Preface

The immediate advance we communicate with this monograph is the discovery of an exact model for a critical spin chain with arbitrary spin S, which includes the Haldane–Shastry model as the special case $S = \frac{1}{2}$. For $S \geq 1$, we propose that the spinon excitations obey a one-dimensional version of non-Abelian statistics, where the topological degeneracies are encoded in the fractional momentum spacings for the spinons. The model and its properties, however, are not the only, and possibly not even the most important thing one can learn from the analysis we present.

The benefit of science may be that it honors the human spirit, gives pleasure to those who immerse themselves in it, and pragmatically, contributes to the improvement of the human condition in the long term. The purpose of the individual scientific work can hence be either a direct contribution to this improvement, or more often an indirect contribution by making an advance which inspires further advances in a field. When we teach Physics, be it in lectures, books, monographs, or research papers, we usually teach what we understand, but rarely spend much effort on teaching how this understanding was obtained. The first volume of the famed course of theoretical physics by L. D. Landau and E. M. Lifshitz [1], for example, begins by stating the principle of least action, but does nothing to motivate how it was discovered historically or how one could be led to discover it from the study of mechanical systems. This reflects that we teach our students how to apply certain principles, but not how to discover or extract such principles from a given body of observations. The reason for this is not that we are truely content to teach students of physics as if they were students of engineering, but that the creative process in physics is usually erratic and messy, if not plainly embarrassing to those actively involved, and hence extremely difficult to recapture. As with most of what happens in reality, the actual paths of discovery are usually highly unlikely. Since we enjoy the comfort of perceiving actions and events as more likely and sensible, our minds subconsciously filter our memory to this effect.

One of the first topics I immersed myself in after completing my graduate coursework was Laughlin's theory of the fractionally quantized Hall effect [2].

I have never completely moved away from it, as this work testifies, and take enormous delight whenever I recognize quantum Hall physics in other domains of physics. More important than the theory itself, however, was to me to understand and learn from the way R. B. Laughlin actually discovered the wave function. He numerically diagonalized a system of three electrons in a magnetic field in an open plane, and observed that the total canonical angular momentum around the origin jumped by a factor of three (from $3\hbar$ to $9\hbar$) when he implemented a Coulomb interaction between the electrons. At the same time, no lesser scientists than D. Yoshioka, B. I. Halperin, and P. A. Lee [3] had, in an heroic effort, diagonalized up to six electrons with periodic boundary conditions, and concluded that their data were "supportive of the idea that the ground state is not crystalline, but a translationally invariant "liquid."" Their analysis was much more distinguished and scholarly, but unfortunately, did not yield the wave function.

The message I learned from this episode is that it is often beneficial to leave the path of scholarly analysis, and play with the simplest system of which one may hope that it might give away natures thoughts. For the Laughlin series of quantized Hall states, this system consisted of three electrons. I spend most of my scientific life adapting this approach to itinerant antiferromagnets in two dimensions, where I needed to go to twelve lattice sites until I could grasp what nature had in mind. But I am digressing. To complete the story about the discovery of the quantum Hall effect, Laughlin gave a public lecture in Amsterdam within a year of having received the Nobel price. He did not mention how he discovered the state, and at first couldn't recall it when I asked him in public after the lecture. As he was answering other questions, he recalled the answer to mine and weaved it into the answer of another question. During the evening in a cafe, a very famous Russian colleague whom I regard with the utmost respect commented the story of the discovery with the words "But this is stupid!".

Maybe it is. If it is so, however, the independent discoveries of the spin $\frac{1}{2}$ model by F. D. M. Haldane [4] and B. S. Shastry [5] may fall into the same category. Unfortunately, I do not know much about these discoveries. Haldane told me that he first observed striking degeneracies when he looked at the model for $N = 6$ sites numerically, motivated by the fact that the $1/r^2$ exchange is the discrete Fourier transform of $\epsilon(k) = k(k - 2\pi)$ in one dimension. Shastry told me that he discovered it "by doing calculations", which is not overly instructive to future generations. If my discovery of the general model I document in this monograph will be perceived in the spirit of my friends comment, I will at least have made no attempt to evade the charge.

In short, what I document on these pages is not just an exact model, but a precise and reproducible account of how I discovered this model. This reflects my belief that the path of discovery can be as instructive to future generations as the model itself. Of course, the analysis I document does not fully reflect the actual path of discovery, but what would have been the path if my thinking had followed a straight line. It took me about four weeks to obtain all the results and about four months to write this monograph. The reason for this discrepancy is not that my writing proceeds slowly, but that I had left out many intermediate steps when I did the calculation. The actual path of discovery must have been highly unlikely. In any event, it is comforting to me that, now that I have written a scholarly and coherent account of it, there is little need to recall what actually might have happened.

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Karlsruhe, April 2011 Martin Greiter

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