

Bollettino

della

Società Paleontologica Italiana

Pubblicato sotto gli auspici
del Consiglio Nazionale delle Ricerche

Volume 43, n. 1-2, 2004

Proceeding of the
1st MEETING OF THE ITALIAN OSTRACODOLOGISTS
“IN MEMORY OF GIULIANO RUGGIERI”

Rimini, February 12-14, 2003

Editors:
Elsa Gliozzi
Antonio Russo

MUCCHI - MODENA

<i>Bollettino della Società Paleontologica Italiana</i>	43 (1-2), 2004	ISSN 0375-7633	1-9	–	Modena, Novembre 2004
---	----------------	----------------	-----	---	-----------------------

Giuliano Ruggieri in the 60's and 70's and the Messinian Salinity Crisis: from an American friend's point of view

† Richard H. BENSON
Smithsonian Institution
Washington, D.C.

KEY WORDS – Ruggieri, Messinian, Salinity Crisis.

ABSTRACT – The influence of Giuliano Ruggieri on the ultimate definition of the Miocene/Pliocene boundary and unraveling of the history of the Messinian Salinity Crisis was one of the tenacity with which he argued for the general eradication of the marine faunas throughout the Mediterranean region and their replacement by ostracode faunas typical of paratethyan lakes. Although he recognized a sharp contrast between marine faunas of the late Miocene and those of the earliest Pliocene, it was his aid to others, including the present author, which led to the identification of the sudden establishment of oceanic conditions in the Zanclean, and the continued presence of deep basins. This is a story about that influence... and as well about Giuliano, the scientist of his time as I saw him.

RIASSUNTO – [Giuliano Ruggieri e la crisi di salinità del Messiniano negli anni '60 e '70: il punto di vista di un amico americano] – L'influenza di Giuliano Ruggieri sulla definizione finale del limite Miocene/Pliocene e sulla soluzione dell'enigma della crisi di salinità del Messiniano, fu dovuta alla tenacia con la quale egli sostenne la scomparsa generalizzata delle faune marine ad ostracodi di tutta la regione mediterranea, e la loro successiva sostituzione con ostracodi caratteristici dei laghi della Paratetide. Nonostante riconoscesse un netto contrasto tra le faune marine del Miocene superiore e quelle del Pliocene basale, egli aiutò tutti gli studiosi, incluso il sottoscritto, ad identificare l'improvviso ristabilimento di condizioni oceaniche nello Zancleano, nonché la presenza continua di bacini profondi. Questa è la storia di quell'influenza... e di Giuliano, lo scienziato di quel tempo come io lo conobbi.

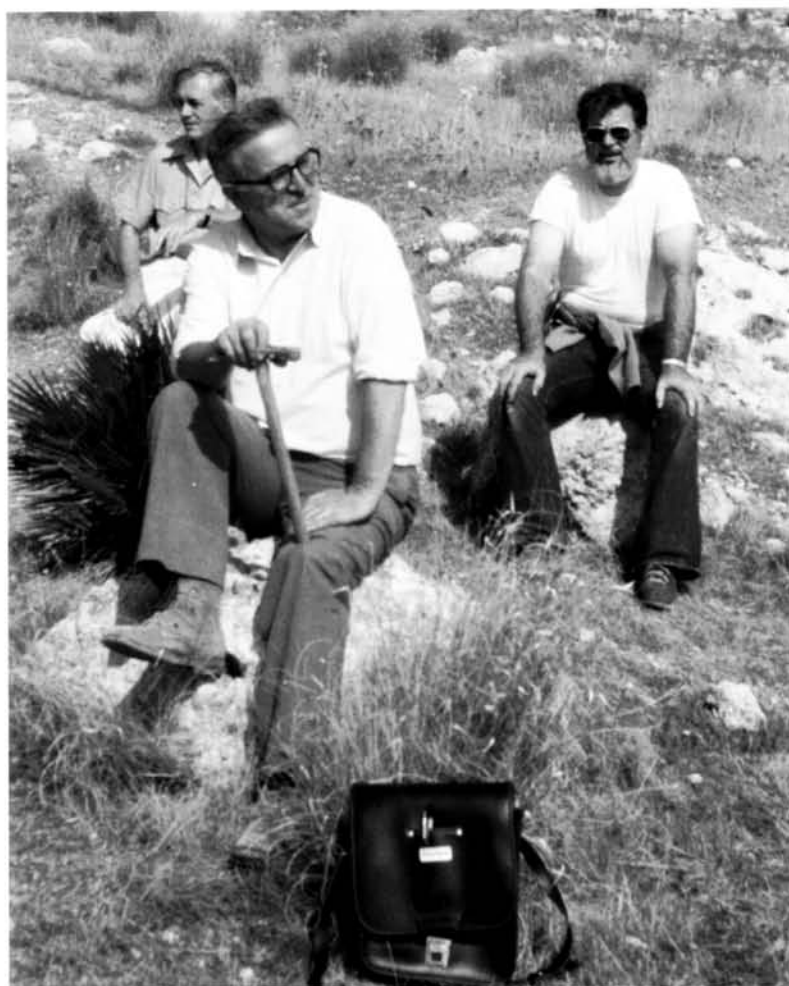
Although Professor Giuliano Ruggieri probably did not invent the term “salinity crisis,” for the massive desiccation event that began in the Mediterranean region at 6.4 Ma and lasted until 5.35 Ma, he championed its stratigraphic significance, and defended its generality as an historical happening. Ken Hsü, in his book *The Mediterranean Sea was a desert* mentions discussing the crisis with Giuliano while the results of the coring in the deep basins by the Deep Sea Drilling Project unfolded. The discovery of the deep-basin origins of the Mediterranean Sea that followed with the Zanclean Deluge would be meaningless without the universality of the crisis that Giuliano advocated. If anyone deserves the title, Giuliano Ruggieri should be remembered as “the *Padrino* of the Messinian Salinity Crisis.”

