it would not require more runs, only more computation. This would have given us less exposure to our model's inevitable inaccuracies and may have produced superior results.

Future models would also benefit from the addition of axial points. While they can be expensive in terms of runs, the addition of axial points would allow us to obtain a better estimate of the curvature of the model than center points alone. Improperly modeled curvature likely had a negative effect on our model's accuracy which would be greatly improved upon with further experimentation.

We believe that with further experimentation it would be feasible to achieve both a high degree of accuracy and a continued high degree of consistency. While this would require a further investment of either runs or computational time, the potential for improvement is large enough to warrant further study. Furthermore, the process knowledge and data derived from this round of experimentation would be able to inform and significantly improve any such future endeavors.

Appendices

A

A.1 Initial ANOVA Analysis

The following ANOVA table was the result of our initial 2^{4-1} experiment. It was these results that encouraged us to conduct a full foldover to de-alias our two-factor interactions.

```
Analysis of Variance
Source                                          F-Value  P-Value
Model                                             56.53    0.103
  Linear                                          86.81    0.080
    A = Pullback Angle                            93.44    0.066
    B = Release Angle                            196.00    0.045
    C = Eyehook Position                          36.00    0.105
    D = Tension Arm Position                      21.78    0.134
  2-Way Interactions                              32.47    0.128
    A = Pullback Angle*B = Release Angle          96.69    0.065
    A = Pullback Angle*C = Eyehook Position        0.69    0.558
    A = Pullback Angle*D = Tension Arm Position    0.03    0.895
  Curvature                                        7.61    0.221
Error
Total

Model Summary

      S    R-sq  R-sq(adj)  PRESS  R-sq(pred)
4.24264  99.78%     98.01%      *           *

Term                                        T-Value  P-Value   VIF
Constant                                      37.83    0.017
A = Pullback Angle                             9.67    0.066  1.00
B = Release Angle                            -14.00    0.045  1.00
C = Eyehook Position                          -6.00    0.105  1.00
D = Tension Arm Position                      -4.67    0.134  1.00
A = Pullback Angle*B = Release Angle          -9.83    0.065  1.00
A = Pullback Angle*C = Eyehook Position       -0.83    0.558  1.00
A = Pullback Angle*D = Tension Arm Position   -0.17    0.895  1.00
Ct Pt                                          2.76    0.221  1.00

Aliases
I +ABCD
A +BCD
B +ACD
C +ABD
D +ABC
AB +CD
AC +BD
AD +BC
```

A.2 Firing Table

Range	Uncoded	Pullback Angle	Release Angle	Eyehook Position	Tension Arm	Variance	95% CI Low	95%CI High
30	(0.79,1,0,1)	168	5	4	4	10.7	23.0	37.0
32	(0.55, 1,0,1)	166	5	4	4	8.6	25.8	38.2
34	(0.31,1,0,1)	163	5	4	4	7.2	28.3	39.7
36	(0.07,1,0,1)	161	5	4	4	6.6	30.5	41.5
38	(0.17,1,0,1)	158	5	4	4	6.7	32.5	43.5
40	(-0.09,1,1,0)	159	5	6	3	6.4	34.6	45.4
42	(-0.49,0,1,1)	155	4	6	4	6.5	36.6	47.4
44	(0.83,1,0,0)	168	5	4	3	5.5	39.0	49.0
46	(0.30,1,0,0)	163	5	4	3	3.6	42.0	50.0
48	(-0.23,1,0,0)	158	5	4	3	3.4	44.1	51.9
50	(-0.75,1,0,0)	152	5	4	3	5.1	45.2	54.8
52	(-0.14,1,0,-1)	159	5	4	2	6.7	46.5	57.5
54	(-0.84,0,1,0)	152	4	6	3	3.1	50.2	57.8
56	(-0.28,0,1,0)	157	4	6	3	2.9	52.4	59.6
58	(0.27,0,1,0)	163	4	6	3	3.9	53.8	62.2
60	(0.83,0,1,0)	168	4	6	3	5.8	54.9	65.1
62	(-1.01,0,0,0)	150	4	4	3	3.3	58.1	65.9
64	(-0.79,0,0,0)	152	4	4	3	2.7	60.5	67.5
66	(-0.57,0,0,0)	154	4	4	3	2.2	62.8	69.2
68	(-0.36,0,0,0)	156	4	4	3	1.8	65.1	70.9
70	(-0.14,0,0,0)	159	4	4	3	1.7	67.2	72.8
72	(0.08,0,0,0)	161	4	4	3	1.7	69.2	74.8
74	(0.30,0,0,0)	163	4	4	3	1.8	71.1	76.9
76	(0.52,0,0,0)	165	4	4	3	2.1	72.9	79.1
78	(0.74,0,0,0)	167	4	4	3	2.5	74.6	81.4
80	(0.96,0,0,0)	170	4	4	3	3.1	76.2	83.8
82	(0.14,0,-1,0)	161	4	3	3	3.1	78.2	85.8
84	(0.28,0,-1,0)	163	4	3	3	2.9	80.4	87.6
86	(0.42,0,-1,0)	164	4	3	3	2.9	82.4	89.6
88	(0.55,0,-1,0)	166	4	3	3	2.9	84.4	91.6
90	(0.69,0,-1,0)	167	4	3	3	2.9	86.4	93.6
92	(0.82,0,-1,0)	168	4	3	3	3	88.3	95.7
94	(0.96,0,-1,0)	170	4	3	3	3.2	90.2	97.8
96	(1.10,0,-1,0)	171	4	3	3	3.5	92.0	100.0
98	(0.37,-1,0,0)	164	3	4	3	3.7	93.9	102.1
100	(0.46,-1,0,0)	165	3	4	3	4	95.7	104.3

