AS-TEXONO/98-08
January 2, 1999

Experimental Neutrino Research in Taiwan : Directions and Prospects [1]

Henry Tsz-king Wong [2]
Institute of Physics, Academia Sinica, Taiwan

on behalf of
The TEXONO [3] Collaboration

Abstract

A Collaboration comprising scientists from Taiwan, mainland China and the U.S.A has been built up since 1996 to initiate and pursue an experimental program in neutrino physics. A pilot experiment to be performed at the Nuclear Power Plant II in Taiwan is under intense preparation. This article summarizes our activities, emphasizing on the motivations of the pilot experiment, as well as the possible future directions, and the strategies and plans for their realization. It is also an "Expression of Interest" for our Collaboration to pursue the investigations of solar neutrino as our long-term goal.

[1] Available from http://hep1.phys.sinica.edu.tw/~exono/TEXT/TEXONO/texono9808.ps
[2] E-mail Correspondence: htwong@phys.sinica.edu.tw
[3] **T**aiwan **EX**periment **O**n **N**eutrin**O**

Contents

List of Tables

List of Figures

1 Purpose and Scope of this Report

Since its "birth" in mid-1996, the activities of the TEXONO Collaboration have generated much interests (and occasional animated debates!) among concerned colleagues, funding agents and review panels. In the wake of the neutrino oscillation "discovery" announcement (which, if confirmed, will be of historical importance) by the Super-Kamiokande Collaboration [1], the enthusiasm inevitably intensified. Questions like "How competitive are we?", "How our activities can fit into the world arena?", "Why did we change the directions?", "What are the values of the pilot experiment?", and "Why we are doing what we are doing?" are naturally raised. We feel that it is necessary to summarize our two-years of efforts and to discuss the possible future directions in a report.

The purpose of this article, written in a more informal tone, is therefore to address the questions raised, especially those on motivations and directions and plans and strategies. The scientific and technical details which can substantiate the discussions can be found in the following complementary reports:

1. "The Starting Up of a Neutrino Project in Taiwan"(AS-TEXONO/97-03) [2], which documents our first year of strategies and activities, which focussed on a boron-loaded liquid scintillator for reactor neutrinos.

2. "A Feasibility Study of Boron-Loaded Liquid Scintillator for the Detector of Electron Anti-neutrinos"(AS-TEXONO/98-05) [3], which describes our R&D efforts in developing such a scintillator.

3. "A Pilot Experiment with Reactor Neutrinos in Taiwan"(AS-TEXONO/98-04) [4], which describes the motivations of our new approach of using CsI(Tl) crystal scintillators for the pilot experiment, the physics program and the conceptual design of the experiment.

4. "A Pilot Experiment with Reactor Neutrinos in Taiwan : A Technical Status Report" (AS-TEXONO/98-07) [5], a long (120+ pages) article giving a comprehensive descriptions on all aspects of the pilot experiment.

5. "A Research Program on Solid State Photo-detector in Taiwan" (AS-TEXONO/98-09) [6] : an introductory note to initiate some feasibility studies.

These articles are available upon request, and can be downloaded in postscript format from Web-site http://hep1.phys.sinica.edu.tw/∼exono/TEXT/TEXONO/ . The replies to a list of "Frequently Asked Questions" are included in the Appendix.

2 History

2.1 Foundation

In 1996, under the initiation of the Academia Sinica (and where Prof. C.Y. Chang has played an indispensable and irreplaceable role) a Collaboration consisting of groups from Taiwan, Mainland China and the United States was built up to pursue the goal of performing a world-standard particle physics experiment in Taiwan. A proposal [7] of doing a "pilot" experiment with the power reactor was submitted and subsequently approved by the National Science Council. The present collaborator list is shown in Table 1.

The pilot experiment will represent the first high-energy-physics-style experiment done in Taiwan, as well as a first-generation large-scale scientific collaborative efforts among Taiwanese and Mainland Chinese scientists. Tremendous pioneering spirits are involved. The potential implications can be profound.

The principal objectives of the TEXONO Collaboration are to initiate and pursue a program in experimental neutrino physics and astro-particle physics in Taiwan, as well as to build up a qualified experimental team for future projects. A good local research program is complementary to the participation of international projects elsewhere, and is essential to the solid foundation of international collaborative basic research in Taiwan. As the field grows and matures, the community has to acquire the experience of conceiving, formulating, building, and then implementing, coordinating, and executing a big project (that is, the experience of being responsible to "generate an experiment and make it work" in its totality).

2.2 Initial 1996 Proposal

Given the above implications and considerations, the field of choice was reactor neutrino [2]. There are operational world-standard power reactors in Taiwan, providing electron anti-neutrino flux comparable to those used by other major players in the field. The mountainous landscape dotted with mines and tunnels makes the construction of an underground laboratory conceivable. The proximity of the reactor locations and the possible underground sites to the city infrastructures provides an additional advantage (Nuclear Power Plants I, II and IV are all about 20-30 km from Taipei city).

Reactor neutrino experiments have been carried out many years world-wide [8]. In fact, neutrino was first observed by Reines and Cowan in 1956 using reactor neutrinos [9]. The central topics in reactor neutrino physics has been the search for neutrino oscillation. A list of previous and current reactor neutrino oscillation experiments [10] and the future projects [11] is given in Table 2. These experiments all made use of the "Reines" reaction:

$$\bar{\nu}_{\rm e} + {\rm p} \rightarrow {\rm e}^{+} + {\rm n} \quad ,$$

followed by the delayed capture of the neutron, as its detection mechanism. Almost all experiments used liquid scintillator to provide an active proton-rich target. They differed in their selected means of neutron capture: ^{3}He proportional counters for Gösgen, lithium-loaded liquid scintillator for Bugey, and gadolinium-loaded liquid scintillator for Rovno, Savannah River, Chooz and Palo Verde. After almost twenty years of efforts and two generations of experiments, the reactor neutrino spectrum is now measured [12] and consistent with theoretical calculations to better than a few % [13]. The best sensitivity in oscillation parameters achieved to date is from Chooz [10], a 5-ton fiducial mass detector at a distance of 1 km from the cores, which has excluded the $\Delta m^2 > 10^{-3}$ eV^2 region.

The most important and glamourous subject in the field of reactor neutrino physics is undoubtedly a very-long-baseline (> O(100 km) with O(kilo-ton) detector) neutrino oscillation experiment. Our 1996-proposal was oriented along this direction. The idea was to develop a boron-loaded liquid scintillator for detection of the neutrinos, again via the $\bar{\nu}_{\rm e}$-p interaction. The first phase of the efforts would be to build a one-ton detector at 30 m from the reactor core in Nuclear Power Plant II to get to a Δm^2 sensitivities of 10^{-2} eV^2.

2.3 Inadequacies of the 1996 Proposal

However, by the end of 1997, it was realized and recognized that this "line of attack" faces several severe and fundamental problems, discussed as follows.

1. **International Competition and Relevance:**

   In mid-1997, first results from the Chooz experiment [10] (5 ton mass, 1 km baseline, Δm^2 at 10^{-3} eV^2) were announced, while two new projects (KamLAND and Borexino) [11], **both fully funded and scheduled to run by 2001**, are positioned to attack a Δm^2 region down to 10^{-6} eV^2 with O(kilo-ton) *unloaded* liquid scintillator detectors and O(100 km) baseline. The key parameters from the various experiments are summarized in Table 2. In the "Neutrino Oscillation Workshop" in September 1998 (where HTW was the convener of the "Future Reactor Neutrino Experiments" session), key members from both new projects were present and all the assumptions and expectations in their proposals were carefully scrutinized [14]. No major flaws were found. It was concluded that both projects, if properly carried out, should be able to achieve the proposal goals of $\Delta m^2 \sim 10^{-6}$ eV^2 by 2002. It is unrealistic for our new Collaboration to compete in this subject with this time-scale.

2. **Scientific Output of the Pilot Efforts:**

   Therefore, the oscillation results from the pilot experiment as proposed in 1996 can at best be *a factor of ten worse than what is already achieved, and probably be a factor of 10^4 away from the frontier by 2002.* The $\bar{\nu}_e$-p interactions have been well-studied by the liquid scintillator techniques for 20 years, and there are no interesting physics to be pursued along this direction - except the long-baseline O(kilo-ton)+O(100 km) experiment. That is, the scientific potentials for the 96-proposed pilot experiment on its own are weak and limited.

   Detector-wise [3], the idea of boron-loaded liquid scintillator does offer some potential advantages - while having its own potential drawbacks. If a large system can be realized, it can be a good neutron detector. As for $\bar{\nu}_e$ detection, the world-direction is to go towards a homogeneous (big unsegmented tank) design with *unloaded* scintillator. The boron-loaded scintillator offers an interesting and new approach, but not necessarily superior performance. Moreover, large volume (kilo-ton) liquid scintillator detectors have been and are being built for low-background experiments (MACRO, LVD, Borexino, KamLAND). More than 20 years of experience and infrastructures have been acquired by the major players. It is unlikely that mastering the skills in this detector technology in our pilot efforts can make our collaboration "stand-out" in the world arena.