I came to know Giuliano Ruggieri as an “outsider,” an American somewhat overwhelmed by all that the Mediterranean means as both the “main street” of the origins of western civilizations, and a rapidly developing geologic and paleoceanographic phenomenon. Giuliano welcomed me. He was patient with my learning. We traveled many miles in the field together in Sicily, in Italy, and in Egypt. Fortunately for me, he liked older American movies. In between discussions of geology and ostracodes, we discussed Clark Gable or Irene Dunn... film stars of long ago. I don't think that Giuliano (Text-fig. 1) would have been offended, if I said that he reminded me of Marlon Brando in the movie “The Godfather.” Giuliano had an acute, an ironical sense of humor. We also shared an interest in sports cars, and driving too fast. He seemed to domi-

nate his mini-Fiat. I felt the irony of all of this, when our car broke down that afternoon in Corleone after a fast drive over the route used for the *Targa Florio*.

I first met Giuliano at a symposium held at the Stazione Zoologica di Napoli during the summer of 1963. This pivotal meeting, overseen by Harbans Puri and made a success by Gioacchino Bonaduce, brought together many specialists to discuss the ecology of ostracodes. Its greatest value was that we had time to get to know each other. Giuliano and Peter Sylvester-Bradley, who was my mentor in ostracode studies, became particularly good friends. Actually, I do not know when their friendship began, but I was a beneficiary. Both were professors, outgoing, and shared experiences of being involved in intelligence activities during the war. Peter was ten years older than Giuliano, who was ten years older than I. In 1969, the three of us were to go to Tunisia together, but something befell Giuliano, and mysteriously, he didn't show up. There was student unrest in Palermo, like many other universities. We were very concerned. Finally, we made contact and then toured Sicily and Italy together. Later in 1971, we all went to Egypt together. For over fifteen years we corresponded frequently, and I returned to Palermo and Giuliano's home in Rimini numerous times.

It was through this relationship, and the hope that we could form an institute for ostracode studies in Dubrovnik, or in Tunis under the aegis of the International Paleontological Union, that gave us focus. The institute was to bring eastern and western specialists together, following the example of the Naples



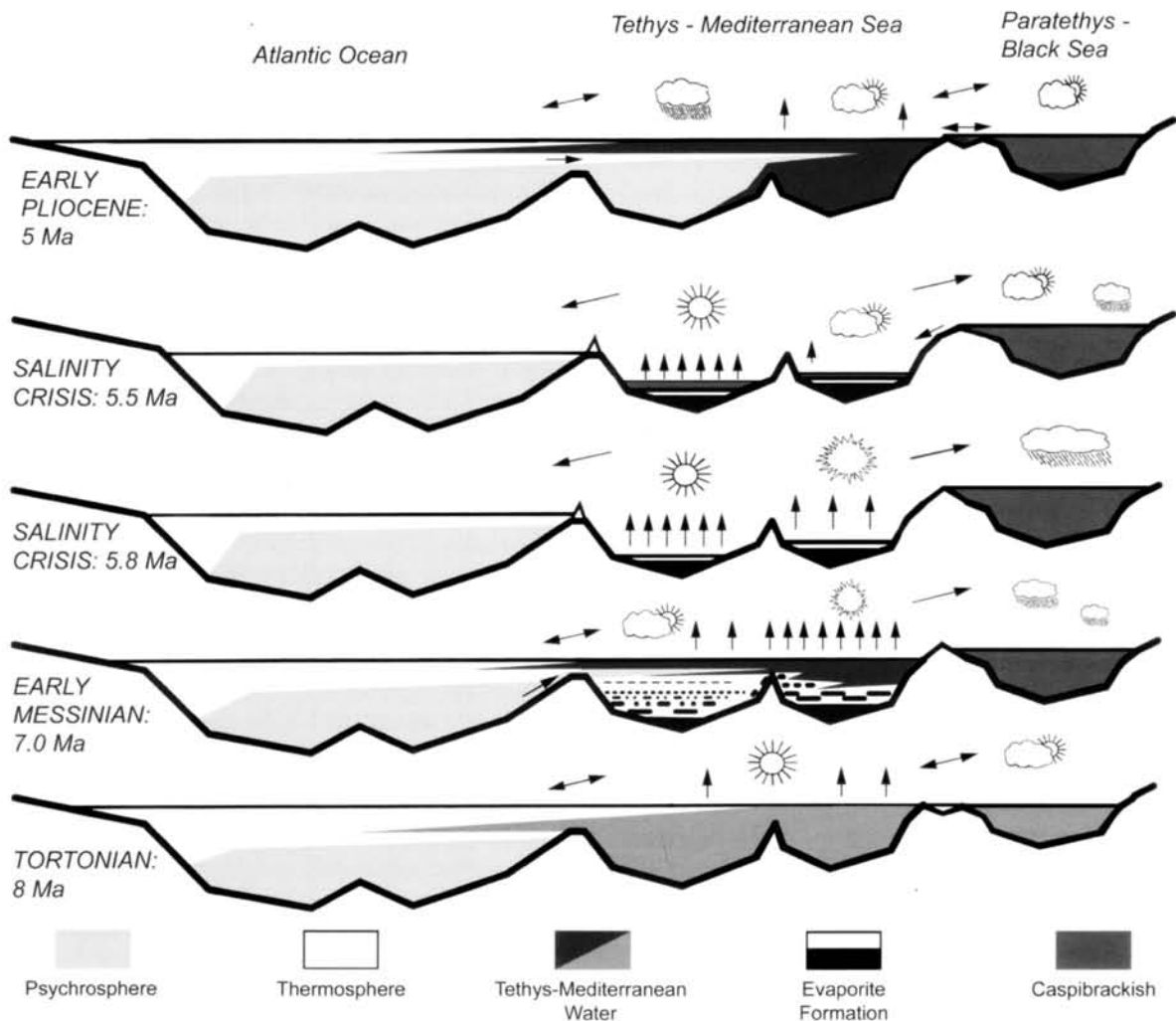
Text-fig. 1 - Photograph of Giuliano Ruggieri (front center) with C.G. "Geoff" Adams (left) and myself (right), taken in the field in 1976 at the Messinian reference section at Falconara along the southern coast of Sicily.

meeting. These ambitions were aided by the visit in Washington in 1967 by Vladimir Pokorný. It was apparent that there was a European nucleus of intellectual energy in ostracodology, if we could just get over the barriers formed by the cold war. America, in cooperation with some selected countries, had "corn money" from the Marshall Plan to pay for the costs. It was a noble, but politically naïve effort... but we kept trying. Thanks to Henri Oertli, I was able to work in France for more than a year. During this time I took my wife, my cat and my VW bus to Rimini to see Giuliano and Titti. He never forgot the cat.

