My research interests over the years (1960-2011) have spanned a number of topics, mostly within the realms of condensed-matter and statistical physics.

My (experimental) MSc thesis, performed under the guidance of the late Professor Solly Cohen and with the help of the late Gideon Gilat, then a senior PhD student, used the angular correlations of two successively emitted **γ**-rays to study the dynamics of the intermediate nuclear state in the solid. The instrument I built to perform the measurements automatically, has worked well for at least 20 years thereafter. A huge hurdle was that it needed two large expensive (by the standards of that time) electrolytic capacitors, and with some effort we got only one. I therefore invented and built a simple "bootstrap" circuit that switched the same capacitor between its two tasks -- something that the experts did not expect to work, but it did. It is still one of the achievements I'm most proud of.

My (mostly theoretical) PhD work, with the guidance of Israel Pelah, dealt with proton dynamics in hydrogen-bonded systems, including some ferroelectrics, as studied by inelastic neutron scattering and infrared spectroscopy. The tunneling of the proton between two minima along the hydrogen bond played an important role there. From that work followed the one (during the postdoctoral period and later) on phase transitions. Of special interest to me were the questions of finite, but long, range order in low-dimensional (low-D) systems and quasi long range order (decaying as a power-law) in 2D x-y symmetry systems, partly in collaboration with Leon Gunther. We also considered the response of a small conducting ring to an Aharonov-Bohm (AB) flux. It was known that if the ring is normal, nothing would happen in the infinite-system limit. We found however that a current would flow for a finite ring at low temperatures, proportional to the inverse of the ring's radius for ideal noninteracting particles. Moreover, for a ring made of superconducting material quite above the nominal Tc, there should still a finite equilibrium current even when the resistance of the wire making the ring was finite. This novel ability of a small system with respect to the infinite one was in fact an important precursor of mesoscopic Physics, but it was not regarded as having any interest by most colleagues at that time. My interest in fluctuations and scaling as function of system's size started then. At the same time, Alan Evans and I studied the pairing theory of the Bose superfluid, as an attempt to provide a, still needed, microscopic theory of the latter.

After joining Tel Aviv University, the research on what happens to phase transitions in finite systems was continued with David Bergman, examining how the "transition" sharpens with increasing size and resulting in a physically motivated first theory of finite size scaling, leading also to insights on the usual scaling laws. Later, came an approximate but surprisingly good picture of "interdimensional" scaling. In addition to studying the renormalization group method and applying it to several problems of critical phenomena, my interest in small superconductors began, much with Bergman, Gunther, Deutscher and Alexander. The model of Josephson-coupled granular system was first introduced then, later to be followed up with Douglas Scalapino and Leon Gunther and, still later, with Myron Strongin. The last paper by Strongin and myself on this subject, which explains interesting universalities observed in high Tc superconductors, is in preparation now.

During a sabbatical at the University of California, interesting results on fluctuations in quasi 1D systems were produced with Douglas Scalapino, along with more work on granular superconductors and, with Phil Pincus, on weakly coupled chains (one of the interesting results of which has been the great sensitivity of these systems to disorder, in agreement with experiment). At that time my interest in disorder started, resulting in general work on phase transitions in those systems with Amnon Aharony and Shang-keng Ma and on the random-field model with Ma, including the mechanism for the destruction of the ordered state, the number of whose physical applications is still increasing. Later, the three of us contributed to the treatment of dimensional reduction in this model. In 1976, Aharony and I had the idea of constructing an analogy between the spin glass physics and Anderson localization. The idea was fruitful, but we did not understand the relevant symmetries for this problem. This was taken up by Wegner, who did the scaling theory of localization correctly, which, together with weak localization, led to the celebrated and very useful "gang of 4" scaling theory (Abrahams, Anderson, Licciardello and Ramakrishnan). In 1979 I have followed up on the latter using ideas due to Thouless, and produced simple consequences on the Anderson transition. A collaboration with Zvi Ovadyahu based on his experiments followed, resulting in confirmation of most salient features, and of weak localization effects and novel observations of the Altshuler-Aronov interaction effects with their dimensional crossover. The Anderson transition should also result in a divergence of the static and $q = 0$ dielectric constant, and in the whole low-frequency and large wavelength dielectric function, with interesting behavior in the critical regime of frequency and wavenumber, due to the scale-dependent diffusion. This very fundamental paper, with Bergman and Gefen was not easy to get published, it finally appeared as a "brief report" in Phys Rev. Interestingly, an adaptation of some of the ideas of that paper to percolating clusters by Gefen, Aharony and Alexander had a much warmer reception by the community. Bergman and I published a few years earlier, results on the divergence of the dielectric function in the classical, percolation transition, but we believe that the quantum case is much deeper.