3. **Technical Difficulties in the R&D program:**

   From our R&D efforts [3] in developing a large-volume boron-loaded liquid scintillator with pulse shape discrimination (PSD) capabilities at the (n,α) capture reaction on ^{10}B, we found out that the electron-equivalence light yield for the neutron capture is only about 50 keV. The PSD at this low energy is extremely difficult and achieve-able only at the expense of good light transmission. In addition, there is a 480 keV γ-ray emitted along with the (n,α) capture at a branching ratio of 93%, thereby having high probabilities of overwhelming the α's PSD signatures. Therefore, we conclude that it would be a formidable challenge to technically realize such a scintillator.

Based on these three principal considerations, all revealing major drawbacks with the initial 1996-proposal, we changed our course in Fall 1997 to the crystal scintillator detector, but retained using reactor neutrinos as the source.

It should be noted that although the 1996-proposal turned out not to be the optimal directions to pursue, it should not be viewed as a "failure" or "waste of time" either. The Collaboration, as well as the essential laboratory infrastructures and expertise and "connections", were built up during this crucial first year. The "snowball" has been initiated. The conception and formulation of the crystal scintillator approach discussed below could not have proceeded so smoothly and efficiently without the foundation already laid in the first year.

3 The "Re-oriented" Pilot Experiment

The scientific and experimental details of the "re-oriented" pilot experiment are documented in a long (120+ pages) technical report [5]. We focussed only on addressing the question "WHY" in this section.

3.1 Motivations (Why the choice of crystals?)

The "pilot" experiment is an essential and indispensable first step for future growth in any directions. The choice of "what exactly to do", however, should preferably satisfy several requirements. It should provide a "platform" for building up a qualified Collaboration and a "launch-pad" for the more ambitious projects beyond, whatever exactly they may be. In addition, it should have its stand-alone and independent physics motivations, while being the first efforts towards some long-term visions. It is essential that the experiment itself is affordable in terms of the available resources and expertise, and can be built up in a relatively short time.

Given these considerations, we adopted crystal scintillator technology [15] for our pilot experiments. The reasons for this choice are given below.

1. **Crystal scintillator is a new approach with new physics potentials.**

   From Table 2, it can be seen that almost all previous reactor neutrino experiments were based on liquid scintillator techniques to study $\bar{\nu}_e$-p. An experiment focusing on gamma detection has never been attempted.

However, gamma-ray spectroscopy has been a standard technique in nuclear sciences (that is, in the investigations of physics at the MeV range). Gamma-lines of characteristic energies give unambiguous information on the presence and transitions of whichever isotopes, allowing a unique interpretation of the physical processes.

Since a crystal calorimeter with improved γ-detection capabilities is a new approach to neutrino experiment, it does offer several physics opportunities if such a detector can be realized.

2. **A 1-ton CsI(Tl) crystal scintillator is realistic, affordable and manageable.**

   The experimental difficulties of building a high-quality gamma detector for MeV neutrino physics have been the large target mass required. However, in the past few years, big electro-magnetic calorimeter systems [16] (with mass up to 40 tons of crystals, in the case for the forthcoming B-factories experiments [17]) have been built for high energy physics experiments, using CsI(Tl) crystals with photo-diodes readout.

   As listed in Table 3, The CsI(Tl) crystal offers certain advantages over the other possibilities. It has relatively high light yield and high photon absorption (or short radiation length). It is mechanically stable and easy to machine, and is only weakly hygroscopic. It emission spectra well matches the response of silicon photo-diode, thus making a compact design with minimal passive volume and efficient shielding configuration possible.

   The CsI-crystal production technology is by now well matured and the cost has been reduced enormously due to the large demands. It become realistic and affordable to build a CsI detector in the range of 1-ton in target mass for a reactor neutrino experiment.

3. **CsI(Tl) detector is relatively easier and quicker to build and operate.**

   Our IHEP-Beijing group already has substantial experience with this detector technology, being a candidate choice as the electro-magnetic calorimeter in their τ-charm factory proposal. Good working relationships have already been established with a Beijing-based crystal manufacturer. This enabled us to minimize the initial R&D "trial and error" phase and proceeded directly into formulating the experiment. Much time (at least one year) has been saved because of this choice.

Operation-wise, a crystal scintillator (especially CsI(Tl) which is only very slightly hygroscopic) is much easier to handle and to maintain stability than, for instance, gas chambers and liquid scintillators. The compactness enables efficient shielding design. The entire detector and shielding system therefore do not contain any liquid or gas (other than dry nitrogen), enhancing the operation safety inside a nuclear reactor.

4. **There are much to explore in the use of crystal scintillator in low-background experimentation.**

   Although the detector may be simple, the background processes for our novel applications are not thoroughly understood. It is a new research domain we have to explore and state-of-the-art techniques will be required in the control, identification and suppression of the background.

   In our first year of mastering the technique, we have already come across several nice features which may make the crystal scintillator technique attractive for low-background experimentation [5]. For instance, α-signals are only slightly quenched and have PSD over γ's, giving a unique handle to measure intrinsic radiopurity of the ^{238}U and ^{232}Th series, such that a modest set-up gives comparable sensitivities to big installations for liquid scintillators [18] .

   In addition, our choice of solid-state readout is also a new attempt, and offers potential advantages such as compactness and better radiopurity to the conventional photo-multiplier readout.

   Therefore, there is a possibility that our involvement in this detector technology (crystal scintillator plus solid-state readout) for low-background experiments may lead us to a distinctive and recognizable position in the world arena.

5. **There are much potentials for future growth.**

   The detector mass for the crystal scintillator technique can conceivably be further scaled up to 50 tons, given the existing experience on the successful construction and operation of crystal electro-magnetic calorimeters with similar tonnage.

   As discussed in Section 5.5, the crystal calorimetry techniques may have much potentials in the studies of dark matter and solar neutrinos. Indeed, NaI(Tl) detectors are being used for dark matter experiments, giving some of the best results in that field [19]. Therefore, our efforts in realizing and operating the pilot experiment are coherent with, and indeed necessary for, the longer-term goals along those directions.

Furthermore, Mainland China has been a world-recognized major player in the crystal-production business, while Taiwan has superb infrastructures and expertise for the silicon technologies. Therefore, the direction of "crystal scintillator with solid-state readout" may match well with the industrial expertise accessibly by the Collaboration.

That is, the choice of this detector technology allows us to "get started" almost immediately, while leaving a host of worthwhile R&D topics to be pursued as we proceed, and offering the potentials of evolving into a world-class project in the future. It may be optimal for our Collaboration, given all the constraints.

3.2 Physics Menu

The detailed discussion on the physics subjects to be addressed by the pilot experiment can be found in Ref. [4, 5]. The topics include -

1. $\bar{\nu}_e$-e scattering : for the measurements of electroweak parameters ($\sin^2\theta_W$, g_V and g_A) at the MeV-scale and for a search of neutrino magnetic moments [20].

2. $\bar{\nu}_e$-deuteron interactions in both charged- and neutral-current modes : which can provides important cross-section measurements for the forthcoming SNO [21] solar neutrino experiment whose detection mechanism is by ν_e-deuteron interactions.

3. $\bar{\nu}_e$-nucleon neutral current excitation : the cross-sections of $\bar{\nu}_e$ on ^{11}B and ^{10}B [22] are sensitive to the axial isoscalar component of neutral current interactions and the strange quark content of the nucleon.

These topics are admittedly less ambitious and glamourous than the frontiers in "neutrino oscillations". However, the physics output are nevertheless relevant and "presentable". The pilot experiment does enable our new Collaboration to "get started" with an affordable and manage-able detector in a relatively short time. This is only the starting point from which we can further grow and launch new research directions.

4 Possible Future Directions

While the pilot experiment allows the Collaboration to get started, its physics potentials are only modest under the spotlights of neutrino physics. Our visions and ambitions should extend beyond the present horizons and the pilot experiment - and experience - can be used as a stepping-stone and "launch-pad" in their pursuit.

The following sections will be an "Expression of Interests" for our Collaboration to pursue the studies of solar neutrinos as our long-term goals, with discussions of the rationale behind and the "world-stage" in which we have to fit in to collaborate and to compete.

4.1 What Field?

Naturally, the Collaboration would remain in the field of "Neutrino/Astro-particle Physics" [23] (which can be broadly defined as the studies *of* and *with* neutrinos and dark matter). A few simple observations:

- neutrinos exist;
- evidence for dark matter is very solid;
- we do not fully understand them;
- their exact properties have profound consequences in particle physics and cosmology,

will convince one that neutrino and dark matter physics will continue to play an central and important role in basic research in the years to come. The Super-Kamiokande results [1] only further consolidate these expectations.