It was well known that Giuliano's heart stayed in northern Italy, geologically near the sections of Forlì. His letters to me frequently began with concerns about life in Palermo. But what a loss to stratigraphy and paleontology had he not gone there. Our knowledge of the Messinian benefited from his scientific rivalry with the "Sellians," as he called the Bologna stratigraphic school, led by his former classmate Raimondo Selli. Also important were his friendships with Enrico Perconig from Spain, and Geoff Adams of the British Museum of Natural History (see

photograph), who supported him in his belief in the importance of the "salinity crisis" as a global event (Text-fig. 2).

Selli, whose representation of Italian reference sections carried considerable weight at international stratigraphic conferences, was treating the Miocene/Pliocene change as a normal, perhaps even minor, transgressive event... secondarily punctuated by evaporites. He advocated the "barred basin model," which was erroneously referred to by him as an "oceanic model." Actually it was a shallow-basin/shallow-water model dependant on local tectonic foundering. He also proposed to the ISC the Messinian section at Capodarso-Pasquasia as the first stage stratotype. In short, he did not believe that the Messinian evaporates represented any more than short-lived events in marginal lagoons, distributed "locally and desultorily" in an otherwise continuous sequence. I remember Selli once saying that the Zanclean beds had more of a Miocene aspect than a Pliocene one. It was also a time when the opinions of Gignoux, in his text on "Stratigraphic Geology," represented the only English summary of the stratigraphy of the classic Neogene.



Text-fig. 2 - A series of hypothetical "deep-basin" water-mass, climate and threshold conditions of the Atlantic Ocean, Tethys-Mediterranean Sea and Paratethys-Black Sea basins taken in profile before, during and after the Messinian Salinity Crisis (after Benson, 2000; also see Benson & Rakic-El Bied, 1991).

Gignoux held that the Pliocene was one of the best developed of all epochs, following the French view of the dominance of megacycles in stratigraphy, beginning with the transgression as shown at Tabiano. The "transgression" in northern Italy later turned out to obscure the significance of the Zanclean Deluge recorded in Sicily.

The present state of Neogene stratigraphy is the product of many not so small struggles between dominant and changing points of view. The arguments often revolve around keeping the reference system stable, so that others can get on with the business of the doing the "real geology." Preserving the "Lyellian aspect" of the Miocene or the Pliocene seems to be very attractive for those who feel that they have a special insight into Charles Lyell's attempt to break away from arbitrary classification (the "age of this," followed by the "age of that"). But Lyell's particular historical perspective of the unfolding of an increasingly modern

world should not impede the basic importance of correlating events and allowing stratigraphy to serve the reconstruction of geologic history. Improvements in stratigraphic practice, with the addition of radiometry, isotopic and geomagnetic time scales, have not only radically altered the time frame to provide for crisis events to happen, but it has challenged many of the basic assumptions of the origins and distributions of the faunas used for correlation.

In the 1960's and 70's, stages were defined by the entirety of a composite section, referred to as the "type-section". Then stages were packets of strata with characteristic fossils. Their definition included all of the faunal and lithological properties typical of that time or state of development. Correlation was done on the basis of the similarity of properties of sequences represented by partial sections, hopefully tied to a type-section. Sometimes there was a "paratype-section," such as the section at Cuevas del Almanzaro for the

Messinian in Spain. This taxonomic approach to stratigraphy was a different concept from that of the "golden spike" idea, wherein the positions of "events", as part of a sequence of events, help to mark and correlate its lower boundary, which in turn defines the unit up to the next lower boundary.

Globorotalia margaritae was first thought to characterize the lower Pliocene. Then its first occurrence in the type-section at Capo Rossello, with the beginning of the Pliocene at 4.6 Ma (presently at 5.35 Ma), was suggested to indeed be a marker for the beginning of the Pliocene anywhere it might first appear. This was a measure of faith, not only in the isochronic distribution of planktonic Foraminifera, but also in the global importance of the sections in Italy. This was also the era of the search for the first appearance of the "northern guests," which were then thought to signal the beginning of the "ice age" cooling. While others searched for *Cyprina islandica* or *Hyalinea baltica* at Catanzaro and Le Castello, our attention, that is Giuliano's and mine, were attracted to the simultaneous disappearance upward of *Agrenocythere pliocenica*. To us, this suggested the ending of the "psychrospheric" environment in the Mediterranean as northern Europe cooled. I believe that this was the first notice of the tie between the Mediterranean, or Tethys, as a "heat source" to the Scandinavian "heat sink" (Benson, 2000).

I recall when Giuliano first took me to the Pasquasia quarry to show me the Mio-Pliocene contact at the new stratotype proposed by Selli. It was in an erratic about the size of a desk. He was trying to impress upon me the need to question the historical completeness of the section. Of course, I took a sample of the lowermost Pliocene for the microfauna, as did many that followed. The next time I visited the locality, with many distinguished stratigraphers, there was no contact left. Also the usefulness of the section for correlation purposes, especially its lower part, was being questioned. The definition of the Messinian was then revised to include the first appearance of *Globorotalia conomiozea*; and the search for another and more complete section that would include the evaporates, such as might occur at Falconara, began. The question of the validity of the FAD of *G. margaritae*, as more than just a regional marker for the beginning of the Pliocene, was put on hold with discoveries of its much earlier occurrence, and its variant *G. margaritae primitiva* in the Messinian of Morocco.