In parallel with the above, I became interested in the nonlinear aspects of Josephson Physics. Chaos, with Braiman, and solitons, with Ben Jacob, were discovered in those systems and a theory of such effects in suitable junctions and including AC and DC squids was produced. Another unrelated noteworthy development was the proof with Moshe Schwartz that the supersolid phase does **not** exist in perfect lattices (in spite of speculations by several prominent scientists), but a finite density of defects is necessary for it. Our speculation on Bose condensation of vacancies was in line with the former ideas of Andreev and Lifshitz. This problem came to the forefront in recent years, due to novel experiments. It appears that these experiments are fully consistent with our ~30 year old statements, even when unknown to the experimentalists…

Is the transition to the spin-glass phase a real, sharp transition? It occurred to me during a sabbatical at IBM Yorktown heights that since the usual susceptibility has only a cusp at the transition, a better way to look into the problem would be by observing the **nonlinear** susceptibility which should diverge at the transition, if any. Experiments by Alex Malozemoff and Bernard Barbara confirmed the predicted divergence and the full scaling with temperature and magnetic field. Our joint paper was the first such evidence for the spin-glass transition in a real system.

Around that time, the technology of small-scale fabrication was developed enough so that electronic devices on the micron and submicron scale could be produced and observed. Thus, the time was ripe for the application of small-size thinking to these problems, *i.e.* for mesoscopic Physics. The motivation for that increased strongly due to the discovery of the Quantum Hall effect around that time. The relationship with the IBM lab, which was at the forefront in these technologies, and especially the collaboration and deep friendship with the late and esteemed friend and colleague Rolf Landauer, and with Markus Büttiker, were extremely important. The first contribution was the full understanding, with them, how to show that the aforementioned finite equilibrium currents in a small resistive ring do not decay (hence termed "persistent currents"). However, it was even more interesting and relevant to consider transport properties. Landauer had created, years before, an innovative interesting presentation of the conductance of a 1D system in terms of the transmission through it. But, it was thought that its applicability was confined to 1D. It was obvious to me that the Landauer picture gave an appropriate tool to study mesoscopic transport phenomena and I pushed that idea on several fronts. An interesting first question was what would be the resistance of a normal ring as a function of an AB flux through it. The original Landauer formula can still be used if the ring is connected to the measurement system with 1D leads. The calculation was done with Gefen and Azbel, resulting in periodic oscillations of the ring's resistance as a function of the flux with the fundamental period ***h/e*** (the single-electron flux quantum). Not too many people believed in this, until the experimental proof, by Webb, Washburn, Umbach and Laibovitz at IBM and by Chadrasekhar, Rooks, Wind and Prober at Yale came about. Two years before our work, a related problem was considered in a beautiful paper by Altshuler, Aronov and Spivak (AAS), using perturbation theory in the disorder. This too gave a periodic AB conductance oscillation, but the fundamental period was found to be ***h/2e*** (the two-electron flux quantum)—the first harmonic of the ***h/e*** period! This prediction was very quickly verified experimentally, on ingeniously prepared long ~micron diameter cylinders, by Sharvin and Sharvin. Is the fundamental flux period ***h/e*** or ***h/2e?*** Fortunately, experimentalists continued the quest for the ***h/e*** period. In the beginning, though, there was a persistent aperiodic component that was **reproducible** for the same mesoscopic sample. Fortunately, Richard Webb insisted and proved experimentally that that was a real effect. I was convinced that it was due to the "non-AB" portion of the magnetic field penetrating the real material and sensitive to the specific defect arrangement of each sample (and therefore would vanish by averaging over an ensemble of many mesoscopic samples prepared with similar macroscopic properties but differing by the detailed defect arrangement—which may be termed "the impurity ensemble") and this was confirmed with numerical simulations by Stone. This started the subject of "mesoscopic fluctuations" which turned out to have been a very fundamental mesoscopic phenomenon. In 1984, I heard from Gefen that a way to understand the ***h/e*** oscillation could be to interpret it as a mesoscopic fluctuation, so it will occur in any sample, albeit with a sample-specific phase (*i.e.* paramagnetic or diamagnetic at small flux) and magnitude. Thus, the ***h/e*** component being a mesoscopic fluctuation, should average out for the impurity ensemble, but some of the ***h/2e*** component would survive. All this was confirmed by Stone's simulations in our joint work, and later by Murat, Gefen and myself. The reason that the AAS calculation produced the ***h***/2e fundamental frequency was that the perturbative calculation used the impurity-ensemble averaged propagators (as appropriate for a very long cylinder which may be thought of as comprised of many connected rings!).