Technically, a wide spectrum of techniques have been deployed in neutrino experiments. Some of the world's most daring and shrewd experimentalists are working on this area. There are potentials for surprises. The field is (to HTW's taste) rich in its "intellectual contents".

There are much future ahead in the inter-disciplinary area between particle physics, astrophysics and cosmology. We have every reason to stay on.

4.2 High or Low Energy Neutrinos?

The expected neutrino spectra from various celestial and terrestrial sources is depicted in Figure 1. The experimentally well-studied range are from $\sim$1 MeV to $\sim$50 GeV, comprising mainly reactor and accelerator neutrinos, as well as the high energy tail of solar neutrinos and the low energy end of atmospheric neutrinos. The future directions will be to study the already-studied neutrinos (accelerators and reactors) with longer baseline and to extend the range of detection capabilities both at the high energy (limited by flux) and low energy (limited by background) ends.

The high energy frontiers will be the domain for the "Neutrino Telescope" projects whose goals will be to detect neutrino point sources from astrophysical bodies - with "km^3" water or ice Cerenkov detectors. The low energy frontiers will be the studies of sub-MeV solar neutrinos, geo-neutrinos (from the Earth's crust and mantle), supernovae neutrinos, dark matter searches and ultimately - the holy grail of observing the "cosmological (relic) neutrinos" (the 1.9 K counterpart for the 2.7 K background microwave radiations).

The Collaboration would continue our focus at the low energy frontiers. The experiments in this arena are technically less complex compared to the ultra-high energy frontiers - though equally, if not more, elegant and challenging. Good ideas and insights, rather than sheer technical prowess, can make major impact. (No-one knows even conceptually how to detect relic neutrinos yet!) There may be more room to "leave a mark" here.

Both ν_μ and ν_τ only interact via neutral currents at low (MeV and lower) energies, so that the experiments will emphasize on the detection of ν_e and $\bar{\nu}_e$. As discussed in Section 2.3, the detection scheme of $\bar{\nu}_e$ (with liquid scintillator) for the studies of reactor neutrinos, geo-neutrinos and part of the supernovae neutrinos are well-matured, to the extent that there are fully funded kilo-ton-scale projects (Borexino and KamLAND) [11] scheduled to be operational by 2001. It is unrealistic for us to compete at this time-scale on a subject where the optimal detector-concept is already proven and known. In contrast, the detection scheme of ν_e for solar neutrinos and supernovae neutrinos are still open, and this is the direction we would like to pursue.

5 Solar Neutrinos ($\nu_\odot$) : Physics and Experiments

The solar neutrino spectrum based on the "Standard Solar Model" [24] is shown in Figure 2, whereas the the comparison with existing results are depicted schematically in Figure 3, showing a deficit of observed neutrinos from the Sun in all experiments. The "Solar Neutrino Puzzle" is a solid and important problem in physics [25]. The present wisdom is that it is impossible to account for all data by modifying astrophysical (solar) scenarios alone, and it is necessary to introduce neutrino masses and mixings. The *preferred* regions in the Δm^2-$\sin^2 2\theta$ parameter space is depicted in Figure 4.

Besides neutrino physics, $\nu_\odot$ also provides the unique probe to the internal mechanism of the Sun, complementary to the "helioseismology" techniques. Photons from the Sun reaching the Earth are generated only millions of years after the actual nuclear fusion in the interior and only gives us information of the Sun's surface. The Sun is our closest star - the only Star in the Universe where we can study its interior physics. Therefore, measurement of $\nu_\odot$ will be a very important contribution to solar-astrophysics in its own right. Items like total $\nu_\odot$ flux, relative fluxes between the p-p chain and the CNO cycles, and the width of the ^{7}Be lines, all reveal the physics and the parameters in the Sun's interior.

5.1 Detection Mechanism

Ideas and experiments to detect of solar neutrinos are very diversified, showing much elegance and vigor. The ultimate goal will be to measure (or derive) the *raw* "solar neutrino spectrum" [$\phi(\nu_e)$] as depicted in Figure 2.

In general, solar neutrinos (that is, ν_e) are detected by three different physics mechanism:

1. **Neutrino-Electron Scattering (ES):** Neutrinos can be detected via neutrino-electron scatterings :
$$\nu_x \ + \ e^- \ \rightarrow \ \nu_x \ + \ e^-$$
where $x = e, \mu, \tau$, or $\bar{\nu}$. The principal merit is that anything can be the target and so there is an unlimited choice of detector technology. The cross-section is also larger per unit mass, from the simple fact that $\rho_e \gg \rho_N$. However, there is no flavor-specific information and only the kinetic energy of the recoil electron T_e is measured, such that the evaluation of the neutrino spectrum (where the physics is) with $T_e \rightarrow \phi(\nu_e)$ is ambiguous requiring convolutions and assumptions on the neutrino properties.

2. **Neutrino Charged-Current Interactions (CC) :** The charged-current interaction :
$$\nu_e \ + \ (A, Z) \ \rightarrow \ e^- \ + \ (A, Z+1) \text{ or } (A, Z+1)^*$$
offers a flavor-specific channel giving direct and unambiguous information on $\phi(\nu_e)$, via
$$E(\nu_e) \ = \ E(e^-) \ + \ E(\gamma' s) \ + \ Q \text{ value} .$$

However, the cross-sections depend heavily on the nuclear structure of (A,Z) and for those with good cross-sections, not all of them can be made into a realistic detector. Therefore, choice of detectors are very limited. In addition, except for light nuclei like deuteron, in general $\sigma(\mathrm{CC}) < \sigma(\mathrm{ES})$ so that methods must be devised to "fish out" the CC events from the ES ones to obtain $\phi(\nu_{\mathrm{e}})$.

3. **Neutrino Neutral-Current Interactions (NC) :** The cross-section for the neutral-current interaction :

$$\nu_{\mathrm{e}} \ + \ (\mathrm{A,Z}) \ \rightarrow \ \mathrm{e}^- \ + \ (\mathrm{A,Z})^*$$

is independent of flavor, and hence the CC/NC ratio offers a powerful model-independent test of the solar models. The signatures of the NC interactions are mono-energetic γ-lines (and neutron emissions for deuteron) folded onto the smooth solar neutrino spectrum. It gives information on the integrated neutrino (all types) flux above the excitation energy threshold. Technically, it encounters similar difficulties as the CC-channel : that the cross-sections depends on (A,Z) and the choice of realistic detector is limited.

5.2 Current and Near-Future Experiments

A summary of existing experiments [26] together with those under construction is given in Table 4. The radiochemical experiments (Homestake-chlorine, SAGE, GALLEX) only measures the integrated total CC cross-sections and do not have timing or spectral shape information. The only real-time counter experiment operational at present is the Super-Kamiokande experiment, but it is sensitive only to ES and with a high threshold, therefore only giving convoluted shape information on the tail of the solar neutrino spectrum.

In the next future, the SNO and ICARUS experiments will give direct spectral shape information on the high energy part of the ^{8}B solar neutrinos. Model independent tests can be made via the NC/CC and ES/CC ratios for SNO and ICARUS, respectively. Borexino, on the other hand, will attempt to measure ES with a liquid scintillator detector down to a 250 keV threshold (very challenging!), and therefore sensitive to the ^{7}Be line (the 862 keV mono-energetic neutrinos will give a 665 keV "Compton-Edge" in ES).

The superior information-content of the CC electron spectrum over the one from ES is illustrated in Figure 5, which depicts the potential measurements from Super-Kamiokande and SNO, and in comparison with the different oscillation scenarios.

5.3 Requirements for Next-Generation Experiments

Assuming all the next-future experiments will achieve design goals (which is a big *IF* itself), the solar neutrino "stage", on which the goals and design of future experiments should be based, would be like :

- the integrated flux of $\phi(\nu_e)$ above 230 keV will be accurately measured from the gallium radiochemical experiments;
- the solar neutrino spectrum $\phi(\nu_e)$ will be measured above $\sim$5 MeV from the SNO and ICARUS CC channels;
- model-independent NC/CC and ES/CC tests will be performed for $\nu_\odot$ above $\sim$5 MeV;
- counter measurements of $\nu_\odot$ down to $\sim$250 keV from Borexino will be achieved, the ^{7}Be line observed and seasonal variation studied.

Therefore, the sketch of the neutrino-oscillation physics may emerge, and many details and improved measurements are required to fill in the gaps. However, the solar-astrophysics program, where the detailed shape of the sub-MeV part is crucial, remains open for future experiments.

