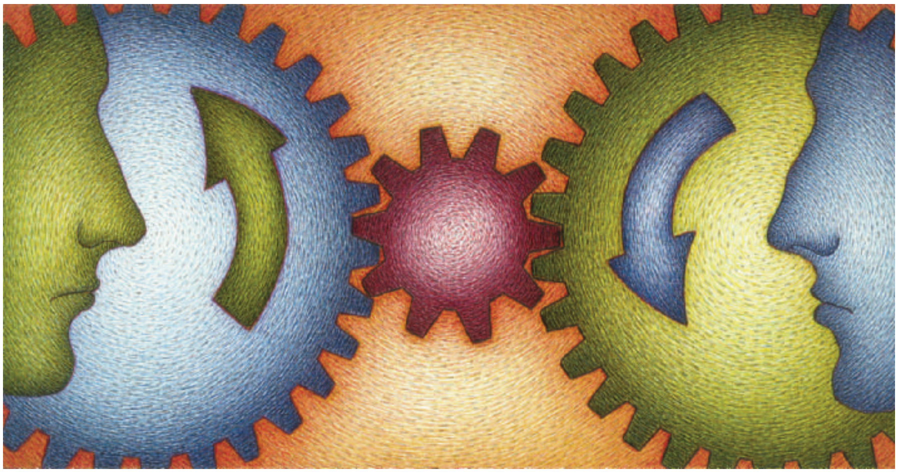
From 'not wrong' to (maybe) right

How ignoring glaring problems can sometimes lead to fruitful theories.

Frank Wilczek



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Savas Dimopoulos is always enthusiastic about something, and in the spring of 1981 it was supersymmetry. He was visiting the new Institute for Theoretical Physics in Santa Barbara, which I had recently joined. We hit it off immediately — he was bursting with wild ideas, and I liked to stretch my mind by trying to take them seriously.

Supersymmetry was (and is) a beautiful mathematical idea. The problem with applying supersymmetry is that it is too good for this world. We simply do not find particles of the sort it predicts. We do not, for example, see particles with the same charge and mass as electrons, but a different amount of spin.

However, symmetry principles that might help to unify fundamental physics are hard to come by, so theoretical physicists will not give up on them easily. Based on previous experience with other forms of symmetry, we have developed a fallback strategy, called spontaneous symmetry breaking. In this approach, we postulate that the fundamental equations of physics have the symmetry, but the stable solutions of these equations do not. The classic example of this phenomenon occurs in an ordinary magnet. In the basic equations that describe the physics of a lump of iron, any direction is equivalent to any other, but the lump becomes a magnet with some definite north-seeking pole.

Understanding the possibilities for spontaneously broken supersymmetry requires model building — the creative activity of proposing candidate equations and analysing their consequences. Building models with spontaneously broken supersymmetry that are consistent with everything else we know about physics is a difficult business. Even if you manage to get the symmetry to break, the extra particles are still there (just heavier) and cause various mischief. I briefly tried my hand at model building when supersymmetry was first developed in the mid-1970s, but after some simple attempts failed miserably, I gave up.

Savas was a much more naturally gifted model-builder, in two crucial respects: he did not insist on simplicity, and he did not give up. When I identified a particular difficulty (let us call it A) that was not addressed in his model, he would say: "It's not a real problem, I'm sure I can solve it," and the next afternoon he would come in with a more elaborate model that solved difficulty A. But then we would discuss difficulty B, and he would solve that one with a completely different complicated model. To solve both A and B, you had to join the two models, and so on to difficulty C, and soon things got incredibly complicated. Working through the details, we would find some flaw. Then the next day Savas would come in, very excited and happy, with an even more complicated model that fixed yesterday's flaw. Eventually we eliminated all flaws, using the method of proof by exhaustion — anyone, including us, who tried to analyse the model would get exhausted before they understood it well enough to find the flaws.

When I tried to write up our work for publication, there was a certain feeling of unreality and embarrassment about the complexity and arbitrariness of what we had come up with. Savas was undaunted. He even maintained that some existing ideas about unification using gauge symmetry, which to me seemed genuinely fruitful, were not really so elegant if you tried to be completely realistic and work them out in detail. In fact, he had been talking to another colleague, Stuart Raby, about trying to improve those models by adding supersymmetry! I was extremely sceptical about this 'improvement', because I was certain that the added complexity of supersymmetry would spoil the existing success of gauge symmetry in explaining the relative values of the strong, electromagnetic and weak coupling constants. The three of us decided to do the calculation, to see how bad the situation was. To get oriented and make a definite calculation, we started by doing the crudest thing, which was to ignore the whole problem of breaking supersymmetry. This allowed us to use very simple (but manifestly unrealistic) models.

The result was amazing, at least to me. The supersymmetric versions of the gauge symmetry models, although they were vastly different from the originals, gave very nearly the same answer for the couplings.

That was the turning point. We put aside the 'not wrong' complicated models with spontaneous supersymmetry breaking, and wrote a short paper that, taken literally (with unbroken supersymmetry), was wrong. But it presented a result that was so straightforward and successful that it made the idea of putting gauge symmetry and supersymmetry unification together seem (maybe) right. We put off the problem of how supersymmetry gets broken. And today, although there are some good ideas about it, there is still no generally accepted solution.

After our initial work, more precise measurements of the couplings made it possible to distinguish between the predictions of models with and without supersymmetry. The models with supersymmetry work much better. We all eagerly await operation of the Large Hadron Collider at CERN, the European particle physics laboratory, where, if these ideas are correct, the new particles of supersymmetry — or, you might say, the new dimensions of superspace — must make their appearance.

This little episode, it seems to me, is 179 degrees or so out of phase from Karl Popper's idea that we make progress by falsifying theories. Rather, in many cases, including some of the most important, we suddenly decide our theories might be true, by realizing that we should strategically ignore glaring problems. It was a similar turning point when David Gross and I decided to propose quantum chromodynamics (QCD) based on asymptotic freedom, putting off the problem of quark confinement. But that is another story... ■

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