In 1985 came the theoretical discovery by Altshuler, and then by Lee and Stone, of the fundamental property of the universal magnitude of the conductance fluctuations (at $\boldsymbol{T=0}$), irrespective of *e.g.* disorder strength (as long as it was not too strong), system's size and material. Using the two-terminal multichannel Landauer formula (see below) and a presentation due to Jean-Louis Pichard, I was able to demonstrate the relationship of the above universality with the random-matrix description of the system's transfer matrix. This started a large chapter of research by numerous people. The semiclassical picture due to Berry was used by Argaman, Smilansky and myself to obtain the spectral correlations for diffusing electrons, and later found to be a useful tool for other properties too. The nontrivial generalization of the Landauer formula to many channels was done with Landauer, Büttiker, my student Pinhas and myself. I then understood a little later the distinction between the two-terminal and four-terminal situations—which even Landauer objected to in the beginning, but did accept later. According to this, I found that even an ideal system should have a finite two-terminal conductance (due to its contact resistances with the outside world, given in fact by the number of conduction channels in units of the quantum conductance unit $\boldsymbol{e^2/h}$. Fortunately, this strange prediction was soon confirmed by experiments in both the Delft and Cambridge groups. The generalization of the above to any number of terminals, done later by Büttiker, enabled the understanding of a number of further mesoscopic effects that were seen experimentally.

This started the golden age of mesoscopic Physics, which also went serious in including sophisticated electron interaction and correlation effects, as well as those coupling the system to a reservoir (see the mention of "decoherence" below).

It would be ideal to combine the theoretical expertise created in the burgeoning field of mesoscopics with the appropriate level experimental research, which necessitates some advanced technology. However, starting with the missing capacitor story of my youth and observing the lamentable situation of experimental condensed matter Physics in Israel, this looked like a very nontrivial task. Negotiations with several institutions took place in 1985, during my sabbatical at Yale (where I taught Solid State theory, helped build their group in this field, and learned from Daniel Prober about the requirements for experimental research in the field). It looked like the only institution in Israel that perhaps had the ability and perhaps the will to do this was the Weizmann Institute, which I joined in 1986 and where I spend a large fraction of my effort for the first few years in convincing my colleagues and the management to go about this. Fortunately, Moty Heiblum was looking for a way to return to Israel, and accepted to come, build and head what became the Braun Submicron Center, which succeeded even more than my expectations, and the rest is history! In parallel, the theory group (comprised in the beginning by Shimon Levit and Yuval Gefen, besides myself) was also strengthened with prominent younger Israelis: Ady Stern and Yuval Oreg and, very recently, Ehud Altman. In addition we had the great benefit of having three outstanding additions from the ex-Soviet Union high-level theorists: Alexander Finkel'stein, and the late Yehoshua Levinson and Arkadi Aronov who also became dear friends. They added a new dimension to our theory research and left their clear marks on the group. Unfortunately, Arkadi was with us only three years and Yehoshua passed away two years ago. In addition to Israel Bar-Joseph who joined the experimental group from the very beginning, and Yacoby who joined for a number of years, Eli Zeldov, whom I convinced to join us, is beautifully active in superconductivity and the younger generations are presented by Danny Shahar and Shahal Ilani.

I was very fortunate to have at that time a few of my best graduate students ever (where they all became good friends and colleagues later). After a first interesting paper on mesoscopic thermoelectric transport by Uri Sivan, work on the magneto-transport in the localized phase was done by him with Ora Entin-Wohlman and Claudio Hartsztein, motivated again to an extent by experimental results due to Zvi Ovadyahu. Uri also did the spectral correlations and ac transport in the localized phase. Upon finishing, he was a postdoc with Moty Heiblum and became a leading experimentalist, and a Professor and founding head of the RBNI Nanoscience Center at the Technion. Ady Stern dealt with the decoherence due to the coupling with the environment. This was thoroughly studied with the help by Yakir Aharonov, and resulted not only in a rather full understanding of the problem including the equivalence of the effects of environment noise **on** the system and the trace left **by** the system, but also in a useful formula for the dephasing rate for weak coupling, which is consistent with the special more complicated earlier important formulation by Altshuler, Aronov and Khmelnitskii. Doron Cohen and I proved from this formulation rather generally that for weak coupling the dephasing rate must vanish in the $T=0$ limit. Since extra dephasing seems to be observed at low, but finite, temperatures in many real conductors, it must be due to an extra mechanism such as a measurement current being not small enough, magnetic impurities or two-level systems (that are ubiquitous in disordered systems). The last one was suggested and worked out with Fukuyama and Schwab. Ovadyahu demonstrated experimentally that reducing the current into the real linear transport regime often eliminates the extra dephasing, although we found that the two-level systems are relevant as well. In the noble metals it was demonstrated by the Saclay-Michigan group that magnetic impurities are the culprit. The pair-breaking due to those was relevant for the explanation of the magnitude of the persistent current mentioned above. Later, Sivan, Aronov and myself showed in agreement with experiment, that in a quantum dot the inelastic electron-electron scattering may cause the spectrum to become effectively continuous above a characteristic energy termed the Thouless energy of the dot. There are some interesting subtleties here, which have been considered by others and are not fully settled yet.

