Introduction: Tudor Ratiu and Alan Weinstein

Jerrold Eldon Marsden, known to all his friends and colleagues as Jerry, was born in Ocean Falls, British Columbia, on August 17, 1942. He passed away at home in Pasadena, California, on September 21, 2010.

Looking at the long and wide-ranging list (numbering 367 in MathSciNet as of January 13, 2011) of published books and articles by Jerrold E. (or J.E.) Marsden, someone who did not know Jerry could be forgiven for thinking that "Marsden" was the pseudonym of a collaborative group consisting of several pure and applied mathematicians. (This may have already been written about others, such as Halmos or Lang, but it applies to Jerry as much as to anyone.) In fact, as the testimonials below illustrate in many ways, Jerry, our dear friend and colleague, was a distinctive individual as well as a great scientist.

Jerry's first publication, written when he was an undergraduate at the University of Toronto, and appearing in the *Canadian Mathematical Bulletin* in 1965 [22], was a note concerning products of involutions in Desarguesian projective planes. It was reviewed in *Mathematical Reviews* by none other than H.S.M. Coxeter. This was followed by two papers in 1966 with Mary Beattie and Richard Sharpe [4, 5], one on finite projective planes and one on a categorical approach to separation axioms in topology. Already in this earliest work, one can see the interest in symmetry which guided Jerry's view of mathematics and mechanics.

Jerry began his graduate work at Princeton in 1965, where his interests in analysis and mechanics were stimulated by contact with Ralph Abraham, Gustave Choquet, and Arthur Wightman (among others). Another result was the beginning of Jerry's long career in book writing and editing. He assisted Abraham in the writing of *Foundations of Mechanics*, [1], Choquet with the three-volume *Lectures on Analysis* [15], and Wightman with his Princeton *Lectures on Statistical Mechanics* [38]. In 1968, he completed his Ph.D. thesis under Wightman's direction on *Hamiltonian One Parameter Groups and Generalized Hamiltonian Mechanics* and came to Berkeley in 1968 as a Lecturer in Mathematics 1968. He was appointed as an Assistant Professor in 1969 and remained in Berkeley until 1995. From 1988 until he left, he was also a Professor of Electrical Engineering and Computer Science.

Jerry's thesis was published as two back to back articles in the Archive of Rational Mechanics and Analysis [23, 24]. This work, on flows "generated" by nonsmooth Hamiltonians, was motivated in part by problems of Hamiltonian dynamics in infinite dimensions, the subject of much of Jerry's work throughout his life. Rereading this work today, one also sees ideas which have reappeared recently in the theory of continuous Hamiltonian dynamics (see [41]).

The next decade was marked by the choice of many of the subjects to which Jerry would devote the rest of his life. The first was his major work with David Ebin [18, 19], applying infinite-dimensional geometric analysis to prove uniqueness and short-time existence of solutions to the Euler equations of motion for ideal, homogeneous, incompressible fluids. This paper remains to this day the model of global analysis techniques in the study of fluid motion; its methods have been extended to incompressible inhomogeneous Euler equations, to the averaged Euler equations, and to the Camassa-Holm equation in arbitrary dimensions. At around the same time as the work on fluids, Jerry started a ten year long collaboration with Arthur Fischer (and later others) on the Hamiltonian structure of Einstein's evolution equations. For this work he received two prizes in the "Relativity Essay Contest" (with Fischer) in 1973 (1st prize) and in 1976 (2nd prize). The second, significantly enlarged to the extent of being encyclopedic, edition of *Foundations of Mechanics* book appeared in 1978 [2] ; it set the tone for what later became a major field of research: geometric mechanics. The book *Hopf Bifurcation and Its Applications* [30] started Jerry's life-long work in dynamical systems and bifurcation theory, to which he later added considerations of symmetry. Towards the end of this decade, Jerry also became interested in elasticity, a subject on which he would work in the years to come in collaboration with Tom Hughes, Juan Simo, Michael Ortiz, and other engineers. His 1983 book with Hughes [28], was the first to link deep differential geometric ideas to nonlinear elasticity and is still today the source and inspiration of much work in this area.

In the 1980s, Jerry added many items to his long list of research subjects. Work with Weinstein on the Hamiltonian formulation of the Maxwell-Vlasov equations of plasma dynamics led to a long collaboration with Holm, Ratiu, Weinstein, and others in which methods of Poisson geometry were used to study the stability of motions in all sorts of continuum systems by the so-called "energy-Casimir method" due originally to V.I. Arnol'd. Other topics included classical field theory, geometric phases, and control theory (with Bloch and others).

Towards the middle of the 80's, Jerry became more and more interested in structure preserving algorithms and engineering problems. As his work on control theory began to move to the center of his attention, his research shifted gradually towards engineering mathematics. One result of this change of focus was his move to the Control and Dynamical Systems Program at Caltech in 1995. During the last fifteen years of his life, Jerry added yet more topics to his list of interests, including nonholonomic mechanics, structure preserving algorithms, variational calculus for mechanical systems (both continuous and discrete), mission design for spacecraft, and Dirac structures. At the same time, he worked on foundational problems in mechanics, all related to reduction, from all possible points of view: Hamiltonian, Lagrangian, Dirac. Stabilization problems and control continued to be a major area of research, as well as dynamical systems, geometric mechanics, global analysis, elasticity, and fluid mechanics. Jerry never dropped a subject, he only added to his list of interests. As a result, he saw bridges between very different areas. For, example, he would apply ideas from relativity to elasticity, thereby arriving at remarkable foundational results. Or he would use geometric mechanics ideas in numerical algorithms. Control theory and dynamical systems techniques would find their way in spacecraft mission design, asteroid orbit analysis, or molecular motion. Discrete differential geometry or locomotion problems would be treated using geometric mechanics and Dirac structures.

For his pioneering work, Jerry won several major prizes such as the Norbert Wiener Prize, the John von Neumann Prize, the Humboldt Prize, the Max Planck Research Award, and the Thomas Caughey Award. He was a member of both the Canadian and the London Royal Societies and the American Academy of Arts and Sciences, and he held an honorary degree from the University of Surrey. Although Jerry certainly appreciated this recognition, his life was enriched as much by the personal interaction which he had with so many colleagues and students, to whose careers he made extraordinary contributions. The pieces below show how others' lives were enriched by his.

A list of Jerry Marsden's coauthors may be found at
`http://ams.rice.edu/mathscinet/search/author.html?mrauthid=120260`
and a list of his Ph.D. students at
`http://genealogy.math.ndsu.nodak.edu/id.php?id=28380`.

Anthony M. Bloch

In 1986, when I was a postdoc at the University of Michigan, I was fortunate enough to meet Jerry at a conference in Boulder. Jerry, who was in the audience, came up after my talk to introduce himself. I was surprised and flattered to learn that he had read a couple of my papers, one on infinite-dimensional Hamiltonian systems and another on coupled flexible and rigid body motion. This was the beginning of a wonderful collaboration and friendship which has enriched both my academic and personal life over the past 25 years.