To some degree these are altered details one should expect with further, and I should say, major exploration. Perspectives were modified by new accessible technologies and procedures. Consider the impact of the SEM and the hydraulic piston corer. The reasons for exploration changed as it progressed. The "crisis" context of Mediterranean history began to need explanations that put a strain on accepted theory. Lyell's model of gradual change, and the vision of the Huttonian

"cannonade" of the sea's coming in and going out, was never abandoned; but it was no longer sufficient. The extinction of an ocean and the origin of a sea should have created a major and immediate evolutionary change in the faunas, but except for the disappearance of the coral reefs, the expected adaptive radiation to follow didn't happen. During the 60's and 70's, there was considerable pressure put on correlation by expanded drilling programs – both commercial and scientific. The Late Miocene is an important production zone globally, and oil companies wanted to know when their drilling passed it. Giuliano's contribution to all of this was to think in terms of big events... to impose catastrophic oceanographic conditions on those with more conventional thought. Giuliano's trips into the field with me were to impress upon me the magnitude of contrast and change represented in the challenge of explaining Sicilian and Italian geology.

To some degree, this is the story of the education of an "innocent abroad," an American trying to find his geologic roots. During this period, I was chosen to represent stratigraphy (Chair of the Stratigraphic Panel) to the Planning Committee of the Deep Sea Drilling Project, and help choose from among 120 stratigraphers, those to man its Cruise Legs. It is noteworthy that at that time, DSDP drilling was having trouble finding stage and epoch boundaries in deep ocean sediments. The structural geologists were not amused by vague stratigraphy. The practice of trying to apply the stage concept, based on statistical similarity and local stratigraphy, was ultimately replaced by "high-resolution stratigraphy," based on the identification of global events (geomagnetic reversals, shifts in carbon and oxygen isotope values) using admittedly diachronic biostratigraphic markers. Giuliano made me conscious of the limitations of the idols of the past, those giants upon whose shoulders I was supposed to stand. He remained respectful of the work of Seguenza, but we could not find Messinian at Messina. Mayer-Eymar apparently never visited Sicily. We passed close to Mount Etna, where Lyell worked, but never suspected the dramatic oceanographic changes that had transpired deeper under his feet (Lyell did not think much of eustatic change).

I came to Italy from the Kansas and the Hedbergian "stable earth" school of layer-cake, lithofacies stratigraphy. The experience of seeing Italian geology, like wandering through Neapolitan streets, was shocking to the ordered mind. American stratigraphy placed heavy emphasis on the value of tracing formations over long distances, sometimes for hundreds of miles. In the Neogene of the Mediterranean, a world of rapidly changing facies, rarely did "rock running" lead me very far, although Gioacchino Bonaduce and I later chased the "Blue Marl" (Mio-Pliocene) across northern Morocco. There were places one could stand on Cretaceous platform carbonates and look down into the abyssal depths of the present Mediterranean. It

was not hard to see why the ancients might have considered geologists as apprentices in the devils workshop, and a workshop it was for me... with Giuliano to guide me. He introduced me to the idea of olistostromes, and the role of the Tortonian blue marls as their transport medium. Later, this knowledge was basic to the understanding of the history of the closure of the Rifian Corridor in Morocco by "rootless nappes," with Jurassic limestones occurring on top of, or in the middle of, the Miocene sequence.

Contrast is a great teacher, and it was becoming easier for me to accept the possibility of deep-sea ostracodes sequentially following ones typical of great lakes. We collected among the solitary corals in the Serravalian of Ragusano (Cava di Biemi; Ruggieri, 1960b) to find the last deep ostracode faunas of Tethys Ocean. Later, similar but much older forms were found in northern Italy. At the same time we stopped to see to the dam engineering problems caused by the structural instability of the beds overlying the Trubi Marls north of Gela (maybe, it was the "Gelasian"). At this time, I was still taking the conservative path of searching for possible *refugia* of Miocene faunas somewhere in the Mediterranean, those that escaped extinction by the Messinian desiccation. Giuliano, on the other hand, was convinced that the Miocene marine fauna had been totally destroyed. In a way, we were both right. There were no refuges in the Mediterranean. But just outside of the Gibraltar Barrier, there was the stock of the Mediterranean Sea fauna, misidentified at the time as Pliocene in age.

In retrospect, it is easy to see how restricted the views of the traditional American "time-rock" stratigraphers had been, but they were changing fast. But then we were just getting used to the idea that continents or plates could separate or collide. The effect of plate separation was easier to imagine than that of plate collision and subduction. Did the Azores-Gibraltar transcurrent fault really run through Sicily? Does it even run through Gibraltar Strait? Before we started drilling the floor of the ocean, we had very little experience with the kinds of evidence one might expect to find that would demonstrate the presence of a former or forming ocean with abyssal faunas. It was a "big deal" to discover that fossil ostracodes, suddenly becoming blind in a cored sequence, could indicate the collapse and subsidence of a stable borderland.

Older paleogeographic maps, even those concerned with "continental drift," concentrated on the position of former landmasses and "seas," not former oceans. Krummelbein had easily matched Upper Jurassic and Lower Cretaceous lake ostracode faunas across the South Atlantic, while Reymont was struggling to understand what would happen to Cretaceous marine faunas trying to cross. We were just exiting a state of paleogeography reminiscent of the world of Strabo in 25 BCE, a world dominated by inadequate knowledge of the existence of the oceans. We were con-

cerned primarily with the surface or structural pathways of seas as they had covered the land. Were the Mediterranean basins "sphenochasms," or exposed stretch-scars of the undercrust of an expanding earth (Carey, 1976)? Brinkmann, in his *Evolution of Europe* (1960), speaks of the Tyrrhenian Sea as having been a landmass before the Pliocene. Van Bemmelen wanted mountains in the Mediterranean. Theses being defended in France were still describing a hypothetical southern landmass, not believing that Corsica or Sardinia could have been torn away from Provence and Catalonia.

