# PHYSUN-II: Impressions and Conclusive Remarks

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*I'd like to collect my impressions, to offer a (brief, personal) summary of the workshop and some open questions for discussion. Please have a look to the excellent summary of Lisi 2008 for PHYSUN – as I did. With congratulations to my Collegues for a very high scientific level and achieved results; with many thanks to the Organizers for their excellent work and especially for creating a special atmosphere conducive to the discussions.*

## Who studies solar neutrinos?

Astroparticle physicist=an astronomer for particle physicist and a particle physicists for the astrophysicists. Nuclear astrophysicists, same story. Even an experiment like LUNA (discussed by Formicola) is astrophysics for the Italian nuclear physicists who pays for it – thus, only partly recognized.

As was put nicely by J.N. Bahcall:

> ***Everyone is in favor of interdisciplinary projects but no one wants the money to come out of his or her discipline's budget.***

From Neutrino Astrophysics, 1989, page 35.

## but fortunately this attitude is changing...

From the web site `http://www.iau.org/public/careers/`, as part of the answer to the straight question *"What is an astronomer?"* we read the following text:

> *Astronomers have to understand the behaviour of matter in conditions that simply do not exist on Earth, whether at extreme temperatures or involving exotic objects and particles. They must use whatever kind of light, from radio to gamma rays, and particles (from cosmic rays to neutrinos)*

I think the new attitude is due to the successes of solar neutrino discipline and to a recognizion of its importance.

## Why studying the Sun?

First of all, it is a unique opportunity to study energy generation in stars and nuclear fusion processes.

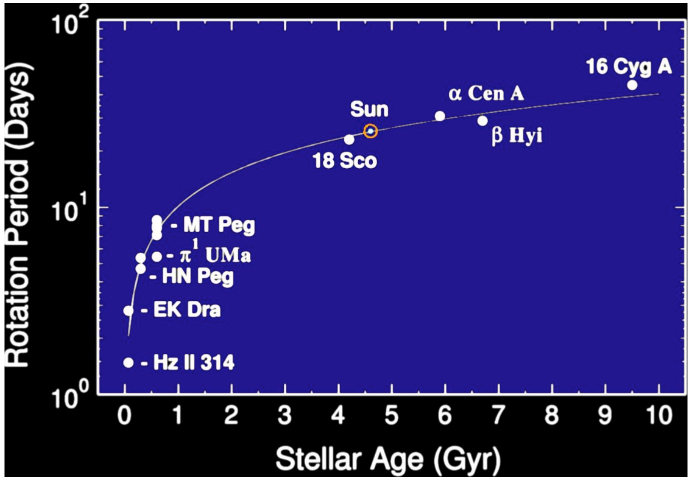
The SSM works pretty well and it is a predictive theory (Serenelli, Villante, Peña-Garay, Palazzo.... any remaining doubts?).

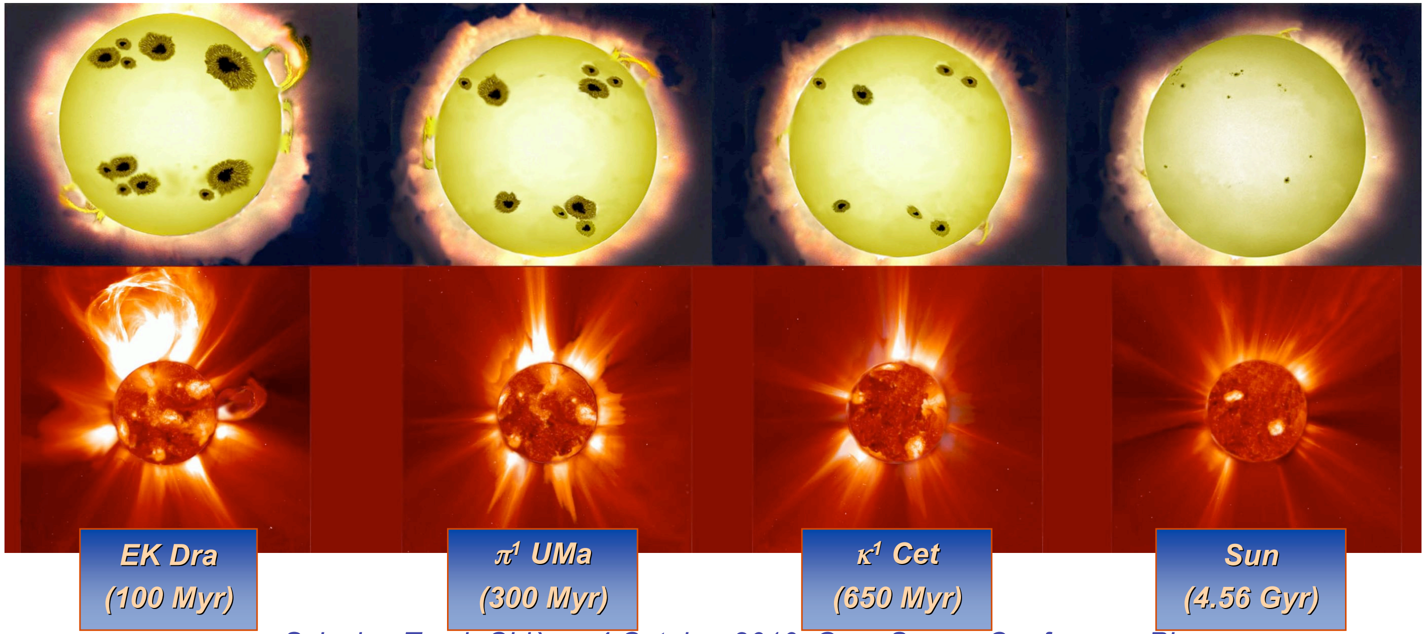
The Sun could be a peculiar star, at least for what regards metallicity (Grevesse, Turck-Chieze, Serenelli)...