Accordingly, the conceptual design of the "next"(probably the ultimate or last) generation of solar neutrino experiments should have the ambition to deliver the following:

1. **Low Threshold:** Almost all neutrinos from the Sun (and hence all the solar physics) are below 1 MeV. The future experiments should be sensitive to the crucial p-p $\nu_\odot$ and therefore should have a threshold of 200 keV or less. The threshold depends on *both* the detector technology and the background control.
2. **Measurement of $\phi(\nu_e)$:** The future experiments should be flavor-specific and give direct measurement of $\nu_\odot$ energy. That is, they should exploit and ν_eN-CC interactions and devise schemes to tag or identify them.
3. **Neutral Current Measurements to the <1 MeV Range:** This will provide model-independent tests on the bulk of $\nu_\odot$. Comparison between measured $\phi(\nu_e)$ and the integrated σ_{NC} can potentially pin-point all the neutrino and solar physics.

Therefore, a next-generation solar neutrino experiment which can meet the specifications above is well-motivated, almost inevitable - a world-class project. The challenges to conceive, formulate and realize such an experiment is enormous yet tempting and rewarding, and deserve the attention of the best and most-qualified groups.

5.4 A Survey of the "Market"

There are many ideas and R&D projects working towards a next-generation solar neutrino experiment [27]. A summary is given in Table 5. A wide spectrum of competing ideas are being pursued: from cryogenic superfluid ^{4}He, to high pressure gaseous ^{4}He TPC, to solid state GaAs, to Gd/Yb/Se-loaded liquid scintillator, to LiI crystal scintillator. It is a dynamic and lively, yet open, field. There is no dominant idea (and no dominant group) that stands out yet, and there are rooms for qualified new-comers to collaborate and to compete with the existing communities.

5.5 Crystal Scintillator for Solar Neutrinos?

5.5.1 Why ?

There are good motivations, elaborated as follows, of exploring the choice of the crystal scintillator technology for the next-generator solar neutrino experiments. Indeed indium borate ($InBO_3$) has been investigated in the past [28] for a $\nu_{\odot}$-detector with ^{115}In as target.

1. **Wider Choice of Isotopes:**

   The ultimate goal of next-generation efforts will be to measure the "Solar Neutrino Spectrum" $\phi(\nu_e)$. It is clear from the discussions in Section 5.1 and Figure 5 that one should detect *and* identify the ν_eN-CC interactions which offer flavor-specific and direct energy information. The choice of detector technology which accommodate a large range of possible nuclei is very limited, the two most prominent ones being loaded liquid scintillator and crystal scintillator.

2. **Intrinsic Properties of Crystal Scintillators:**

   Several properties of crystal scintillators make them attractive candidates for low-background applications. Crystal production is a purification process on its own. The α's are only slightly quenched and possess PSD characteristics, thereby allowing very high sensitivities to the radiopurity level of ^{238}U and ^{232}Th series [5, 18]. They are compact, giving rise to more efficient shielding configurations. Being solid, there are less problems of having radioactive radon gas diffusing into the system, unlike the gaseous or liquid detectors. Radioactivity in cascades can also be localized since the daughter isotopes are trapped within the crystal lattice.

3. **There Exist 50-ton Detector Systems:**

   The target-mass requirement is always a challenge to all neutrino experiments. Unlike the other contenders listed in Table 5, the world does have much experience in producing and operating 50-ton-range crystal calorimeters. The technology is proven to be stable and robust at the harsh accelerator environment.

4. **Coherent with our Pilot Experiment:**

   Everything that we do and learn in the pilot experiment will be relevant and our experience will put us in a favorable position to play a major role in this $\nu_{\odot}$ crystal calorimeter. Therefore, while the pilot experiment is an independently-motivated experiment in its own right, it also servers as a "launch-pad" for this future ambitious project.

5. **Idea not Fully Explored yet:**

   While the use of large water cerenkov and liquid scintillator in neutrino (low-background) physics are well established, the crystal scintillator technique for such applications is much less matured. There are several groups using crystals for relatively modest dark matter searches [19]. There are room for new groups starting afresh, with ample R&D topics and directions to explore. Potential spin-offs in other applications are also conceivable, as discussed in Section 5.5.3.

5.5.2 How ?

A 50-ton low-background crystal calorimeter experiment to be placed underground will inevitably require several years of intense and internationally-coordinated R&D efforts to formulate. We cannot (and arguably should not!) tackle such a big experiment alone.

At present, the Collaboration have identified certain lines of worthwhile R&D directions, on the crystal itself or on its readout device. After these R&D projects are successfully launched, we should actively participate in the world's discussions on the future of solar neutrinos (that is, "making our bid" in the international forum). That will be both a collaboration with and meanwhile a competition against the international communities towards the convergence of a detector technology and the realization of an experiment. All options are open as to the actual location where such an experiment will be performed.

5.5.3 What ?

In order for the crystal scintillator idea to be accepted as a legitimate contender for the next-generation $\nu_\odot$ experiment, two experimental challenges must be dealt with : crystal scintillator and readout device. The scale of a solar neutrino experiment is big enough such that its realization can conceivable drive a new technology, much like the numerous previous examples in high energy physics detectors.

On the crystal front, we are conducting feasibility studies on two candidate crystals - lithium iodide and GSO - relying much the experience and connections already established by the pilot experiment, to further investigate the various scientific issues. The properties of these two crystals, together with the commonly used NaI(Tl), CsI(Tl) and BGO, are listed in Table 3.

I. LiI(Eu) : The idea of using LiI(Eu) crystal scintillator for solar neutrino studies were first proposed by our University of Maryland group in 1994 [29]. Its merits are based on the fact that both ^{7}Li (isotopic abundance (IA)=93%) and ^{127}I (IA=100%) have large $\sigma_{\rm CC}$ with low Q-value thresholds:

$$\begin{array}{llll} \nu_{\rm e} \ + \ ^7{\rm Li} & \rightarrow & {\rm e}^- \ + \ ^7{\rm Be} & ({\rm Q} = 862 \ {\rm keV}) \\ \nu_{\rm e} \ + \ ^{127}{\rm I} & \rightarrow & {\rm e}^- \ + \ ^{127}{\rm Xe} & ({\rm Q} = 789 \ {\rm keV}) \ , \end{array}$$

thile ^{7}Li has the largest $\sigma_{\rm NC}$ among the nuclei:

$$\nu_{\rm x} \ + \ ^7{\rm Li} \ \rightarrow \ \nu_{\rm x} \ + \ ^7{\rm Li}^* \qquad ({\rm E}_\gamma = 478 \ {\rm keV}) \ .$$

The crystal is also very attractive for applications in neutron detection, from the ^{6}Li (IA=7%) content through:

$${\rm n} \ + \ ^6{\rm Li} \ \rightarrow \ \alpha \ + \ ^3{\rm H} \qquad ({\rm Q} = 4.79 \ {\rm MeV}) \ .$$

II. GSO : It was recognized recently [30] that certain double beta decay isotopes may give rise to targets where a *tagged* $\nu_{\rm e}$N-CC signature is possible. Requirements of low Q-value thresholds point to the candidate isotopes ^{176}Yb (IA=12.8%), ^{160}Gd (IA=21.8%) and ^{82}Se (IA=9.4%). As an illustration, in the case of ^{160}Gd [31]:

$$\nu_{\rm e} \ + \ ^{160}{\rm Gd} \ \rightarrow \ {\rm e}^- \ + \ \gamma'{\rm s(prompt)} \ + \ ^{160}{\rm Tb}^* \qquad ({\rm Q} = 169 \ {\rm keV}) \ ,$$

followed by, in the 100 ns time-scale,

$$^{160}\mathrm{Tb}^* \rightarrow {}^{160}\mathrm{Tb} + \gamma \qquad (\tau_{\frac{1}{2}} = 60 \text{ ns} \; ; \; \mathrm{E}_\gamma = 63.7 \text{ keV}) \; .$$

This characteristic time-correlated double signals provide the long-sought-for flavor-specific signatures and hence may lead to a direct measurement of $\phi(\nu_e)$. In addition, the space-and-time correlated signals can reduce the radiopurity requirements by a factor of 10^6, from the typical 10^{-16} g/g level of Borexino to the 10^{-10} g/g for the ^{238}U and ^{232}Th series.

The challenge becomes how to devise a realistic detector from these candidate isotopes. Liquid scintillator loading is now being pursued by the LENS group. Another line of attack can be with the GSO crystal ($Gd_2SiO_5(Ce)$) which was first developed in the 1980's [32]. Various studies have been performed at the prototype [33] and detector [34] levels. From the properties listed in Table 3, it can be seen that besides $\nu_\odot$ detection, GSO may offer an attractive alternative to BGO in high energy physics and medical applications, having almost the same radiation length, but higher light yield, faster decay constant, and excellent radiation resistance.

It should also be noted that both Gd and Yb are "rare earth" elements, where Mainland China supplies 90% of the world market. Should either of these elements blossom into a realistic experiment, our Collaboration will be able to hold key positions in terms of the provisions of raw materials.