I cannot remember whom it was that Giuliano told me coined the phrase "salinity crisis," even though, as I said earlier, I credit him with championing it. He once told me that a professor from Taranto invented it. Ken Hsü found traces of usage in H. G. Wells — and of course there is Noah, one of the first experimental ecologists concerned with crises. As I indicated above, we were not accustomed to think in crisis terms. In fact, as my discussion throughout suggests, although crises as disruptions to natural systems are common in Mediterranean history, and key to the proper use of "crisis" to describe the desiccation event is understating its broad effects. A crisis is a condition from which recovery is possible, but the Mediterranean Sea did not recreate itself. All marine life in Tethys perished. The life-systems that populated the Mediterranean Sea were "transplants" from the Atlantic. No doubt, "The Messinian Catastrophe" sounds too alarming for sensitive Italians... as it was for the English during the time of Cuvier. I once heard it said that during one of our many UNESCO meetings in Italy, a local dignitary, having graciously welcomed our IGCP Working Group on the "salinity crisis" to his town, said aside to his wife. "First a crisis in sugar, and now there is going to be another with salt?"

The change in paleoceanographic perspective of the region now called Mediterranean Sea, the *Mare Internum*, which I have referred to as the main street of immersing civilizations, came rapidly with the drilling and coring of its bottom sediments by DSDP, thanks to the fertile and creative imaginations of Hsü, Ryan & Cita (1973). But I must say it was also the conflict in the interpretation of the origins of the ostracode faunas in these cores that brought to a head the issue of the depth of the basins and their connection to the Atlantic. Also were the ostracodes from the Paratethyan lakes to the east, which could have been flown in by birds, restricted to the same shallow depth that they are today, as in the Sea of Azov?

The presence of oceanic ostracode faunas indicated that the deep basins had to have low sills or thresholds to allow cold waters to enter. Furthermore, the waters had to make their way through structurally complex Mediterranean basin systems and still remain cold. The thresholds, presumably dominated by the ones in the Betic-Rif tectonic complex, had to be raised and then lowered (intermittently according to Hsü), while those

to the east controlled the amount and nature of a major source of diluting fresh-water. These historical reconstructions had to be backed by faunal evidence and at the same time support the geological evidence, such as the proposition of a bull's-eye pattern in the deep-basin evaporites, the presence of gypsum in DSDP Site 134, and deep canyon erosion, all of which suggested a "drawdown" of sea level in the order of 2000 meters.

While I have been credited several times as having been the one who first identified the shallow "lago mare" fauna in the deep-marine, deep-basin faunal sequence, it was Giuliano who worked hard to show the significance of the Paratethyan *Melanopsis* clays at the top of the Messinian. These are the beds that contain the *lago mare* fauna characterized by the caspi-brackish ostracode *Cyprideis* that I described in the DSDP cores (Sites 129, 134, 372, 375, 376). Giuliano wrote to me complaining that the "Sellians" were claiming that these "continental clays" contained marine faunas. While he proved that they were in fact reworked or actually Pliocene, he wanted me to take on and defend the deep faunas (letter 9/9/74) such as with the blind, deep marine ostracode *Oblitacythereis*. I thought that *Oblitacythereis* would hold the key to the question of continuity of marine conditions through the "salinity crisis." I, and Antonio Russo, showed that it went outside and changed, then came back in.

As my study with Sylvester-Bradley (1971) shows, the deep-sea ostracode faunas containing *Agrenocythere*, occur in the Mediterranean region, at least on the generic level, before and after the salinity crisis. Concerned with more precise identifications, Giuliano sent me lists of shallow marine ostracode species that failed to survive the salinity crisis (including *Rectotrachyleberis* aff. *batei*, *Carinocythereis galilea*, and *Aurila albicans*). The Pliocene fauna, he said, "is completely new." "New" in the Mediterranean, but getting older, as we later found just outside of the Gibraltar Barrier in late marine Messinian Atlantic. In fact the differences although not great are significant. *Agrenocythere pliocenica*, however, might just as well have been called "*Agrenocythere miocenica*," if Seguenza had lived in western Morocco.

Looking back on the history of my participation in all of this, I realize my "deep" involvement in the crisis problem; that is, helping to demonstrate that the waters and basins had been deep, very deep before and after the "drawdown," transpired, because of an accident of scheduling of priorities on the *R/V Anton Bruun* cruises in the Indian Ocean in 1964 – a year after the Naples meeting. Discoveries are often the consequences of uncontrolled circumstances like that. In 1964, I spent five months helping to dredge the bottom of the western Indian Ocean (IIOE cruises 7-9). I went there intending to study the shelf faunas in the belief that I would find a cline of coastal ostracode species connecting the Arabian Sea

neomonoceratinids with the southern ostracode *Sulcostocythere*. However, others on the cruise wanted the *R/V Anton Bruun* to stay well offshore to collect pogonophores, annelid worms, sharks and fish from abyssal depths. Fortunately for the history of Messinian Salinity Crisis, they prevailed. Anton Bruun, was the Danish ichthyologist, who invented the concept of the "two layer ocean" (thermosphere and psychrosphere). He had a friend, Olga Hartmann, on board.

I say that this "discovery" was accidental, because I was forced, out of sheer boredom, to look for ostracodes in the abyssal biogenic oozes dredged up with Olga's worms. There were large and beautiful examples of *Quasibuntonia*, and what at that time looked like *Cythereis diction* (Brady) *pliocenica*, (Seguenza). Also there was *Bythoceratina scaberrima*, that very distinctive ostracode used as the first biologic subject for the earliest Scanning Electron Microscope. These ostracodes came from depths well over 1500 meters. They were abundant, contrary to expectations. I did not know it at the time, but this was to be the same basic fauna Giuliano would later send to me, the one he had illustrated from the white Trubi Marls of Buonfornello and Enna (Ruggieri, 1960a), and the upper Pliocene section mentioned earlier from Calabria (Ruggieri, 1953). It was this deep-sea ostracode fauna, at least on the generic level, that is found in the Serravalian of Ragusa and overlying, by only a few centimeters, the *Lago Mare* caspi-brackish *Cyprideis* assemblage of the Salinity Crisis. Point made! Even if it was a lucky point!