A couple of years later, Jerry invited me to visit him and Phil Holmes at Cornell University. This turned out to be a great experience for me both professionally and personally, and I continued to see much of Jerry after that. We met at Berkeley and Ohio State, and later at Caltech and Michigan, as well as at various conferences around the world. I first met Jerry's wife Barbara at Cornell, and I spent many happy hours in subsequent years with them both. I also met Tudor Ratiu, and this in turn led to many wonderful collaborations with both Jerry and Tudor.

Jerry had a panoramic view of mathematics, and one could talk to him about almost any aspect of mathematics, physics, and mathematical engineering. Indeed, he introduced me to a great cross-section of the mathematical community involved in mechanics and geometry. Jerry had a wonderful way of interacting with people both mathematically and personally.

Jerry's generosity was both personal and mathematical, ranging from hosting me at his house when I visited, to helping my graduate students. One thing I remember in particular in Ithaca was Jerry offering to videotape my then 3-year-old son; we have some wonderful video from that time which also features Jerry, Tudor and Krishna (P.S. Krishnaprasad).

Despite doing so many things, Jerry took great care in everything, and this included meticulous care in writing. In all our collaborations, he held me and anyone else involved to the highest standards. This was true in particular with the book [7] which we wrote on nonholonomic systems. Writing a book involves a lot of things, and Jerry was a marvel at all of them. We are currently doing a second edition, and I will very much miss his input.

Jerry's view of mathematics influenced a great deal of research in geometric control theory. This includes the work of Grizzle and Marcus on symmetry and reduction for nonlinear control systems, and the work of Roger Brockett, Peter Crouch and Arjan van der Schaft on Lagrangian and Hamiltonian control systems (see e.g. [13]). The work of Grizzle and Marcus in turn influenced the work of Jerry and his student Gloria Alvarez Sanchez on Poisson control systems. This work, together with earlier work of Krishnaprasad, inspired our joint work on controlled Lagrangian systems (see e.g. [11]). This work used the geometry of mechanical systems, and specifically the mechanical connection, to construct a controlled system that remained Lagrangian. Its stability could then be analyzed by the so-called energy-Casimir or energy momentum method which Jerry did so much to pioneer. A related project which I very much enjoyed was our work with P.S. Krishnaprasad and Tudor Ratiu on dissipation-induced instabilities (see [10]). We showed there that, if the energy momentum method failed, the smallest amount of dissipation would give instability. This work used a beautiful combination of classical mechanics and modern geometry, which was typical of Jerry.

One of the last projects I worked on with Jerry (together with Arieh Iserles, Tudor Ratiu and Vasile Brînzănescu) was the integrability of a new class of flows on symmetric matrices (see [8]). I remember well the moment when we were able to prove the involution of the integrals. Jerry's thrill at this moment matched mine, and I shall never forget it.

Jerry and I were also working with Dmitry Zenkov and Jared Maruskin on a paper on transpositional relations in mechanics; the three of us are in the process of completing this, sadly, without Jerry's participation.

It is impossible to say how much I will miss Jerry's presence in my life.

Hernan Cendra

Jerrold Eldon Marsden's papers, conferences and books will remain among the most influential applied mathematical (therefore, mathematical) works written in the second half of the 20th century and part of the 21st century. Their combination of mathematics with models of reality from an astonishing variety of fields is unique. Just one example of Jerry's fundamental works (in collaboration with Alan Weinstein) is Marsden-Weinstein reduction theory. Historically rooted in mechanics and inspired by pioneering works of Smale and of Arnold, it is both a powerful tool and a source of inspiration to discover the role of symmetry in an increasing number of fields, driven by new questions arising in models of reality or of a pure mathematical nature. The latter include highly nontrivial and very interesting generalizations of the theory, like its extension to singular cases and the development of the Ortega-Ratiu optimal momentum mapping.

Many people working in engineering, physics, celestial mechanics, robotics, numerical analysis and other fields, have realized that reading some of Marsden's papers or books opens a new window for them. The unmistakable Marsden style can be easily appreciated in those works, every one of them connected to some important idea, motivating, not artificially complicated, and making evident the relevance of some fundamental mathematics coming from differential geometry, poisson geometry, symplectic geometry, riemannian geometry, or Lie groups. I know an increasing number of mathematicians, especially geometers, who have discovered that some questions and ideas from their own research are deeply rooted in such long-standing sciences as mechanics or electromagnetism, a connection made crystal clear in the geometrically inspired work of Jerry and some of his students.

One of Jerry's well known books, *Foundations of Mechanics*, in collaboration with Ralph Abraham, remains one of the most important books on mechanics written in the 20th century. Jerry's undergraduate books are also written in Marsden's style and have played an important role in the teaching of calculus to many generations of students in several parts of the world.

It is impossible to imagine Jerry's way of doing mathematics, with the participation of so many students and collaborators from all around the world and coming from such a diversity of fields and cultures, without thinking about the qualities which made this possible: his ability to create lively collaboration among people, human sensibility, understanding, clarity, and wide and deep mathematical knowledge. He was always kind to everybody and had time for coffee and a talk about different topics.

In the Fall of 1984, I was in Berkeley as a visiting scholar, working on integration in finite terms. In a course taught by Jerry, I asked him about the inverse problem in the calculus of variations. He mentioned a well known book about this topic, and then he suggested that I take a look at a paper by Seliger and Whitham on fluid mechanics, as well as at his paper with Weinstein on coadjoint orbits and Clebsch variables. I hardly knew at that time what a coadjoint orbit was, but I somehow perceived that that book and those articles were of an entirely different nature!. This is how I started a long collaboration with Jerry, which was a great opportunity for me. Once a year for 15 years, through 2010, I visited Caltech, generously invited by Jerry. Tudor was also there, and we worked for a month. Those were great days. While we were writing our long article on reduction by stages [14], I imagine that we all had in mind some kind of solvability of the equations of mechanics, in the spirit of differential Galois theory, but we never really talked about it. As I start studying this kind of question now, the memory of Jerry saying: "I won't do that unless I have a good example!" comes to my mind.

I also remember with pleasure the many occasions when he invited me and Tudor and colleagues for a party at his home in Pasadena, with his kind and joyful wife, Barbara.

With the highest respect and appreciation, I would like to say that it seems to me that, for the benefit of future generations, the momentum of his mathematical life and creations is conserved, that is, alive.

Alexandre Chorin

In the late '70's I had the pleasure of writing a book with Jerry, *A Mathematical Introduction to Fluid Mechanics* [16], which became quite successful. The book is different in style and approach from the way either one of us usually wrote: it is a genuine compromise between different styles and points of view, and it is the effort to bridge the differences that made the book accessible to many readers. I greatly admired Jerry's thoroughness and hard work, the depth of his insights, and his willingness to see other points of view and to explain his own as many times as needed. The book was meant to be quite introductory, but to my surprise it is also widely cited by researchers.

Our other collaboration was a paper on product formulas (also with Tom Hughes and Marge McCracken), which included a discussion of a formula Jerry originally named after me but that I had not thought of, certainly not in the general form Jerry put it into. This is also quite widely cited, and I have felt that Jerry was very generous in the assignment of credit.