In parallel, we are investigating the directions of solid state photo-detectors as readout devices for the crystal scintillator. Solid-state readout offers the advantages of being compact and involving much less (and in principle cleaner) materials than photo-multipliers. Taiwan, having advanced infrastructures and matured experience in silicon-device fabrication, may be ideal to pursue such an R&D program. The belief is that there must be much room for major improvements and innovations beyond what is currently available. There will be a huge market world-wide for solid state photo-detectors which can compete with photo-multipliers. Introductory notes have been written [6] to discuss the various considerations of this direction.

5.6 A Possible Scenario

A possible scenario, by no means unique or final or even optimal, where our Collaboration can proceed may be described as follows:

Phase I:

- $\odot$ Pilot experiment with 500 kg of CsI(Tl) at reactor;
- $\odot$ Initiate R&D programs on crystal and photo-detectors;
- $\odot$ Modest physics on neutrino cross-sections;
- $\odot$ Get program started and Collaboration built;
- $\odot$ Learn the basics.

Phase II:

- $\odot$ A 1 ton detector at underground site;
- $\odot$ Deploying candidate crystal and readout device for $\nu_{\odot}$'s;
- $\odot$ Dark Matter searches and background studies;
- $\odot$ A superb double beta decay experiment on ^{160}Gd for the GSO option;
- $\odot$ The demonstration experiment for final solar neutrino project.

Phase III:

- $\odot$ Solar neutrino experiment;
- $\odot$ Commissioning 50 tons of crystals in stage.

The actual evolution will almost certainly be different from these simple-minded steps. However, they do serve as an "Expression of Interest" on how our Collaboration would like to navigate ourselves as we proceed. It should be noted that every of the "phases" can be considered to be an independent program which can stand on its own, such that, for instance, the value of Phase I, if successfully performed, does not depend exclusively that we will actually evolve to Phase III.

Soon the Collaboration will have to participate in the world-wide discussions on the future of solar neutrinos. Naturally the fine-structures of the future evolution will depend critically on what will *or* will not be happening in the rest of the world.

6 Outlook

Neutrino physics and astrophysics will remain a central topics in experimental particle physics in the coming decade and beyond. There are room for ground-breaking technical innovations - as well as potentials for surprises in the scientific results.

In the lapse of two years, the TEXONO Collaboration has succeeded in conceiving and formulating a manage-able and worthwhile pilot experiment, and getting itself organized and built up above the "critical mass" to be able to perform the experiment on our own.

Looking ahead beyond the horizon, we are interested in the use of crystal scintillator as a next-generation solar neutrino experiment. The technical feasibilities and scientific merits will be studied and explored. R&D efforts are being planned. Our pilot experiment and experience will put us in a favorable position in this program.

The Collaboration invites all interested colleagues to comment/enhance/scrutinize on the directions and strategies of our program - and to join us in their formulation and realization.

A Frequently Asked Questions

A.1 Can we join an experiment abroad instead?

Yes. We could have done so. In fact, quite a few groups have invited us to join their program. Those experiments, however, are already at a matured stage, with the basics almost rigidly-defined after many years of experience and painstaking R&D program. There is not much room for us as a new group without previous credentials to contribute and make an impact at the fundamental level. Another important consideration is that it is not possible to build up a Collaboration above the "critical mass" and with access to a broad spectrum of expertise simply by joining an experiment abroad. We chose the more challenging path: "establish ourselves first with a local but qualified experiment".

A.2 Can we simply pursue straight ahead on the future "big" project and skip the pilot experiment?

No. Skipping the difficult but necessary "foundation phase" would not be appropriate. The experience we acquire from the pilot experiment will be crucial for any future endeavors from technical know-hows to an effective working relationship among collaborators. All these will take a solid and concrete experiment to nurture. The future "big" project alone simply cannot support 50 people working together at its embryonic stage. There are numerous examples to support that R&D program must be incorporated within the framework and infra-structures of an on-going experiment in order to bear fruit. The construction and operation of the pilot experiment will enable us to set up facilities to "try out new ideas" and to "expose students to the sophistications" so that we will be well-prepared to compete and collaborate at the international level.

A.3 What are the potential rewards from the pilot experiment?

Much international attention (and in some cases, respect) on our efforts have already been evident. We have been invited to report on our work in five international meeting in 1997-98, including major occasions like WIN97 and ν98. The feedback to our efforts and plans have been mostly positive and encouraging. The physics output from the pilot experiment, while modest, are nevertheless presentable and publishable - *if* the difficult measurements are indeed achieved. A well-performed pilot experiment can earn our Collaboration what is needed to proceed into the spotlights in the international arena - a solid base at home and reputable credentials abroad.

References

[1] Y. Fukuda et al., Phys. Rev. Lett. **81**, 1562 (1998);
Y. Fukuda et al., Phys. Rev. Lett. **81**, 1158 (1998).

[2] C.Y. Chang, S.C. Lee and H.T. Wong, AS-TEXONO/97-03, in *Procs. of XVI Int. Workshop on Weak Interactions and Neutrinos 1997*, Nucl. Phys. (Procs. Suppl.) **B 66**, 419 (1998).

[3] S.C. Wang et al., AS-TEXONO/98-05, submitted to Nucl. Instrum. Methods (1998).

[4] H.T Wong and J. Li, AS-TEXONO/98-04, LANL e-print hep-ex/9805024, to be published in *Procs. of the Workshop on the Japan Hadron Facilities Science* (1998), also in *Procs. of the XVIII Int. Conf. in Neutrino Physics and Astrophysics* (1998).

[5] C.P. Chen et al., TEXONO Collaboration, Technical Report, AS-TEXONO/98-07 (1998).

[6] H.T. Wong, Technical Notes, AS-TEXONO/98-09 (1998).

[7] C.C. Chang et. al., AS-TEXONO/96-01, Proposal to the National Science Council (1996).

[8] For an overview of current status of reactor neutrino experiments, see, for example, Y. Déclais, in *Procs. of XVI Int. Workshop on Weak Interactions and Neutrinos 1997*, Nucl. Phys. (Procs. Suppl.) **B 66**, 294 (1998).

[9] F. Reines and C.L. Cowan, Phys. Rev. **90**, 492 (1953);
C.L. Cowan et al., Science **124**, 103 (1956).

[10] G. Zacek et al. (Gösgen), Phys. Rev. **D 34**, 2621 (1986) ;
A.I. Afonin et al. (Rovno), Sov. Phys. JETP **67** (2), 213 (1988);
G.S. Vidyakin et al. (Krasnoyarsk), JETP Lett. **59**, 364 (1994);
B. Achkar et al. (Bugey), Nucl. Phys. **B 434**, 503 (1995);
Z.D. Greenwood et al. (Savannah River), Phys. Rev. **D 53**, 6054 (1996);
M. Apollonio et al., (Chooz) Phys. Lett. **B 420**, 397 (1998);
F. Boehm et al. (Palo Verde), Palo Verde Proposal (1993).

[11] F. Suekane (KamLAND), in *Procs. of VI Conf. on Intersection of Particle and Nuclear Phys., Big Sky, Montana (1997)* ; also as preprint Tohoku-NSS-97-01 (1997);

S. Schoenert and L. Oberauer (Borexino), Nucl. Phys. (Proc. Suppl.) **B 70**, 195 (1998).

[12] B. Achkar et al., Bugey Collaboration, Phys. Lett. **B 374**, 243 (1996).

[13] P. Vogel et.al., Phys. Rev. **C 24**, 1543 (1981);
H.V. Klapdor and J. Metzinger, Phys. Rev. Lett. **48**, 127 (1982); Phys. Lett. **B 112**, 22 (1982);
P. Vogel, Phys. Rev. **D 29**, 1918 (1984).

[14] S. Schoenert, F. Suekane, and H.T. Wong, "Future Reactor Neutrino Oscillation Experiments", in *Procs. of the Neutrino Oscillation Workshop, Amsterdam, September 1998*; also as AS-TEXONO/98-06 (1998).

[15] For a recent review on the crystal scintillator detector, see, for example,
M. Ishii and M. Kobayashi, Prog. Crystal Growth and Charact., **23**, 245 (1991), and references therein.

[16] For a recent review on the applications of crystal scintillator in particle physics, see, for example
G. Gratta, H. Newman, and R.Y. Zhu, Ann. Rev. Nucl. Part. Sci. **44**, 453 (1994).

[17] Letter of Intent, Belle Coll., KEK (1993);
Letter of Intent, BaBar Coll., SLAC-443 (1994).

[18] U. Kilgus, R. Kotthaus, and E. Lange, Nucl. Instrum. Methods **A 297**, 425, (1990);
R. Kotthaus, Nucl. Instrum. Methods **A 329**, 433 (1993).

[19] M.L. Sarsa et al., Nucl. Phys. **B 35**, 154 (1994);
N.J.C. Spooner et al., Phys. Lett. **B 321**, 156 (1994);
R. Bernabei et al., Phys. Lett. **B 389**, 757 (1996).