In no way could the earliest Pliocene of the Mediterranean be shallow. In no way could the Serravalian or the Tortonian, as it was known then, be formed in shallow flooded continental basins that would founder to their present depths during the Pliocene or just after the *gessoso-sulfifera* deposition. Tethys Sea had to have been, in fact psychrospheric. Tethys was an Ocean ... and furthermore, the Mediterranean Sea was again oceanic after the desiccation of the last stages of Tethys. [Not to detract from this enthusiasm, but we now know that it was, as shown to us by Dan Bernoulli in the long sections near Lake Como, from the Triassic to the Miocene]. Peter Sylvester-Bradley, who proclaimed that Tethys was a "lost ocean" in 1968, and I published the Tethys Ocean ostracode evidence in 1971. However, we had the conviction that a complete break with the World Ocean during the salinity crisis was not yet proven. It was not until the cores of the Leg 13 and 42A of the Deep Sea Drilling Project were studied (Initial Reports, 1970 and 1978) that this break became probable. As they say, a lot of ink has been spilled over both sides of the issue of the deep-basin versus the shallow-basin models.

It is important to note how little we actually knew about the ostracodes of the deep oceans in the beginning of the 1960's. Although Brady had described the deep-sea ostracodes of the Challenger Expedition in

1880, they were thought to be relatively rare, unfamiliar and complicated. Elolfson in 1941 had suggested their chief characteristic was that they were larger than shallow marine ostracodes, and either smooth or very ornate. The term "ornate" bothered me... even as hyperbole. It was discussions with Giuliano that encouraged me to look for more meaningful architectural properties of the carapace, and to prove mathematically (RFTRA; Siegel & Benson, 1982) that *Costa* and *Oblitacythereis* came back in from the Atlantic when the Mediterranean was formed (Benson, 1976, 1977). He encouraged me to explore the redundancy of structures that strengthened the carapace in ostracodes like *Oblitacythereis*. It was important to explain how their morphology might have reacted to, and be changed by, conditions before and after the salinity crisis. This eventually took me to search for the linkage in Morocco, following in the footsteps of Antonio Russo and Alessandro Bossio, who had also worked with Giuliano.

At Giuliano's urging (we both dabbled in engineering), I entered more deeply into the world of architecture. It was a way to independently confirm systematic relationships. His letters to me always had sketches with key morphological differences (he was more than just a capable illustrator). I even gave a lecture, at the request of Giuliano's daughter Maria Chiara Tricoli, to the School of Architecture of the University of Palermo on the resemblance of the reinforced concrete design principles of P.L. Nervi (Italian developer of reinforced concrete) for the interpretation of microstructures in deep-sea ostracodes. With Giuliano's guidance, I became a kind of specialized tourist in Italy, visiting many of the domes of classical buildings to look for structural analogues (particularly the catenary sections in the domes of the Pantheon and of the Roman baths near Pozzuoli) with the ostracodes. I climbed up in the domes of the Basilica di San Pietro of Michelangelo, to see the cracks, and the Basilica di Santa Maria del Fiore in Florence by Brunelleschi, to see its oak ring element used to counter spreading tension – like the chitin of the carapace duplicature. It became possible to show that the structure of the ostracode carapace could choose alternate pathways to retain strength.

However, it was not architecture, but the wish to announce that the Messinian of the Mediterranean basins represented a complete break in ocean history, that took Giuliano and I to an invitational meeting in Cairo in 1971. Peter, Giuliano and I were to present papers and help celebrate the anniversary of Egyptian Geological Survey. However, the Israeli Army had just crossed the Suez Canal and every thing was being shut down. This war and architecture kept us in Egypt three weeks longer than expected. Our brief paper, written to impress those who were reading about the discoveries of Hsü, Ryan and Cita, was published two years later.

A post-meeting field trip was supposed to take us

to the Faiyum region to collect, but it was cancelled. It seemed that everyone with any reasonable intelligence was leaving, not coming to Egypt. Peter went home, somewhat appalled at the state of conditions in Cairo at the time. We were in a taxi that was suddenly stopped in front of a hotel because of the assassination, as he was coming out, of one of the delegates at a UAR meeting. The Russians were leaving Egypt *en masse*. Rushdi Said, the director of the Geological Survey, made arrangements for Giuliano and I to take a tour of the ancient monuments on the Upper Nile. There were no tourists around, and the hotels were empty. Frantically, we spent three days cramming impressions of ancient Egyptian artifacts at the Museum in Cairo. Many of the ancient statues were being protected from air attack by sand bags. We bought all the guidebooks on Egyptology we could afford and carry. Giuliano, who knew more about digestive problems than I did, introduced me to fried mozzarella cheese. It was obviously impossible to understand enough of this incredible subject in the time allotted, so we settled on trying to perceive something of the geometric problems that concerned the builders of the ancient tombs and temples... something a geologist-engineer might understand.

Perhaps it was the experience of Giuliano's work on measuring slump movement above the dam north of Gela that gave us the sense of the need of taking sites along moving targets. Or maybe it was those nights at Aswan, where the stars were so many and bright. Or maybe it was the Nilometer, the well on Elephant Island that Eratosthenes used to measure the diameter of the earth, whose shaft at one time cast no shadow at summer solstice. Or maybe it was just seeing the problem of getting the sun's light down into the tombs to paint the murals. Whatever! By the time we got to Luxor, we were ready for the story about the gradual displacement of the sun's penetration along the main corridor of the temple. Formerly this was a religious event marked by the ascendance of the brightest star, *Sirius* the Dog Star, used to celebrate the coming of the Nile flooding. But in time, the precession of the equinoxes, caused by the earth's orbital motion, changed the date of alignment of the corridor with the sun and the stars. One can only imagine how the ancient priests used this event, which lit up the "holy of holys" to mystify the masses with their forecasts. Years later this experience became the inspiration for one of us to calculate the harmonics of precession, and other changes caused by variation in earth-orbital motion, to calibrate the changing statistical effects of seasonal winds in Morocco... and ultimately, for the first time, to measure more precisely the duration of the Messinian Salinity Crisis.