In the course of these collaborations, I came to admire Jerry not only as an exceptional mathematician but also as a person. He was consistently open-minded, generous, patient and fair. I was very sorry when he left Berkeley. It was a great loss for us, for the obvious academic reasons, but also because his departure left our department poorer in human terms.

Michael Dellnitz

Being in Toronto while writing these words makes it particularly hard for me. Here, as well as in nearby Waterloo, I have spent many days together with Jerry when he was director of the Fields Institute. I also remember the Workshop on Geometry, Dynamics and Mechanics in honor of his 60th birthday at the Fields Institute as if it were yesterday. It is terrible that we cannot celebrate Jerry's 70th birthday in his presence.

The first time I met Jerry was during his visit to Hamburg in 1989 as a Fellow of the Alexander von Humboldt Foundation. I still remember being very excited that I had been given the opportunity to meet this outstanding mathematician. All the more, I appreciated his patience and the attention Jerry paid by listening to the report on my PhD thesis. In fact, based on what he heard, this was the first time for me that Jerry suggested investigating an important mathematical problem: the classification of Hamiltonian Hopf bifurcations in the presence of symmetries. A couple of years later, this work (performed together with Jerry and Ian Melbourne) eventually led to the equivariant version of the celebrated Darboux Theorem. It took me a while to understand that Jerry was aware of this potential right from the beginning.

Those of us who have experienced Jerry in discussions know how his brilliant ideas and suggestions came about. Typically they started with the statement, “That is (very) interesting,” followed quite rapidly by a suggestion like, “I believe that it is worthwhile to consider”. I don't know any other mathematician who had so many great ideas and such an unerring feeling for important developments in mathematics. Many colleagues, postdocs and PhD students have profited from this expertise. Thus, without Jerry, we would certainly not have made such significant progress in mathematics over the last decades.

Allow me to close with a paragraph of even more personnel comments. Over the last 20 years, for me Jerry has not just been an admirable mathematician; both he and his wonderful wife Barbara also became close friends of mine. I will never forget the exciting rounds of golf that we played in Pasadena, the boat trip to the Channel Islands, or the very enjoyable visits to their house. Jerry, I miss you as an outstanding mathematician but not less as an outstanding friend.

Mathieu Desbrun

As a young researcher in computer science and computational geometry, I was not familiar with Jerry's books: my lack of training in mathematics made them seem unattainable and/or nearly irrelevant to my interests. My first encounter with Jerry irreversibly challenged these preconceived notions. Imagine my surprise when "the" Jerrold E. Marsden, accomplished scientist and prolific author, came to see me after one of my talks with his warmth and caring nature evident in his signature smile. He proceeded to convey to me that my approach bore resemblance to exterior calculus and geometric mechanics. When confronted by my blank stare, he patiently spent the next five minutes recommending books with undecipherable titles, and convincing me that under the "mathematical mumbo-jumbo", I may find key concepts at the core of my research goals. It took me a while, but enthused by his encouragements, I soldiered through the books—and found what Jerry promised, and more.

Since then, Jerry has had such an immense influence on both my students and me that it would be difficult to describe without sounding hyperbolic. As a collaborator, he had an uncanny ability to see beyond mathematical minutiae, yet he could extract the mathematical essence of our confusing questions in a matter of seconds. He could explain deep, abstract concepts with a single figure or a simple example: his mastery of science bordered on artistry, and we would relish each of his geometric insights, as well as his detailed explanations of the roots of this principle or that equation. Consequently, a 30-minute meeting with Jerry usually ended with renewed enthusiasm and a sense that his elucidative comments saved us months of desperate pondering. It also had the unexpected effect of making us fearlessly build intellectual bridges across fields: geometry being at the roots of so many topics, Jerry's guidance would often lead us down paths we would not have otherwise dared approaching.

Jerry was a mentor, a role model, a source of inspiration, and a great supporter, all rolled up into one singular character. Having such a colleague was precious, and wonderfully humbling. I think I speak on behalf of many people when I say that Caltech will be forever indebted to Jerry. I know I am.

Marty Golubitsky and Barbara Keyfitz

One benefit of academic life is the chance to meet, interact with, learn from, and become friends with a large number of fascinating and gifted people. Jerry Marsden was extraordinary even in this rather unusual group — he worked with and mentored an uncommon number of people and at all levels. We had the chance to see Jerry in action in a number of ways and we shall always be grateful to have had this opportunity.

Marty's introduction to Jerry was, like that of many, through one of his books (in this case the one published by Ralph Abraham on Hamiltonian mechanics, but based on course notes taken by Jerry). Barbara and Jerry had known each other even longer, as students at the University of Toronto, where Jerry, even as an undergraduate, was a wonderful mentor and study-mate. Our friendship continued over the next decade through repeated contacts at meetings. Jerry was delighted that his two friends – Barbara and Marty – decided to marry each other.

Our first substantive introduction to the Marsden sphere occurred in spring 1982. Jerry arranged for us to spend six months at Berkeley. In his typical way, Jerry not only did the inviting but also found us a house to rent (we were coming with two young children: Elizabeth aged three and Alex aged six months). Our visit to Berkeley was, in addition to the opportunity to get to know and to work with Jerry, a chance over time to meet many in his Berkeley orbit.

What impressed us then and what continued to impress us throughout Jerry's career was the unusual number of people who worked with Jerry and the unusual number of different projects (research, book writing, and eventually editorial) which he managed to carry out. To work like this, one had to be a clear thinker — it also helped to be a clear expositor. Marty once asked Jerry how he was able to get so much done. Jerry's response: if you are having trouble sleeping, get up and work until you are ready to go back to sleep. At one meeting, the organizers decided to have a prize for best lecture based on a participant poll. Jerry, in his low-key and clear way, won hands down.

There were other experiences that have made wonderful memories. Marty spent a summer month in 1988 together with Jerry and Alison, then 12, traveling around China (arranged by Li Kaitai) attending meetings, giving lectures, and sightseeing — from Beijing to Xian to Chengdu, by boat from Chongching to Wuhan through the now nonexistent Three Gorges, and to Shanghai. Jerry chronicled the trip with then new technology and created eight hours of videotape. It was a delightful trip with a few memorable moments. One occurred when we were separated on our trip from Xian to Chengdu; Marty was on an early flight and Jerry and Alison on a flight an hour or so later. Marty arrived in Chengdu and was taken to the university guesthouse — but Jerry and Alison did not appear that evening. The next morning, Jerry related that their plane had some mechanical difficulties after takeoff. Jerry had been approached by the stewardess who asked: "How do I explain to the passengers in English that the door is improperly closed and that we have to return to Xian without getting them upset?"

As is well-known, in the early 1990's Jerry helped create a new mathematics institute in Canada – the Fields Institute – and became its first director. At its inception, the institute was located in Waterloo. Jerry arranged for us both to visit in spring 1993. It was an exciting time; there were many visitors and lots of new contacts. At Jerry's invitation, Barbara was able to bring a postdoc from Houston, and to embark on a new project. Both of us realized during this visit the wonderful potential that institutes have. Jerry's incredible energy, vision, political acumen and reputation deserve much credit for making the Fields Institute happen.