...and we could be missing some physics too, espe. of Sun's infancy (Turck-Chieze).

Existing open problems in the understanding of the Sun makes its study more interesting (Grevesse, Chaplin, Turck-Chieze).

The young Sun rotated about 10 times faster than today and had enhanced magnetic activity




Sylvaine Turck-Chièze, 4 Octobre 2010, Gran Sasso, Conference Physun

# What is the nature of the problems we face?

*I liked the statement of CHAPLIN: "maybe the solution will be complex".*

Luminosity, age, radius are known.

Nuclear physics inputs seem adequate; a well-done job (FORMICOLA). Still, $S_{1,7}$ and $S_{1,14}$ down to 5% would be useful (PEÑA-GARAY)

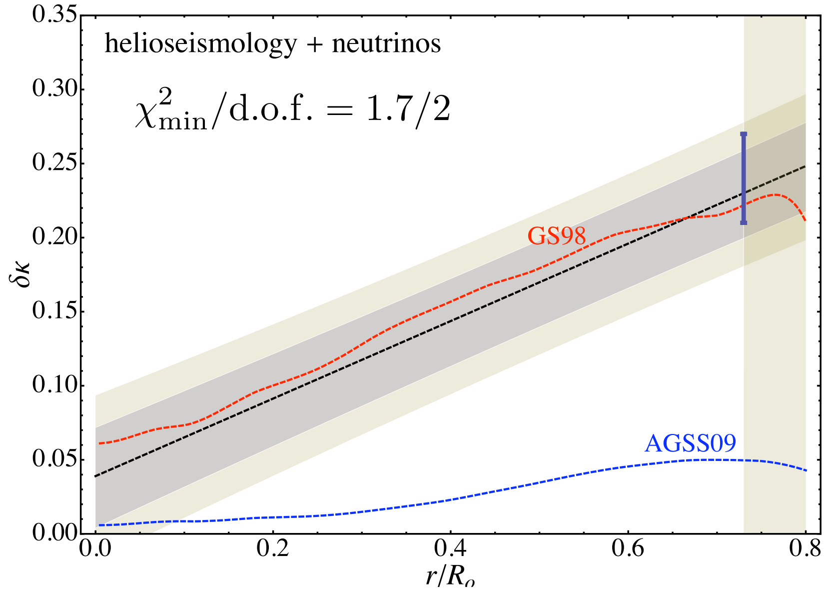
Diffusion is no longer considered a major problem (or at least this is not much discussed this time; see TURCK-CHIEZE).

Metals (espe., C, O and Fe) and opacities are the least stable input, we need to test–or at least to enucleate their role (SERENELLI; PEÑA-GARAY).

*What is the nature of the errors? What are the a priori conservative estimates for these quantities? And even more precisely: how far is the modification of opacity proposed by VILLANTE from what we know?*

# A new game in the town

The linear solar model (VILLANTE) makes it easy to explore the SSM predictions and to identify the criticalities and possible failures.