I hope it is apparent, through all of this description of my relationship with Giuliano, that there has been the overriding question of rates, timing and duration of the events we were studying. The issue of oceanic depth was not difficult to resolve, not nearly

as difficult as convincing others that it was true. But time and its relevance is another matter. How long did it take for the crisis to take place? What rates of change are recorded in the upset condition that makes a crisis into a catastrophe? Earlier stratigraphers dealt with time in bundles. But they were not actually chronostratigraphers.

I have heard it said that life was not complete for a student of the Neogene until he writes one paper attacking Charles Meyer-Eymar. Why should we divide the Miocene into six parts, instead of three? Why can't we make the stages more equal in duration... in order to make the rates of change in structural deformation more obvious from geologic maps? The same could be equally, but carefully stated about that of the series or epoch classification of Charles Lyell. The stock market ignores months, will chronostratigraphers and astrostratigraphers ultimately abandon stages and epochs?

I once argued that the lower boundary of the Pliocene should be lowered and moved out of the Mediterranean, in order to facilitate correlation of the global effects of the salinity crisis; but cooler heads prevailed... those who argued that this would put the salinity crisis in the Pliocene. They said, in effect, that so long as the estimation of the ages of the lower boundaries are accurate, continue to honor the names and divisions or the historical system given to us by Meyer-Eymar and Lyell, especially by Lyell. Think about our calendar, and why the stock markets abandoned it!

Would have Giuliano agreed? He enjoyed disagreeing. Both he and Peter were provocative... dialectic in their approach to science. They felt comfortable with contrast. And like me, sometimes they were purposely argumentative, and accustomed to being ignored... well, almost accustomed.

To continue... there is the problem of the selection of a time reference that marks only the beginning of a history, and excludes the evidence of that history's process of becoming. It has the effect of blinding the historian to the causes of that history. For this reason, the ICS recommends that the bases of reference sections of chronostratigraphic units should be in continuous sections. Now the GSSP ("golden spike") of the Messinian has been established in the continuous Oued Akrech section in Morocco [described in considerable detail by myself and Kruna Rakic-El Bied], although frankly speaking, it does not have very much of a historical record below the golden spike. The circular reasoning in correlation that began with the unconformity of base of the Pliocene at Tabiano (Lyell's choice) was extended to the base of the Zanclean at Capo Rossello. The correlation of the base of the Pliocene now at Eraclea Minoa, with the Paratethyan reference sequence of the type-sections of the Pontian and Kimmerian on the Kerch Peninsula in Crimea is difficult (*fide* V. Semenenko), because there are no externally caused events shared by both areas. I would

hope that Giuliano would think that insistence on usage of the classic stages very far beyond the Mediterranean region is nonsense.

Writing about Giuliano is inherently interwoven with the processes, the perspectives, and the context, as well as the events of discovery. The catastrophic ending of the Tethys Ocean merges with the sudden origin of the Mediterranean... and unraveling this history continues. It is a big story, and many people played important roles in its unraveling. If one stands on the temple steps at Agrigento, where Empedocles once stood, and one looks back on the ideas of the world of Homer, Herodotus, Strabo, and Ptolemy, he, or she, cannot help but be impressed at the magnitude of change in ideas of the historiography that this vista toward Eraclea Minoa represents. In conclusion, I wonder if there will ever be as exciting and challenging a time in the future as that which Giuliano shared with all of the creative forces surrounding the "salinity crisis." Behind all of the studies of the origins of the Mediterranean Sea, its waters and its basins, the rapidly changing geography between two continents, not to mention its sheer beauty, is the extraordinary fact of its beginning as a world borne of crisis, a world that became the cradle of civilization. All further explanation may rest as footnotes to the basic idea that nothing ever begins or ends... it is just transformed (Empedocles).

SELECTED BIBLIOGRAPHY

The following is but an abbreviated list of publications alluded to in this article. For a more complete list see Benson & Rakic-El Bied, 1991 cited below (*).

- ADAMS, C.G., BENSON, R.H., KIDD, R.B., RYAN, W.B.F. & WRIGHT, R.C., 1977, The Messinian Salinity Crisis and Evidence of Late Miocene Eustatic Changes in the World Ocean: *Nature*, 269: 383-386, 2 figs.
- BENSON, R.H., 1972a, Ostracodes as Indicators of Threshold Depth in the Mediterranean during the Pliocene. In Stanley, D.J. (ed.), *The Mediterranean Sea*. Stroudsburg, Penn.: Dowden, Hutchinson & Ross, Inc.: 63-73.
- , 1972b, The *Bradleya* Problem with Descriptions of Two New Psychrospheric Ostracode Genera, *Agrenocythere* and *Poseidonamicus* (Ostracoda: Crust.): *Smithsonian Contribut. Paleobiol.*, 12: 1-138.
- , 1973, Psychrospheric and Continental Ostracoda from Ancient Sediments in the Floor of the Mediterranean. In Ryan, W.B.F. et al. (eds), *Init. Repts. Deep Sea Drill. Project*, 13. Wash., D.C.: U.S. Govern. Print. Office: 1002-1008.
- , 1973, An Ostracodal View of the Messinian Salinity Crisis. In Drooger, C.W. (ed.), *Messinian Events in the Mediterranean*, Amsterdam: North-Holland Publ. Co.: 235-242.
- , 1975, Ostracodes and Neogene History. In Saito, T. & Burckle, L.H. (eds), *Late Neogene Epoch Boundaries*, *Micropaleontol. Spec. Publ.*, 1: 41-48.
- , 1976a, Changes in the Ostracodes of the Mediterranean with the Messinian Salinity Crisis: *Palaeogeogr., Palaeoclimatol., Palaeoecol.*, 20 (1/2): 147-170.
- , 1976b, The Evolution of the Ostracode *Costa* Analyzed by "Theta-Rho Difference." In Hartmann, G. (ed.), *Evolution*