Two other endeavors continue this theme. The first is Jerry's editorial work with Springer on two text series and then on the Journal of Nonlinear Dynamics. The second is Jerry's work with Ivar Ekeland as scientific co-chair of ICIAM 2011, scheduled to take place in Jerry's hometown of Vancouver in July. We both served on the steering committee for this meeting. Despite his illness, Jerry was a part of every phone call of the committee. He did his share of appointing committees and balancing speaker lists. Ivar spoke truly when he said, "Jerry was the heart and soul of the meeting".

We are proud to be part of some of Jerry's legacies – Fields and ICIAM 2011. We will miss him.

Mark Gotay

Calgary, in the fall of 1979, was an exciting venue mathematically. There were symplecticians (J. Śniatycki, W. Tulczyjew, and myself) and several people in related fields (such as dynamical systems and relativity) on the faculty then. But what made that semester *really* special was that Jerry Marsden was in residence on a Killam Fellowship.

Jerry was coming off a *decennium mirabile* of work in mathematical physics; the second edition of *Foundations of Mechanics* surveys the state of the art in 1978. Notable interests of Jerry's included—among many others—the dynamical structure of general relativity, the Dirac theory of constraints, geometric aspects of classical field theory, and momentum maps. It was the right place and the right time for us all, and we spent an intense autumn exploring these topics. This was the beginning, for me, of a life-long collaboration with Jerry on the mathematical aspects of classical field theory.

I sketch the state of the main thrust of our investigations, as of 2010: for a given Lagrangian field theory, we have shown how to construct a *multisymplectic* or "covariant Hamiltonian" formulation of it, which encodes, in a clear and concise way, the interrelations between the dynamics, the initial value constraints, and the gauge ambiguity of such a theory. In a nutshell, this amounts to writing the evolution equations for the dynamic fields ψ and their conjugate momenta ρ in the "adjoint form"

$$\frac{d}{d\lambda}\binom{\psi}{\rho} = \mathbb{J}\cdot\sum_i \left[D\Phi^i(\psi(\lambda),\rho(\lambda))\right]^* \alpha_i(\lambda),$$

where λ is a slicing parameter ("time"), the adjoint is taken relative to an L^2-inner product on the symplectic space of Cauchy data, $\mathbb{J}$ is a calibrated almost complex structure, the components of the "energy-momentum mapping" Φ comprise the totality of first class constraints, and the "atlas fields" α are twisted combinations of generators of the gauge group and nondynamic fields. Other joint interests focused on stress-energy-momentum tensors and general covariance.

Having met Jerry towards the end of my grad student years, I was always impressed by his approach to both mathematics and mentoring. He was an inspiration: he taught us wisdom, enthusiasm and persistence. (The proof that crashed was never a cause for disappointment, rather it provided an opportunity to learn something interesting—a new way forward.) With Jerry, one was welcomed to the "symplectic group" whole-heartedly, and he was always available to listen patiently and criticize gently. He was also well-known to be imperturbable: once while working on my lana'i he calmly completed a calculation even as floodwaters threatened to inundate the house!

It is sad that Jerry is no longer around to chart the course and take the helm, and most of all to enjoy as a friend. I miss him immeasurably.

Jim Isenberg

I had been sitting in Jerry Marsden's office for several weeks before I really got a chance to speak with him. No, it was not that he was too busy to spend any time with me. I had arrived in Berkeley in late May of 1980 to begin my postdoctoral position with Jerry, and he was away for the first half of that summer. When I asked the Berkeley Math Department staff about an office for me, they said that there was nothing available until at least September. A few days later, when I spoke to Jerry and mentioned what had happened, he said that I should just use his office for the rest of the summer. At the time, I was very surprised by his generous offer. As time went on and I got to know Jerry, I became less and less surprised by his wonderful generosity.

A couple of years earlier, I had met Jerry briefly at a meeting in Calgary. (This Calgary meeting was effectively the birthplace of the "GIMMSY" program, which uses a multisymplectic approach to study the dynamics of general relativity and other classical field theories whose gauge transformations include the spacetime diffeomorphisms.) I was a physics graduate student, working on gravitational physics, and I was interested in learning much more of the mathematics of general relativity. At the time, Jerry was among the world's leaders in mathematical relativity, having done beautiful work (with Arthur Fischer and Vincent Moncrief) on the stratified structure of the space of solutions of the Einstein constraint equations, among other important and exciting work on the dynamical nature of Einstein's equations. So when I applied for a Chaim Weizmann postdoctoral fellowship (which, back in those days, could be done with any chosen advisor at one of forty chosen US institutions), I asked Jerry to be my designated advisor. I am very happy that he agreed to do it.

Besides his remarkably broad and deep mathematical knowledge, there are two things that made Jerry a great advisor and collaborator. The first is the completely free, honest, unpretentious, and noncompetitive nature of mathematical discussions with him. We spent countless hours talking about a wide range of mathematical projects, with a very wide range of other colleagues, and during these sessions it was ok (and encouraged!) to ask any question, seek any clarification, and raise any issue relevant to the mathematics under discussion. This may sound like a recipe for wasting a lot of time, but I can attest that it was (and is!) just the opposite. There was no worrying about "dumb questions", and there was always a delightful and productive give-and-take. This feature made Jerry a wonderful collaborator as well as advisor. The other thing that made Jerry a great advisor was his zeal and skill in helping to find good positions for his advisees. Many advisors believe that their job is done once they have written a decent letter of recommendation. For Jerry, that was the beginning of a long and carefully planned campaign involving calls to strategic colleagues, advice on writing grant proposals, and long hours coaching the preparation of a collection of audience-targeted job talks.

My relationship with Jerry continued long after those wonderful postdoc years, and was not free of rough spots. However, both for me and for many others I know, he was a tremendous advisor and collaborator whose wonderful influence will long be felt.

P.S. Krishnaprasad

Meeting Jerry Marsden in the pages of the first edition of *Foundations of Mechanics* (FoM) was a pivotal moment in my life. At that time, I was trying to understand the geometry of rational functions under the guidance of my thesis advisor, Roger Brockett. Taking a Hamiltonian point of view and learning the Arnold theorem on cylindrical decompositions from Abraham-Marsden led me eventually to construct a foliation of the space Rat (p, q) of real rational functions of McMillan degree (p + q) and Cauchy index (p-q). Thus, flows on rational functions were my entry into Hamiltonian mechanics.

Jerry and I met in person at a celebration of the centennial of Case Institute of Technology in 1980. Following his wide-ranging lecture [25] on bifurcations, chaos and control in infinite dimensions, Jerry responded over coffee to my questions on symplectic structures. He then arranged to send me a draft of his CBMS lecture notes. By then I had become a fan of the second edition of FoM. Thus began my appreciation of the enormous range of Jerry's interests and contributions. In the years immediately following our first meeting at Case, I had become interested in nonlinear control theory from a geometric viewpoint. It was inevitable that I should be drawn to the gyroscope as a model problem, "an emissary from the six-dimensional symplectic world to our three-dimensional one," in the words of Manin [21]. I worked out the Poisson structure and stability theory of a rigid body controlled by spinning reaction wheels. Jerry reacted enthusiastically to this work and invited me to Berkeley. This was the beginning of a most stimulating collaboration.

