   b. Predictions from the tables will be good so long as the reference or calibrating measurement has a typical percentage of resistance effect
   c. Thanks to this, tables may yield decent predictions, say from 30 to 60 deg, even when resistance effects are highly pronounced
3. The numbers in the Tables are thus presented in just the wrong form for purposes of bringing empirical data to bear on the theory!
   a. They understate discrepancies, whether from resistance or from any other uniform error, and hence allow experimental results to appear to provide stronger confirmation for the theory than they really do
   b. They **mask** any systematic discrepancies that could be used to argue that the theory is false, or is open to refinement
   c. Better off for purposes of marshaling empirical evidence if discrepancies exposed as completely and clearly as possible, if only to pursue improved, more telling experiments
4. This is a general feature of "engineering" oriented idealized theories and models that are first calibrated before being applied, where the calibration serves to compensate for neglected effects
   a. The fact that they work in the practical realm provides some evidential support for the theory
   b. But generally less support than it appears to, for more often than not the calibrated theory works to the extent it does too much for the wrong reason
   c. And almost nothing is learned from meticulously comparing theory with observation in these cases, except limits of their practical value
5. Galileo shows little sign of seeing this, but others we will be studying saw the difference quite clearly
   a. In 1670's Collins, among others, attempted to provide better tables for artillery, building on Galileo's theory
   b. Clear by then that Galileo's tables were proving to be of limited military value
   c. A continuing question: what is the trajectory a projectile follows in air
   d. A question of importance in the 19th and 20th centuries -- Babbage and Eckart, the designers of digital computers in the two centuries, focused on the ballistics problem

F. Galileo’s Suppressed "Ski-Jump" Experiment

1. Drake (as well as Hahn and Damerow *et al*) have argued from a sheet in Galileo's notebooks that he indeed did perform the "ski-jump" experiment (folio 116v, which Drake assigns to 1608 (p. 129f))
   a. That is, let a sphere acquire its horizontal speed along a table from descending along an inclined plane and then measure the distance -- i.e. amplitude -- covered horizontally versus the height of further descent after it leaves the table
   b. As shown in the figure in the Appendix, taken from Galileo’s notebooks and cleaned up by Hahn
2. My table in the Appendix compares what Galileo measured against what his theory would have predicted: [(*theory* – *observed*)/*theory*]

a. His numbers in notebook indicate a fairly consistent, slightly greater than 17 percent discrepancy between observed and predicted amplitudes (as a fraction of the theoretical amplitudes)
b. If the horizontal speed was acquired by the ball rolling on its bottom down an inclined plane, as Drake naturally proposes, then the rolling-falling discrepancy is 15.5 percent, i.e. $1-\sqrt{(5/7)}$
c. Hahn (see Appendix) has argued that the greater than 15.5 percent discrepancies fit well with Galileo having used an inclined-plane groove in this experiment in which the sphere does not roll on its bottom, but on tangent points between the bottom and its middle
d. In particular, if the width of the groove is 4/9 the diameter of the rolling sphere, the discrepancy between theory and observation should be 18.3%

3. At the very least, then, we have reason to think that Galileo really did do this experiment and got the results he quoted; yet he decided not to publish those results, presumably because they were incompatible with his theory
   a. Drake thinks Galileo attributed this to a loss of speed in the transition from inclined plane to horizontal, but he might as well have attributed at least part of it to the effects of air resistance
   b. But in a situation in which there were too many other things to attribute the discrepancy to, and he chose not to follow up the result with a series of further complementary experiments, nor did he publish the idea of the experiment, giving others a chance to follow it up
   c. A result that remains unpublished becomes merely a part of Galileo's biography, and not part of science
4. Still, we should notice that this is just the sort of further experiment, beyond the inclined-plane experiment described in "The Third Day," to complement it and provide a cross-check on it
   a. The deep lesson here, which Galileo seems not to have appreciated, is that getting well-established results from experiments requires more than single experiments in isolation -- the very practice Kepler had followed in pursuing cross-checks for results in *Astronomia Nova*
   b. It requires a host of complementary experiments, cross-checking one another in order to provide safeguards against being misled by tacit, unrecognized assumptions in a single experiment of just the sort that occurred with the rolling-falling lacuna in his inclined-plane experiments
   c. In particular, when discrepancies appear, follow-on experiments exploring those discrepancies are the best hope for sorting out what is going on
   d. Especially experiments like this one that eliminate time as something that has to be measured!
5. The fact that Galileo appears not to have pursued such further experiments says something about the limitations of his conception of using experiments to establish conclusions
   a. Insofar as Galileo's sublimity does make the experiment obvious, what is more interesting to me is whether others subsequently performed it and elected not to report the results
   b. That would be quite a comment on experimental practice during the 17^{th} century