Amir Yacoby did his MSc work with me clarifying the conditions for getting a quantized conductance, obtaining the leading corrections to it and understanding (with Norton Lang), the focusing of electrons emitted by a source). This was a rather high-level theoretical endeavor and with a little more work, it would become an excellent theoretical PhD. However, Amir felt that his real vocation is experiment, started an experimental Phd from scratch with Moty Heiblum and, like Uri Sivan, became a leading experimentalist and a Professor at Weizmann. More recently, he accepted such a position at Harvard. In the late part of his PhD work, Amir tried to measure the transmission phase of a quantum dot embedded in an AB interferometer, by looking at the phase of the AB flux oscillations discussed above. It became immediately clear that this can not be done with a closed interferometer (conserving electrons), where Onsager symmetry locks the phase of the AB oscillations. Therefore, the interferometer has to lose electrons to another lead. These losses must be strong enough, but not too strong in order not to perturb the phase shift. With Aharony, Entin-Wohlman, Levinson, Schiller and, later with Professor Bert Halperin, we worked out the precise details of how and to what extent the interferometer should be opened to effect a good measurement. We also found that the transmission phase can be determined by fitting the detailed measurements to the theory for a closed interferometer (but not via a naïve shift of the AB phase). The AB interferometer is also a nice system for studies of dephasing, and controlled decoherence by "which path" detection was thoroughly studied there. The measurements of the transmission phase by the Heiblum group discovered several unexpected phenomena. A notable one was the "phase jump"— a sharp decrease of the transmission phase by $\boldsymbol{\pi}$ near the middle of the Coulomb blockade valley between consecutive transmission resonances. Many explanations have been advanced to this phenomenon, and it is becoming clear that the crux of the matter is a switching of the occupation of a broad and a narrow level, suggested by Peter Silvestrov and worked out in our joint papers, including the effect of spin. This switching is due to a combination of the interaction and the coupling to the leads. Earlier, it was suggested by Weidenmuüler et al that a similar occupation switching might be more simply due to a peculiar shape of the dot. It seems, though, that the considered dots do not have the necessary shape for this.

It is interesting that when the single electrons are localized by disorder, two electrons can propagate together provided their combined energy is high enough (but still much below the mobility edge), I interpreted it in terms of the Thouless' picture for localization and the increase with energy of the two-electron density of states (DOS). Interestingly, my association with Silvestrov started from his criticism on this work! Later, Aharony, Entin Wohlman and I provided some exact results for a related problem.

In the later 90's I also dealt with two further major topics: mesoscopic superconductivity, with my student Moshe Schechter and with Levinson and quantum noise also with Levinson and with our joint student Uri Gavish. Moshe found novel results in nano-superconductivity, the most surprising one being that the superconducting correlation energy of a nanograin can be relatively much larger than that of the bulk. Together with Jan von Delft of Munich, we identified two pairing energy scales for such systems. These results are crying for a follow up, and I do hope to get to that soon. With Uri and Levinson we understood the Physics of noise correlators for both equilibrium and shot-nose which gives insights on their detectability. Interesting limits on the latter as obtained with an amplifier in the quantum realm were later confirmed and generalized, in collaboration with Bernie Yurke of Bell Labs. An important generalization of the fluctuation-dissipation theorem to non equilibrium steady states (*e.g.* nonlinear differential conductance) was found as well.

Two recent problems I have been active on are the interpretation of the Casimir force as the balance between radiation pressures, and the physical understanding of the quantization of the ac conductance (discovered by Büttiker and confirmed experimentally by Glattli's group) in terms of "delayed currents" – a picture due to Zohar Ringel, a joint MSc student of Entin-Wohlman and myself.

The more recent work, on the mesoscopic persistent currents, slow relaxation in glassy systems, thermoelectricity and molecular transport are reviewed in my homepage.

MAIN REFERENCES

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A Full list of publications can be found in:
https://dl.dropbox.com/u/16058514/Imry%20stuff/pubrec.pdf