- of Post-Paleozoic Ostracoda, Abhandl. Verhndl. des Naturwiss. Ver. (Hamburg), 18/19: 127-139.
- , 1977, Evolution of *Oblitacythereis* from *Paleocosta* (Ostracoda: Trachyleberididae) during the Cenozoic in the Mediterranean and Atlantic: Smithsonian Contrib. Paleobiol., 33: 1-47.
- , 1978, Paleocology of the Ostracodes of DSDP Leg 42A. In Hsü, K., Montadert, L. et al., (eds), Init. Repts. Deep Sea Drill. Proj., 42, Wash., D.C.: U.S. Govern. Print. Office: 777-787.
- , 1981, Form, Function and Architecture in Ostracode Shells: Ann. Rev., Earth and Planet. Sci., Stanford, 9: 59-80.
- , 1991, Messinian Salinity Crisis. In Neirenberg, W.A. (ed.), Encyclopedia of Earth System Science, vol. 3, Acad. Press, San Diego: 161-167.
- , 2000, Neogene temperature variation and forcing in Tethys and Paratethys: Proc. Interim Colloq. RCMNS, Patras, Greece, 1998, Geol. Soc. Greece, Sp. Publ., 9: 19-35.
- & HAYEK, L.-A., 1997, "An Application of Spectral Analysis: Tying Iterative Changes in the Earth's Magnetic Field to Rhythms in Earth-Orbital Motion." Stats 18 (1997): 3-8.
- , HODELL, D.A. & RAKIC-EL BIED, K., 1995, Extending the climatic precession curve back into the late Miocene by signature template comparison: Paleocyanography, 10 (1): 5-20.
- (*) — & RAKIC-EL BIED, K., 1991, Biodynamics, Saline Giants and late Miocene catastrophism: Carbonates & Evaporites, 6 (2): 127-168 (See for a more complete bibliography on the salinity crisis).
- , — & BONADUCE, G., 1991, An Important Current Reversal (Influx) In the Rifian Corridor (Morocco) At The Tortonian-Messinian Boundary: The End of Tethys Ocean: Paleocyanography, 6 (1): 164-192.
- & RUGGIERI, G., 1974, The End of the Miocene, a Time of Crisis in Tethys-Mediterranean History: Annals of the Geological Survey of Egypt, 4: 237-250, 3 figs.
- & SYLVESTER-BRADLEY, P.C., 1971, Deep-Sea Ostracodes and the Transformation of Ocean to Sea in the Tethys: Bull. Centre Rech. Pau-SNPA, 5: 63-91, 1 pl., 15 figs.
- BRINKMANN, R., 1960, Geologic Evolution of Europe. (J.E. Sanders, English translation) Ferdinand Enke Publ., Stuttgart, Hafner Publ., New York, 161 pp.
- CAREY, S.W., 1976, The Expanding Earth: 10 of Developments in Geotectonics: Elsevier, Amsterdam, 488 pp.
- GIGNOUX, M., 1950, Stratigraphic Geology. (translation from the 4th French Edition) W.H. Freeman & Co., San Francisco, 682 pp.
- HSÜ, K.J., 1983, The Mediterranean Sea was a desert: In A Voyage of the Glomar Challenger: Princeton Univ. Press, Princeton N.J., 197 pp.
- , RYAN, W.B.F. & CITA, M.B., 1973, Late Miocene desiccation of the Mediterranean Sea: Nature, 242: 240-244.
- RYAN, W.B.F., 1976, Quantitative evaluation of the depth of the western Mediterranean before, during, and after the Late Miocene salinity crisis: Sedimentology, 23: 791-813.
- RUGGIERI, G., 1953, Età e fauna di un terrazzo marino sulla costa ionic della Calabria: Giornale di Geologia, Ann. Mus. Geol. Bologna (ser. 2), 23: 20-168.
- , 1960a, Segnalazione di *Globoquadrina altispira* nei trubi di Buonfornello (Palermo): Rivista Mineraria Siciliana, Anno XI, 61: 3-7.
- , 1960b, Ostracodi Miocenici del Ragusano: Rivista Mineraria Siciliana, Anno XI, 63: 123-129.
- , 1967, The Miocene and later evolution of the Mediterranean Sea: Syst. Assoc. Publ., 7: 283-290.
- & SPROVIERI, R., 1976, Messinian Salinity Crisis and its paleogeographical implications: Palaeogeogr., Palaeoclimatol., Palaeoecol., 20: 13-122.
- SELLI, R., 1964, The Mayer-Eymar Messinian, 1867. Proposal for a neostatotype. In Int. Geol. Congr., 21st, Copenhagen, 1960, Rep. Session, Norde, 28: 311-333.
- , 1973, An outline of the Italian Messinian. In Drooger, C.W. (ed.) Messinian Events in the Mediterranean: North Holland Publ. Co., Amsterdam: 150-175.
- & FABBRI, A., 1871, Tyrrhenian: A Pliocene deep sea: Accad. Nazionale Lincei (ser. 8), 50: 104-116.
- SIEGEL, A. F. & BENSON, R. H., 1982, A robust comparison of biologic shapes: Biometrics, 38: 341-350.
- SYLVESTER-BRADLEY, P. C., 1968, Tethys: the lost ocean: Science Journal, 4: 47-53.

(manuscript received July 24, 2002
accepted January 15, 2003)

† Richard H. BENSON

Smithsonian Institution
Washington, D.C., United States