While the flowering of symplectic geometry in the 20th century was in part inspired by the problems of celestial mechanics, already in the work of Routh [42] one recognizes a new class of many-body problems with links to modern day pre-occupations of engineers concerned with the design of mechanisms, spacecraft with articulated rigid and flexible components, robots and such. These newer problems proved to be a fertile field for geometric thinking, including symmetry principles, reduction, conservation laws, stability, bifurcations, and control [29]. I had the special privilege of engaging Jerry's interest in these problems and collaborating with him for well over a decade. For me the high points of collaboration included visits to Berkeley, periods of serene immersion in the beauty of momentum maps, Poisson brackets, Casimir functions and energy methods, meeting Alan Weinstein, Tudor Ratiu and Darryl Holm, participating in workshops and visits to the Mathematical Institutes at Berkeley, Minnesota and Cornell, Fields Institute (then at Waterloo), and Oberwolfach. At Cornell, we spoke at length about geometric phases and associated optimal control problems. The role of connections in this context was a precursor to a full-bore attack on problems of nonholonomic mechanics and newer bundle-like notions of momentum maps adapted to this setting [9]. Perhaps this and related work on dissipation and feedback control system design persuaded skeptical practitioners of the value of geometric methods in mechanics.

As a teacher, collaborator, and friend, Jerry remained close, and offered a personal example through the generosity of his spirit and the obvious delight he took in exploring the natural and the engineered worlds. For me, he lives on in his work and in the perspectives he shared.

Alison Marsden

I have always felt lucky to be the daughter of Jerry Marsden. Not only was he a brilliant mathematician, but he was, as many of his colleagues also know, kind, generous, and a gentle soul.

A wonderful and caring father, he encouraged me in the most gentle of ways, always pointing out things to explore in nature and the world around us. He encouraged my interest in math and science, not by pushing me towards success, but by allowing his own love and curiosity to rub off on me. When I was a young child, he would take me up to his study and make me hand drawn workbooks where we would do multiplication using pieces of toast and other silly pictures. He would take me in the backyard to check our rain gauge, and then I would be assigned to do a coloring project to graph the weather of the day. When we would go on walks, he would tell me things like "Look at this line of ants here. How do you think they know to stay in a line?" In my schoolwork, he encouraged me to take my time, never to rush, and to always make sure I understood things. I came to understand that this was the way he approached his work, and this became an important lesson in life. He took time to contemplate things, to wonder about them, and to think about them deeply. He developed this trait at a very early age. A 1961 article on his high school science fair project notes, "he spent over half an hour every night for five months counting cosmic rays with his homemade Geiger counter," and I imagine him as a teenager outside in the dark wondering about the universe.

I have always admired my dad's ability to maintain a calm outlook on life amid the hectic pace that we all too often find ourselves caught up in. This admiration has never been greater than it is today, now that I find myself as a a mother of two children and an assistant professor of mechanical engineering. While my dad clearly influenced my choice to become an engineer and enter academia, we also had many other fun times together. He taught me how to ski, showed me the best strategy in Monopoly, and told silly bedtime stories that he would make up on the spot. He loved science fiction, contests, and had a keen sense of humor. He was legendary in the family for being absent minded, and once left the car at the grocery store, walked home, and then thought it was stolen.

Because of his world-wide reputation in academia, people have always asked me, "Is it hard being the daughter of Jerry Marsden?" On the contrary, I have always felt lucky to have him as a guide, mentor, role-model, loving father, and a grandfather to my children. He will be greatly missed by family, friends, students, and colleagues as the years go on.

Richard Montgomery

Jerry was my advisor at U.C. Berkeley from 1982 to 1986, towards the end of the heyday of his collaborations with Alan Weinstein on symplectic reduction and its applications. He was an inspired and devoted advisor.

Jerry had a buoyant optimism about each new research project, coupled with a near-mystic belief in overarching theories within which all techniques or disciplines could be placed. His fervor for grand theories was strongly tempered with an insistence that any theory worth its salt must be tested on examples (and later on, applications) from the very beginning. He urged his students to build up an intimate acquaintance with examples from mechanics.

Here are some maxims he passed on. Be generous with credit. Write so that at least you yourself can read and understand what you wrote five years later. Talk to people in other fields. So, if you want to work in mathematical biology, or mathematical physics, talk to practicing biologists, or physicists.

He made strong, daily efforts towards mentoring and towards nurturing research collaborations at all levels, from the undergraduate, through postdoctoral, all the way to people like me, tenured and past mid-career. I learned of the following example after his death, just a week ago as I write, from one of my main collaborators, Misha Zhitomirskii of Technion (Haifa). Misha wrote me: "I never met Prof. Marsden, but exchanged an email message with him. In 1998 I asked him about the possibility to spend my sabbatical year at his university, without much hope of a positive answer. His answer was better than positive: he advised that I contact you. I do not think I would have asked you about this first sabbatical if Prof. Marsden did not advise me to do so: our collaboration before 1998 was not that productive and I was afraid I could not collaborate with you at all. Therefore Prof. Marsden changed my life very much for the better."

Michael Ortiz

I was a graduate student in engineering at UC Berkeley while Jerry was there, and his influence was tremendous. He, perhaps more than anyone else in the department, was responsible for getting engineering students interested in advanced mathematics and then fostering mathematical education among those students. His influence over students was tremendous, and he very much "schooled" them in mathematics, in the sense that the students would always carry with them a certain "Marsdenian" way of thinking throughout their subsequent careers.

As Jerry's colleague later at Caltech, I would also add that he was extremely supportive of junior faculty, and that he worked hard and cared seriously for their advancement.

Tudor S. Ratiu

Jerry's disappearance marks the end of an era. The applied mathematics branch anchored in geometry, which he helped create and was instrumental in its development, has suddenly lost its leader; Jerry died when he was very productive and had many projects going, doing work till the last days of his life. He gave ideas to many people, he influenced other sciences - pure and applied - with his projects and leadership, he provided guidance to scores of young scientists. He leaves a vacuum that will be impossible to fill in a large and very active field of research.

For me, personally, this loss is even harder, because Jerry was also one of my closest friends and someone who truly changed my life. This is the story I would like to tell, since it speaks volumes about Jerry's influence on others. It relates to trust, encouragement, and friendship.

My contact with Jerry predates our personal meeting in 1975 at Berkeley. It took place in the summer of 1972, at the end of my third year as an undergraduate at the University of Timişoara in Romania, during an extremely difficult time in my life, when all seemed lost. I will not go into the details of the horrible political troubles, interrogations, and detentions to which I was subjected at the time by the Communist dictatorship. It suffices to say that, during that summer, I was informed that my studies would be stopped and that I would never be able to have a graduate education.