[20] B. Kayser et al., Phys. Rev. **D 20**, 87 (1979);
P.Vogel and J.Engel, Phys. Rev. **D 39**, 3378 (1989).

[21] G.T. Ewan et al. SNO Proposal (1987).

[22] H.C. Lee, Nucl. Phys. **A 294**, 473 (1978) ;
J. Bernabéu et al., Nucl. Phys. **B 378**, 131 (1992);
K. Kubodera and S. Nozawa, Int. J. Mod. Phys. **E 3**, 101 (1994).

[23] For an overview on the subject of neutrino physics, see, for example, "Neutrino Physics", ed. K. Winter, Cambridge University Press (1991); "Physics of Massive Neutrinos", 2nd Edition, Felix Boehm and Petr Vogel, Cambridge University Press (1992).

[24] For the recent work on Standard Solar Model, see, J. N. Bahcall, S. Basu, and M. H. Pinsonneault, Phys. Lett. **B 433**, 1 (1998); J. N. Bahcall et al., Phys. Rev. Lett. **78**, 171 (1997).

[25] For an overview on solar neutrino physics and experiments, see, for example, "Neutrino Astrophysics", J. N. Bahcall, Cambridge University Press (1989).

[26] For an overview of current solar neutrino experiments, see, for example, J.N. Bahcall et al. Physics Today July 1996, 30, and references therein (1996).

[27] For a review on the future solar neutrino projects, see R.E. Lanou Jr., LANL e-print hep-ex/9808033; also in *Procs. of the XVIII Int. Conf. in Neutrino Physics and Astrophysics* (1998), and references therein.

[28] R.S. Raghavan, Phys. Rev. Lett. **37**, 259 (1976); J.P. Chaminade et al., J. Cryst. Growth **99**, 799 (1990).

[29] C.C. Chang, C.Y. Chang, and G. Collins, Nucl. Phys. (Proc. Suppl.) **B 35**, 464 (1994).

[30] R.S. Raghavan, Phys. Rev. Lett. **78**, 3618 (1997).

[31] K.D. Schilling et al., Nucl. Phys. **A 299**, 189 (1978); C.W. Reich, Nucl. Data. Sheets **78**, 547 (1996).

[32] K. Takagi and T. Fukazawa, Appl. Phys. Lett. **42**, 43 (1983).

[33] C.L. Melcher et al., IEEE Trans. Nucl. Sci. **37**, 161 (1990); M. Moszynski et al., Nucl. Instrum. Methods. **A 372**, 51 (1996); N. Tsuchida et al., Nucl. Instrum. Methods. **A 385**, 290 (1997).

[34] S. Nakayama et al., Nucl. Instrum. Methods. **A 404**, 34 (1998); M. Tanaka et al., Nucl. Instrum. Methods. **A 404**, 283 (1998).

The TEXONO Collaboration

C.P. Chen, M.L. Chu, G.C. Jon, G.C. Kiang, W.L. Lai, W.P. Lai, S.C. Lee,
P.K. Teng, C.W. Wang, S.C. Wang, T.F. Wang, H.T.K. Wong
Academia Sinica, Taipei, Taiwan

D.I. Lee, D.S. See, Y.T. Yang, T.R. Yeh
Institute of Nuclear Energy Research, Lungtan, Taiwan

Y.C. Tan
National Chiayi Teachers College, Chiayi, Taiwan

C.L. Kao, H.B. Li,
National Taiwan University, Taipei, Taiwan

J.H. Chao, K.P. Chen, C.Y. Liang, H.M. Liu
National Tsing Hua University, Hsinchu, Taiwan

R.F. Su
Nuclear Power Plant II, Kuosheng, Taiwan

L. Hou, H.Q. Tang, Z.Y. Zhou
Institute of Atomic Energy, Beijing

Y.F. Lai, J. Li, Y. Liu, J.G. Lu, Z.P. Mao, L. Meng, J.F. Qiu,
H.Y. Sheng, H.Y. Wang, P.L. Wang, X.M. Xia, D.X. Zhao,
J.W. Zhao, P.P. Zhao, S.Q. Zhao, B.A. Zhuang
Institute of High Energy Physics, Beijing

T.Y. Chen
Nanjing University, Nanjing

M. He, X.Y. Zhang
Shandong University, Jinan

X.Q. Yu
University of Science and Technology, Hefei

Z.Y. Zhang, Z.S. Zhao
Institute for Radiation Protection, Taiyuan

G.C.C. Chang, C.Y. Chang
University of Maryland, Maryland, U.S.A.

Regional Co-ordinators :

Taiwan - Henry T. Wong, Academia Sinica (htwong@sinica.edu.tw);
Mainland China - Jin Li, Institute of High Energy Physics (lijin@bepc5.ihep.ac.cn);
U.S.A. - George C.C. Chang, University of Maryland (chang@enp.umd.edu).

Table 1: The institutes and collaborators list of the TEXONO Collaboration.

Experiment	Neutron Capture Isotopes	Target Mass	Distance from Core (max.)	Δm^2 (90% CL) (min.)	Status
Gösgen	^{3}He	320 kg	65 m	0.019	completed 1986
Rovno	Gd	190 kg	25 m	0.05	completed 1988
Krasnoyarsk†	^{3}He	458 kg	231 m	0.008	completed 1994
Bugey	^{6}Li	555 kg	95 m	0.01	completed 1995
Savannah River	Gd	250 kg	24 m	0.1	completed 1996
Chooz	Gd	5 ton	1 km	0.9×10^{-3}	completed 1998
Palo Verde	Gd	12 ton	750 m	$10^{-3\ddagger}$	data taking
Borexino	proton	300 ton	1000 km	$2 \times 10^{-6\ddagger}$	start 2001
KamLAND	proton	1000 ton	160 km	$10^{-5\ddagger}$	start 2001

† All experiments except the Krasnoyarsk detector adopted liquid scintillator as a proton-rich active target. The Krasnoyarsk experiment used polyethylene as passive target.
‡ Goals

Table 2: A summary of previous and current reactor neutrino oscillation experiments as well as future projects.

Properties	**CsI(Tl)**	**NaI(Tl)**	**LiI(Eu)**	**BGO** $Bi_4Ge_3O_{12}$	**GSO** $Gd_2SiO_5(Ce)$
Density	4.51	3.67	4.08	7.13	6.7
Relative Light Yield	0.45	1.00	0.35	0.1	0.2
Radiation Length (cm)	1.85	2.59	2.5	1.12	1.38
Emission Peak (nm)	565	410	470	460	440
Decay Time (ns)	1000	230	1400	300	30-60
Refractive Index	1.80	1.85	1.96	2.15	1.85
Radiation Hardness (rad)	10^3	10^3	N/A	10^{4-5}	$> 10^8$
Hygroscopic	slightly	yes	very	no	no

Table 3: Characteristic properties of the crystal scintillators discussed in text.

Experiment	**Detector**	**Channel**	**Mass**	**Threshold**	**Status**
Homestake	Radiochem.	CC on ^{37}Cl	615 tons C_2Cl_4	0.814 MeV	Since 1967
SAGE	Radiochem.	CC on ^{71}Ga	60 tons Ga	0.233 MeV	Since 1990
GALLEX †	Radiochem.	CC on ^{71}Ga	30 tons Ga	0.233 MeV	Since 1992
S.-Kamiokande ‡	Water Cerenkov	ES	50000 tons	6.5 MeV	Start 1996
SNO	D_2O Cerenkov	ES	1000 tons	5 MeV	Start 1999
		CC on ^{2}H		6.5 MeV	
		NC on ^{2}H		2.22 MeV	
Iodine	Radiochem.	CC on ^{127}I	100 tons I	0.662 MeV	Expect 1999
Borexino	Liq. Scin.	ES	300 tons	0.25 MeV	Expect 2001
ICARUS	Liq. Ar-TPC	ES	600 tons	5 MeV	Expect 2001
		CC on ^{40}Ar		4.4 MeV	

† Will evolve to 100 tons of gallium in GNO.

‡ Superceding Kamiokande, a 2200 tons water Cerenkov detector.

Table 4: Summary of the essential features of the current and near-future (being built) solar neutrino experiments.

Project	**Detector**	**Channel**	**Mass**	**Features**
HERON	superfluid ^{4}He	ES	10 tons	Ultra-pure, good E.-reso.
HELLAZ	^{4}He gas at at 77 K+5 atm.	ES+$\theta_\odot$	6 tons	Provide E_ν, exce. spatial reso.
GaAs	GaAs	ES+CC+NC	125 tons	All channels, exec. E.-reso.
LENS	Liq.Scin.+ ^{176}Yb/^{160}Gd/^{82}Se	ES+CC	10 tons	Tagged ν_eCC, direct $\phi(\nu_e)$.
LiI	LiI(Eu) crystal	ES+CC+NC	33 tons	All channels, large σ_{NC}

Table 5: Summary of the essential features of the next generation of solar neutrino experiments. All of them are real-time counter experiments aiming to be sensitive to the p-p neutrinos, that is, with thresholds of $\sim$100 keV.