A.3 Experiment Results

Standard Order	Run Order	Center Point	Blocks	A = Pullback Angle	B = Release Angle	C = Eyehook Position	D = Tension Arm Position	Distance
2	1	1	1	1	-1	-1	1	110
8	2	1	1	1	1	1	1	18
6	3	1	1	1	-1	1	-1	104
1	4	1	1	-1	-1	-1	-1	63
4	5	1	1	1	1	-1	-1	53
7	6	1	1	-1	1	1	-1	35
3	7	1	1	-1	1	-1	1	37
5	8	1	1	-1	-1	1	1	34
9	9	0	1	0	0	0	0	63
10	10	0	1	0	0	0	0	69
4	11	1	2	1	1	-1	1	33
1	12	1	2	-1	-1	-1	1	65
8	13	1	2	1	1	1	-1	47
5	14	1	2	-1	-1	1	-1	64
3	15	1	2	-1	1	-1	-1	51
2	16	1	2	1	-1	-1	-1	160
6	17	1	2	1	-1	1	1	51
7	18	1	2	-1	1	1	1	36
9	19	0	2	0	0	0	0	78
10	20	0	2	0	0	0	0	75

A.4 Competition Results

Team Names	Target I: 32”				Target II: 64”				Target III: 98”				
	Attempt #			STD DV Avg	Attempt #			STD DV Avg	Attempt #			STD DV Avg	Overall Avg
	1	2	3		1	2	3		1	2	3		
Ian	34.5	29.5	30	2.75	60	69	64.75	4.50	87	89	96	4.73	3.99
Van	2.5	2.5	2	2.33	4	5	0.75	3.25	11	9	2	7.33	4.31
Kat	33	35	34	1	75	77	76	1	125	120.5	120	2.75	1.58
Constance	1	3	2	2	11	13	12	12	27	22.5	22	23.83	12.61
Jamie	27.5	33	35	3.88	71	65	59	6	105	104.5	104	0.5	3.46
Tyler	4.5	1	3	2.83	7	1	5	4.33	7	6.5	6	6.5	4.56
Rose	45	40	40	2.89	58	60	61	1.53	84	81	81	1.73	2.05
Anthony	13	8	8	9.67	6	4	3	4.33	14	17	17	16	10.00
Larson	33	27	31	3.06	69	72.5	72.75	2.10	107	104.5	106	1.26	2.14
Catherine	1	5	1	2.33	5	8.5	8.75	7.42	9	6.5	8	7.83	5.86

A.5 Main Effects Plots

Figure 2: Main Effects Plot for Distance

Figure 3: Interaction Plot for Distance

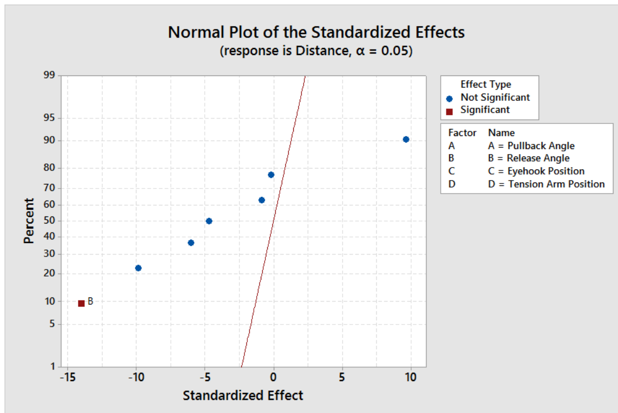

Figure 4: Normal Probability of Effects for 1st Half-Fraction

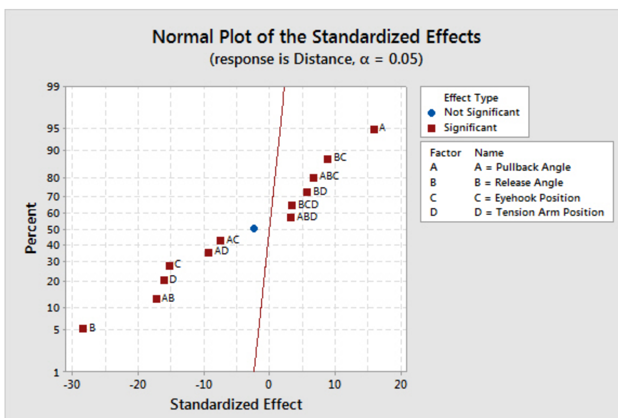

Figure 5: Normal Probability of Effects for Full Model

A.6 Residual Plots

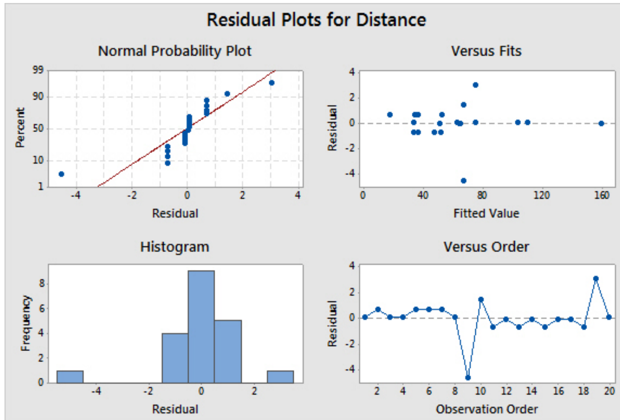

Figure 6: Residual Plots for Final Model