During the same period, I was struggling with the first edition of *Foundations of Mechanics*, trying to learn, at the same time, the abstract setting and putting things in the context of the very heavy classical theoretical mechanics course I had just taken that year. Of course, I was lost and did not understand several things, so I decided to write to Jerry for clarifications. I did not really expect an answer, for two reasons. First, at that time, any letter leaving Romania, was opened, read, and filed with the secret police as a negative point for the sender. Often, the letter would not even leave the country. Second, I really had my doubts that Jerry would bother answering someone from some obscure place in Eastern Europe. But I was lucky: the letter did go through, and an answer came. In it, Jerry not only clarified the points I raised, but also promised to send me his most recent work which then started to come at regular intervals; every time he had more than one paper, a large envelope would arrive at my house containing his last preprints. So, I am probably one of the few who read the preprint of the famous reduction paper he wrote with Alan Weinstein [37].

Jerry's answer and subsequent letters completely changed my life. In hindsight, it was a major bifurcation point, as he would say, an event that influenced everything that happened to me later. First, I set as my goal to really understand the *Foundations of Mechanics* book. Second, I informed the mathematics department at my university that my BA diploma work would be in the area known today as geometric mechanics. Needless to say, I had a very hard time, since no one knew what I was talking about. Third, I decided to risk all in order to come to Berkeley to study and work in this area. This may seem unrealistic, but coming to this conclusion was not so difficult. After all, I knew already that I would not be allowed to study for a Ph.D. and that I would be sent after graduation to work somewhere totally cut off from mathematics. This actually happened, and I worked for one year punching computer cards. My own (and my family's) political situation was horrible, with frequent interrogations and detentions so that, in some sense, there was no additional risk, short of prolonged jail time. In any case, I could not continue to work as a mathematician, and this pushed me over the brink. But during all this time, I continued to receive short encouraging notes from Jerry, along with various preprints that I was trying to understand. These were among the very few rays of hope during those days. By 1975, it became increasingly clear that a crisis point was coming, and, sure enough, by the end of June I was expelled from Romania. To be fair, I was given a choice: leave the country in 48 hours with no right to ever return, or go to an extermination camp.

I met Jerry personally for the first time in the Fall of 1975. He took me aside and told me that, during my first year, he expected me to finish all requirements of the Berkeley Ph.D. program, with the exception of the thesis. Till then, there was no point in discussing with me any research project, since I would not be able to work on it. I followed his advice to the letter and in June of 1976 showed up again in his office to inform him that all of this was done. The first serious discussion with him left a lasting impression on me. He began to talk to me about various broad areas of mathematics, trying to gauge my personal taste, and, at the same time, presenting to me the major questions in every field in order to arouse my interest. I learned during that half hour more mathematics than I could read in months. Then he told me to study his recent book on the Hopf bifurcation and gave me some of his notes in fluid dynamics. I was supposed to come back periodically and tell him about my progress as well as ask questions on points I did not understand.

During one of the early meetings, I noticed on his desk the *Foundations of Mechanics* book, totally cut up with some pages glued on sheets with various notes scribbled on them. I asked him what this was, since I was so interested in this book; it was, ultimately, the cause of my presence in Berkeley. He informed me that he and Ralph Abraham just started work on the second, vastly enlarged, edition. I told him that I would like to discuss certain parts of the book where I thought the exposition could be improved. His reaction was totally unexpected. He picked up the first stack of papers, handed them to me and said that these were the first sections of the book, that I should now read also this material, and that he expected me to come back with a list of serious comments. I was stunned and protested that I could not take the only existing copy and that I really was not prepared to suggest improvements. He just smiled and said that he trusted me not to lose the manuscript and that he was sure that I would do much more than just reading the text. It is hard to explain what this meant to me, what his trust in my abilities as a mathematician did to my self-confidence. At the next meeting I presented a list of discussion topics concerning the text. We went point by point, in the typical systematic style of Jerry, and at the end he handed me another pile of papers, this time copied, but gave me different instructions: now I was supposed to change the text, add or cut, whatever I liked. Again, this took me by surprise because I never thought to be allowed to actually intervene in the text. But I felt more relaxed since this was, after all, a copy and he could just throw it away if he disagreed with my changes. The contrary happened at the next meeting. Again, we went point by point through the manuscript and he showed me why some of my writing was clear and why other parts were not up to his standards. This marked the beginning of my schooling in mathematics writing, and I could not have had a more talented teacher. At the end of the meeting, he handed me a third stack of papers, again with different instructions. This time we went through the entire text together beforehand, and I noticed that some parts were sketchy, hardly containing some ideas. My job was now to actually write about these subjects and present a proposal. Again, I was taken by surprise and told him that I simply did not know enough to be able to contribute in a significant way. He just looked at me and said that he was sure I was wrong. “Let’s see what you can really do” were his parting words. There could not have been a stronger encouragement possible, and I plunged into reading entire shelves of the Berkeley Mathematics Library in order to be worthy of this trust.

As time progressed, Jerry gave me more and more independence while at the same time, the frequency of the meetings increased. They became the highlight of every week. We would go through what I wrote and later, through the new text he or Ralph produced, and discuss everything; at the end, we would speculate on various open problems in geometric mechanics. The amount of mathematics I learned in these sessions was enormous. But he taught me much more: why does one ask a certain question, why is it important, does it bear on some other field of mathematics or some other science, how does one recognize a good question, what is fashion and what is fundamental? So I learned from him geometric mechanics and all the other mathematical areas connected to it in the best possible way: Jerry gave me the opportunity to learn the subject by doing it. When the book was published in 1978, we celebrated with a tremendous sense of achievement. Soon thereafter, I got my Ph.D., with Jerry as principal and Steve Smale as secondary advisor. I followed Jerry’s advice and specialized in integrable systems. Three years later, we were again meeting almost daily during my summer vacation that I always spent in Berkeley, to understand nonlinear stability in Poisson systems, a project that also involved Darryl Holm and Alan Weinstein. By that time, we were already close friends, we had just finished a book on infinite dimensional manifolds [3], were working on another one on geometric mechanics [31], and were in constant contact that was never interrupted, no matter on what continent each one of us happened to be.

The day I was told that Jerry passed away was one of the saddest of my life, comparable only to those when a close family member died. The sense of loss was tremendous. The entire day, I could only remember scenes of various joint events with Jerry. I suddenly realized that, with the exception of my marriage, no single serious decision in my life was taken without discussing it first with Jerry. He was always present, on the phone, through email, or personally, ready to talk. He always had time for me, something I never understood how he managed to do, because I am in a constant time crisis. He always listened, gave advice and examples from his own life, trying to really understand the problem that was bothering me, be it mathematical research, academic administration, or just personal. I miss these discussions very much.

Thank you Jerry, for the lifelong warm friendship you gave me.

Geneviève Raugel

In 1985, all I knew about Jerrold Marsden was his 1983 paper with M. Golubitsky on the Morse Lemma [27] and his 1976 book *The Hopf Bifurcation and its Applications* [30], co-authored with M. McCracken, in which I had studied center manifolds and Hopf bifurcations in infinite dimensions. At that time, it was one of the rare books dealing with bifurcation theory and the extension of finite-dimensional techniques to the infinite-dimensional setting, which was essential for applications to partial differential equations. Even today, this book contains elegant results that are hard to find elsewhere.

Having worked in numerical analysis and related bifurcation questions, I intended to explore more theoretical problems in bifurcation theory. I therefore asked Jerrold Marsden, without much hope, whether I could visit him for six months in 1986. I was very fortunate that he accepted me as a post-doc.