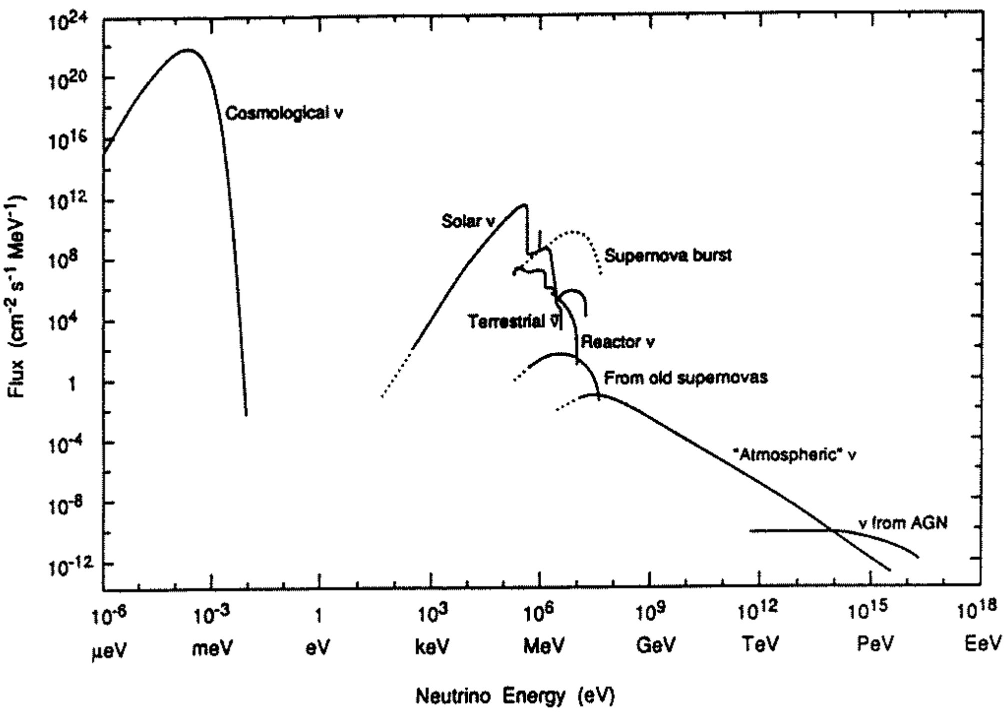


Figure 1: The expected neutrino spectra from various celestial and terrestrial sources.

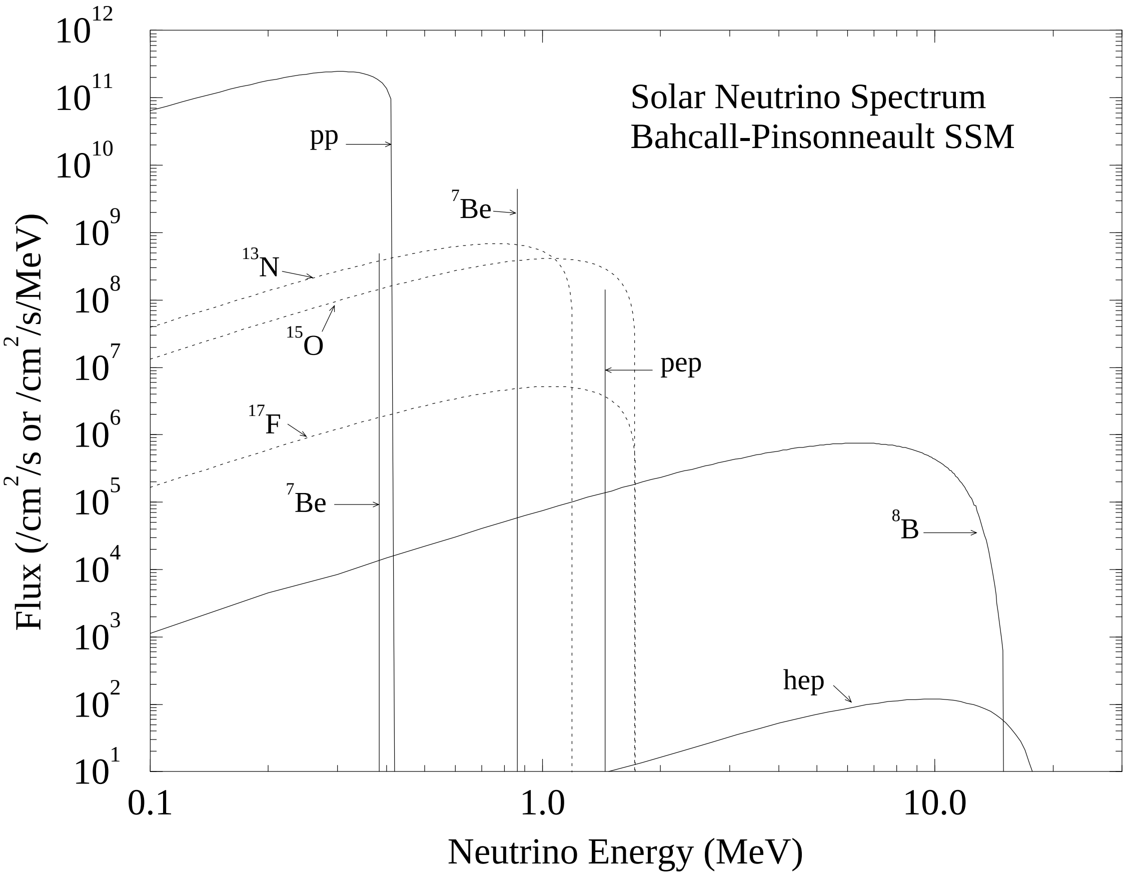


Figure 2: The predicted solar neutrino spectrum based on the Standard Solar Model. The solid and dotted lines indicate contributions from the p-p chains and from the CNO cycle, respectively.