I feel important to explore its potential, to compare it with other supposedly more complete approaches, to identify its shortcomings if any.

| Reaction | $S(0)$ in keV b<br>Solar Fusion I, 1998 | $S(0)$ in keV ]<br>Solar Fusion II, 2010 |
|---|---|---|
| $\mathrm{p(p, e^+\nu_e)d}$ | $(4.01 \pm 0.02) \times 10^{-22}$ | $(4.01 \pm 0.04) \times 10^{-22}$ |
| $\mathrm{^3He(^3He, 2p)^4He}$ | $(5.4 \pm 0.4) \times 10^3$ | $(5.21 \pm 0.27) \times 10^3$ |
| $\mathrm{^3He}(\alpha, \gamma)\mathrm{^7Be}$ | $(0.53 \pm 0.05)$ | $(0.56 \pm 0.03)$ |
| $\mathrm{^3He(p, e^+\nu_e)^4He}$ | $2.3 \times 10^{-20}$ | $(8.6 \pm 2.6) \times 10^{-20}$ |
| $\mathrm{^7Be}(\mathrm{p}, \gamma)\mathrm{^8B}$ | $0.019^{+0.004}_{-0.002}$ | $(2.08 \pm 0.16) \times 10^{-2}$ |
| $\mathrm{^{14}N}(\mathrm{p}, \gamma)\mathrm{^{15}O}$ | $3.5^{+0.4}_{-1.6}$ | $1.66 \pm 0.12$ |

Table 1: *Previous and updated nuclear S-factors, compiled by* FORMICOLA.

## Which experiment do we really need today?

More/better helioseismological measurements (Chaplin, Turck-Chieze)

More astronomical observations, atomic/plasma physics and opacity studies (Grevesse, Turck-Chieze)

$\nu$ flux measurements (Peña-Garay, Serenelli, Villante, Maltoni...)

*What is their cost and what are their synergies? Are we ready to pair the experimental efforts with the relevant theory and ancillary measurements (e.g., detection cross sections)?*

## Why studying solar neutrinos?

⋆ To monitor the nuclear energy production in a star.

⋆ To get precise measurements of fluxes (Testera, Yamada, Chen).

⋆ To see CNO $\nu$'s; $L_{\rm cno} <$ few $\times L_{\rm cno}(\rm SSM)$ (Maltoni, Peña-Garay...).

⋆ To study other terminations (Testera, Chen).

⋆ To find some MSW: upturn (Chen), $A_{D/N} = -2.3 \pm 1.3\%$ (Yamada) or even to test it (Peña-Garay).

⋆ To search for new physics (Yamada, Frandsen, Palazzo, Peña-Garay...).

⋆ To have multipurpose detectors (Testera, Chen, Fargion...).

*My impression is that this is a mature but still well-fit field.*

## What do we know on $\theta_{13}$?

- From solar neutrino+KamLAND and other data, weak but persistent and well-understood hint (PALAZZO; MALTONI; YAMADA)
- Interesting/important since no strong theoretical reason for $\theta_{13} = \emptyset$.
- If large as the hint suggests (many degrees) we will be measured it in the next few years: T2K, Double-CHOOZ, etc.
- This will open the way to measure the mass hierarchy and the phase of the leptonic CP violation.
- and manifestations of new physics are not impossible (PALAZZO; FRANDSEN; ...)

Rich possibilities for research (for neutrino detectors in part.) and in a timescale of just few years.

## "Conventional" neutrino properties?

The discussion on oscillations is already over? In a paper with Strumia aimed mostly at these issues we asked "which solar $\nu$ experiment after KamLAND and Borexino?"; assuming LMA, favored already 10 years ago, we stated:

***KamLAND [...] will measure $\Delta m^2$ with few per-cent accuracy and even $\theta_{12}$ [...] it will be a real challenge for sub-MeV experiment to improve on these measurements.***

*Generally, or in first approximation, conventional oscillations are not anymore felt as a major issue for solar neutrinos. Do you feel risks in such an attitude?* (Palazzo, Peña-Garay, Chen)

## What is the ultimate low energy $\nu$ detector?

Answer is still open, however, SUPER-KAMIOKANDE Collaboration continues to progress impressively (YAMADA): Low energy threshold now set at 4.5 MeV. Neutron from IBD can be measured.

*Slowing down of the Megaton project? What about Gd doping?*

Ultrapure scintillators (CHEN, TESTERA) and heavy water are strong competitors. I would like to use this occasion to congratulate with my Colleagues from BOREXINO for what they did and want to do.

*Feasibility and interest of pursuing more techniques?* (CHEN)

Major emphasis on DM detectors nowdays at least in EU.

# Outlook

Rereading the book of BAHCALL, published 20 years ago, one realizes how deep and important has been his contribution to solar neutrinos.

And how slow is our progress. However, in these 20 years we made big steps forward and we learned for the first time to see with neutrinos.

Nobel prize of 2002 marked the path of neutrino astronomy and today the main hopes and most discussion concerns high energy neutrinos.

But we should not forget that all we have are solar neutrinos, a handful of events from SN1987A, and now, the first geo-neutrino events.

*Thanks to everybody and see you again!*