So, one day in the middle of August 1986, I entered Jerrold Marsden's office. After having assigned me Tudor Ratiu's office, he said: "For your entertainment during your stay, I was thinking about linearization of Hamiltonian systems along given trajectories." Then followed a long explanation on the blackboard, full of terms like Lie-Poisson brackets, symplectic connections, adjoint representations, ... With a trembling voice, I confessed that I did not understand a single word. Marsden did not look disappointed. He kept quite cool, gave me a pile of various lecture notes and papers, and told me with his typical smile, "You have plenty of time (two or three months) to learn all the needed stuff." Leaving his office, I rushed to the nearest bookstore and bought the book, *Foundations of Mechanics*, by Abraham and Marsden.

Six wonderful months followed, during which Jerrold Marsden introduced me into the fascinating "symplectic" world of Marsden-Weinstein symplectic reduction, Clebsch variables, momentum maps, the energy-Casimir stability method, etc. Many years have elapsed, but I still see Jerrold Marsden, with his legendary mug of already cold coffee with milk and sugar, either in his hands or on the edge of his desk, talking about Einstein's equations or stability questions in Hamiltonian mechanics. The geometric interpretation of the Euler equations, in Ebin and Marsden's masterpiece "Groups of diffeomorphisms and the motion of an incompressible fluid," [18] was a real revelation for me.

In the pleasant working atmosphere in Berkeley, Jerrold Marsden gave me the great opportunity to meet mathematicians like Alan Weinstein, Tudor Ratiu, and Darryl Holm. This resulted in a long collaboration and friendship with Jerry (as Jerrold had now become for me) and Tudor.

Gradually, I became aware that Jerry was not only a great mathematician and a living encyclopedia, but also a generous, patient, fair, and easily approachable person. And my admiration grew! Jerry taught me to be generous in giving credit to others. He also advised me to give a chance to *a priori* less gifted students and to accept some of them as graduate students.

Jerry had a passion for understanding, whether it was mathematics, mechanics, weather phenomena, astronomy, physics, etc. I remember Jerry explaining with enthusiasm how he measured the diameter of the moon at different hours of the night, when he was a student, in order to prove that the perceived difference in size was just an optical illusion.

I never met anyone as efficient, fast, and well-organized as Jerry. He answered e-mail messages, and more generally all requests, almost immediately. He could follow a talk, ask deep questions, and, at the same time, carefully proofread one of his manuscripts. Once, when I had complimented him for this ability, he smiled and replied, "Life is so short..." (was it a premonition?) "and we have so much to discover in mathematics". That did not prevent him from devoting his time generously to students, post-docs, co-workers, and colleagues. If you wanted to discuss something with him, he always found a slot in his tight schedule. I remember that, on a three-day meeting with Jerry and Tudor at Cornell, during which we hardly found any time for eating, Jerry took us to visit a former post-doc of his, who had been hospitalized for a few days. After hard work, there was also room for recreation. I look back with emotion to the nice dinners I had with Jerry and his lively wife Barbara at their home in Pasadena and elsewhere. Among other recollections, I vividly remember Jerry showing us the impressive cliffs in the sunset light at La Jolla, during a conference in San Diego in January 1987.

Jerry was a true friend. Even if I had not been in touch with him for months, I knew that I could always rely on him. The last time I met Jerry was in June 2006 at Lausanne. We worked with Tudor on Euler equations for thin spherical shells, and Jerry had a decisive idea. In these too short days, as usual, Jerry gave me friendly advice and enthusiastically talked to me about mathematics – this time about Lagrangian coherent structures. All these happy and bright days are engraved forever in my memory.

Jürgen Scheurle

I would like to express here my fond remembrance of Jerry as an outstanding scholar and a very nice human being. I am one of his many friends and colleagues all over the world who were deeply shocked by his early death.

I first got to know Jerry personally during my visit to Berkeley in 1982. This was the beginning of a close friendship and collaboration. At that time, I had just finished my Habilitation thesis on "Bifurcation of quasiperiodic solutions for reversible systems" [43]. We expanded this work by studying the codimension two bifurcation to tori of B. Langford, G. Iooss and J. Guckenheimer. This led to our first joint paper [45]. The paper includes a computable criterion for the existence of stable tori carrying quasi-periodic flow. This was successfully applied to the Brusselator, a PDE model in reaction diffusion theory, as well as to various fluid flow problems.

Later, we continued to visit each other quite frequently, in Berkeley and at other places in the USA, where I worked and lived for some time, and later in Germany. Our wives Barbara and Karin became good friends, too. The children loved Jerry. He always took them seriously, even at a very young age.

Professionally, I more and more became a member of Jerry's applied mathematics and geometric mechanics school. We continued our joint work by developing a quite general theory concerning the construction and smoothness of invariant manifolds based on the deformation method, with special emphasis on optimal smoothness results [33]. In particular, we gave a new proof of the center manifold theorem which has created a lot of interest. Together with P. Holmes, we subsequently embarked on the ambitious project of studying the exponentially small splitting of separatrices in rapidly forced dynamical systems. For instance, such systems come up in KAM theory as well as in the analysis of degenerate bifurcation problems. The usual transformation methods for showing transversal intersection of separatrices do not work here. Indeed, the failure of the usual theory is precisely the reason the proofs in H. Poincaré's famous 1890 paper on non-integrability of the three body problem are not complete. As a kind of prototype model, we studied the so called rapidly forced pendulum equation containing the same essential difficulty; namely, the splitting of the separatrices is of exponentially small order with respect to the forcing period. We were successful in this project and rigorously derived exponentially small upper as well as lower bounds for the splitting distance, and thus proved the transversal intersection of the separatrices in non-trivial cases [20], [44], [46]. Our method, inspired by classical work of Perron together with the complex embedding of trajectories, used a new expansion in which we showed that each term is of exponentially small order and in which we were able to control the error. In the meanwhile, quite a number of papers have been written about our methods and results, extending them in various ways, but our original technique appears to remain as a fundamental advance.

While I was holding a professor position in Hamburg, Jerry was there on an Alexander von Humboldt Research Prize in the first half of 1991. At that time, he wrote one of his trend setting books, *Lectures in Mechanics* [26]. In the course of writing this book, he gave a series of lectures on the subject in Hamburg. Attending these lectures I got introduced into the area of geometric mechanics. We started to work on our paper on the double spherical pendulum and Lagrangian reduction [34]. The Lagrangian reduction methods we subsequently developed [35], [32] turned out to be particularly important in nonholonomic mechanics and control theory; a number of people, including A. Bloch and P. S. Krishnaprasad, have adapted our ideas to solve some basic problems about nonholonomic constraints. In Hamburg, Jerry and I also started a project on the subject of "pattern evocation" which is a theoretical-numerical method for detecting discrete symmetries of solutions in mechanical systems with symmetries. If one visualizes solutions naively, then one might see "chaos", but in the right rotating frame, very interesting patterns emerge. We showed how one can construct these rotating frames using ideas of connections and geometric phases [36].