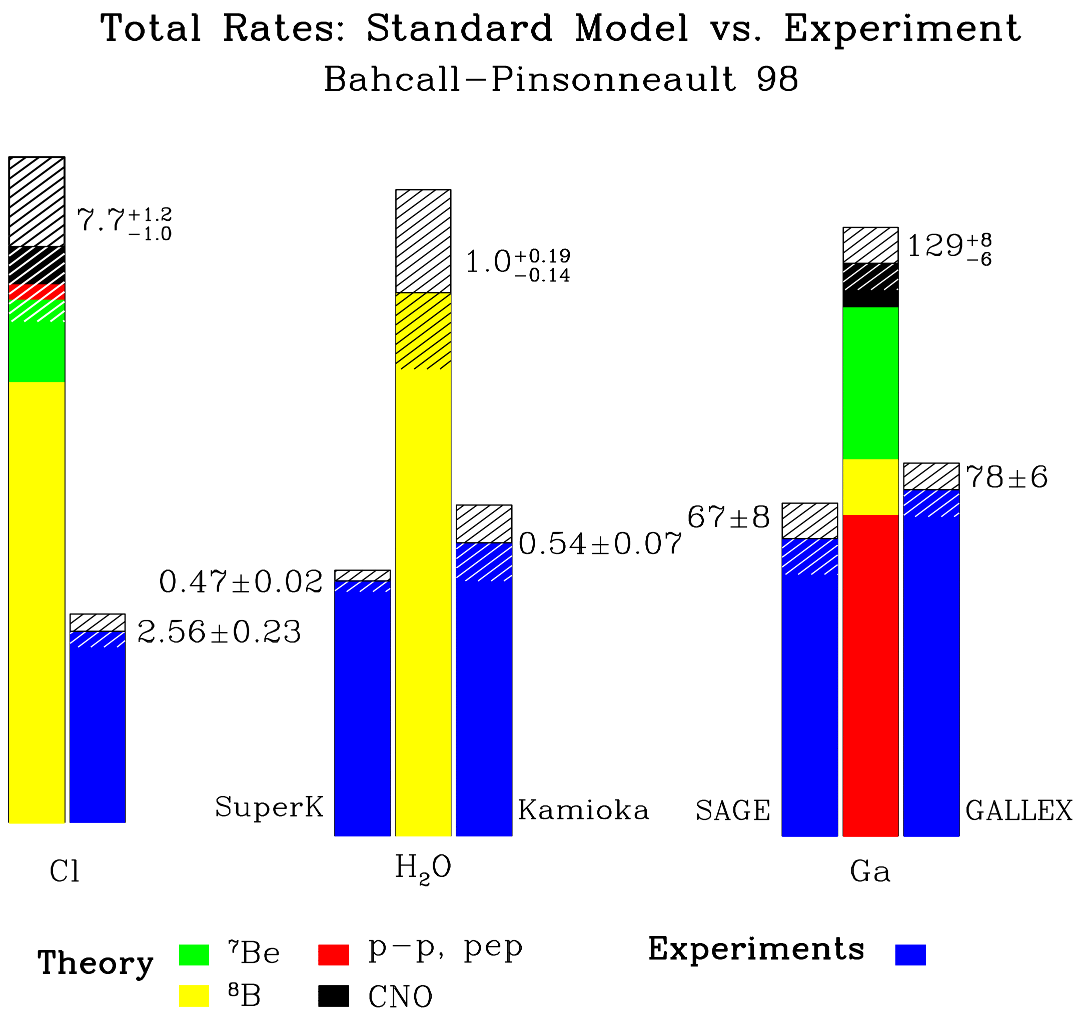
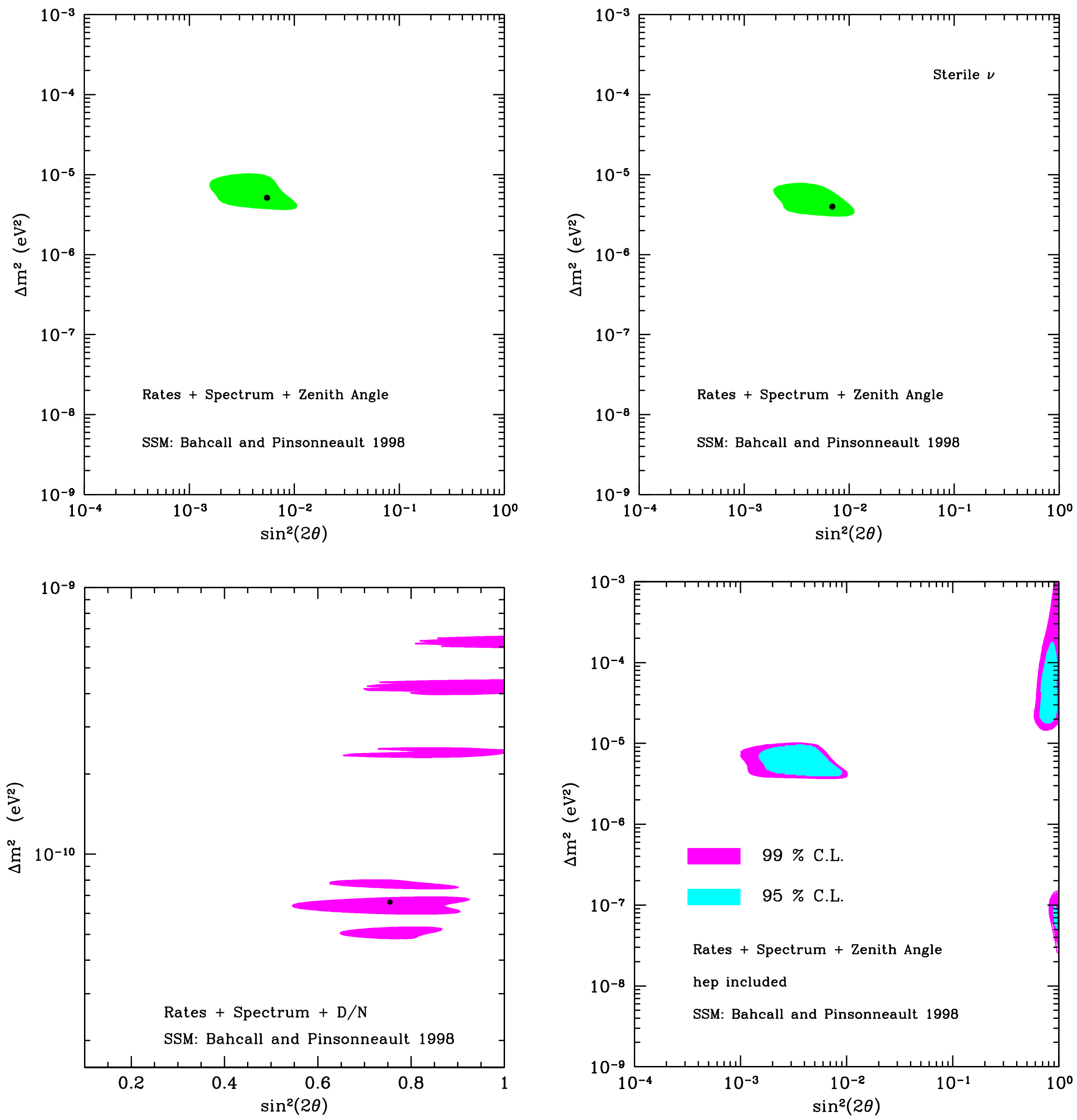
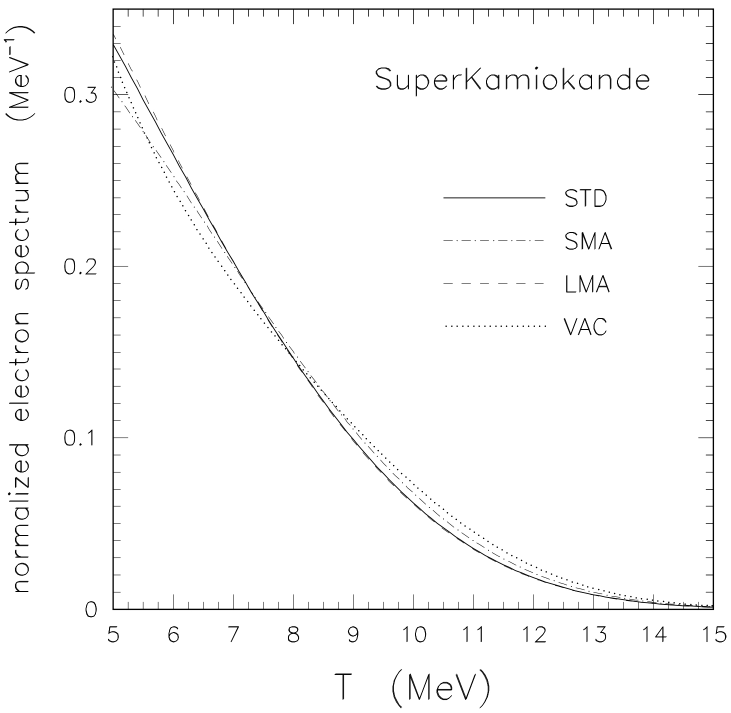


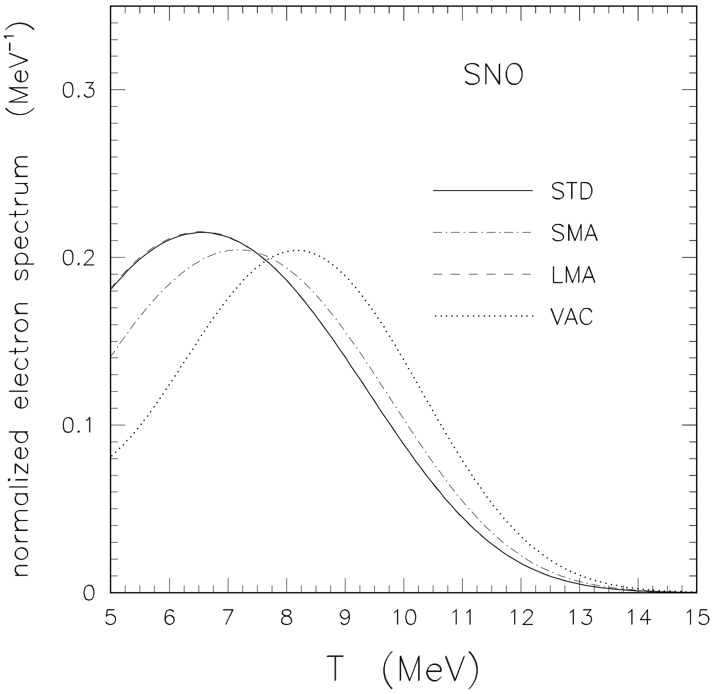
Figure 3: Comparison between predicted solar neutrino flux from the Standard Solar Model with experimental measurements.



Global best fit (99% C.L.) for various oscillation scenarios : **Top Left:** MSW resonant oscillations ; **Top Right:** Sterile neutrinos ; **Bottom Left:** Vacuum oscillations ; **Bottom Right:** MSW resonant oscillations with hep neutrinos flux as a free parameters.

Figure 4: The various solutions to the solar neutrino puzzle.





Measured electron energy spectra for **Top:** Super-Kamiokande (ν-e scatterings), and **Bottom:** SNO (ν_eCC on deuteron), for various oscillation scenarios.

Figure 5: Comparison of ES and CC spectra with different oscillation scenarios.