Jerry's frequent presence and scholarly activities in Germany were actually very beneficial to many people. Especially, he helped several junior people from Germany to start and to establish a scientific career. He always invited a large number of junior people to the Oberwolfach conferences on Dynamical Systems and Geometric Mechanics which he regularly organized together with changing co-organizers, starting with K. Kirchgässner. In 2000, the very prestigious German Max Planck Research Award was granted to him in order to honor him for his great contributions to the applied mathematics and mechanics community.

It has been a great pleasure to have such a close personal and scientific relation to Jerry for so many years. I am very grateful to him, indeed. All of us will miss Jerry very much, inside and outside of Germany.

Alan Weinstein

Jerry came to Berkeley as a Lecturer in 1968 and was appointed as an Assistant Professor the following year. I joined the Department in 1969, and we quickly found ourselves to have many mutual interests, with our first joint publication [49] appearing in 1970.

Soon thereafter, Jerry and I attended Steve Smale's special topics course on the topology of the n-body problem. In those lectures, Smale introduced the energy-momentum map (see [47, 48]) for the lift to phase space of a group action on configuration space, and he gave a unified geometric explanation of many reduction constructions in classical mechanics, including "fixing the center of mass" and Jacobi's elimination of the nodes. Combining this idea with Lie, Kostant and Souriau's notion of momentum map for general Hamiltonian actions of groups, we arrived at our construction of reduced symplectic manifolds [37]. For each of us, according to Google Scholar, this is by a good margin our most cited paper. (There were 839 Google citations on 1/24/11, the most for me and the most among all of Jerry's papers (but not books). This idea of reduction (also found by Meyer [39] in a slightly different setting) has had immense ramifications in our own work, as well as throughout geometric mechanics (and related areas like representation theory and algebraic geometry).

Through the 1970's, Jerry continued working on general relativity and spaces of metrics, fluid dynamics, and the analysis of general nonlinear evolution equations, and he began a collaboration with Phil Holmes on bifurcation theory. At the same time, my own interests drifted toward microlocal analysis. Then, in the early 1980's Jerry and I once again were inspired by attending a seminar together, this time the "Dynamics Seminar" organized at Lawrence Berkeley National Laboratory by Allan Kaufman, a plasma physicist. The subject was the discovery by Phil Morrison, a student of the Princeton physicist John Greene, of a Hamiltonian formulation of the Maxwell-Vlasov plasma evolution equations in terms of a noncanonical Poisson bracket on a space of charge densities and electromagnetic fields [40] Morrison had found his Poisson bracket by trial and error and had verified by two months of calculation that it satisfied the Jacobi identity.

Jerry and I felt that there should be a geometric explanation of Morrison's bracket. After around six months of work, we found that it was obtained by reduction of the canonical bracket on the cotangent bundle of the product of the group of phase space symplectomorphisms and the space of electromagnetic potentials. This six months of work reduced Morrison's two month verification to five minutes. By itself, that was not a clear improvement, but we did find along the way that Morrison's bracket (which did not quite coincide with ours) did not actually satisfy the Jacobi identity. More important, our geometric approach led to Hamiltonian structures for a wide variety of evolution equations in continuum mechanics, to which we could apply a method developed by Arnold to establish stability of stationary solutions. This led to many years of collaboration with Darryl Holm, Tudor Ratiu, and others. For myself, the work with Jerry on fluids and plasmas stimulated an interest in the geometry of Poisson structures themselves.

By the end of the 1980's, our research interests diverged again, but each of us was now and then inspired by the work of the other. For example, Jerry's work on discrete reduction led me to an interpretation in terms of Lie algebroids and groupoids, which was then picked up by his Caltech student Melvin Leok. More significantly for me, I can say in retrospect that a large fraction of my research in the past 25 years has its origin in the interest in Poisson structures and their applications which arose in our joint work.

Hiroaki Yoshimura

I first met Jerry Marsden at Caltech in July 1997, when I was interested in nonholonomic Lagrangian mechanics in conjunction with *non-energic systems*, a term coined by Birkhoff [6]. As known, electric circuits can be understood as *interconnected systems* through non-energic multiports. Until then, it was known that interconnection could be represented by the *Dirac structures* developed by Courant and Weinstein [17]. However, I could not explain this well to him in the Lagrangian context at that time. Later, I had a chance to spend a sabbatical year from September 2002 at Caltech as a visiting faculty member, sponsored by Jerry. Then, we started to explore Lagrangian systems in the context of Dirac structures, circuits and nonholonomic systems. We discussed enthusiastically two or three days a week and noticed that circuits are a typical degenerate Lagrangian system. Then, we developed a notion of *implicit Lagrangian systems* [50] in the context of Dirac structures. These systems are triples (X, L, D) which satisfy

$$(X, \mathbf{d}E_L|_{TP}) \in D_{\Delta_Q}$$

where D_{Δ_Q} is a Dirac structure on T^*Q induced from a distribution Δ_Q on a manifold Q, L is a Lagrangian on TQ, possibly degenerate, $E_L = \langle p, v\rangle - L(q, v)$ the generalized energy, $X : TQ \oplus T^*Q \to TT^*Q$ is a partial vector field, and P is the image of Δ_Q under the Legendre transformation. It follows that

$$p = \frac{\partial L}{\partial v}, \quad v = \dot{q} \in \Delta_Q(q), \quad \text{and} \quad \dot{p} - \frac{\partial L}{\partial q} \in \Delta_Q^\circ(q).$$

For unconstrained cases, we can derive *implicit Euler-Lagrange equations*, consistent with the Hamilton-Pontryagin principle:

$$\delta \int_a^b L(q, v) + \langle p, \dot{q} - v\rangle \, dt = 0.$$

We had a chance to present our idea at the Alanfest held in Vienna, August 2003, in honor of Alan Weinstein's 60th birthday. Jerry gave a lecture on our idea. In Vienna, I spent ten days with him mostly from morning to night, which was one of the greatest moments in my life.

During my sabbatical, I learned a lot of things in mathematics and mechanics directly from Jerry. After that, we continued our collaboration to establish Dirac reduction theories under the assumption that a Lie group G acts freely and properly on Q; namely, *Lie–Dirac reduction* [51] for the case $Q = G$ as well as *Dirac cotangent bundle reduction* [52] in the case where there is an associated principal bundle $Q \to Q/G$. These Lagrange-Dirac reduction theories succeed in accommodating Lagrangian, Hamiltonian, and a variational view of reduction simultaneously.

Near the end of Jerry's life, we started to develop the general theory of Dirac reduction in the context of *Dirac anchored vector bundles* with Hernan Cendra and Tudor Ratiu. We also began to consider the field theoretic analogue of Dirac structures called *multi-Dirac structures* with Joris Vankerschaver. It was March 2010 that I saw Jerry last at Caltech to discuss the project on interconnection of distinct Dirac structures and associated Lagrange-Dirac systems with his Ph. D. student, Henry Jacobs. Until his final days, we continued to discuss by email, and he never lost his enthusiasm for research. I received the news of his passing the day before I would talk on our new theory at a conference in Greece. Many things remain unfinished, which are left to us to complete.

— Jerry, thank you very much.

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